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THE

NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

JULY, 1874.

ORIGINAL COMMUNICATIONS.

ARTICLE I. *The Vital Statistics of New Orleans, from* 1769 *to* 1874. By STANFORD E. CHAILLÉ, A.M., M.D., Professor of Physiology and Pathological Anatomy, Medical Department, University of Louisiana.

EXPLANATORY.

In the January No., 1870, of the *N. O. Journal of Medicine*, the writer published Article I. (of pp. 67), on the "Vital Statistics of New Orleans from 1769 to 1869, and more especially for the five years [preceding the war] 1856-1860;" and in the July No., 1870, of the same Journal, Article II. (of pp. 36), on the "Yellow Fever and Vital Statistics of New Orleans during its military occupation, the four years 1862–5," was published. The object of the present article is to complete the subject by this the third and last article on the Vital Statistics of New Orleans, for especially the five years succeeding the war, 1866–70, and thus to fulfil the purpose expressed in the two preceding articles. The postponement of its publication to the present time has been due chiefly to delay in procuring the necessary data from the U. S. Census of 1870, and to the non-existence of any medical journal in this city during the years 1871 and 1872.

While this article presents the statistics of the five years,

1866-'70, it is also, in large part, a resumé of the two preceding ones, and therefore treats of the Vital Statistics of New Orleans from its earliest statistical records (1769) to the present time, but more particularly for the fifteen years, 1856–1870. It is believed that the statistics of no period in the history of this city are likely to prove of such advantage for reference, as of those fifteen years which show the contrast between series of years, which were stamped with such momentous political changes. As one result of the war, the records of these facts were fast disappearing, and one object of these articles was to rescue them from oblivion for the instruction of the future. The labor undertaken is now gladly terminated, and, for the years subsequent to those herein reported, the student of statistics will find, in the Reports of the Board of Health, which, published since 1869, have annually improved in value, all of the necessary facts which are procurable.

This article is divisible into three chief parts; the first part states facts as to the population, the second as to mortality, and the third gives eight statistical tables of figures derived from the official reports. This last part is of course the most important, for it is the source of most of the facts and conclusions presented in the two other parts. Frequent use is made of such figures as 1866-70, for the purpose of designating the five years, therefore in all such cases both years given are included. The fifteen years 1856–70 are frequently referred to as "the thirteen years 1856–70;" in all such cases the two years 1861 and 1862 are to be excluded, for the reason that there are no complete official reports of these years, and that the facts referred to, being incompletely reported for these years, are therefore excluded. Such data as are given as to these two years can be relied on. In order to accord with the United States Census, the term "colored" is often employed as synonymous with what is designated by the Board of Health "Blacks and Mulattos," and by people of education generally negros or Africans.

In concluding this Introductory, particular attention is called to the fact, that if any one practical lesson is specially taught by the study of the Vital Statistics of New Orleans, this lesson is—that no one thing is so essential, for the rectification of the sanitary evils of New Orleans, as its proper drainage from the river to the lake. To secure this, no pecuniary sacrifice would be too great, provided that the necessary funds could be rightly ex-

pended, and not diverted into the repleted pockets of rapacious officials.

POPULATION, &c.

Jean Baptiste de Bienville, Commandant General of the King of France for the *Colony* of Louisiana, founded the city of New Orleans in 1718. Its site was inundated in 1719. In 1723 it became instead of Mobile the capital of Louisiana, and then consisted of about one hundred log cabins. In 1728 it had become an important commercial port. In 1762 Louisiana was ceded by France to Spain, but the Spanish Governor, O'Reilly, did not take possession until August 18th, 1769. In this year a census of the population was taken for the first time: it numbered 3190; of these, 1225 were slaves, and there were 468 houses. In 1775, as also in 1785, the population was augmented by Acadians (refugees of French descent) from British America, and in 1791 by refugees from Jamaica. In 1797 the population was 8056. Spain, in 1800, ceded Louisiana back to France, which did not take possession until Nov. 30th, 1803, and retained it only twenty days; for on April 30th, 1803, France had ceded Louisiana to the United States, which took possession on Dec. 20th, 1803. March, 1804, the United States divided the Province of Louisiana into two territories, and designated the portion now known as Louisiana the "Territory of Orleans," which was admitted into the Union as the State of Louisiana on April 8th, 1812. In this year (January 10th) the first steamboat arrived at New Orleans, and the second war with Great Britain began, ending January 8th, 1815, with the Battle of New Orleans.

Incorporated as a city in 1805, the population in 1810 was 17,242, which in 1850 had increased to 116,375. In 1852, the Fourth District (formerly known as Lafayette), with a population in 1850 of 14,190, was annexed to New Orleans. This population of 130,565 in 1850 had increased in 1860 to 168,675, and in 1870 to *only* 173,763. In 1870 the Fifth District (know as Algiers, or "Orleans Parish, right bank"), with a population of 6819, and the Sixth District (known as Jefferson City), with a population of 10,836, were annexed to New Orleans, and increased the 173,763 of the old city to the 191,418 given in the United States Census. Act 71, March 23d, 1874, added a Seventh District by annexing Carrollton, with its population in 1870 of 6495. Thus the New

Orleans of 1874 had in 1870 a total population of 197,913, which now probably amounts to from 210,000 to 215,000.

These various annexations have been sources of some statistical confusion. To avoid this, it must be remembered that New Orleans, as it was bounded from 1852 to 1870, contained 168,675 population in 1860, and only 173,763 in 1870; but that, for the mortality statistics, Jefferson City, with its population of 10,836, is considered as having been a part of New Orleans. There is thus given, as a basis for the pro ratâ estimates of the population from 1860 to 1870 a total in 1860 of 168,675, and in 1870 of 184,599. These are the only figures which can be properly used in calculations of the death-rate, and if not remembered, the estimates of population, as given in the Tables, &c., will give rise to useless discussion.

As to the estimate of the civil population during the four war-years, 1862–5, the following facts deserve consideration. Jan. 26th, 1861, Louisiana seceded from the United States, April 25th, 1862, it was captured by the United States Navy, and on May 1st, 1862, it was taken possession of by the United States Army. Between these dates the population was much diminished by thousands of citizens who became Confederate soldiers, and by thousands of refugees. From April 9th to May 26th, 1865, all of the Confederates surrendered, and New Orleans was repleted to a greater extent than ever before by its returning citizens, and by the freedmen who then flocked to it in large numbers. Thus it is certain that in 1862 its civil population was at its minimum, and in 1865 at its maximum. The general opinion of intelligent citizens who remained in New Orleans is that the population was much diminished from May, 1862, to May. 1865. Dr. Harris, in July, 1865, estimated that *then* "the total population, including the permanent or the transient military forces, was little less than 200,000." Dr. J. J. Woodward, U. S. A. Surgeon General's Office, writes: "As to the civil population of New Orleans during the period referred to, I have myself no doubt at all that the army of camp followers, sutlers, traders, etc., far exceeded the number of fugitives, and should not be surprised if, in fact, the civil population were shown to be really larger than before the war, but I know of no reliable reports bearing on the case." Table No. 5 demonstrates a fact inconsistent with Dr. Woodward's opinion, for it shows that the most marked diminution in the deaths, during this time, occurred among those between the ages

of twenty and forty, and therefore in that class especially which had abandoned the city, and which would have been replaced by those from twenty to forty years of age, if replaced by Dr. Woodward's "army of camp followers, sutlers, traders, etc." For these reasons, it is confidently believed that the estimates of the civil population during the years 1862–3–4–5, as given in Table No. 1, &c., are over rather than under estimates, and that they are approximations to the true figures, as favorable to the death-rate as the facts and official reports permit.

These approximative estimates, even if they largely underrate the true population, serve the interésting purpose of correcting a misrepresentation, which, originating in 1862, has gained such currency that it has become a part of the popular history of the war, as written north of the Potomac. It is asserted with great confidence, that the United States military authorities in New Orleans did, by an efficient sanitary police, etc., not only protect it from yellow fever (an assertion which the facts fail to prove, as will be shown farther on), but also greatly improved its general health and notably diminished its death-rate. Now, the only official reports of the mortality of 1862–5 were furnished me by the courtesy of Surgeon General Barnes and Dr. J. J. Woodward, U. S. A., were first published in my article of July, 1870, and are now republished in the various tables of this article. These, the only official figures, prove beyond question that the death-rate of the civil population of New Orleans was, during its military occupation, absolutely increased rather than diminished, and this to a notable extent, if these non-epidemic war years be compared, as in fairness should be done, with those non-epidemic years which immediately preceded and succeeded the war. The correctness of these conclusions is established by additional facts, which fortunately prohibit the usual quibbles (so popular when death-rates are discussed) about the estimates of population. For no one can claim that during the military occupation of New Orleans, its population of children under 10 years, of men over 70 years, and of females was increased, and yet there was an increased mortality of these three classes of the population. Staid truth from the official figures has in this case, as in so many others, a tedious chase to catch the flying falsehood of vain-glorious opinion.

In New Orleans, as in other cities, there has always been dissatisfaction with the census, and if popular opinion is to be ac-

cepted, then the population was very much under-estimated in 1870, as also always before; and therefore the death-rate necessarily becomes greatly overrated. Of course, no one pretends that the official figures are absolutely correct, nor denies that the actual population may be greater than enumerated. But for the calculation of the death-rate and all other practical questions, it is of little consequence if the enumeration be not correct, even if it furnishes an under-estimate; for the question of preëminent importance is, whether the enumeration for New Orleans is as correct as for other cities of the United States; or, in other words, whether the census of New Orleans is *comparatively correct.*

The strongest argument against the United States Census is that it has always been taken during the summer, when its population was at its minimum. This argument has not the strength popularly assigned it: for, in the first place, the census of all other cities is also taken in the summer, and therefore is *comparatively* correct for New Orleans, except in so far as it may have a larger number of summer absentees than other cities; and in the second place, the apparent reasonableness of this objection is invalidated by the method of taking the census. By the law, "Assistant Marshals duly qualified" are required to certify on oath to the following, among other things; that they have visited every house, and have enumerated the name of "every person whose *usual* place of abode," "including the names of those *temporarily absent,*" was in said house, or family; and in addition, every census taker is allowed "as compensation for his services, after the rate of two cents for each person enumerated." These facts fail entirely to justify the assertions that a summer census is necessarily an under-estimate, and that the census of New Orleans is *comparatively* incorrect.

The invectives of this city against the census were as violent as to preceding enumerations as to that of 1870. To illustrate how little reason New Orleans has had to complain of the census officials of the United States, and of a summer census, the following facts are cited. During 1847–1859 the city officials took the census three times, and the State officials once. Three of these four enumerations gave a much less numerous population than the United States Census. The fourth, in 1852, reported 8000 or less than six per cent. more than is yielded by a pro ratâ estimate of the two United States Census of 1850 and 1860; and since

the latter was taken after the four destructive epidemics of 1853–4–5–8, it becomes evident that even the city census of 1852 cannot be claimed as having given any larger population than was indicated by the United States Census. As to the objections to a summer census, the city enumeration of 1847 was in March, and gave 94,526, whilst the United States census indicated 112,000; and the city in 1859 enumerated itself in February 138,277, whilst the United States Census indicated 164,400.

The newspapers have repeatedly urged that the United States Census certainly underrated the population, for such reasons as that an under-estimate was proved by the number of its adult male population, of its voters, of its school children, of its houses, of its superficial extent, of the number of names in its annual Directories, &c. Every one of these reasons has been thoroughly examined, so that I assert without hesitation, that any intelligent man can readily convince himself that they are all unfounded, and that the citizens of New Orleans have thus far failed to advance a single valid proof that the population has not been enumerated by the United States Census with *comparative correctness.* Even if this be disbelieved, it remains true that the student *must* resort to the United States Census, for there alone will he find the data which are necessary, and which are consistent with each other and with the well established laws of population. This last jewel of consistency will be sought for in vain in the ipse dixit estimates proclaimed in the newspapers, and at the street corners. The tendency to exaggerate self and its surroundings is natural, but weak; and no intelligent citizen should encourage an over-estimate of population, for this produces two evils—it encourages the present gross abuse of illegal voting, and it causes an under-estimate of the death-rate, and therefore ill appreciation of the true sanitary condition. Recognition of this must precede its correction.

All will admit that great changes in the population must have been caused by the war which robbed New Orleans of a large number of its white males from 20–40 years of age; and by two results of the war, viz., by the comparative cessation of foreign immigration, which diminished especially this same class of the population, and by the greatly increased immigration of Africans. That these well known results are fully shown by a comparison of the census of 1870 with that of 1860, constitutes a strong confirmation of the correctness of the census. Notwithstanding the fact that

in 1870 the population of New Orleans was 22,743 more than in 1860, yet in 1860 there were 11,100 more white males from 20–40 years of age than in 1870. The total white population in 1870 was absolutely less by 3678 than in 1860, and this deficiency was supplied, and the sole addition gained has been, by the great increase of the African race. Of this population New Orleans had 24,074 in 1860, and 50,495 in 1870. In the doubling of this population from 1860 to 1870 by the United States Census is found another striking confirmation of its correctness; for the mortality statistics derived from a totally different source (the city sextons), report that the deaths of this population during 1866–70 doubled the deaths during 1856–60. Those who discredit the census must acknowledge that this is, to say the least, an extraordinary coincidence. Cities seem naturally to attract an excess of African females, and in 1860 there was in New Orleans an excess of this class. The reduction of the white male population would also be naturally attended with a comparative increase of white females. For the various reasons now given, it is not singular that the census, which reported that New Orleans in 1860 had 1537 more males than females, should report in 1870 that it had 10,860 more females than males. Of this 10,860 female excess, 8,065 were from 15–30 years of age; 3663 were white, and 7797 were colored females; and of these 7797 there were 5050 from 15–40 years of age. An estimate based upon the total population and the total deaths under 1 year of age in 1870 yields for the number of births about 6350, and a similar estimate gives 4700 as the number of births in 1860. This great increase in the number of births confirms the census report of a much increased female population of child-bearing age.

Thus the Census shows that, comparing 1870 with 1860 New Orleans slightly lost in its white and doubled its "colored" population; lost most largely in white males from 20-40 years of age, and gained largely in females from 15–40 years of age, and especially in "colored" females.

Mortality.

Table No. 4 proves conclusively that, comparing the five years 1866–70 with the five years 1856–60, there has been a decided diminution of the death-rate not only for the deaths from all causes, but also for the non-epidemic deaths. It is believed that this improvement is in part only apparent, and really due to the

diminution of the unacclimated population of foreign birth, and to the excess of the female population, for the death-rate of females has always been less than that of males. Some real improvement is no doubt due to the facts that, the area between the rear of the city and Lake Pontchartrain has been better drained and cultivated; and greater attention has been paid to sanitary matters. To this latter cause, however, no very beneficial effects can be reasonably attributed so long as our privies, gutters and streets, maintain their unenviable condition, and thus continue to poison the most important of all foods, the air.

Mortality by Sexes and Nativities.

Table No. 5 presents all of the data procurable for calculations of the death-rate of these classes. The general difference in non-epidemic years between the death-rate of males and females is illustrated by the death-rates for the two years 1860 and 1870. In 1860 the death-rate for the total population was 43.5, for the males 52.8, for the females 34.1 per 1000; and for 1870 the figures for these three death-rates were respectively, 38.6, 49.4, and 28. In these corresponding results of the census and of the sextons is found another confirmation of the comparative correctness of the census. If yellow fever epidemic years be taken, the difference is still greater between the male and female death-rates. In non-epidemic years the death-rate of the foreign born is somewhat greater than that of the "natives of the United States," but in yellow fever epidemic years the foreign born death-rate is very much greater. These facts prove that the least mortality occurs in native born females.

Mortality by Races.

The mortality of the negro has always exceeded that of the white population, except during yellow fever epidemic years; for this disease attacks the whites more especially. This greater mortality existed in New Orleans and other cities—certainly in Charleston, Washington, Baltimore and New York—before the war. Comparing the five years of freedom 1866–70 with the five years of slavery 1856–60, it will be found that the death-rate remains about the same; but if the comparison be made, for these two periods of time, between the colored and white deaths, then it will be found that the colored death-rate has relatively in-

creased very much. For instance, during the four years 1856–60 (for 1858, a yellow fever epidemic year, is excluded), the colored death-rate was about 44 and the white 39 per thousand; while during the four years 1866–70 (for 1867, a yellow fever epidemic year, is excluded), the colored death-rate was 43 and the white only 30 per thousand. To what causes is this greater mortality of the colored due? The official reports furnish such scanty data on this subject that they are extremely unsatisfactory. However, some of the causes are certainly—the greater ignorance and improvidence of this race, and the greater mortality by Small Pox, Choleraic Diseases, Consumption, Trismus Nascentium, Still-births, and of children under two years of age. The future of this race is involved in the question, whether it is naturally increasing. The reports of the Board of Health for the two years 1872 and 1873 are the only two which throw some light on this question, for these alone report the number of deaths respectively of the white and of the colored children under two years of age. Accepting, for 1872 and 1873, the population in 1870 of these classes, the result yielded is that in 1872 there were 154 deaths in every 1000 of the white children under two years of age, and 298 of the colored children; and in 1873 these same data were 181 of the white and 335 of the colored. Thus it is manifest that, here in New Orleans, the mortality of the colored children under two years of age is enormous, when compared with the mortality of the white children.

Mortality by Ages.

Table No. 5 furnishes all of the data procurable as to the distribution of the population and of the deaths by ages. From these can be readily constructed the numerous and important tables of ratios which are ordinarily compiled for the purpose of comparison.

If the five years 1866–70 be compared with the five years 1856–60, the former show a marked diminution in the mortality of the sum total of the children of both races, and therefore this diminution indicates a very great improvement as to the white children; for, as has been shown, the colored population has more than doubled, and the mortality of its children under two years of age is comparatively enormous.

The estimated births in 1860 are 4700, and the similarly esti-

mated births in 1870 are 6350. In 1860 there were 49,300 females from 15–55 years of age, and in 1870, 60,465. These figures indicate that in 1860 there was one birth to every 36 of the total population, and 95 births to every 1000 females from 15 to 55 years of age; while in 1870 these numbers were respectively 30 and 105. This increase in 1870 of the births to the total population was due to the greatly increased population of females; and the increase of births to the number of females from 15–55 years of age was due to the unusually large number, in 1870, of females from 18–40 years of age. That these different facts are consistent with the Census furnishes additional confirmation of its correctness.

Hygienists concur in regarding the mortality of infants under one year of age, as one of the best tests of the sanitary condition of a place. In 1860 there were about 280 deaths of children under one year of age to every 1000 children born *alive*, and in 1870 this number was reduced to 190. This indicates not only a great improvement, but also a comparatively very favorable sanitary condition.

Another hygienic test is derived from the average duration of life, and one of the factors in this problem, the relative number of centenarians, has been seized upon here in New Orleans to prove its healthfulness. In the article of January 1870 the statistics of longevity were thoroughly examined as to the Census both of 1850, and 1860. These proved conclusively, 1st, that whenever there was an excess of centenarians, there ought certainly to be found a corresponding excess of those from 90 to 100 years of age; and that this was not the case as to New Orleans; 2d, that any comparative superiority of New Orleans was to be found (in the Census) exclusively in its colored population; and 3d, that examination of the Census would prove that throughout the United States the *reported* number of centenarians was excessive in proportion to the ignorance of the population, being greatest among the Indians, who are the most ignorant; and that everywhere the more ignorant colored centenarians exceeded the white, and the more ignorant female the male population. If the Census of 1870 be compared with those of 1850 and 1860, there will be found a confirmation of these views to such extent that further discussion is deemed useless. The census-takers record the age as given by the person questioned; and since every one familiarized by personal experience with the negro race, knows well its ignorance of

dates, and its love for the remarkable, it is my conviction that the Census is entirely unreliable in its records of the remarkable longevity of an ignorant, and wonder-loving population. All other data indicate, that here in New Orleans, the duration of life is much less, and the mortality of the negro race much greater than of the whites; and yet the Census of 1870 reports that the 140,923 white population had only 35 persons from 90 to 100 years of age, and 7 centerarians, whilst the 50,495 colored population had 81 persons from 90 to 100 years of age, and 32 centenarians, and that 50 of the 81, and 21 of the 32 were colored *females*. It may be, that in the United States, Indians enjoy the greatest longevity, that negros excel the whites, and females the males, but, in my opinion the Census indicates too much of this, and therefore superiorities which are incredible, since they are inconsistent with all other facts.

Mortality by Months and Seasons.

For the detailed facts the student is referred to Table No. 7, and is warned that the reports of the months, as given, are really of weeks, and therefore that some months embrace four, and others five weeks.

If the year be divided, so that one half includes the six months November–April, and the other half the six months May-October, the former will include those months during which the population is at its maximum, and during which the least mortality occurs, except as to the months of May and November, for generally the deaths in November somewhat exceed those in May. Now, if the more populous and healthy half-year, be compared with the less populous and more sickly half-year, the result is that, while the total deaths during the thirteen years 1856–70 were 96,538, there were 39,732 of these in the six months November–April, and 56,806 in the six months May–October. If the three most healthy be compared with the three least healthy months, the result is that there were during said 13 years, 18,002 deaths in the months of January, Febuary and March, and 30,947 in the months of July, August and September. This mournful contrast is largely due to epidemics, but if all the deaths by yellow fever, cholera and small-pox be deducted, even then the sad result is that there were still 25,318 deaths during the three most sickly months to contrast with 16,997 deaths during the three most healthy months.

These facts constitute the most forcible of all arguments for drainage.

Mortality of Special Diseases.

YELLOW FEVER.

From 1796, when the first epidemic of yellow fever occurred, to 1859, a period of sixty-three years, thirty-four epidemics ravaged New Orleans. The records indicate that some cases occurred in every one of the remaining twenty-nine non-epidemic years. No part of these sixty-three years can be compared in fatality with the six years 1853–1858, during which occurred four violent epidemics, 1853–4–5–8; and of these "*the great epidemic*" of 1853 well deserved its title, whilst the epidemic of 1858 caused a mortality never surpassed, except by its notorious predecessor of 1853. During the fifteen years 1859–73, New Orleans has enjoyed an exemption, unprecedented in its history, from yellow fever epidemics; for notwithstanding the fact that there have been cases every one of these years (except perhaps 1861), there has been but one epidemic—that of 1867.

The first cases have been repeatedly traced to the shipping; and in many other years there has been as absolute proof, as a negative proposition admits of, that there was no such connection, and a conspicuous absence of any facts justifying the assignment of the origin of the disease to importation. The preceding indisputable facts are frequently ignored by those who advocate the protective virtues of Quarantine, of an efficient sanitary police, and of carbolic acid and other disinfectants. Some of the facts relative to each one of these three prophylactics will be stated.

Quarantine.—This is based on the theory that yellow fever is imported into New Orleans, that it is communicable from person to person, and therefore that by excluding every case of the disease the city can be protected from its ravages. What are the facts?

The first Quarantine established was maintained only four years, 1821–4, having been abandoned early in 1825 from the general conviction that it had proved worse than useless, for yellow fever was present every year, and to the extent of a very violent epidemic in 1822, and an epidemic in 1824. After thirty years

discontinuance, the Quarantine, which is still enforced, was re-established in March, 1855. Very violent epidemics occurred in 1855, 1858, and 1867, and (excepting 1861) there have been deaths by yellow fever every year of the existence of the present Quarantine.

Thus including the whole time, seventy-eight years, 1796–1873, there have been twenty-three years with Quarantine. During these there have been deaths of yellow fever every year (1861 excepted), and there have been five epidemics of which four (1822–55–58–67) were very violent.

These facts render it manifest, that after twenty-three years' trial, Quarantine has annually failed in its sole object—to keep all cases of yellow fever out of the city. During the military occupation of New Orleans the experiment was tried, whether this failure was attributable to defects of the law, or of its execution; for "by the exercise of absolute and relentless military authority, an *impregnable* system of Quarantine was maintained," and notwithstanding its remorseless rigidity there were cases of yellow fever among the civil population every year. Worse even than this for the advocates of Quarantine, for in 1863 and 1864 it prevailed especially on board the vessels of the United States. Now as to these cases, it is asserted that "the official usages and the armed discipline of the naval fleet in the harbor of New Orleans and upon the river enabled the medical officers to trace to its source every case of yellow fever," that when the disease appeared on one vessel, all other vessels were prevented "by armed surveillance and discipline" from communicating with the infected vessel, and that in spite of all this, the disease could not be traced to importation. In fact there was in 1863 but one vessel (the Spanish man-of-war Pizarro) which even approached the port of New Orleans with yellow fever, and this vessel was kept at the Quarantine, sixty-five miles below the city, thus rendering communication with the "river fleet" impossible; and in 1864, when twenty-five vessels, iron-clad gunboats, &c., in the river and the lake, were attacked with yellow fever, not one infected vessel from a foreign port arrived, even at the Quarantine Stations.

Avoiding discussion of the communicability of yellow fever, and therefore of the theoretical value of Quarantine, the conclusion from the facts given can not be escaped—that Quarantine has entirely failed, even under circumstances very exceptionally favorable to it, to prevent the occurrence of yellow fever, and

has had no practical value in protecting this city. That New Orleans has the capacity to originate yellow fever just as well as Havana or Rio Janeiro, is an assertion which I deem indisputable, and which was often made by the deceased Prof. Stone, whose ability as also his experience in this disease were unequalled in this city. The experience of the United States "river fleet" at New Orleans in 1863 and 1864 confirms many other indications that yellow fever is especially prone to originate in the *holds* of vessels.

An Efficient Sanitary Police.—The topography of New Orleans is such as to render its proper drainage very difficult and expensive; its warm, moist climate, is most favorable to vegetation and to putrefaction; and its houses are constructed without regard to hygiene. Therefore it is not singular that from the earliest records to the present day this city should have been characterized by ill-drained, overflowed and filthy streets, gutters and privies. Purity of atmosphere is impossible under these conditions. Until the last few years, there has not been any sanitary police at all, and no one can claim that it is now or has been at any time efficient, except during the time of the military occupation of the city. From 1862 to 1865 it is asserted that it enjoyed "a sanitary police so efficient;" and "sanitary regulations so excellent," that "so clean a city had never before been seen upon the continent." The exemption of New Orleans from epidemics of yellow fever during this time has been so generally and confidently attributed to this cause, that it has gained a place in our medical text-books. This conclusion is certainly hasty and unjustifiable. For, if this exemption in 1862-3-4 was due to this very efficient sanitary police, then to what cause was due the exemption in 1859–60–61, as also in 1865–6–8–9–70–71–72–3, during which eleven years, there has been either no sanitary police, or one notoriously very inefficient? Plainly, these three years immediately preceding, and these eight years succeeding the three years 1862–3–4 invalidate the above hasty conclusion, which cannot be accepted until it has been conclusively proved (as is very far from having been done) that no clean city has ever been attacked by a yellow fever epidemic.

Disinfectants.—A similarly hasty and unjustifiable conclusion, based on the use of carbolic acid during the last two or three years only, has been maintained by some, who apparently forget

the many preceding non-epidemic years before the use of disinfectants. An efficient sanitary police and the proper use of disinfectants deserve the earnest support of every enlightened citizen, and therefore, such hasty conclusions and illogical pretensions in their behalf, as endanger their being brought into popular disrepute, are to be deprecated.

Many seem disposed to forget that, in the history of yellow fever, it has repeatedly abandoned cities which it had afflicted as severely as New Orleans; and that its mysterious departure from these cities was very certainly not due either to Quarantine, or to an efficient sanitary police, or to disinfectants. No advocate of the protective powers of either of these can possibly prove that the exceptional exemption of New Orleans since 1858 is not due to causes similar and as yet inexplicable; and hence true science is forced to acknowledge its ignorance, and to reject all unproved explanations. I can recal but one condition common to New Orleans and to other cities which yellow fever has ceased to ravage, viz., the better drainage, cultivation, and general improvement of the suburbs. It is certain that no explanation can be found in the meteorological records.

In concluding with yellow fever, attention is called to the following three facts: (1) Even contagionists admit that here in New Orleans it certainly fails, as a general rule with few exceptions, to manifest the *catching* characteristics of those diseases which are universally admitted to be contagious or infectious. (2)During the past thirty years, and for all *recorded* years, an epidemic has never prevailed when the first case occurred later than June; except perhaps for the year 1822, as to which the date of the first case is uncertain, but it was certainly "as early as the beginning or middle of July." First cases have occurred in June without being followed by epidemics. (3) In view of the discussions about acclimation, and the liability of children to *undiagnosed* yellow fever, it is worthy of attention that in 1858, 1867, 1870 and 1873, the mortality of children under 10 years of age was notably increased during the months when yellow fever prevailed, and especially as to children over 2 years of age.

The statistics of yellow fever will be found, as complete as the records permit, partly in Table No. 1, but especially n Table No. 2.

CHOLERA.

The statistical facts are, for the limits of this article, sufficiently given in Table No. 3.

SMALL POX.

The annual deaths by this disease since 1856 will be found in Table No. 4. The greatly augmented mortality since 1864 is notable, and due to the great increase at that date of our colored population, which ignorantly neglects, even refuses the protection of vaccination. I have not been able to secure the deaths by races except for the years 1870 and 1873; these are significative, no doubt, of the general facts in all other years.

	Total deaths by Small Pox.	Whites.	Colored.
1870	528	110	418
1873	505	107	398
	1033	217	816

An estimate from these figures proves that, if equal numbers of the white and colored population be compared, there are ten and a half times more colored than white deaths by small pox.

STILL BIRTHS.

For the 5 years 1856–60 there were about 80 premature and still-births to every 1000 births, and for the 5 years 1866–70 about 85. This increase was probably due to the facts, that the population of colored child-bearing females had very much increased, and that the number of colored still-births is, relative to the population of the two races, much larger than those of the whites. Table No. 6 proves that the ratio of still-births to births is, in the Charity Hospital, 103 to every 1000, and therefore considerably worse than in the balance of the city.

Still-births are excluded from the mortality statistics of many places, and therefore their addition to the deaths in New Orleans augment *comparatively* its death-rate. Yellow fever, cholera and variola are only occasional visitants, and it is desirable to show the death-rate of the city for what may be considered its ordinary and permanent causes. Hence Table No. 4 was constructed, ex-

cluding the above four causes; and it teaches the ordinary and comparative death-rate for the last 18 years—1856–73.

Malarial Fevers.

"Swamp Poison" is a less appreciated but a greater enemy to the health and lives of the inhabitants of New Orleans than yellow fever. During the 18 years 1856–1873 the total deaths by yellow fever were 9459.

During the 16 years 1856–1873 (less 1861 and 1862) there were 5817 deaths by malarial fevers, which would indicate for the 18 years 6543 deaths. But important additions belong to this sum total. 1. Every physician admits that malarial poison is at the bottom of or singularly mixed up with very many diseases not diagnosed malarial; and that very many deaths caused by other diseases are due to the deleterious influence of swamp poison on the general health. 2. Unquestionably many of the deaths *reported* as due to "Congestion" and "Inflammation of Brain," "Meningitis," "Teething," "Infantile Convulsions," "Debility," "Marasmus," &c., were really due to Malaria. 3. Still less questionably were due to this cause a very large proportion of the deaths *reported* as "Fever," "Nervous," "Brain," "Continued," "Typhoid," Fever, &c. The deaths by these "fevers" during the 18 years exceeded 3000.

If due consideration be given the above figures and facts, it will be admitted that during the past 18 years malaria has destroyed fully as many lives as the 9459 dead by yellow fever, and that this estimate of the mortality fails to give a full idea of the deterioration of health and the amount of sickness due to this cause.

The following facts illustrate to some extent the injury thus inflicted. There were in the Charity Hospital during the 5 years 1856-60 and the 5 years 1866–70 a total of 93,068 "deaths and discharges." Of this total number of cases, 26,309 (or somewhat less than one-third of the whole) were cases of malarial fever. During the year ending September 30th, 1870, there were of 747 "sick or wounded" Metropolitan Policemen 236 (or somewhat less than one-third) cases of malaria. During the last six months of 1867, there were of an average daily number of 761 white soldiers at New Orleans 368 cases of malarial fever, which were about one-fourth of the cases of sickness by all diseases, excepting yel-

low fever. Of 313 colored soldiers, there were 155 cases of malaria, which also were about one-fourth of all the sickness except by yellow fever.

The above illustrating examples are only confirmations of all other facts, and convincingly enforce the necessity for drainage.

CONSUMPTION.

It has been often asserted that the annual deaths in New Orleans are notably increased by the deaths of strangers by consumption. If this be so, then an excess of such deaths ought surely to be found at that portion of the year when these strangers are in New Orleans. Any large amount of this mortality must occur, if at all, during the six months November–April; the half year during which the deaths by consumption are ordinarily more numerous, without regard to any additions made by strangers.

The statistical facts prove conclusively that the popular assertion is founded in error, and are as follows. During the 13 years 1856–70 (less 1861 and 1862) there were 9331 deaths by consumption, an annual average of 718. Of the 9331, there died during the six months November–April 4855, and during the six months May–October 4476, which gives an annual average excess of *only* 29, during that half year which is most unfavorable to consumptives, and during which there naturally occurs a preponderance of their mortality. The lesson taught by this aggregate of 13 years is also taught by each of these years separately, and in my opinion places this issue beyond farther discussion.

The average annual deaths during the 5 years 1856–60 were 743, the 3 years 1863–5 were 753, the 5 years 1866–70 were 674, and the 3 years 1871–3 were more than 800. The mortality in 1873 was unusually large, 850, and of these 348 were colored. This indicates an excessive preponderance of colored over white deaths, since the population of the latter is nearly three times larger than that of the colored.

PNEUMONIA.

The statistics of the Charity Hospital indicate that this disease is in New Orleans the most fatal of all of the so-called curable diseases, except Chronic Dysentery and Diarrhœa.

During 10 years (the 5 years 1856–60 and the 5 years 1866–70) there were in the Charity Hospital two deaths in every five cases. If it be supposed that this unfavorable indication may be due in part to errors of diagnosis, and to rectify this there be added to the cases of Pneumonia all those by "Congestion of the Lungs," and "Bronchitis," (the only two diseases likely to have been confounded with Pneumonia,) the result would still remain most unfavorable, viz., one death in every four cases.

Abscess of Liver.

One hundred and fifty post mortems during two years demonstrated a larger number of Abscesses of the Liver than the official reports of those years gave for the total mortality of the city. Satisfactory data for even approximative estimates are wanting, but from such facts as I have, it is my conviction that there are annually not less than 100 deaths by Abscess of the Liver, instead of the *reported* average for thirteen years of only 10.

Albuminuria and Dropsy.

The official reports indicate that during the 5 years 1866–70 there was a great increase of the deaths by Albuminuria, and a corresponding diminution of the deaths by Dropsy, when compared with the deaths by the same causes in preceding years. This apparent increase of renal disease is probably due only to a better diagnosis of Dropsy; and it is believed that accurate diagnosis would still farther increase the deaths by Albuminuria, diminishing at the same time those by Dropsy; so that the annual average of 40 deaths during the 5 years 1866–1870 should probably be about 60.

Sunstroke.—The total deaths in "the 13 years" were 285. The number was unusually large in July, 1860, August, 1865, and August, 1866. Sunstrokes were also unusually numerous in August, 1850, and in June, 1854.

Measles.—Total deaths in 13 years 724. It prevailed chiefly in 1857–63–66–69, and in every one of these four years during the six months January–June.

Scarlet Fever.—Total deaths in 13 years 1038. It prevailed

chiefly in 1859–60–64–5–6–70, and especially during the four months April–July.

Roseola prevailed, and deaths by *Inflammation of Throat* were unusually numerous in 1858.

Diphtheria.—Total for 13 years 1201. It prevailed chiefly in 1859–60–3–4–6, and did not manifest any decided preference for particular months or seasons. This disease (as thus named) first appeared in New Orleans in 1853 or 1854.

Hooping-Cough.—Total deaths for 13 years 527. It was most fatal in 1856–60–3–9.

Croup.—Total for 13 years 799. It was especially fatal in 1858. It also prevailed in 1864–6–7.

Dengue prevailed in the fall of 1860, and of 1873. It is said to have first appeared in the United States in 1820, and in New Orleans in 1829 or 1830. As far back as 1848, it does not seem to have prevailed in any year until 1860.

TABLE NO. I.

Historical Table of the Population and Mortality of New Orleans.

ABBREVIATIONS: Est.—estimated and not official; S.—sporadic; M.—mild; E.—epidemic; V.—violent, and V. V.—very violent; Y. F.—yellow fever; C.—cholera; Av.—average; U.—unknown; U. S. C.—United States Census.

YEARS.	TOTAL POPULATION BY U. S. CENSUS.	TOTAL DEATHS BY BOARD OF HEALTH, ETC.	NO. OF DEATHS TO EVERY 1000 POPULATION.	OCCURRENCE OF YELLOW FEVER AND CHOLERA.	REMARKS—ESPECIALLY AS TO EVENTS SUPPOSED TO INFLUENCE POPULATION AND MORTALITY.
1769	3190	.....	.....	Y. F. S. perhaps.....	Y. F. E. Biloxi, 1702, and Mobile, 1705.
1785	4980	.....	.....	.....	
1788	5331	.....	.....	.....	
1787–97	(10 yrs.)	*488 av.	*69.5 av.	Y. F. S. 1791	*Estimates given by Dr. E. H. Barton for the ten years.
1796	.....	.....	....	First Y. F. epidemic.....	1794-97—Carondelet Canal dug.
1797	8056	.....	.....	....	
1799 and 1800	.....	.....	.....	Y. F. epidemics.....	1801—Y. F. S.
1801–4–9	.....	.....	.....	" "	1807–8—Embargo.
1810	17,242	.....	.....	....	1812–13–14—War.
1811–15	(6 yrs.)	*989 av.	*34.2 av.	1811 and 1812 Y. F. E.....	*Estimates of Dr. E. H. Barton for the six years.
1816	.....	..... .	....	....	Inundation in the spring by crevasse at Carrollton. Year very healthy.
1817	....	.....	.....	Y. F. V. E.....	1142 total deaths in five months, viz., August to December, inclusive.
1818	25,000 est.	1151	46.	Y. F. E...	
1819	26,000 est.	2190	84.2	"	485 deaths in September.
1816–20	.....	*1517 av.	*39.5 av.	.....	*Estimates of Dr. E. H. Barton of annual averages.
1820	27,176	.....	....	Y. F. E.....	
1822–25	(4 yrs.)	*2085 av.	*47.2 av.	Y. F. E. 1822–4–5.....	*Dr. Barton's estimates: Quarantine, 1821–25.
1826–30	.....	*1707 av.	*36.1 av.	" 1827–8–9.....	1824–28—Gormley's Canal dug.
1830	46,310	.....	.....	"	1825–28—Melpomene Canal deepened and cleaned. [Year healthy.]
1831–3–4–5	.....	*3503 av.	59.2 av.	Y. F. E. V. 1833, E. 34–5.....	1831—Inundation in Aug. to Dauphin St. by violent storm from Lake.
1832	.....	.....	.....	Y. F. E, and V. V. C. E.....	1832—First appearance of Cholera, Oct. 25th. 1832–5—New Basin and [N. O. Canal dug.
1836–8–9–40	(4 yrs.)	*2942 av.	*39.6 av.	V. Y. F. E, 1839, S. C. 1835, 1836.	*Dr. Barton's estimates.
1837	.....	U.	.....	V. Y. F. E.....	Inundation in October by violent storm from Lake.
1840	102,193	.....	.....	....	102.193 pop. U. S. C. of City and Parish of Orleans, and 3207 for Lafayette.
1841–5	...	*3993 av.	*44.8 av.	1841 V. E., 1842–3 E.	*Dr. Barton's average estimates.
1845	108,000 est.	2783	25.7	... .	
....	.....	.....	.....	.....	For all the years which follow the population and deaths are for N. O. *and Lafayette*, which were not legally consolidated until 1852. N.B.—"Immigration from abroad averaged about 20,000 per annum (1845–50), very few arriving in summer and fall."—*Barton.*
.....	.....	.....	.....	.....	
.....	.....	.....	.....	....	
....	.....	.....	.....	.....	

FOR NEW ORLEANS AND LAFAYETTE.

1846	117,000 est.	4500 est.	38.5	160 deaths by Y. F.		Population in 1846, estimated from U. S. C.; deaths estimated from reports of 1770 deaths for last four months.
1847	120,000 est.	9336	77.8	2804 " " "		Inundation in April to Burgundy st., by violent storm from Lake.
1848	123,500 est.	8191	66.8	872 " " "	924 by C.	1846-7-8—War with Mexico.
1849	127,000 est.	10,661	84.	752 " " "	3285 "	Cholera began December 11th; numerous California travelers.
1850	130,565 U.S.C.	8086	62.	107 " " "	1851 "	Inundation 6 weeks, May and June, to Dauphin st., by Sauve's crevasse.
Total for 5 yrs.	618,065	40,774	66.	4695 " " "	6060 "	Population 130,565, viz: New Orleans 116,375, and Lafayette 14,190.
1851	133,940 est,	7275	54.3	17 deaths by Y. F.	688 by C.	
1852	137,400 est.	8693	63.3	456 " " "	1319 "	
1853	140,960 est.	15,633	111.	7849 " " "	607 "	7849 to 7970 Yellow Fever deaths by this "*The Great Epidemic*" of 1853.
1854	144,600 est.	10,800 est.	74.7	2425 " " "	950 "	
1855	148,400 est.	9000 est.	60.7	2670 " " "	883 "	March 1855—The present Quarantine and Board of Health established.
Total for 5 yrs.	705,200	51,401	72.8	13,417 " " "	4447 "	
1856	152,325 est.	5689	37.3	74 deaths by Y. F.	46 by C.	
1857	156,180 est	5581	35.7	200 " " "	89 "	
1858	160 240 est	11,721	73.1	4855 " " "	26 "	
1859	164,400 est.	6847	41.6	91 " " "	27 "	
1860	168,675 U.S.C.	7341	43 5	15 " " "	30 "	
Total for 5 yrs.	801,820	37,179	46 3	5235 " " "	158 "	
1861	172,000 est.	5772	33 5	No deaths by Y. F.....		January 25th, 1861, Louisiana seceded from the United States.
1862	170,000 est.	6278	37.	2 deaths by Y. F.....		April 26th, 1862, New Orleans captured by the United States.
1863	172,000 est.	7172	41.7	2 deaths of civil pop'n by Y. F...		About 100 cases of Yellow Fever occurred in the river fleet.
1864	173,000 est.	8498	49.	6 deaths of civil popul'n by Y. F.		About 200 cases and 57 deaths of Yellow Fever from gunboats, &c.
1865	180,000 est.	7016	39.	1 death by Y. F		Date of War. April 12th, 1861, Fort Sumpter bombarded, and April 9th, 1865, General Lee surrendered.
Total for 5 yrs.	867,000 est.	34,736	40.	11 deaths by Y. F.		
1866	181,000 est.	7754	43.	192 deaths by Y. F.	1294 by C.	Cholera began July 14th.
1867	182,000 est.	10,096	55.4	3107 " " "	581 "	
1868	183,000 est.	5343	29.	5 " " "	129 "	October (1st week).—Inundation from Lake, extending for a few hours as high as Burgundy street.
1869	184 000 est.	6001	32 6	3 " " "	4 "	Inundation from Lake.
1870	191,418 U.S.C.	7391	38.6	567 " " "	3 "	Algiers and Jefferson City annexed to New Orleans in 1870, and deaths in Algiers first reported by Board of Health.
Total for 5 yrs.	921,418 est.	36 585	39,7	3894 " " "	2011 "	
1871	195,000 est.	6059	31.	54 deaths by Y. F.	6 by C.	Inundation from River and Lake.
1872	200,000 est.	6122	31.	39 " " "		
1873	205.000 est.*	7995	39.	226 " " "	41 "	
1874	215,000 est.	....	.....	....		Carrollton annexed to New Orleansin 1874; population in 1870, 6495. April,—Extensive inundation of Louisiana, *not* of New Orleans.

* Soards' New Orleans Directory of 1874 estimates population of 1873 as 230,986, exclusive of Carrollton.

TABLE NO. II.

Table of the Annual and Monthly Mortality by Yellow Fever in New Orleans, 1796-1873.

In the seventy-eight years since the first epidemic of Yellow Fever, viz., 1796-1873. *there have been thirty-five epidemics, in the following years, viz:* 1796-9; 1800-1-4-9; 1811-12-17-18-19; 1820-2-4-5-7-8-9; 1830-3-4-5-7-9; 1841-2-3-7-8-9; 1853-4-5-8; 1867. *Of these, twelve at least were violent epidemics, viz: those of* 1817-22-33-7-9-41-47; 1853-4-5-8; 1867. *During the past eighteen years, viz:* 1856-1873, *there have been but two epidemics, viz:* 1858 *and* 1867. *N.B.—French embargo* 1807-8. *War with England* 1812-14. *Quarantine* 1821-24. *Quarantine which now exists was established March,* 1855.

YEARS.	Jan.	Feb.	Mar.	April	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mon. of oc'rnce not known.	TOTAL FOR YEAR.	REMARKS.
1817	.....	.....	.....	.....	.....	.....	.....	304	.....	.....	... ..	.....	519	823	First case June 18th.
1819	.....	.....	... ..	.....	.....	.....		.....	.....	.....	.....	.....	.....	U.	First 2 cases May 7th and 12th; last, Dec. 9th.
1823	.....	.....	.....	.....	.. ...	.....	.....	...	....	.....	.....	.....	.....	800-2000	First case prior to July 15th.
1839	.....	.....	.....	.....	.....	.....	.....	...	.....	.....	.....	.....	....	1325	
1841	.....	.....	.....	.....	.....	.....	.....	.....	.....	... ..	.....	.....	.....	1800	
5 epidemics— 1837-9-41-2-3	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	av'ge 5 epidemics 1100	
1844	.....	.....	.....	.....	.....	.....	1	4	54	... ..	.....	.....	89	148	(Deaths for New Orleans alone;* 1st case July 20th.)
1845	.....	.....	.....	.....	.....	.....	.....	.....	.. ..	..	----	.....	.....	2	
1846	.....	.....	.....	.....	.....	.....	.....	1	8	118	33	.. ..	.....	160	
1847	.....	.....	.....	.....	.....	.....	74	965	1100	198	12	10	545	2804	First case June 21st.
1848	. ....	.....	.....	.....	.....	4	33	200	467	126	20	.....	22	872	First case July 6th
1849	.....	.....	.....	.....	.....	.....	.....	11	194	396	143	8	.. ..	752	Deaths for New Orleans alone, 1st case July 28.
1850	1	.....	2	.....	1	.....	4	62	33	4	.....	.....	.....	107	" " " " 1st death of season May 29th. There were 3 to 4 cases in Charity Hospital in February, 1850.
Total for 5 y'rs.	1		2		1	4	111	1239	1902	842	208	18	567	4695	
1851	.. ...	.....	.....	.....	.....	.....	.....	8	6	2	1	.....	.....	17	
1852	.....	.....	.....	.....	.....	.....	2	8	91	198	105	11	41	456	Some say first death in first week of August.
1853	1	.....	.....	.....	2	31	1521	5133	982	147	28	4	....	7849	Some report 7970 deaths; first case May 22d.
1854	.....	.....	.....	.....	.....	2	29	532	1234	490	131	7	.....	2425	First death June 12th.
1855	.....	.....	.....	.....	.....	5	382	1286	874	97	19	7	.....	2670	First case June 19th.
Total for 5 yrs.	1	0	0	0	2	38	1834	6967	3187	934	284	29	41	13,417	

* By "for New Orleans *alone*" is meant that Lafayette is *not* included.

1856	.....	.....	.....	.....	.....	.....	.....	14	40	16	4	.....	... .	74	First case June 28th.
1857	1	.....	... .	.....	.....	1	1	1	8	98	82	8	.....	200	First undisputed cass died September 20th.
1858	2	.....	.....	.....	.....	2	132	1140	2204	1137	224	15	.....	4855	First case June 20th.
1859	.....	.....	.....	.....	.....	.....	.....	1	59	28	3	.....	....	91	
1860	.....	.....	.....	...	.....	. ....	3	7	5	.....	.....	.....	.....	15	
Total for 5 yrs.	3	0	0	0	0	3	136	1163	2316	1279	313	23	0	5235	
1861	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	0	
1862	....	.....	.....	.....	.....	.....	1	.....	1	.....	.....	.....	.....	2	Both cases said to be imported.
1863	.....	.....	.....	.....	.....	.....	.....	.....	2	.....	.....	.....	.....	2	Neither case imported; both were "boat-hands from a river-tug." About 100 cases occurred in U. S. "river fleet," and were not part of the civil population.
1864	....	.....	.....	.....	.....	.....	.....	.....	.....	4	1	1	.....	6	Abont 200 cases and 57 deaths from the U. S. gunboats and river fleet.
1865	.....	.....	.....	.....	.....	.....	.....	1	.....	.....	.....	.....	.....	1	
Total for 5 yrs.							1	1	3	4	1	1		11	
1866	.....	.....	.. ...	.....	.....	.....	.....	5	52	97	36	2	.....	192	First case August 9th.
1867	.....	.....	.....	.....	.....	3	11	255	1637	1072	103	26	.....	3107	First case died June 10th.
1868	1	.....	.....	.....	.....	1	1	1	.....	1	.....	.....	.....	5	
1869	.....	.....	.....	.....	.....	.....	1	.....	.....	2	.....	.....	.....	3	
1870	.....	.....	.....	.....	.....	.....	.....	3	231	242	106	5	.....	587	First case August 19th.
Total for 5 yrs.	1					4	13	264	1920	1414	245	33		3894	
1871	.....	.....	.....	.....	.....	.....	.....	2	9	22	19	2	.....	54	
1872	.....	.....	.....	.....	.....	.....	.....	1	5	24	7	2	.....	39	
1873	.....	.....	.....	.....	.....	.....	3	19	108	79	17	.....	.. ..	226	First case July 4th.
Total for 3 yrs.							3	22	122	125	43	4		319	

TABLE NO. III.

Annual and Monthly Mortality by Cholera.

This disease first appeared in New Orleans October 25th, 1832, *and* M HALPHEN, D.M.P., *of New Orleans, reported to the Paris Academy of Medicine, that in the first twenty days there were* 6000 *deaths!! It prevailed as a violent epidemic several months, and sporadically until early in* 1836, *when it finally disappeared. It reappeared December*, 1848, *July*, 1866, *and mildly in the spring of* 1873.

YEARS.	Jan.	Feb.	Mar.	April	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Month not known.	ANNUAL TOTAL.	REMARKS.
1832	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	... ..	.....	.. ....	Unknown.	Began Oct. 5th; very violent epidemic.
1833-36	.....	.....	... ..	.....	.....	.....		.....	.....	.....	.....	.....	.....	"	Prevailed sporadically. Disappeared early in 1836.
.....	.....	.....	.....	.....	.. ...	.....	.....	....	.....	.....	.....	.....	.....	.....	
1848	.....	.....	.....	.....	.....	.....	.....	....	.....	.....	.....	924	....	924	[1st case Dec. 11. Max. day Dec. 28, 92 cholera deaths]
1849	628	222	813	405	507	323	311	7	3	8	107	122	.....	3176	
1850	128	363	415	75	66	40	12	8	45	101	367	231	.....	1851	This total includes 1013 of "cholera Asiatic," and 838 of "cholera" & "cholera morbus."
.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	
Total for 3 Yrs.	.....	.....	.....	.....	.....	...	.....	.....	.. ..	..	----	.....	. .....	5951	
1851	30	29	11	108	121	131	125	14	5	9	82	23	.....	688	
1852	5	.....	.....	.....	240	455	153	45	116	60	158	87	... .	1319	Began first week of May.
1853	25	3	2	11	9	14	5	1	.....	6	177	322	32	607	
1854	21	U.	U.	U.	U.	U	U	U.	U.	.....	.....	.....	.. ..	about 950	"Over 900 in last seven months."
1855	.....	.....	....	.....	235	570	49	7	13	4	4	.....	.....	833	Began about May 15th.
Total for 5 y'rs.	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	4447	
Total for 5 Yrs. 1856-1860	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	158	'Cholera' *reported* for 1856, 46 deaths; 1857, 29; 1858, 26, 1859, 27; 1860 30. Total 5 years 158. In my detailed report Table No. 8, classed with Cholera Morbus.

1861	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	U.	
1862	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	U.	
1863	.....	.....	.....	1	2	.....	.....	.....	.....	1	.....	.....	.....	4	97 of C., Chol. Morb. and Infant.
1864	.....	.....	.....	1	3	.....	.....	.....	.....	1	.....	.....	.....	5	103 of C., Chol. Morb. and Infant.
1865	.....	.....	.....	.....	.....	3	1	.....	1	2	.....	2	.....	9	168 of C., Chol. Morb. and Infant.
Total for 5 yrs. 1861-5	.....	.....	.....	2	5	3	1	.....	1	4	.....	2	.....	18†	
1866	.....	.....	.....	.....	.....	.....	5	617	431	167	56	18	.....	1294	1490 of C., Chol. Morb. and Infant. 1st case of C, July 14th.
1867	5	.....	1	.....	.....	4	36	39	15	37	234	210	.....	581	726 of C, Chol. Morb. and Infant.
1868	120	1	.....	.....	.....	3	1	1	.....	1	1	1	.....	129	236 of C., Chol. Morb. and Infant.
1869	.....	.....	.....	.....	2	.....	.....	.....	.....	1	1	.....	.....	4	69 of C., Chol Morb. and Infant.
1870	.....	.....	.....	.....	1	1	.....	.....	.....	.....	1	.....	.....	3	83 of C., Chol. Morb. and Infant.
Total for 5 yrs. 1866-70	125	1	1	.....	3	8	42	657	446	206	293	229		2011	
1871	.....	.....	.....	2	1	1	.....	.....	1	1	.....	.....	.....	6	112 of C., Chol. Morb. and Infant.
1872	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	0	72 of Chol. Morb. and Infant
1873	.....	3	4	28	97	9	.....	.....	.....	.....	1	.....	.....	142	39 of C., Chol. Morb and Infant. 267 of above 359 died in April and May, 1873.

TABLE NO. IV.

Annual Deaths in New Orleans for the Eighteen Years 1856-73, with the Annual Deaths by Yellow Fever, Cholera, Small Pox, and the Still and Premature Births; and the Annual Death-Rates with and without the deaths from these four causes.

YEAR.	Total Deaths.	Deaths by				Annual Deaths, exeluding Epidemies and Still-Births.	Annual Death-Rate, including deaths by all causes.	Annual Death-Rate, excluding deaths by Yellow Fever, Cholera, Sm'll Pox and Still-Births.
		Yellow Fever.	Cholera.	Varioloid and Small Pox.	Premature and Still-Births			
1856	5689	74	0	2	403	5200	37.3	34.1
1857	5581	200	0	103	379	4899	35.7	31.3
1858	11,721	4855	0	108	383	6370	73.1	39.7
1859	6847	91	0	43	386	6327	41.6	38.4
1860	7341	15	0	22	434	6970	43.5	41.3
Totals for 5 Years.	37,179	5335	0	278	1890	29,766	46.3	37.
1861	5772	0	0	Unknown.	Unk. say 352	say 5420	33.5	31.5
1862	6278	2	0	"	say 376	say 5900	37.	34.7
1863	7172	2	0	2	308	6860	41.7	40.
1864	8498	6	0	605	388	7499	49.	43.3
1865	7016	1	0	613	401	6001	39.	33.3
Totals for 5 Years.	34,736	11	0	1220	1825	31,680	40.	36.6
1866	7754	192	1294	188	485	5593	43.	30.9
1867	10,096	3107	581	47	562	5799	55.4	31.8
1868	5343	5	129	14	568	4627	29.	25.3
1869	6001	3	4	141	521	5332	32.6	29.
1870	7391	587	3	528	559	5714	38·6	29.8
Totals for 5 Years.	36,585	3894	2011	918	2695	27,067	39.7	29.3
1871	6059	54	6	2	516	5481	31.	28.
1872	6122	39	0	29	530	5524	31.	27.6
1873	7995	226	142	509	528	6590	39.	32.1

TABLE NO. V.

Deaths by Sexes, Nativities, Races and Ages, During the 5 Years 1866-7-8-9-70. Also the Annual Averages of such Deaths During the 3 Years 1863-5, the 5 Years 1856-60, and the 5 Years 1866-70.

	1866	1867	1868	1869	1870	Totals for 5 Years 1866-70	Annual averages of the 3 years 1863-5	Annual averages of the 5 years 1856-60	Annual averages of the 5 Years 1866-70	Pop. U. S. C. 1860	Pop. U. S. C. 1870
Total Deaths	7754	10,096	5343	6001	7391	36,585	7714*	7427	7317	168,675	191,418 †
DEATHS BY SEXES.											
Males	4372	6239	2854	3276	4466	21,207	4382	4535	4241	85,106	90,279
Females	3052	3566	2275	2598	2838	14,329	3093	2792	2866	83,569	101,139
Not Stated	330	291	214	127	87	1049	239	100	210	. . ..	.....
DEATHS BY NATIVITIES.											
Born in United States	4145	4990	3308	3971	4753	21,167	3911	3843	4234	104,054	142,943
Foreign Born	1968	3624	1218	1218	2064	10.092	1417	2440	2018	64,621	48,475
Not Stated	1641	1482	817	812	574	5326	2386	1144	1065	.....	.....
DEATHS BY RACES.											
Whites	4984	7866	3503	3757	4602	24,712	5245	†6327	4943	141,601	140,923
Blacks and Mulattos	2392	1931	1676	2092	2560	10.651	2266	1100	2130	24,074	50,495
Not Stated	378	299	164	152	229	1222	203	.....	244	.....	.....

* The annual average of the deaths of the *civil* population was 7562. The 7714 deaths, as given, include a part of the deaths of "soldiers," and this number is given because in the specifications which follow, the civil population cannot be separated from the "soldiers."

†The annual average of white deaths for the 4 years 1856-60, excluding from these 5 years the epidemic year 1858, was 5267.

TABLE NO. V.—CONTINUED.

Deaths by Ages,	1866	1867	1868	1869	1870	Totals for 5 Years 1866-70	Annual Averages of the 3 Years 1863-5	Annual Averages of the 5 Years 1856-60	Annual Averages of the 5 Years 1866-70	Pop. U. S. C. 1860	Pop. U. S. C. 1870
Premature and Still-Births	485	562	568	521	559	2695	366	378	539	.....	.....
Under 1 Year	1039	1301	1040	864	1028	5272	865	1286	1054	3637	5135
1 to 2 Years	518	529	364	472	410	2293	692	548	460	{	4183
2 to 5 Years	510	566	229	493	353	2151	743	584	430	18 874 }	15,387
5 to 10 Years	360	491	152	201	180	1384	449	276	276	19.038	21,114
Total from	**Totals from 0 Years of Age to Over 100 Years of Age.**										
0 to 10 Years	2912	3449	2353	2551	2530	13,795	3106	3072	2759	41,549	45,819
10 to 20 "	416	719	222	251	402	2010	443	396	402	30,960	40,583
20 to 30 "	1069	1981	508	529	1073	5160	745	1308	1032	36,100	34,340
30 to 40 "	991	1441	588	581	929	4530	744	1108	906	30,963	28,894
40 to 50 "	737	946	488	570	754	3495	654	712	699	18,185	21,967
50 to 60 "	520	604	396	445	559	2524	487	350	505	6770	12,267
60 to 70 "	333	344	266	276	348	1567	293	187	313	2890	5374
70 to 80 "	147	163	136	133	189	768	166	98	155	810	1711
80 to 90 "	51	70	62	45	64	292	87	52	58	255	398
90 to 100 "	19	12	22	30	24	107	36	19	21	85	116
Over 100 "	10	10	9	15	21	56	16	5	11	23	39
Not Stated	549	357	293	575	507	281	931	120	456	80	.....

TABLE NO. VI.

Still-Births During 21 Years in New Orleans Charity Hospital.

YEARS.	Length of Time.	Total No. of Still-Births.	Total No. of Births.	Total No. of Male Births.	Total No. of Female Births.	No. of Twins.
1856-60	5 Years.	60	570	330	240	5
1866-70	5 Years.	33	390	194	196	4
{1852-3-4-5 {1861-2-3-5	8 Years.	127	1077	563	514	15
1871-3	3 Years.	19	284	135	149	7
Totala 1852-73	21 Years.	239	2321	1222	1099	31

TABLE NO. VII.

Monthly Report of the Five Years 1866-70, Consolidated and Arranged in two Series of the more healthy Half Year November-April, and the less healthy Half Year May-October.

N.B.—Each Half Year contains 26 Weeks. Some of the Months have Five, others Four Weeks, which respectively is not known.

Months.	Nov.	Dec.	Jan.	Feb.	Mar.	Ap'l.	6 Mos. Nov.-April.	May.	June.	July.	Aug.	Sept.	Oct.	6 Mos. May-Oct.	12 Mos. 1866-70
Total deaths for month of	3170	2805	2252	1958	2335	2390	14,910	2588	2746	2608	932	5408	4611	21,675	36,585
Total deaths by Yellow Fever, Cholera and Small Pox	581	358	212	101	171	170	1593	119	96	82	3714	2375	1626	5230	6,823
Total deaths exclusive of above 3 epidemics.	2589	2447	2040	1857	2164	2220	13,317	2469	2650	2526	2782	3033	2985	16,445	29,762
Yellow Fever	245	33	1	0	0	0	279	0	4	13	264	1920	1414	3615	3894
Malarial Fevers	191	81	49	40	63	47	471	83	128	199	377	518	429	1734	2205
"Fever," Brain, Nervous, &c	20	7	17	12	13	31	100	37	58	46	43	42	22	248	348
Typhoid Fever	37	36	26	22	35	17	173	25	32	22	58	58	69	264	437
Cholera	293	229	125	1	1	3	652	1	8	42	656	446	206	1359	2011
Variola and Varioloid	43	96	86	100	170	167	662	118	84	27	12	9	6	256	918
Scarlet Fever	7	9	7	11	11	17	62	18	11	14	7	15	10	75	137
Measles	2	0	8	20	46	62	138	93	60	27	3	2	2	187	325
Congestion of Brain	45	43	38	30	36	35	227	50	36	49	63	67	61	326	553
Encephalitis, Meningitis, &c	66	41	28	37	51	41	264	53	59	61	60	67	53	353	617
Apoplexy	44	63	26	29	42	34	238	26	35	39	46	42	38	226	464
Tetanus	80	60	30	30	23	31	254	41	37	55	51	75	89	348	602
Trismus Nascentium	74	105	81	67	57	72	456	49	68	80	112	114	96	519	975
Convulsions, Infant	105	83	92	84	63	105	532	111	105	90	89	88	130	613	1145
Phthisis Pulmonalis	297	314	238	270	308	294	1721	281	271	288	245	276	264	1625	3346
Bronchitis and Catarrh	40	40	48	52	48	55	283	30	36	23	19	38	54	200	483
Pneumonia, Inf., and Congestion of Lungs	107	163	169	182	209	198	1028	136	104	74	55	125	123	617	1645
Croup	16	17	26	13	18	10	100	9	12	4	15	16	10	66	166
Teething	34	23	27	16	25	21	146	45	54	42	37	35	33	246	392
Cholera Infantum	49	22	12	4	7	38	132	74	74	34	33	23	24	262	394
Inf. Marasmus and Debility	119	176	79	48	70	94	516	103	141	126	97	90	107	664	1180
Still and Premature Births	259	269	199	183	199	178	1278	157	224	211	271	264	290	1417	2695
Gastritis	8	10	7	10	9	6	50	6	10	15	14	9	13	67	117
Gastro-Enteritis	36	39	30	20	27	29	181	49	55	32	26	33	51	246	427
Enteritis, Inf., and Congestion of Bowels	47	19	35	25	35	36	197	71	76	56	49	39	30	321	518
Cholera Morbus and Colic	20	13	14	8	8	6	69	20	14	18	7	14	14	87	156
Diarrhœa, Acute and Chronic	89	84	58	40	45	58	374	101	114	95	77	99	92	578	952
Dysentery, Acute and Chronic	104	74	60	25	38	46	347	61	116	100	106	80	85	548	895

TABLE NO. VII—CONTINUED.

MONTHS.	Nov.	Dec.	Jan.	Feb.	Mar.	Ap'il.	6 Mos. Nov.-April.	May.	June.	July.	Aug.	Sept.	Oct.	6 Mos. May-Oct.	12 Mos. 1866-70
Entero-Colitis	12	14	3	4	5	9	47	11	21	21	9	15	14	91	138
Disease of Liver	38	33	35	19	21	26	172	24	30	35	25	37	32	183	355
Disease of Heart and Endocarditis	49	68	71	59	59	66	372	58	60	74	60	69	65	386	758
Drowned	22	9	11	18	19	23	102	39	44	35	50	32	25	225	327
Total of above Diseases	2598	2203	1736	1479	1752	1855	11,623	1980	2181	2047	3036	4757	3951	17,952	29,575
" " other "	572	602	516	479	583	535	3287	608	565	561	678	651	660	2723	7010
Total of all Diseases	3170	2805	2252	1958	2335	2390	14,910	2588	2746	2608	3714	5408	4611	21,675	36,585

TABLE NO. VIII.

Mortality Report of New Orleans for Thirteen Years, 1856-1870.

(Nomenclature and Classification of the Royal College of Physians, England.)

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Population of New Orleans by U. S. Census of 1860 was 168,675,
" " " from 1863-5 is estimated at from 170 000 to 180,000.
" " " by U. S. Census of 1870 was 191,418.

GENERAL SUMMARY IN SEVEN COLUMNS.

Column 1. Total Mortality for the 3 Years 1863-4-5, i. e., during the military occupation of N. O.
" 2. " " " " 5 Years 1856-60 " preceding the War.
" 3. " " " " 5 Years 1866-70 " succeeding the War
" 4&5. " Deaths and Discharges in N. O. Charity Hospital during the 5 Years 1856-60.
" 6&7. " " " " " " " " " " " 1866-70.

N.B.—"Deaths in the Charity Hospital" are a part of the "Deaths in New Orleans," and are therefore included in the Total of "Deaths in New Orleans."

Class.	Order.	DISEASES BY CLASSES AND ORDERS.	Deaths in New Orleans during the 3 Y'rs 1863-5	Deaths in New Orleans during the 5 Y'rs 1856-60	Deaths in New Orleans during the 5 Y'rs 1866-70	Deaths and Discharges in N O Charity Hospital during the 5 Y'rs 1856-60. Deaths.	Deaths and Discharges in N O Charity Hospital during the 5 Y'rs 1856-60. Dis-charg'e's	Deaths and Discharges in N O Charity Hospital during the 5 Y'rs 1866-70 Deaths.	Deaths and Discharges in N O Charity Hospital during the 5 Y'rs 1866-70 Dis-charges
1		General Diseases A.—"Zymotic, apt to be epidemic '.....	4835	9462	10939	2398	17,574	1936	12813
2		General Diseases B.—"Constitutional, apt to be inherited ".....	3106	4806	4577	1123	6476	899	5242
		LOCAL DISEASES, VIZ:							
3	1	Nervous system.....	2792	5822	5176	331	1039	258	724
	2	Eye.....	....	0	0	0	1283	0	635
	3	Ear and Nose.....	....	1	0	1	41	0	24
	4	Circulatory System.....	465	640	929	118	231	166	189
	5	Absorbent System.....	1	10	4	0	41	0	29
	6	Ductless Glands.....	1	0	0	0	2	0	0
	7	Respiratory System.....	1743	2620	2650	472	2251	292	817
	8	Digestive Sys em.....	6074	6544	4608	1607	7033	838	2951
	9	Urinary System.....	115	124	261	59	136	94	150
	10	Generative System.....	105	184	194	28	1952	14	1546
	11	Locomotory System.....	6	20	21	4	231	22	142
	12	Cellular Tissue.....	0	0	0	5	10	0	0
	13	Cutaneous System.....	28	30	53	26	2607	23	1699
4		Conditions unclassified, i. e., "not necessarily associated with General or Local Diseases.".....	2412	3696 643	4624 170	147 358	503 1786	167 53	444 711
5		Poisons.....	102						
6		INJURIES.							
6	1	General Injuries.....	435	1125	810	42	236	30	127
	2	Local Injuries.....	257	328	274	201	4000	109	2101
7		Surgical Operations.....	3	4	5	6	85	2	68
8		Parasites.....	22	30	12	1	58	0	68
9		Congenital Malformations.....	5	16	18	0	0	0	1
10		Conditions unclassifiable—"Unknown,".. "Not Stated," uncertain, &c.....	635	1028	1260	65	1725	49	1343
		Total Deaths of Citizens and Soldiers.....	23,142						
		Total Deaths of "Soldiers included".....	456						
		Total Deaths of Citizens only.....	*22686	37,131	36,585	6992	49,300 6992	4952	31,824 4952
		Total Discharges and Deaths in N. O. Char. Hosp'l for 5 Years 1856-60.....					56,292	5 y'rs 1866-70	36,776

* 131 should be added for defect of report of 3d week in August, 1863.

TABLE NO. VIII--Continued.

Class.	Order.	DISEASES BY CLASSES AND ORDERS.	Deaths in New Orleans during the			Deaths and Discharges in N. O. Charity Hospital during the			
						5 Y'rs 1856-60		5 Y'rs 1866-70	
			3 Y'rs 1863-5	5 Y'rs 1856-60	5 Yrs 1866-70	Deaths	Dis-charg's	Deaths	Dis-charg's
1		General Diseases A.—"Zymotic, apt to be epidemic."							
		Small Pox	926	262	898	0	23	2	54
		Varioloid	294	16	20	1	32	0	60
		Measles	163	236	325	9	93	3	53
		Scarlet Fever	404	497	137	13	34	2	5
		Other Exanthemata	.....	2	3	0	11	.....	.....
		Diphtheria	629	398	174	1	11	3	7
		Hooping Cough	107	217	203	5	21	0	3
		Cholera	.....	.....	2011	.....	.....	316	81
		Gangrene	39	71	38	16	6	11	7
		Erysipelas	61	84	52	28	209	5	117
		Puerperal Fever	41	59	73	4	3	3	0
		Pyæmia	14	11	36	1	.....	16	2
		Typhus Fever	25	68	58	28	34	2	0
		Cerebro-Sp. Meningitis, (or Spotted Fever)	0	3	27	.....	.....	3	1
		Typhoid Cont'd, Enteric, &c., Fever	679	802	437	346	687	94	135
		Brain and Nervous Fever	107	95	182	.....	.....	.....	.....
		Fever	117	80	166	0	41	3	9
		Malarial Fever	441	333	704	99	14,350	283	11120
		Malarial Congestive Fever	473	819	828	145	63	122	35
		Pernicious Fever	306	163	673	30	15	35	9
		Yellow Fever	9	5242	3894	1672	1483	1033	1102
		Dengue	.....	4	.....	0	453	0	13
		Total of Class 1	4835	9462	10939	2398	17574	1936	12813
		General Diseases B.—"Constitutional, apt to be inherited"							
		Rheumatism	49	81	66	22	2362	19	1412
		Gout	6	4	19	0	7	0	5
		Syphilis	19	43	78	27	2475	25	2767
		Cancer	169	232	387	33	74	71	96
		Tumors	11	11	16	3	16	8	34
		Leprosy (Elephantiasis)	1	4	5	2	6	2	3
		Scrofula, (Tubes Mesent, Rickets, &c)	78	73	113	24	129	10	62
		Phthisis Pulmonalis (Tuberculous)	2258	3727	3378	940	1037	715	717
		Hectic Fever	6	27	2	4	2	.....	.....
		Diabetes	...	4	2	1	12	.....	.....
		Purpura and Scurvy	18	19	30	6	65	9	47
		Anæmia, Chlorosis, Leucocyth	74	48	93	18	183	17	56
		Dropsy, Anasarca, Œdema	417	533	388	43	108	23	43
		Total of Class 2	3106	4806	4577	1123	6476	899	5242
3		Local Diseases, Class 3.—with 13 Orders							
	1	Order 1—Dis. of Nervous System							
		Disease of Brain	14	37	8	.....	.....	.....	.....
		Congestion of Brain	289	647	553	67	17	32	3
		Softening, Abscess of Brain	37	133	140	26	14	17	2
		Inflam. of Brain	166		171			9	3
		Meningitis	173	473	446	15	18	23	4
		Apoplexy	236	584	464	38	12	42	5
		Sun Stroke	29	127	29	50	34	13	34
		Hydrocephalus	66	169	70	0	2	.....	.....
		Mania, Insanity, Dementia, &c	23	24	23	5	99	4	65
		Diseases of Spine	19	40	17	3	9	1	5
		Paralysis	100	118	153	38	171	54	188
		Tetanus	268	315	602	41	6	32	13
		Trismus Nascentium	341	947	975	5	1	2	0
		Convulsions	.....	62	255	14	13	10	2
		Convulsions Adult	59	.....	.....	.....	.....	.....	.....
		Convulsions Infantile	836	2031	1145	.....	.....	.....	.....
		Epilepsy	123	97	97	24	85	17	70
		Hydrophobia	3	5	18	.....	.....	1	0
		Hysteria, Chorea, Catalepsy, Par Agit	4	6	7	1	69	0	62
		Neuralgias	6	7	3	4	439	1	268
		Total of Class 3, Order 1	2792	5822	5176	331	1039	258	724
3	2	Eye Diseases	0	0	0	0	1283	0	635
3	3	Diseases of Ear and Nose	0	1	0	1	41	0	24
3	4	Circulatory System	.....	.....	.....	.....	.....	.....	.....
		Pericarditis	15	36	24	11	11	6	4
		Hydro-pericardium	0	2	12	2	0	.....	.....

TABLE NO. VIII--Continued.

Class.	Order.	DISEASES BY CLASSES AND ORDERS.	Deaths in New Orleans during the			Deaths and Discharges in N. O Charity Hospital during the			
						5 Y'rs 1856-60		5 Y'rs 1866-70	
			3 Y'rs 1863-5	5 Y'rs 1856-60	5 Y'rs 1866-70	Deaths	Dis-charg'ds	Deaths.	Dis-charg'ds
		Endocarditis	3	.....	22	1	11	4	5
		Disease of Heart, Organic, &c	407	501	736	78	132	133	127
		Angina Pect	7	40	24	2	3	2	4
		Cyanosis	11	9	26	.....	.....	0	1
		Aneurism	17	39	51	14	9	19	17
		Phlebitis	2	5	21	1	7	0	4
		Phleg Dolens	0	1	.....	1	2	0	6
		Varices	0	.....	1	0	55	0	21
		Gangrena Senilis	3	7	12	8	1	2	0
		Total of Class 3. Order 4	465	640	929	118	231	166	189
3	5	Diseases Absorbent System	.....	.....	.....	.....	.....	.....	.....
		Parotitis, Adenitis, &c	1	10	4	0	41	0	29
3	6	Diseases of Ductless Glands	.....	.....	.....	.....	.....	.....	.....
		Goitre	1	0	0	0	2	0	0
3	7	Diseases of Respiratory System	.....	.....	.....	.....	.....	.....	.....
		Laryngitis, &c	2	6	46	8	67	5	33
		Croup	200	433	166	.....	.....	3	1
		Catarrh	108	154	96	8	511	1	107
		Bronchitis	206	352	387	61	795	26	253
		Asthma and Emphysema	59	68	111	16	57	31	79
		Disease of Lungs	.....	24	1	11	3	3	0
		Abscess, and Gangrene of Lungs	6	42	28	11	3	5	3
		Pneumonia	936	1164	1471	300	501	184	235
		Congestion of Lungs	73	179	174	18	4	9	1
		Hæmoptysis	39	68	87	5	26	4	13
		Pleurisy	41	52	52	28	277	16	88
		Empyema	0	11	6	3	3	3	1
		Hydrothorax	23	67	25	3	4	2	3
		Total of Class 3, Order 7	1743	2620	2650	472	2251	292	817
3	8	Dis. of Digestive System	.....	.....	.....	.....	.....	.....	.....
		Diseases of Mouth and Tongue	10	8	10	6	16	1	18
		Diseases of Throat, Pharynx and Œsophag	245	165	16	3	148	3	10
		Teething	482	897	392	3	5	1	1
		Dyspepsia Gastrodynie Enteralgia	4	8	18	9	456	1	141
		Epistaxis	.....	.....	.....	.....	.....	4	25
		Hæmatemesis	17	11	18	0	5	1	1
		Ulcer, Soft'g Gangrene of Stomach	4	21	13	4	0	.....	.....
		Gastritis	74	179	117	29	286	8	48
		Disease of Bowels	16	24	16	.....	.....	3	0
		Gastro-Entritis	266	341	427	37	42	7	2
		Enteritis	416	625	38	33	92	13	28
		Entero Colitis	.....	.....	133	.....	.....	.....	.....
		Inf. and Cong. of Bowels	283	148	480	0	0	0	0
		Chol. Morbus, Colic, Cramps	151	299	156	40	198	10	149
		Cholera Infantum	227	517	394	17	2	3	0
		Diarrhœa	.....	1442	228	713	3327	.....	.....
		Diarrhœa Acute	861	.....	215	.....	.....	90	1083
		Diarrhœa Chronic	1603	.....	509	.....	.....	266	211
		Dysentery	.....	1187	303	404	1104	.....	.....
		Dysentery Acute	782	.....	324	.....	.....	104	515
		Dysentery Chronic	270	.....	263	.....	.....	212	128
		Hernia	.....	.....	.....	0	102	0	49
		Hernia Strangulated	13	22	23	6	24	7	2
		Strict., Intussusc., Obstruct. of Intestines	2	7	16	3	7	1	1
		Constipation	4	11	7	1	281	0	125
		Hæmorrhoids	0	0	0	2	121	0	102
		Fistula, Strict. Prolaps of Anus and Rect'm	3	0	4	0	99	0	84
		Biliary Derangement	.....	.....	.....	2	254	.....	.....
		Disease of Liver	15	28	41	1	0	6	3
		Inf. Congest. Enlarg. of Liver	120	246	171	48	104	15	67
		Abscess of Liver	26	62	42	19	4	11	4
		Cirrhosis	31	47	76	35	22	28	27
		Jaundice	29	54	25	27	133	8	58
		Inf. Enlarg. of Spleen and Pancreas	2	7	2	5	26	2	24
		Peritonitis	73	170	116	45	15	13	13
		Ascites	.....	18	.....	115	160	20	32
		Total of Class 3.—Order 8	6074	6544	4608	1607	7033	838	2951
3	9	Dis. of Urinary System	.....	.....	.....	.....	.....	.....	.....
		Dis. of Kidneys	19	.....	17	.....	.....	0	3

TABLE NO. VIII--Continued.

Class.	Order.	DISEASES BY CLASSES AND ORDERS.	Deaths in New Orleans during the			Deaths and Discharges in N. O. Charity Hospital during the			
						5 Y'rs 1856-60		5 Y'rs 1866-70	
			3 Y'rs 1863-5	5 Y'rs 1856-60	5 Y'rs 1866-70	Deaths.	Dis. charg's	Deaths.	Dis. charg's
		Brights Dis., Albuminuria, Nephritis	62	81	177	50	71	78	98
		Uræmia	0	.. ..	10	.....	.....	3	1
		Suppression of Urine	2	.... ..	6	...	.....	0	2
		Hæmaturia	.....	.....	2	.....	.....	0	3
		Disease of Bladder	.....	9	.....	1	24	.....	.....
		Cystitis	23	28	37	5	33	8	22
		Calculus- Gravel	1	4	2	0	6	2	12
		Rupture Bladder, Urethra	5	2	10	3	2	3	0
		Retent. Incont. Urine, Dysuria	3	.....	.. ...	.....	.....	0	9
		Total of Class 3, Order 9	115	124	261	59	136	94	150
3	10	Dis. of Generative System							
		Gonnorrhœa, Balanitis, Gleet	.....	.....	.....	4	488	0	526
		Strict. of Urethra	1	... .	4	3	88	3	169
		Dis. Testicles, Orchitis, Epididym. &c	3	.....	.....	4	249	0	212
		Dis. of Prostate Gland	.....	.....	.... ..	.....	.....	2	5
		Vaginitis and Leucorrhœa	0	.....	1	3	82	0	57
		Inf. of Ovary and Uterus. Dropsy of Ovary	25	38	34	4	53	1	26
		Tumors of Ovary and Uterus	2	.....	9	.....	.....	0	3
		Prolapsus, Versions, Flexions of Womb	...	.....	.....	0	47	0	35
		Amenorrhœa and Dysmenorrhœa	9	3	2	5	99	0	19
		Abortion	0	.....	1	0	26	1	16
		Pregnancy	0	.....	0	0	236	0	82
		Parturition	23	73	44	0	550	3	328
		Puerperal and Uterine Hæmorrhage	10	32	25	1	7	0	1
		Menorrhagia	.. ...	.....	.....	0	21	0	17
		Rupture of Womb	1	.....	.....	....	.....	.....	....
		Retained Placenta	.....	.... ..	.....	.. ..	.....	0	2
		Vesico-Vaginal Fistulæ	2	. .	1	.. ...	.....	1	17
		Puerperal Convulsions	28	33	73	2	2	2	0
		Puerperal Mania	1	.....	0	2	4	1	1
		Total of Class 3, Order 10	105	184	194	23	1952	14	1546
3	11	Dis Locomotory System							
		Dis. of Bones Caries, Necrosis, &c	3	12	10	2	226	14	138
		Pelvic Abscesses, Psoas, Lumbar, &c	3	8	11	2	5	8	4
		Total of Class 3, Order 11	6	20	21	4	231	22	142
3	12	Dis. of Cellular Tissue	....	... .	.....	5	10	.....	.....
8	13								
		Dis. of Cutaneous System							
		Abscesses	10	11	7	11	621	9	373
		Ulcers	10	12	21	11	1684	10	1169
		Carbuncle (Anthrax)	2	4	13	1	0	3	13
		Roseola	.....	.... ..	.....	0	17	.....	.....
		Other non syphilitic cutaneous eruptions	6	3	12	3	235	1	144
		Total of Class 3, Order 13	28	30	53	26	2607	23	1699
4		Conditions not necessarily associated with General or Local Diseases							
		Hæmorrhage from Navel	.....	.....	14	.....	.....	.....	.....
		Premature Birth	230	126	411	....	.....	... ..	.....
		Still-Born	867	1764	2284	.....	.....	.....	.....
		Infantile Debility	150	417	122				
		Debility	....	.....	431				
		Debility Marasmus	533	773	627	123	484		
		Adult "	74	61	148			14	3
		Debility (Adult ?)	231	192	149			72	390
		Inanition, Atrophy	94	.....	110	.....	.....	11	3
		Old Age	233	363	528	24	19	70	48
		Total of Class 4	2412	3696	4624	147	503	167	444
5		Poisons	2	19	33	2	12	8	29
		Mercury-Salivation	1	1	2	1	64	3	34
		Lead Colic and Palsy	1	18	4	5	167	1	46
		Chloroform	.....	.....	.....	2	0	.....	.... .
		Alcohol. Del. Tremens, Intemperance	98	610	131	348	1543	41	602
		Total of Class 5	162	643	170	358	1786	53	711
6		Injuries							
	1	General Injuries							

TABLE NO. VIII—Continued.

Class.	Order.	DISEASES BY CLASSES AND ORDERS.	Deaths in New Orleans during the			Deaths and Discharges in N. O. Charity Hospital during the			
						5 Y'rs 1856-60		5 Y'rs 1866-70	
			3 Y'rs 1863-5	5 Y'rs 1856-60	5 Y'rs 1866-70	Deaths.	Dis-charg's	Deaths	Dis-charg's
		Injuries and Casualties	4	280	32	1	0	.....	.....
		Drowned	199	402	327	.....	.... ..	.....	.....
		Asphyxia, Suffocation	37	43	36	....	....	2	0
		Lightning	1	2	3	....	.....	1	0
		Burns and Scalds	61	168	154	40	236	25	127
		Exposure, Privation, Want	9	15	12	1	0	.....	.....
		Killed Accidentally	73	.....	103	.....	.....	.....	.....
		Killed or Murdered	26	88	24	.....	.... ..	.....	.....
		Executed	.....	5	.....	.....	.....	.....	.....
		Suicide	22	105	61	.....	.....	2	0
		Infanticide	2	17	6	.....	.....	.....	.....
		Malpractice	1	.....	2	.....	.....	.....	.....
		Total of Class 6, Order 1	435	1125	810	42	236	30	127
6	2	Local Injuries				.....		23	812
		Wounds—Cont'd, Lac'd Inc'd. Punctur'd, Pent'g, Poisoned	24	164	66	73	3070	2	652
		Gunshot Wounds	175	.....	95	25	160	11	155
		Compn. Concus. of Brain Fract. Skull and Spine	45	146	91	78	60	28	25
		Fractures and Dislocations	11	17	22	29	707	44	457
		Other Local Injuries	2	1	0	5	3	1	0
		Total of Class 6, Order 2	257	328	274	201	4 00	109	2101
7		Surgical Operations							
		Amputation, Lithotomy	3	4	5	6	85	2	68
8		Parasites	.....	.....	...	.....	....	.....	.. ...
		Worms	22	30	12	1	13	.....	.....
		Tænia	.....	.....	.....	.....	.....	0	5
		Scabies	.....	.....	.. ..	0	45	0	63
		Total of Class 8	22	30	12	1	58	0	68
9		Congenital Malformations	2	3	4	.. ..	.... ..	0	1
		Imperf. Anus	3	13	14	.. ..	.....	.....	.....
		Total of Class 9	5	16	18	.....	..... .	0	1
10		Unclassifiable Diseases							
		"Unknown," "Not Stated," "Nihil" &c.	635	1028	1260	65	1725	49	1343

ARTICLE II. *Dengue, a Typical Eruptive Fever: Its thermometrical Semeïology.* By Dr. H. C. d'AQUIN.

The thermometrical charts of a few observations of Dengue, taken during the epidemic of 1873, demonstrate that it is a typical fever; and the accompanying eruption, places it among the eruptive fevers. Its thermometrical scale has the greatest analogy with that of varioloid, whilst its eruption ranges nearer to that of measles, or scarlet fever.

The charts have been made with observations belonging to civil practice; but the temperatures were taken with the greatest care, twice, and sometimes three times a day, at about the same hours.

The dotted lines, that are continuous with those of the temperature, represent missing visits, figured through strict information taken at the bed of the patient. The other dotted parallel lines figure the number of pulsations corresponding with each temperature. All the patients were acclimated, with the exception of No. v., coming from Galveston, the day previous to the invasion of his case; No. xii., born in northern Louisiana; Nos. iv. and xiv., who left Shreveport seven days before being taken sick, and No. viii., a stranger, living in the city for a year, but not yet acclimated.

Dengue, as a type, is a fever with two paroxysms, separated by a short remission, and lasting five full days.

The first paroxysm, *period of invasion*, sets in abruptly with a high temperature, the exact degree of which could not be ascertained, on account of the difficulty of seeing the patient at the very moment of the invasion. The *defervescence* that terminates this period, is either slow, or rapid, and ends in a period of calm, or *remission* of short duration, which is immediately followed by a quick, or a lingering rise—*effervescence*, terminating by a high temperature the acme of the second paroxysm or *exacerbation*. The second defervescence, which is generally lytic, without, or with very small evening rises, comes after, and terminates the disease by a fall to the normal point, in the early part of the morning that follows five complete twenty-four hours, which is generally the morning of the sixth day, or the seventh when invasion took place after mid-day on the first day.

An eruption which could not be detected in all the cases, mixed as it were with Lichen Agrius and other eruptions, the product of excessive summer heat, appears at the very fastigium of the invasion, follows it through all its periods, becoming paler during first defervescence and remission, stronger marked during effervescence and fastigium of exacerbation, disappearing, and desquamation taking place during and after second defervescence.

Observation No. vii., though not complete, was a good illustration of the course of temperature, together with that of the eruption which was scarlet-like and confluent.

The paroxysms and eruptions of Dengue have been described by authors in former epidemics, without the use of the thermometer; the charts only confirm their observation (see Jules Roch-

ard, Article "Dengue, Dictionnaire de Medicine et de Chirurgie pratique).

Observation No. xx., which is that of a very mild though confluent case of varioloid, shows how much Dengue resembles an eruptive fever by the course of temperature and eruption. At the highest fastigium of the period of invasion, appear the first papules; then begins a first defervescence which terminates by a remission, when the eruption is complete and vesicular; then comes the effervescence of secondary fever, whilst the variolic exudation is transforming into pus, and when all the pustules are mature, it stops, and second defervescence begins, ending at a normal temperature when dessication is nearly completed, after twelve days evolution. Change the form of the eruption, and take half the time of the evolution of the disease, and no two typical fevers will resemble each other more.

If we examine with care the thermometrical charts of Dengue, especially the cases observed on the first day, or the beginning of the second, we remark:

1st.—That the remission between the two high temperatures rarely comes to the normal point; but stops at a few tenths of a degree above or below 38° Cent., 100.4° Far., not being a period of apyrexia, but a diminution of heat of short duration.

2d.—That the remission does not always fall on the same days, but sometimes on the third or on the fourth day.

3d.—That the exacerbation shows a high peak also on different days, on the third, the fourth and even the fifth.

4th.—That notwithstanding this fall of remission and exacerbation on different days, the disease completes its evolution in five times twenty-four hours, in nearly all the cases; the time of the excursus of the effervescence and defervescence constituting the paroxysms, being longer or shorter, in order to compensate the time of the complete evolution of the disease.

5th.—That each of the paroxysms lasts separately from forty-eight hours to three days and a half; but that the time of their evolution is not generally the same.

6th.—That the pulse follows faithfully the rise and fall of the temperature, throughout all the periods, the two lines being parallel mostly all the time.

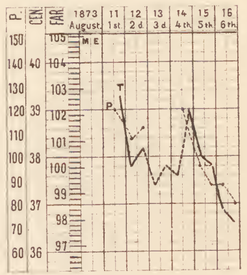
These minute remarks confirm the disease in its type, and estab-

lish the thermometrical diagnosis between Dengue and other typical fevers resembling it, such as Yellow Fever and Malaria.

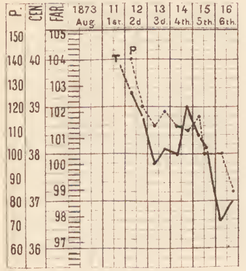
The period of invasion of Yellow Fever resembles that of Dengue to such an extent, that the two have been confounded; nay, some physicians have considered Dengue as a miniature Yellow Fever. The two charts of Yellow Fever, No. xviii., from Dr. Layton, and No. xix., from Dr. Touatre, give us the following difference between the two diseases. There is but one paroxysm in Yellow Fever, as Dr. Faget has judiciously observed, that of invasion; it lasts from two to three days or more, showing one higher peak on one of those days. The defervescence that follows is sometimes quick, sometimes slow; but there is no remission followed by an exacerbation. If by chance it happens that there is a second rise of temperature it comes at no specified time, and is a sign of the congestional state of some organs, and not the consequence of a regular febrile process. What is more remarkable, and belongs especially to Yellow Fever, as a type, is the absence of parallelism between the number of pulsations and the degree of heat. In the first days always, and generally throughout all the disease, (this is again an observation due to Dr. Faget) the pulse decreases regularly morning and evening, whilst (as Dr. Touatre has shown in his charts) the temperature rises and descends, regardless of the line of the pulse. This absence of parallelism, between pulse and temperature, that of an exacerbation at a determined period, and of a regular evolution, during a certain number of days, give us the difference between the two types.

In Dengue, the sudden invasion of the first paroxysm, culminating rapidly to an hyperpyretic temperature, produces a chilly sensation in the same way that chill happens whenever fever comes to extreme heat in the course of a few hours. The slow defervescence that follows, the remission, and effervescence that precedes the exacerbation, constitute altogether a period of calm and relief for the patient, who realizes two paroxysms, separated by a period of diminished pyrexia. The physician who was not present at the invasion, and did not follow the course of the disease with the thermometer, would be apt, on this information, to diagnose an intermittent malaria. But compare the charts Nos. xxi., xxii. and xxiii., copied from Wunderlich. There, each paroxysm lasts from six to twenty-four hours, the effervescence and defervescence are each equal and critical, that is, fall in one nearly per-

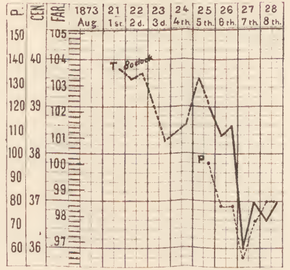
No. I.
Female Born N. O., 14 years old.



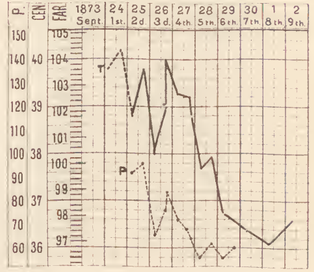
No. II.
Female N. O., 10 years.



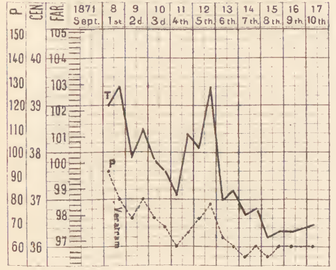
No. III.
Female N. O., 34 years.



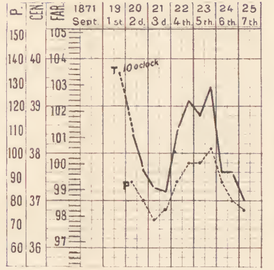
No. IV.
Male Kentucky not acclimated.



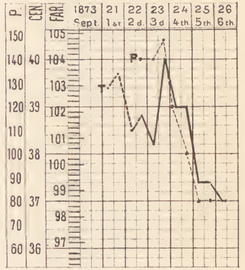
No. V.
Male North U. S., 29 years, not accl.



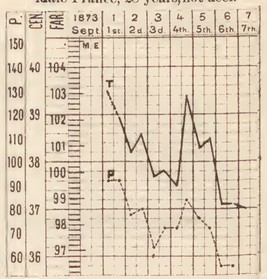
VI.
Female N. O., 14 years.



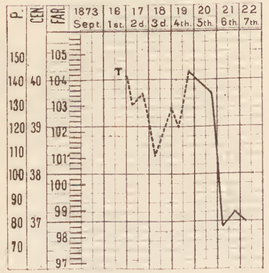
No. VII.
Male N. O., 6 years.



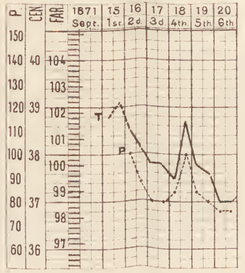
No. VIII.
Male France, 28 years, not accl.



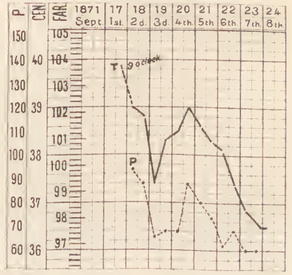
No. IX.
Female N. O., 13 years.



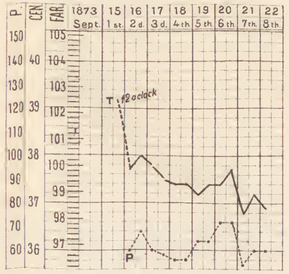
No. X.
Female N. O., 7 years.



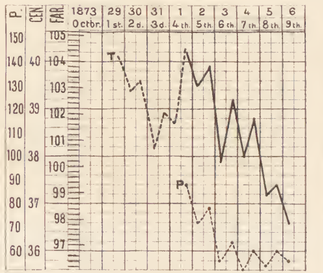
XI.
Female Louisiana, 40 years.



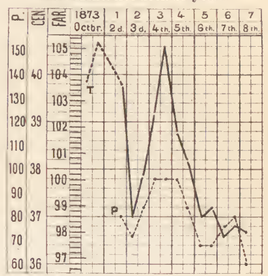
No. XII.
Male Louisiana, not accl. 45 years.



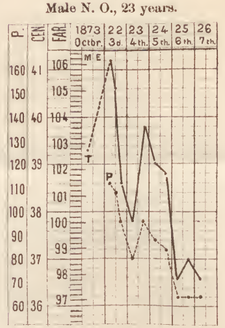
No. XIII.
Male N. O., 19 years.



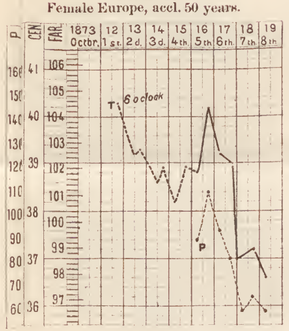
No. XIV.
Male Kent. not accl. 23 years.



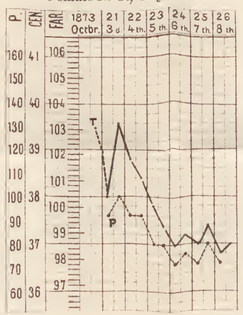
XV.
Male N. O., 23 years.



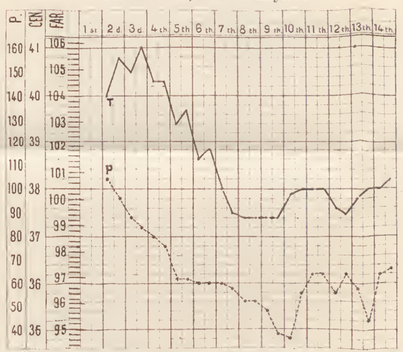
No. XVI.
Female Europe, accl. 50 years.



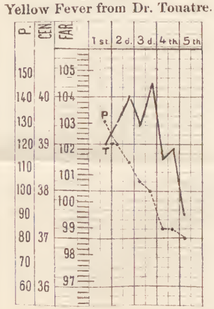
No. XVII.
Female N. O., 40 years.



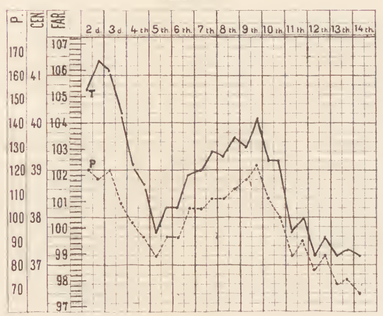
No. XVIII.
Yellow Fever, from Dr. Layton.



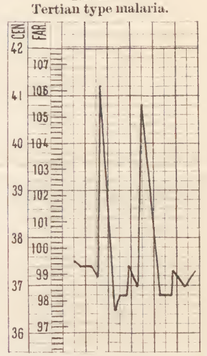
No. XIX.
Yellow Fever from Dr. Touatre.



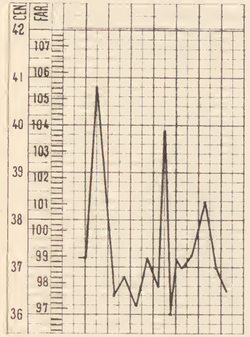
No. XX.
Varioloïd.



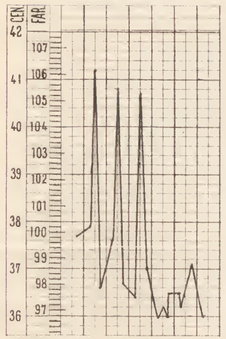
No. XXI.
Tertian type malaria.



No. XXII.
Quaterly type malaria.



No. XXIII.
Daily Malaria.



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H. Wehrmann, Lith. 30 Beekman Alley N.Y.

pendicular line. Each paroxysm is separated by a period of real apyrexia, varying from twelve hours to three days or more according to the types; there is an unlimited number of paroxysms. With Dengue, the paroxysms last a longer time, from forty-eight hours to three days, their effervescence and defervescence are sometimes lytic, sometimes critical; the number of paroxysms is unlimited; they are not equal one to the other; there is no period of real apyrexia, only a calm and remission, and though the disease evolutes in five days, its periods do not show the same regularity as those of malaria.

The authors have described a second exacerbation of Dengue. In some cases, it seems as if there was really a third paroxysm. Observation No. xiii., which was a severe case, and terminated thirty-six hours later than the regular cases, shows that it was due to the defervescence of exacerbation, which, instead of being lytic or critical with small evening rises, was intermittent with a great difference between the morning and evening temperatures.

The exacerbation seems to have missed sometimes. We see upon examination of observation No. xii., that this absence of second paroxysm, must have been only apparent in many cases, where observations with the thermometer were not taken. After the invasion, the temperature was never febrile, but remained sub-febrile, during the five full days. We find a remission on the fourth day, an exacerbation on the sixth, and a fall below normal early on the morning of the seventh day. The invasion having taken place at twelve o'clock on the first day, we count five full days, and a fall below normal on the following morning for the complete evolution of this case, whose chart of temperature represents in miniature the same figure as that of other cases of Dengue.

Observation No. xiv., would simulate at first sight an intermittent fever; for the defervescence of first paroxysm is rapid and reaches the normal temperature, and the excursus of effervescence and defervescence of exacerbation are equal to each other, and composed of three observations each. But remark that the remission is so short, that in a few hours temperature rises to 100° *Far.*, 37.8° *Cent.*, the effervescence takes thirty-six hours to rise to the acme of exacerbation, and the defervescence the same number of hours to come to the normal point; so that the whole paroxysm measures seventy-two hours, which is quite uncommon for a spell of Malaria.

PROCEEDINGS OF THE NEW ORLEANS MEDICAL AND SURGICAL ASSOCIATION.

CASES REPORTED BY JOSEPH HOLT, M.D.—*Case No. I—A Singular Tumor occurring during Gestation, and supposed to be of Traumatic Origin.*

Gentlemen: About six weeks since, I was called to see Mrs. L. in labor, at term, her second child, vertex presenting, l. o. a. General condition excellent, and labor in every respect normal. After an easy and rather rapid second stage, she was delivered of a girl weighing seven pounds and a half.

During descent the scalp over the vertex was wrinkled, but no caput succedaneum formed. The child, after delivery, was found to have over its right occipital protuberance a fleshy tumor, as large as a hen's egg, covering an area of skull two inches by one inch and a half, freely movable over the bone, but involving the scalp, and sufficiently pendulous to press down the brow and upper lid, closing the eye.

Examining it closely, I discovered the whole tumor to be excessively vascular, the venous element seeming to predominate, as was particularly manifested by its becoming turgid, cyanotic, and enlarged, whenever the child held its breath, as in straining or in crying. The right temporal artery, greatly enlarged and pulsating forcibly, entered its base. Over its upper aspect was a superficial or cutaneous nævus, one inch and a half by one inch in extent. The vascular development of the tumor, and perfect adaptation to it of the scalp without stretching, proved it to be of intra-uterine origin and of long standing.

History.—Gestation had progressed from beginning to end pleasantly, with but a single mishap; and to this I attribute the tumor. About the middle of the eighth month the lady picked up her boy, about three years of age, and held him clasped to her bosom. He being fretful and becoming impatient, kicked violently and screamed to be let loose. He happened at the time to be shod with a pair of heavy metal-tipped shoes, and in his fury drove the point of his toe against his mother's abdomen, on her right side, just above the middle of Poupart's ligament. The blow hurt her exceedingly. She instantly dropped the boy, and placed her hands over the point of injury, and felt there distinctly the smooth globe of the child's head. Her impression at the time was that the child must surely have been injured.

In order to avoid unnecessary shock, before allowing her to see the child I apprised the mother of the existence of the deformity, which was certainly hideous in appearance. She being remarkably intelligent, I explained to her fully my opinion: first as to its traumatic origin. The position of the child at birth was

such as to make the tumor exactly correspond with the point of the mother's person receiving the blow. It is a fact recognized by every accoucheur that in very many instances, the fœtus being of full size, the liquor amnii not abundant, the occiput will maintain its position to the left or right during the last six weeks of pregnancy. I expressed my belief that the tumor began as an extravasation, for the most part gradually absorbed, the remainder becoming partially organised; that the vessels ramifying through the injured tissues had undergone excessive development, furnishing eventually the principal factors in its structure; and, finally, that it would disappear, under appropriate treatment, in the course of two or three months. The prognosis has thus far been verified, the tumor having diminished two-thirds.

The treatment has been, the application with a hair pencil, four times daily, of a solution of Liquor Ferri Persulph, fort., in Glycerine, equal parts of each. I tried gentle elastic pressure, but soon discontinued it, because of the fretfulness occasioned.

There are two points of interest in this case: First, the prognosis. Tumors about the head, developed during gestation, are not frequent; and in the majority of instances, indeed, nearly always, disappear during the early months of infantile life. Surgical interference is never warranted, until time at least has demonstrated the permanency of the growth, and until the age and strength will permit an operation.

The second is of especial interest to the obstetrician himself: Had the case been such as to require the use of the forceps, no amount of argument, nor of demonstration, could have convinced the family and friends, headed by an ignorant midwife and a few old aunts, but that the instrument, perhaps unskilfully used, was the sole cause of the deformity. Dr. So-and-So used instruments with Mrs Johnson, and her child was not marked at all. Thus the innocent physician finds himself damned without defence; and doubly, too, if there happens to be called, surreptitiously or in consultation, a so-called brother, but really one of those professional wretches who damn by silence, or by vague remarks remotely equivocal.

Such an unfortunate issue is by no means a conjecture of the fancy; this very lady suggested it to me by remarking, that had instruments been used I would have received all the credit and plenty of blame.. Knowing the influence of some of her gossiping friends, she inquired of me "how in the world" I could have defended myself against such an injurious charge?

Case No. II—Fatty Degeneration and Rupture of the Heart.

Gentlemen: I wish to relate to the Association some interesting circumstances connected with the sudden death of one of our most esteemed citizens. About the 18th of April I was called to Mr. S. J., an elderly gentleman, nearly seventy years of age. I

had attended him some months ago for a mild attack of cervical neuralgia.

This last seizure was apparently of the same nature, except that branches from the brachial plexus on each side were involved. It seemed to have been induced by his having stood for several hours in wet shoes on a cold floor. The administration of an anodyne, containing Chloral and Battley's Sedative, at night, and Quinine during the day, constituted the medication. He progressively improved until the morning of the fifth day, when he appeared convalescent. Every evening, about six o'clock, during the four days of treatment, there was a recurrence of the paroxysms, each less severe than its predecessor. This is mentioned, as furnishing that portion of his previous history with which we are interested for the present.

Now, as to the manner of his dying. Early on the morning of the 22d, he dressed himself and partook of a light breakfast. During the forenoon he amused himself in walking about the house, in reading, and in conversation with Mrs. ———, his daughter-in-law. About 4 p. m., feeling weary, he determined to lie down and rest. At this moment the lady was called down stairs. She had hardly reached the lower hall, when hearing an unusual sound above she ran up in haste to her father's room, and discovered the old gentleman lying on his bed, gasping, and in the agony of death. He looked at her intelligently and endeavored to speak, but could not. I happened to be passing at the time, and hearing the alarm, ran in and reached his bedside just at the closing of this terrible scene. He was pulseless, deathly pale, gasping, and rapidly sinking into unconsciousness. In less than a minute after he was dead. The time, from the beginning to the end, was about four minutes. The question from all around now asked was. "What was the matter with him?" To answer this required deliberate thinking. An absolute reply was impossible; but from negative evidence, or rather by exclusion, I was presently enabled to give an almost positive opinion. In the first place, the suddenness of his death located the cause in some one of the great vital organs. It was not in the brain; there had never been the slightest indication of any lesion of that organ; the manner of his dying, his intelligent look, his trying to speak, his placing himself after his seizure in his usual position in bed—all precluded the idea of apoplexy. His lungs were perfectly healthy. It could have been no valvular lesion of the heart, inasmuch as there had never been the slightest indication of heart trouble. But yet, all the positive evidence in the case pointed to the heart as the centre of disaster. There was but one lesion of the heart that could harmonize with his death, fatty degeneration with rupture. His advanced age, and sedentary life for many years past, together with a manifest disposition to general formation of fat, sustained this theory. Having expressed this as my positive opinion, viz, death from fatty degeneration with rupture of the heart, I urged

the importance of an autopsy. This was readily granted, as is frequently the case in the higher circles of society. Drs. Holliday and Herrick assisting, we proceeded to the examination about thirty-six hours after death. Upon raising the sternum, the pericardium was exposed, greatly distended. Opening it, we discovered an immense black clot of blood enveloping the heart. Exposing this, we found the left ventricle torn open from point to base. The fissure, as we saw it in the contracted state of the heart, was two inches in length. Fatty degeneration of the organ was so complete the finger was pushed with ease through the thickest part of its wall, and when withdrawn was smeared with grease. Nearly an inch of the apex, when cut away, was found to be almost pure fat, containing hardly a trace of muscular fibre.

In conclusion, and as suggested by this case, allow me to direct the attention of the Association to the necessity of more frequent post mortem examination in cases of sudden death, and also in more deliberate ending wherein the nature of the disease is obscure. Negligence on the part of practitioners in this country has much to do with the prejudice of the laity in this particular, simply because the latter are not educated to it by custom. They are seldom asked to permit it, and as seldom have the importance of it explained to them. One thing is certain, such investigation, though a stern teacher, is the surest in giving fixedness to our knowledge. It is the final arbiter, confirming diagnosis, reproving us of errors, oftentimes unveiling the mystery of sudden death, and sometimes of murder.

On the Action of Digitalis when combined with Veratrum Viride in certain diseased conditions of the Heart. By Warren Stone, M. D.

It is an aphorism fraught with the deepest significance, which says that we cannot accurately determine the therapeutical action of medicines independent of pathological conditions, and it certainly must be a want of proper appreciation of this fact which accounts for the differences of opinion as to the application of many of our most valuable remedies. The difficulties attendant upon the drawing of nice distinctions in pathology—distinctions which go to make up a sum of differences that influences very materially the ultimate issue as to life or death—add such innumerable obstacles to the advancement of therapeutics to that state of perfection which could challenge dispute, as to somewhat discourage the practical as well as conscientious thinker on this subject, in his attempt to delineate in their proper and unmistakable colors what to his mind seem to be facts. I will, however, undertake the task of adding fresh testimony in support of the

action of Digitalis when combined with Veratrum in certain morbid conditions of the heart.

The idea of this combination originated with the late Professor Warren Stone, a few years before his death, and he wrote a brief article on the subject which was published in New York. Little or no attention having been paid it, I trust a revival of what I deem so valuable a subject will not prove amiss.

Much has been written about organic disease of the Heart. The symptoms, modes of diagnosis under acoustic and hydrostatic laws—treatment only carried into palliation – all have been well and nobly described, only, however, after irreparable damage has been done; but little has been accomplished towards recognizing many causes that gradually lead on to the horrid consequences of organic cardiac changes, and thus learning to anticipate. I, of course, refer not to such causes as inflammations, atheromatous degenerations, but to that chain of causes which, I believe, is equally as frequent; I mean functional disturbances which ultimately result in the most serious changes in the texture of this most valuable organ, even if not eventuating in sudden death.

I venture to say that, were we to accurately secure the true histories of a very fair majority of cases of heart disease, we would find them to have been *ab initio* functional.

It is to this fact that I wish to draw attention, for a clear recognition of it puts us on our guard and enables us to anticipate, which in the end means everything.

The relation of a few cases will perhaps better enable me to practically elucidate the views I advocate than a set sermon on the subject.

Among the earliest cases in which was observed the action of this combination were the following, to which I think reference has already been made in print. A gentleman whose business required of him to lead a rather confined life, became, under this regimen, a dyspeptic, and was at times subject to what so frequently occurs and is generally passed over as naught—palpitation of the heart. One day, after committing imprudences at the dining table, it happened to him to be subject to severe emotional excitement, which produced the most violent, tumultuous, and irregular action of his heart. The irregular, or, if I may so term it, *choreic* action of the organ, was such as to muffle all the natural sounds which auscultation gives, and it was not long before marked signs of obstruction to the circulation were made manifest. Gastric engorgement, the result of his dietetic imprudence, was relieved by small doses of calomel, followed with saline. The same heart symptoms still continuing, stimulants were given, but to no avail. The heart's agitated and feeble action still held its own ground. Pulmonary engorgement ensued, until specks of blood were expectorated, the face became cyanosed, urinary secretion was suppressed, and death seemed imminent.

At this time, bad as affairs looked, it was determined to give the digitalis and veratrum. Accordingly, one fluid ounce of the tincture of the former with twenty-four minims of Norwood's tincture of the latter were ordered, and a teaspoonful of the mixture directed to be given every hour, its effect being closely watched in the meanwhile.

The pulse soon became more regular and distinct, then diminished in frequency and gained in volume, color and respiration became natural, and the kidneys resumed their function.

The medicine was continued, but at longer intervals, and in twenty-four hours the heart's action was normal. It was feared that there might have been a rupture of some one of the valves in the previous excitement, but examination revealed that everything was organically perfect, and that the proper inference to draw was, that all the disturbance was caused by some nervous derangement which spent its force upon the heart, giving rise to feeble and irregular action. The digitalis evidently stimulated the organ, and the veratrum controlled its stammering.

This case soon led to its trial in other analagous cases. A lady well advanced in years had been much troubled in mind for some time, and was gloomy and despondent. There was loss of appetite, constipation of the bowels, and she used to have suffocating spells, as the family termed it, which were at first taken to be hysterical in their character, and such remedies as Hoffman's anodyne and assafœtida were administered, but with no benefit. A few days after, on visiting the house, she was found in one of her so called suffocating spells, and on examination there was found the same feeble and irregular action of the heart as characterizes cardiac asthma, or what, as I would rather term it a chorea of that organ, as just described, organic lesion of the heart was at first suspected, although auscultation revealed nothing but muffled sounds such as would come from such an organ while stammering. Twenty minims of the tinct. digitalis, with three of the tinct. veratrum were ordered to be given three times a day. The relief was prompt and the medicine was continued for some time after in smaller doses, as we looked for some permanent effect. Some weeks after she was seen and found perfectly relieved, only at times having, as she termed it, a slight quivering in her breast, which was at once relieved by a dose of her medicine. Not long since, a gentleman about middle age came to me suffering with an attack of spasmodic asthma, as characteristic as I ever saw, for which I ordered a palliative, with directions to call the next day. Accordingly, he did, and gave me the following history: About two years ago he began to suffer with spells of neuralgia of the head and face, which would last for a day or two and then leave him as suddenly as they came, giving him a perfect respite for a month or more. About six months after his neuralgia left him, as he says, of its own accord, to give place to the spasmodic asthma, and since then they had alternated one with the other. When he would be

having his periodical attacks of neuralgia, he would be free from the asthma, and vice versa. I will here mention that the palliative given him when I first prescribed for his asthma consisted of a full dose of quinine, combined with fifteen drops of Battley's Sedative, and one fluid drachm of Hoffman's Anodyne, which secured perfect relief.

Acting upon the hint given me by this result, I concluded that his asthma and neuralgia were partners, one relieving the other in their morbific work, and placed him upon arsenic, combined with iodine and the iodide of potassium, in small doses. Everything went on smoothly for some weeks, when one day he came staggering into my office, gasping for breath. At first sight, I of course thought he was laboring under another attack of his spasmodic asthma, but I soon found a different state of affairs. Not an asthmatic sound discernible, but a typical case of what I have termed chorea of the heart—a term I think well calculated to convey the idea of its action under this peculiar condition. He had been suffering for twenty-four hours. Did not seek advice, thinking he was only on the verge of one of his attacks of asthma, and, accordingly, took the prescription just referred to as his palliative. It did no good, and in that short space of time so great was the interference in the circulation that œdema of the feet had commenced, and the right side of the heart so crowded as to give rise to jugular pulsation. Ten minims of the tinct. digitalis and two and a half minims of the tinct. veratrum were ordered to be taken every hour. He began to feel relief after the first dose, and when four had been taken he was resting quietly. The next day the clicks of his cardiac valves were as clear and distinct, and the action of his heart as rhythmical as one could wish.

We will often find in organic diseases of the heart, where auscultation gives us a loud murmur, that the patient will suffer little or no inconvenience until this nervous disturbance sets in. The loudness of a murmur is no guide as to the amount of organic trouble. This can only be estimated by the results of the obstruction.

Therefore, I say that much can be done towards comforting patients and prolonging life, by watching this functional trouble when it supervenes upon organic lesion, but much more can be accomplished if we are equally as vigilant in detecting and controlling what is purely and essentially a functional disorder. Be the cause what it may, while it is essential for us to remove it if possible, it behooves us on the one hand to avert imminent danger, and on the other, all possible damage to so important a member. Patients may die in an attack of cardiac asthma, or if that be not the result, there is always more or less damage done to the organ, and if the attacks are not controlled dilatation will ensue.

Let me mention one more case in illustration of this. I had an opportunity of watching this patient daily during his severe at-

tack, which occurred about 5 or 6 years ago, and as he is still living here I see him from time to time.

The gentleman had been subject to irregular action of the heart for some years before it culminated in the serious trouble to which I now refer, but little or no attention had been paid to it. While engaged in political pursuits, he was for a period of some months subjected to great excitement, and his attacks of palpitation became more frequent and serious, until finally dilatation took place to such an extent as to produce insufficiency of the tricuspid valve, and all the other most aggravated symptoms of obstruction to the circulation.

To have seen the case as it then stood, any one would have pronounced it a case of organic disease in its last stage.

To make the story short I will say, that under the steady use of digitalis and veratrum all the trouble ceased. The right heart contracted down, jugular pulsation ceased, and since then he has been able to attend to a large business without any trouble. At times he will feel the irregularity, but a dose of his medicine soon controls it.

Divulsion in Stricture of the Urethra. By Samuel Logan, M.D.

The subject of Urethral stricture is an extensive one, quite too extensive for a paper the reading of which is to be accomplished in one evening. When a writer whose style is so condensed as that of Sir Henry Thompson, requires a volume of considerable size to do justice to the subject, and when our own agreeable, practical and sagacious Gouley, of New York, also demands that space for its proper discussion, it would be futile for me to attempt to express my own peculiar views concerning it—in its totality—in such limits as would be appropriate on this occasion.

There are few surgical affections which cause more suffering, and which have, partly on that account, and partly because it presents such favorable opportunities for affording effective relief, received more attention. It has always engaged my most careful consideration.

Much has been accomplished in this field by modern surgery. We may, indeed, affirm that much hitherto in dispute has been settled of late years. But we still have some points of considerable importance—"sub-judice." This remark applies more particularly to the treatment, and it is to one of these points that I propose to invite your attention on the present occasion.

While it will be inexpedient to consider in detail the whole subject of the treatment of Stricture of the Urethra, it will nevertheless be advisable in order that I may be clearly understood in what I propose to advance this evening, that I should preface my remarks with a condensed statement of what I consider the general principles of treatment most worthy of approval.

In the first place, I would lay down the important proposition, that a tight, hard stricture, as a rule is incurable. At first glance, this statement may appear both startling and misplaced; startling, from its apparent contradiction of well known facts of familiar and numerous so-called cures after treatment, and misplaced, because it appertains more to the prognosis than the treatment of stricture, while I started with the avowal that my theme this evening referred to its treatment only.

But let me explain myself. Admit for the present the incurability, in its full sense, of stricture, and the corollary at once follows, that the dilatation which has been practiced for the relief of the patient must be a life time business, or the trouble will return; if this be so, then the proposition bears directly on the treatment, and hence my excuse for introducing it here. Every surgeon of much experience will agree with me, that we cannot be too explicit, previous to discharging the case, in urging on those who have had such strictures the great importance of continuing to pass instruments at intervals, even for the balance of their lives. Cases requiring these instructions cannot be considered absolutely cured. They are not. There is a constant tendency to a recurrence, slight, it may be, and in some cases showing itself in palpable results only after a long time, but I believe always present. "Once organic stricture, always organic stricture," as Pemberton expresses it. We have dilated, but not absolutely cured the stricture, and the patient must keep up the dilatation. Now let it be distinctly understood that the method of treatment to which I propose to call your attention this evening, as appropriate to certain forms and certain cases only, claims to do no more than dilate, not cure in the full sense of the latter term; in other words, it claims to do no more than any other plan, which has been adopted for the like purpose. As specially illustrative of the, in this sense, incurability of tight organic strictures, I will refer to case No. iii., reported in this paper, as well as to the general literature of the subject.

My second propositions is, that all sound systems or plans of treatment are definable by the single word—dilatation. In short, this is the one chief indication in the treatment of organic stricture, i. e., to dilate the coarctation to the full extent of the normal urethral diameter when the canal is distended, and to keep it so dilated. All plans of treatment must in the end point to this object.

We hear of the treatment of stricture by dilatation, by divulsion, and by incision, external and internal; but, nevertheless, the very writers who thus classify the treatment never fail to enjoin on the reader the necessity after both disruption and incision, of persisting in the use of dilating instruments. The divulsion or the incision as the case may be, is then merely an adjunct to the dilatation. Now this is no unnecessary refinement. The prompt and complete relief of distressing symptoms after the rupture or

incision of a stricture is often deceptive to the patient, and sometimes to the inexperienced surgeon. The patient is so quickly relieved that he can hardly be persuaded he is not permanently cured, while the seeming cure sometimes continues for so long a period, that the surgeon himself may be deceived. One of the smallest strictures that I have had to treat this year had been thoroughly dilated 17 years ago by the distinguished Ricord; gave the patient no trouble whatever for 10 years, and then took 7 years to become reduced to such a size as to excite serious alarm.

My third proposition is to the effect that the plan of gradual dilatation should be the rule, and a resort to the other expedients the exception, in the treatment of strictures. I am aware that some surgeons of experience and deserved authority do not admit this. I do not propose on the present occasion to enter into the merits of the discussion, but presume it will be admitted that the weight of authority preponderates largely towards the adoption of this proposition as an axiom in urethral surgery.

Of course there are many other points in the treatment involving minor details and relating to accident and complications, which may be considered as equally well established as the above. But I think these three propositions will, in a general way, cover over the whole field. With this general survey of the subject, therefore, I am prepared to pass on to the more particular consideration of divulsion as one of the means to be resorted to in certain cases of stricture. My views are illustrated by the following cases.

Case I.—B. C., colored, aged 40; in good health till about 2 years before consulting me when began to suffer from symptoms of stricture; underwent treatment from several "physicians" for "gravel;" that scapegoat, almost as useful in genito-urinary diseases as "dyspepsia" in gastro intestinal troubles. The difficulty increased till, after violent straining 3 weeks before coming to me, he felt more than usual pain, and the next morning he perceived a hard swelling just behind the scrotum, which had since gradually increased.

Consulted me June 10th, 1869. A hard tumor, decidedly circumscribed, about the size of a myrtle orange or a small apple, presents itself in the above mentioned locality. Passes a very small stream: A tough and grisly stricture is found in the bulbous portion of the urethra, which only admits No. 1 steel sound. This was retained 10 minutes.

June 13th. Had experienced no irritation from operation; size of stream slightly increased; passed No. 1 steel sound through stricture again. The method of gradual dilatation was then continued for three weeks, the instruments being usually passed every other day. No. 4 then easily passed, and the tumor had gradually subsided to about the size of a small English walnut. Any attempt to pass a larger number, however, produced much pain, and was followed by considerable irritation and some hæmorrhage. This number was therefore adhered to for two

weeks more, the patient being so comfortable as to have resumed work as a labourer. The tumor subsided to a mere nodule the size of a marble, but no larger instrument could be passed, soft as well as solid bougies having been tried. The stricture grasped the former so tightly as to leave marked indentations in the instrument. Divulsion with the medium sized cylinder was performed in the manner and with the instrument to be described. Some 15 or 20 drops of blood escaped from the meatus. A No. 9 steel bougie was readily passed into the bladder. Not the slightest local or general trouble was experienced. No. 9 and afterwards No. 10 were passed every 3 or 4 days; the patient was instructed in its use, and charged to pass the No. 10 once a week for 2 or 3 months; then once in two weeks, once a month, &c., and was discharged with the usual cautionary advice three months after the treatment was begun. I occasionally meet him on the streets, and he reports himself in very good health.

Case II.—B. S., white; aged 37; had gonorrhœa several times "before the war" and once in the first year of it (1861): was treated for stricture in the army in 1862. The surgeon had only one "medium sized" gum instrument, which, after many trials and much bleeding, was passed, and its use was continued for about two months, when he was informed that he was cured. Had no more decided difficulty for about 2 years, though feeling that he was not "all right." His trouble now returned, and he was obliged to undergo gradual dilatation again. This time the surgeon got his canal up to the calibre of No. 10, and discharged him as cured, assuring him that he would now have no further trouble. In about 3 years he was obliged to resort to another surgeon for relief, the symptoms having again recurred to such an extent as to alarm him. This time he was under treatment by gradual dilatation for about 4 months, when he was again discharged with a capacity of No. 10 and instructed to report again if he had any more trouble; not a word of caution was given him as to the necessity of keeping up the dilatation.

Oct. 10, 1869—Applied to me with the above history, and suffering from difficult micturition. No. 2 English scale gum instrument could barely pass a stricture just in front of the bulbous portion of the canal. It was exceedingly tough, hard and annular. It was highly sensitive, and the patient was nervous and somewhat excited. The urine however contained no albumen or tube casts, and there had been no symptoms of vesical or other trouble. I tried gradual dilatation for three weeks, only being able to pass the instrument every 4 days. I gained but one size during this time, and then determined on divulsion. I used Gouley's dilator, opening the blades pretty fully. In withdrawing the instrument it came a little and was arrested, evidently by having caught some fold of tissue between its blades, although I had not closed them entirely. I had some difficulty in disengaging the instrument, as will be explained further on. There was no serious trouble, however, in the after treatment, No. 10 gum being passed

with ease, and he was duly given the necessary caution and instructions to guard against recontraction. His dearly bought experience has still failed to bear satisfactory fruit, for on meeting him about a month ago he informed me that he had not passed the instrument for a year and over;—he "felt so well that he had forgotten to do so."

Case III.—C. S., age 25; white; clerk. Stricture for about one year, gradually supervening after gonorrhœa and giving more and more trouble; a constant slight gleet.

Consulted writer Jan. 15th, 1870; urination gives great trouble, though still attending to duties; gleet; frequent micturition, great straining, very small stream. Examination with olive-pointed bougie discovers stricture 3½ inches from meatus through which No. 2 English grade is passed after careful and gentle efforts. Gradual dilatation was practiced till the canal admitted No. 5, beyond which point it was very difficult to progress, the parts becoming very sensitive, even after 4, 5 and 6 days intervals between the operations. Divulsion was performed (Feb. 25th, 1870), using the largest cylinder. A few drops of blood escaped. He was taught to draw his water with a gum catheter, which he did for 3 days after the divulsion. On the 5th day I passed No. 12 metallic, and repeated its introduction at intervals of 3 or 4 days for a month, when he was cautioned as usual and discharged perfectly relieved.

Case IV.—Dr. J. D. B., from the country consulted me, Feb. 20th, 1870, with an irritable, tough, annular stricture 1½ inches from meatus. He had been suffering from it for three or four years, partially dilating it himself, and permitting it to recontract, partly from carelessness and partly from the great pain accompanying every renewed effort at dilatation. The stricture admitted only No. 3 gum, and there was much troublesome micturition. I recommended either urethrotomy or divulsion, and was glad when he chose the latter. We tried gradual dilatation with gum bougies, however, for 2 weeks. The excessive soreness and painful micturition were unabated, and we had only advanced one grade at the end of that time.

March 9th, divulsion performed with the next to the largest cylinder of Holt's instrument. The urine was drawn off with a gum catheter every 3 or 4 hours for 2 days. On the 2d day a slight attack of urethral fever came on, which however, yielded readily under the use of opium and quinine. There were some swelling and heat about the penis, but the patient was on the whole more comfortable, except when actually in the febrile condition, than he had been for a long time. Gradual dilatation was resumed on the 5th day, and was well borne; and the patient took charge of the case himself, leaving the city three weeks after the operation. Two months afterwards he reported by letter that he had no trouble in keeping up the calibre to No. 11 (of scale 12 in No).

Case V.—J. F. colored; 45 years of age; has been troubled

with difficulty "about his waterworks" for 2 years; taken quantities of medicine from all sorts of doctors; never consulted a surgeon before. Consulted me July 31st, 1872. Found one of the toughest strictures I ever felt; it was situated at the bulbous portion. Only No. 2 could pass. Tried gradual dilatation for over 3 months; only gained 2 grades, and after another month's trial still effected no further progress. I would say here, that in all cases where I try the method of gradual dilatation, i. e., in the vast majority of cases, I proceed very slowly and cautiously; lengthening the intervals, if necessary, to 5, 6, 7 and even 10 days in irritable cases; never passing the instrument oftener than every other day. No case will be benefitted by daily operations. I fully agree with Sir Henry Thompson in this respect. But to resume. Seeing that gradul dilatation had done all it could, I operated by divulsion, Nov. 10th 1872. I used only a small cylinder, and No. 8 solid sound was passed immediately after. On the 3d day, No. 8 failed to pass, and I was obliged to content myself with No. 7. The gradual dilatation was resumed at this point, but I could get only one grade higher. Feeling perfectly comfortable, he could not be persuaded to attend regularly, and gradually neglected to put in an appearance at all. I regretted not having ruptured this stricture with a large cylinder at once. The fact is that the coarctation presented so much of a tubular or "tunnelled" character, perhaps a half inch in length, that I was fearful of making too large a rent. I am now convinced that to secure the full benefit of the method the divulsion should always be made with the largest cylinder, or at least the second in size.

Case VI.—A. P., rather delicate; aged 50; white; consulted me March 10th, 1873, on his way to his home in Tennessee from a visit to Texas. Has had a stricture for nine years, which has at times given him much trouble. Occasionally it has been partially dilated; never thoroughly. For the last two weeks micturition very slow and painful; has now passed no water for 12 hours, and cannot do so, though the desire is very urgent. Only a capillary whale-bone guide can be made to enter the bladder. Passed Holt's modified instrument over it, and ruptured with the next to the largest cylinder. Immediately on withdrawing it passed a No. 11 gum catheter, and drew off about a pint of urine. Patient continued for 2 days to draw off his urine at such intervals as desire prompted. Not a bad symptom supervened, and at the close of 4 days he determined to remain in the city no longer. Feeling perfectly comfortable he resumed his journey home. Of course I gave him my usual cautionary advice as to keeping up the dilatation. I have not since heard from him.

The notes of the next three cases operated on by me at the Charity Hospital were taken by Mr. E. M. Miles, interne, to whose judicious and faithful attention to the treatment the satisfactory results are greatly due.

Case VII.—Patrick H., aged 45 years; admitted to Charity Hos-

pital, ward 4½, Oct. 2d, 1873, with stricture of the urethra of 12 years standing, following an attack of gonorrhœa. Seat of stricture just in front of membranous portion of urethra. Twelve months before admission stricture closed and a perineal fistula formed. On admission urine passed through meatus by drops, the greater quantity passing through the old fistula. Until Oct. 17th, no instrument had been introduced. On morning of 17th, divulsion was performed after a capillary whale-bone guide had been insinuated with some difficulty through the stricture. Hemorrhage slight; No. 10 metallic bougie passed at once. On third morning after operation No. 9 metallic bougie passed with some bleeding, which never occurred afterwards. Oct. 30th, No. 9 metallic now passes with ease.

November 14th.—No. 10 gum elastic bougie passes readily.

The fistula healed gradually during the after dilatation. For some time patient (now a nurse in the hospital) has been passing himself a No. 10 metallic bougie.

Case VIII. Jeremiah S., aged 28, admitted to ward 3, Charity Hospital, January 23d, 1874, with stricture of urethra at junction of bulbous with membranous portion. Patient gives history of gonorrhœa three years ago, followed by gleet of twelve months' standing. In March, 1873, patient began to show symptoms of stricture, which became more and more aggravated until December, 1873, when he was unable to pass water except by drops. About this time incontinence of urine—really overflow—began, and has continued up to time of operation.

January 31st, 1874; divulsion performed with slight hemorrhage (teaspoonful). No. 10 metallic bougie passed at once. On third day after operation No. 9 metallic bougie passed with some bleeding. The hemorrhage never occurred afterwards on passing the instrument.

April 7th.—No. 10 metallic can now be passed with ease.

Case IX.—Chas. S., admitted to ward 3, Charity Hospital, March 18th, 1874, with stricture of the urethra of 9 years standing, following gonorrhœa. Seat of stricture, spongy portion of the urethra, about one and a half inches from meatus. On admission stricture just large enough to admit a small silver probe. On March 23d, divulsion was performed with Holt's instrument. It was followed by very little hemorrhage, even less than in either of former cases. No. 10 metallic bougie passed with no hemorrhage; and there was none ever afterwards.

April 18th.—No. 10 gum elastic bougie now passed very readily.

In relation to these nine cases I would invite special attention to the following points: 1st, They extend over a period reaching from 1869 to 1874, about five years of the most active portion of my professional life. In five years, then, I have been called on to perform this operation only nine times; six in private practice, and three in the hospital. It is, then, in a very small proportion of cases that I resort to this expedient. During that period I have treated in private practice some forty odd

cases, and in the hospital about sixty or a little over. In that time I have performed internal urethotomy but three times, for strictures at or near the meatus. Merely slitting a narrow orifice is not included in these. No death has occurred, and only a few cases of slight urethral fever in some of the cases where gradual dilatation was being carried out, and in case No. 4 as reported. In one hundred cases reported by Stokes, of Dublin, he treated seventy-six by gradual dilation, twelve by internal urethrotomy, and eight by Holt's divulsion. In from ten to twenty per cent. of stricture cases, then, I think you will find it advisable to assist dilatation by other proceedings.

In the next place it will be observed that I have generally resorted to divulsion in preference to cutting either internally or externally. I see no reason thus far to regret the choice.

The method of rupturing strictures is one which, in my opinion, has not received the credit it deserves as a substitute, not for gradual dilatation, but for cutting operations, either external or internal. The great popularity enjoyed for a time by external urethrotomy began to wane even before the death of its distinguished advocate, Professor Syme. My objections to it are the following, and these objections apply more or less to any of the modifications.

1st. There is more cutting—a larger operation—than is necessary.

2d. It is often an exceedingly difficult operation, and the difficulty presents itself in the latter stage of the procedure, after the chief incisions have been made. I allude, of course, to the great trouble sometimes experienced in finding the urethra—occasionally even necessitating the abandonment of the operation by the most skillful.

3d. There is a decided liability to a long continued fistulous canal, sometimes extremely difficult to heal, even though the proper dilatation be kept up. It is not strange that this should be so, when we recollect how much indurated the parts through which the incision has been made have become, as, for example, in Case I. This induration is not confined to the immediate envelopes of the canal, but in cases where urinary infiltration of the parts has occurred it involves all the tissues beneath the skin. Incisions made through tissues thus indurated are necessarily in many cases difficult to close.

It is true that Maston, of Mobile, proposes, by means of a kind of external subcutaneous urethrotomy, very similar in principle and in fact to "le boutoniere," to, in a measure at least, obviate this tendency, but his procedent cannot do so entirely.

My objections to internal urethrotomy are not so strong; I prefer it to the external operation, and even to divulsion, in strictures at or very near the meatus. But when compared with divulsion, in so far as troublesome strictures farther back are concerned, it is liable to the following disadvantages.

1st. As in external urethrotomy, though to a lesser degree,

there is an unnecessary amount of injury inflicted. We cannot confine the cutting to just such limits as are necessary. I cannot admit of the propriety, much less the advisability, as some advocates of this method suggest, of cutting any more than the actually indurated stricture substance. The wound you make in the healthy tissues must itself heal by granulation. But this must add to the existing amount of adventitious fibrous material, and thus tend to increase the pathological condition which is at the bottom of all the trouble.

2d. It is an operation much less easily performed than that of divulsion, and therefore the latter is more suitable for the general practitioner, who cannot be expected to possess the skill of the surgeon in active practice.

3d. It is more apt to produce a perhaps serious and troublesome hemorrhage than divulsion.

Such, then, in a necessarily condensed form, are my objections to external and internal urethrotomy. But I must not be understood as recommending that these operations be entirely set aside. By no means. There are some cases in which one or the other of these plans may be adopted with advantage, as I have already indicated.

I now desire to call your attention to the special advantages of divulsion.

1st. It is an operation requiring no special skill in its performance, though it does require the same degree of skill and judgment in its preliminaries as in like cases which would be subjected to urethrotomy.

2d. But very little more than the diseased tissue is divided. I cannot agree with the special advocates of urethrotomy who contend that near a half inch of the canal before and behind the stricture should be split up. Under such circumstances additional cicatricial tissue must be formed, for the whole of the wound heals, under the subsequent dilatation, by granulation, not by first intention. You have thus added to the case all the elements of a traumatic stricture, a form of stricture often of the most obstinate character. If Oliver Pemberton and others are right in the assertion that new fibrous tissue under such circumstances is not absorbed, even after many years, then the result of any unnecessary cut through healthy parts, which cut can heal only by the slow process of granulation, with a corresponding increase of bioplastic elements, will surely extend along the canal the area which will be subject to the recurrent tendency. This accounts, at least in a great measure, for the fact that we so often find strictures which have recurred after urethrotomy involving so considerable a length of the canal—"tunnelled" strictures, as Teevan calls them. It is more than likely that the acknowledged advantages of gradual dilatation, as compared with either divulsion or cutting, is mainly due to the fact that little or no traumatism is added by it to the pathological condition present.

In divulsion, however, but little more than the indurated tissue

is torn; the normal parts, being more elastic, give way around the instrument. In urethrotomy, on the contrary, the wound cannot be so limited. This more accurate limitation of the effects of the violence to the diseased structure constitutes, in my opinion, the chief advantage possessed by divulsion, as compared with either external or internal urethrotomy.

3d. All the condensed tissue is more apt to be divided than by urethrotomy. The knife may frequently only penetrate and not pass through the whole thickness of the diseased mass, especially if it happen to be directed towards that portion where it is most thickly developed; whereas the divulsion, acting with equal force on all sides of the canal, is more apt to entirely rupture the thinnest portion of the circumferential induration, and thus more fully accomplish the object in view with a less depth of wound. To a force acting centrifugally, the thinnest part, as offering least resistance, must give way first. This accomplishes all we desire, i. e., the complete disruption of the encircling band of induration. In this view of the subject, the instrument possesses, so to speak, a happy power of selection, choosing for disruption just that point where the least amount of injury will be inflicted.

4th. I have yet to find recorded a case of serious hemorrhage after the operation of divulsion; whereas, though rare, it is known to sometimes follow urethrotomy.

Let us now examine the objections which have been urged against this operation.

1st. Objection is made—particularly by Dr. W. Stokes, in the *Dublin Quarterly* for February, 1871—that we have a lacerated, instead of a clean cut wound. But we must remember that the chief advantage of a clean cut is lost when the further treatment of the case requires that the lips of the wound be repeatedly pressed asunder, as necessarily occurs in keeping up the dilatation after the operation. In other words, the clean cut wound can heal only by granulation and cicatrization, and if it be more extensive than the lacerated one, as is apt to be the case in urethrotomy, then there will necessarily be more cicatricial tissue, and the tendency to recontraction will thus be proportionally aggravated.

2d. It is said by some that the lacerated wound would render infiltration of urine more liable to occur. This objection is not based, so far as I have been able to ascertain, on any clinical facts; it is stated as only a theoretical objection. Clinical observation shows that, provided the obstruction be thoroughly divided, there is but little risk of infiltration in either divulsion or urethrotomy, especially if for two or three days after either operation the urine be drawn off at proper intervals.

3d. Another objection to divulsion, in common with dilatation and cauterization, is made in the following language by one worthy of respectful attention. I allude to Dr. C. H. Maston, of Mobile. We must bear in mind, however, that he is quite an

enthusiast on the subject of urethrotomy. In his report to the Medical Association of the State of Alabama, he says, referring to urethrotomy:

"No other operation has furnished more satisfactory results. Dilatation, divulsion and cauterization, each leave the urethra with a tendency sooner or later to recontract and narrow down its calibre; so that none of them are entirely free of objections. Hence, we consider it at least the part of sound judgment to select internal urethrotomy as the one, which, upon rational grounds, offers the best chance of success."

That internal urethrotomy is followed by more lasting results than divulsion, is here advanced as a mere *ipse dixit.* The writer gives us no facts to substantiate the assertion. Indeed, the proper comparative statistics to elucidate this phase of the subject of urethral surgery, are yet to be gathered. In the meanwhile, we cannot accept a simple assertion, from however worthy a source, as anything more than the mere impression or opinion of the writer. If, by the statement as to the liability of a stricture to recur after "both divulsion and cauterization," Dr. Maston would have us infer—an inference warranted by the manner in which he expresses himself—that his favorite method of internal urethrotomy does not leave behind a similar tendency, we have only to say, that the general experience of the profession opposes most distinctly such a belief. Some of the worst cases of recurrent stricture we have, are those artificially traumatic cases, which had been subjected to urethrotomy. "Once organic stricture, always organic stricture," as it is tersely put by Prof. Pemberton, (see his address on surgery, before the British Medical Association) is an axiom to which almost all experienced surgeons assent.

The recurrence is slower, in all probability, for it is hardly possible to prove such a point statistically, in those cases which, during the treatment, have been subjected to the least degree of traumatism. Gradual dilatation, when carefully conducted, should, according to this law—if such it be—stand first; and, as I contend, divulsion, as involving less of the healthy parts than urethrotomy, will probably take the next rank.

4th. On another occasion, Dr. Maston writes: "As to divulsion, on general principles I am opposed to it." The objection is couched in such "general" terms that I might reply with equal propriety in his own words, and say that "on general principles" I would prefer it to urethrotomy. In both cases this language merely amounts to the expression of our individual views, and therefore embodies no argument pro or con.

5th. Mr. S. Teevan, whom we must all acknowledge to be excellent authority, objects in the following strong terms: "Surgical pathology settles the question at once, for we know that there is more contraction after a laceration than after a cut," &c. (See *Lancet* for July, 1871, p. 353, American reprint.) Perfectly true; but why is there less contraction after a cut? Simply because,

as a rule, the cut heals up more promptly, and therefore with less intervening new tissue. It heals, as a rule, by prompt union, while the laceration usually heals more tardily, and therefore by granulation. But do we procure this prompt and more direct union in the parts divided in urethrotomy? If we do, in what respect is the stricture benefitted, puckered up to its previous dimensions as the canal would necessarily be by the immediate reunion of the parts which had been divided? In fact, the after treatment, consisting in the regularly repeated introduction of large instruments, practically converts the incision into a lacerated wound, so far as slow healing and the consequent production of new fibrous tissue is concerned; and "surgical pathology settles the question" that the tendency to recontraction resides in this new tissue, whether it be the result ot chronic hyperplasia, or of traumatic origin.

6th. Sir Henry Thompson ("nomen clarum et venerabile") says, in a recent lecture: "It is an operation generally involving but little risk, although it has been sometimes followed by fatal consequences. My principal objection to it is that the benefit obtained is certainly not very enduring in severe cases of stricture, and it is for such only that operative measures are necessary." This distinguished surgeon elsewhere states, however, that he has not had much experience in the operation, and, as will be shown further on, his instrument is decidedly faulty, and therefore what little experience he had was just such as would be most likely to lead to an unfavorable impression. This will, however, be better understood when I come to speak of the instruments to be used. That the operation should have been sometimes followed by fatal consequences is not strange when we remember, that even simply passing a bougie or catheter has been followed by fatal consequences. As to the results in "severe cases" not being very enduring, we have to say that, in the first place, as performed by his instrument, the results are well calculated to be of only a transient character; and, in the second place, that we must remember that in all operations for "severe strictures" the coarctation is very apt to return, and often pretty promptly, too, if the patient neglects the necessary precautions. No writer has been more emphatic on this point than Sir Henry himself. The comparative permanence of the dilatation effected by this method, as contrasted with other operations, is a matter which is necessarily exceedingly difficult to determine. Cases differ so markedly in respect to the degree of tendency to recur inherent in them, irrespective of any special method of treatment, and the after circumstances, habits and influences vary so very much, that statistics on the subject would necessarily prove very unreliable. To give them any value the tables should be constructed of a vast number of cases, all of which should have been under careful observation for years. The objectors to divulsion have given us no such tables, while its advocates assert that it has met with as permanent success in

their hands as urethrotomy, with which, to a certain degree, it stands in contrast and rivalry.

In every allusion made by Sir Henry Thompson to disruption we observe, notwithstanding his mild objections, a marked undercurrent of approval. Besides what I have already quoted, permit me to make the following extracts from his article on Genito-Urinary Diseases, in Holmes' System of Surgery, vol. iv., pp. 962–3, 2d edition. * * * "An old and obstinate stricture may be dilated up to a certain moderate degree, beyond which the slightest advance sometimes produces severe constitutional symptoms: the same stricture may be ruptured at once up to the natural calibre of the urethra without any such symptoms. I have seen this as the result of Mr. Holt's method, and also from the use of an instrument employed by myself, which distends the stricture to a calibre of 14 or 16 of the catheter scale."

And further on he says:

"To rupture a stricture appears at first sight a harsh proceeding, but a careful and unprejudiced examination of its results on a large scale, and some personal practice of it, convinces me that it deserves the character here accorded to it."

Mr. James Spence, F.R.C.E., in his Lectures on Surgery (Edinburg, 1871), says: "For the last nine or ten years I have practiced Holt's method, and with great success, so that I feel no hesitation in recommending it as at once efficient and safe—that is, as free from danger as any operation on diseased urinary organs can be."

Bumstead, of New York, in the edition of his work on Venereal Diseases published in 1866, uses the following language: "With the very strong testimony adduced in favor of this method, I am happy to reverse the judgment expressed in the first edition of this work, and to state my belief that, in this instance as in many others, experience proves the value of a mode of treatment which at first has been rejected on theoretical grounds." And, after mentioning the fact that Mr. Holt had stated, in a discussion before the Royal Medical and Chirurgical Society (April 14th, 1863), that he had operated on over 250 cases "without any complications of either infiltration of urine, abscess, swelled testicle, or inconvenience of any kind, further than the occasional supervention of a rigor or a mild attack of urethral fever," he further on adds that he had "also had the opportunity of testing Mr. Holt's method in three cases of stricture with the most satisfactory result."

Nunnelly, in his Address to the British Medical Association, published in the *British Medical Journal* for August, 1869, page 145, cordially approves of the operation in appropriate cases. The late lamented Mr. Christopher Heath, a most cautious surgeon and sagacious clinical observer, in an article in the same Journal for July, 1867 (p. 55), suggests that the absence of severe symptoms after divulsion may be explained by the fact that in some cases operated on by Holt's method, and examined after

death from other causes, only the submucous hard stricture-substance was torn.

The question now naturally arises—in what cases should we resort to this operation? I will answer it in but a few words. In all cases of organic stricture where gradual dilatation fails, or is impracticable, except in those at or very near the meatus. In these positions they are quite readily cut, and divulsion is unnecessary.

I must, however, be a little more explicit in my reply to the above question.

In the *first* place, some strictures reach a certain point under gradual dilatation, and then either refuse to yield any more, or become exceedingly irritable even though reasonable intervals are allowed to elapse between the introduction of the instruments. In such cases, as a rule, I would advise divulsion. In the *next* place, we sometimes find that the stricture presents from the first a marked resiliency, which so interferes with the dilatation that hardly any progress is made. If, after a reasonable time—which is dependent in some degree on the circumstances of the case, the facilities for treatment, the amount of leisure the patient may be able to allow himself, &c.—spent in faithful and careful efforts at gradual dilatation with little or no satisfactory progress, or if what progress is made is of only a very transient character, I would resort to disruption in aid of the dilatation.

Again: In cases of retention from tight stricture, where, with great trouble, a small guide only can be made to enter the bladder, I would often resort to this expedient. It is frequently impracticable, or at least imprudent, under such circumstances to await the slow process of gradual dilatation, unless, indeed, a small catheter can without much force be run down on the guide, when it may be possible that, with the stricture thus somewhat opened and the urgent symptoms relieved, no more retention may occur. We must always remember, when deciding the question in such cases, that the engorgement of the parts from the manipulation may itself tend to close the canal. This class of cases usually calls for the exercise of a nice judgment and a delicate discrimination on the part of the surgeon. Here, personal experience is of very great value, while it is difficult to lay down positive rules for our guidance. I would say, however, that if the retention had come on very gradually, if the patient was passing urine by a very fine stream, or perhaps guttatim, for days or weeks before, I would advise the use of the divulsor at once. If, on the other hand, the retention had developed suddenly—say after a debauch, as so often happens—and you ascertain that ordinarily he had been passing a decided stream, sufficient at least to relieve the bladder even though it took an abnormally long time to do so: if a catheter can be pushed into the bladder over the guide, or by other well known means, such as ice in the rectum, warm baths, opium, the aspirator, &c., the organ can be relieved of its contents; then, with continued rest,

sedatives, &c., I would expect that the engorgement of the mucous membrane at the seat of stricture would in a few days be relieved, and gradual dilatation become practicable. In most cases of this description divulsion would be unnecessary. The cases bordering on the line which divides these two classes will often present debatable problems in practice. Fortunately, however, either plan of treatment would usually be followed by pretty good results; or, when the milder plan fails, then the more severe expedient may be adopted. I am almost inclined to think, however, that further experience may warrant us in extending the application of the divulsion much further in this direction than I would at present positively recommend.

A few words now concerning the operation itself.

First, as regards the *instruments*. A detailed history of their invention may be found in the standard authorities, and I need not dwell on this phase of the subject. I may simply state that instruments with expanding blades were probably first used by Luxmoor, in 1812; that some years after Leroy d'Etiolles espoused the same principle, and that M. Perrive did the same in 1847.

Of one point, however, I deem it advisable to take special cognizance. If Mr. Holt did not originate the idea of *forcible dilatation* he did, so far as I can ascertain, originate the idea of *divulsion*. Forcible dilatation is a wretched procedure, in absolute contradiction to all recognized pathological and physiological principles. There is no credit to be attached to it. Disruption, or divulsion by force applied only centrifugally—and not longitudinally also, as by Wakely's method—constitutes the essence of Mr. Holt's plan, and fully deserves the enconiums it has received. These two terms, "forcible dilatation" and "divulsion," have been carelessly used by many of even our best writers, as if they were almost synonymous. They have very different meanings, as we will better understand further on.

A number of instruments for rupturing strictures of the urethra have been offered to the profession. I need not mention all. I prefer that of Mr. Holt, now well known everywhere, but as almost everybody has his own modification of it, I, too, have mine. The nature of this modification I will explain in a few moments.

There are two classes of instruments now used for the rupture of strictures of the urethra, and there is an essential, and in my judgment a very important, difference in the mechanism of the instruments belonging to these respective classes. In one class the force is exerted by a lever of some sort, which expands the blades slowly and with a succession of movements, thus applying the force more or less gradually. I now hold in my hand an instrument of this class. It is that of Professor Gouley, of New York, or rather that of Sir Henry Thompson, modified by Professor Gouley. You will perceive that by turning the screw in the handle the blades are gradually separated. For the disruption of a stricture I object to all instruments of this mechanism.

If the object were to forcibly dilate, it is just what would be wanted; but for divulsion it cannot compare with those of the second class, i. e., those in which the blades are sprung apart suddenly, the force being applied once for all and with a single movement, by means of a cylinder run down over a central rod, as in the instrument I now show you. It is that of Mr. Holt, with a slight, but as I consider it, important, modification by myself, and is the one I prefer. For the idea which suggested the modification I am indebted to Professor Gouley, for you will perceive that it is merely the application of his admirable modification of Sir Henry Thompson's dilator, which he prefers to Mr. Holt's divulsor which I prefer. We are indebted to Professor Gouley, of New York, for the introduction to American surgery of the plan of tunnelling the end of various instruments for the management of very fine strictures. Tubes slid over fine guides had long before been used it Europe, i. e., by Desault, in 1797; by Buchanan, of Glasgow, in 1831; by Hutton, of Dublin, in 1835; and by Maisonneuve, in 1845; and, indeed, Professor G.'s plan is only a modification of the principle of the instruments used by these European surgeons. Prof. Gouley has tunnelled sounds, tunnelled catheters, and a tunnelled dilator, specimens of each of which are on the table. The dilator is, so far as the remaining portion of the instrument is concerned, essentially the same as Sir Henry Thompson's. Nor was the use of very fine bougies for passing through very small or tortuous strictures, and to be used as guides for larger instrument—a new thing in surgery. I need but mention, for example, Maisonneuve's very flexible filiform bougie, whose distal end is so arranged as to admit of being screwed on to the extremity of his urethrotome, or the rather clumsy instruments of Wakely. In Maisonneuve's instrument the conducting bougie is so flexible that it is expected, when pushed in front of the urethrotome, to roll itself up in the bladder. The objections to such a guide are two-fold: it is too apt to double on itself in the urethra, during the attempt to push it in front of the shaft of the urethrotome, and its too flexible character makes it more difficult for the operator to pass it through a very dense and tight stricture, often, at this stage also, doubling on itself. These very fine whale-bone, olive-pointed bougies of Gouley, possess, however, strength enough for the surgeon to use them with confidence, and, at the same time, are sufficiently flexible for all necessary purposes. It very rarely happens that one of these fine and yet strong guides may not, with patience and perseverence, be insinuated gently through the worst stricture. If there are false passages, and a guide becomes engaged in one of them, it should be left there, and a second guide be passed down the canal. If this also goes astray into the same or some other false passage, it should be left in situ, and a third be passed down; and so on till one enters the bladder. If there be any doubt in the mind of the operator in regard to the position of the last passed; then, after withdrawing all the

others, Gouley's small tunnelled catheter may be run down over the guide which remains, and which it is thought has probably entered the bladder. If urine escape, then, in all probability, the guide is in the natural canal. Holt's instrument, modified as I suggest by giving to it Gouley's tunnelled extremity, should then be slid down over the whale-bone guide, with the blades of course closed. If much force be required to do this, it should be drawn out, and one of the tunnelled sounds, smaller than the point of the divulsor, should be slid down in its stead, in order to open up the stricture sufficiently for the ready passage of the divulsor. It may be necessary to use two or three of these tunnelled sounds of successively larger size before the contraction is sufficiently patulous for the divulsor. When the stricture is thus to a certain degree forcibly dilated—in fact partially dilated and partially torn—the divulsor should be carried well into the bladder over the same guide. This is then removed, and all is ready for the rupture of the stricture. At this stage we should remember that our object is to rupture, not merely dilate. We should therefore, as a rule, choose the largest cylinder, and press it home *decisively*, indeed rather rapidly, holding the shaft and handle very steadily while doing so.

Just here comes in the superiority of Holt's instrument, or its modifications by Mr. Burkley Hill, Dr. Bumstead, &c. The blades are expanded by a single quick movement; in Thompson's, Gouley's, and others of their class, they are expanded more or less gradually. The former are, therefore, almost certain to rupture the stricture-substance, while the latter are too apt to merely stretch it.

It will be observed that the essential part of this operation, i. e, the rupture of the stricture, is the easiest portion of the procedure, the true difficulty being most frequently found in passing the stricture with the guide. A few words more in regard to this stage of the operation will, therefore, be appropriate.

Tight strictures are difficult to navigate, if I may use the expression, mostly from three causes, acting either singly or variously combined. These causes are (1st), false passages; (2d), extreme diminution of the calibre of the canal, from either temporary hyperæmia of the mucous membrane about the stricture, or progressive and extreme contraction of the strictnre itself; and (3d) tortuosity of the passage through the seat of stricture. How may we best overcome these respective difficulties?

Firstly. If the tronble is caused by false passages, chiefly or in part, I have already mentioned the remedy. Fill up the false passages—as first suggested by Professor Gouley—with as many of the whale-bone guides as may be necessary. The true passage will at last be the only route left for the next and last guide. If you have any doubts in the matter, however, pass down over this last a tunnelled catheter; and if you fear that the fine calibre of this catheter may be clogged with mucous, pus, or blood, and a larger one cannot be passed without too much violence, attach to

its distal end an elastic bottle, such as is often used for injecting cavities, or any other instrument which is capable of exerting a suction power sufficient to draw out the urine, and by this means reassure yourself. The aspirator may be applied in this way.

Secondly. If extreme diminution of the canal, from either hyperæmia or gradual contraction, or both, be present, patience and perseverance will usually enable you to get through with one of these very fine, flexible, and olive-pointed whalebone guides. If, however, after faithful efforts, you fail to do so, you may, before resorting to extreme measures, try two or three horse hairs, which Dr. Mitscherlich, of Berlin, reports to possess peculiarly happy insinuating qualities in such cases. He uses two or three strands which are rolled together in wax. Over this the tunnelled instrument may then be passed. But if you still fail to pass—an exceedingly rare contingency with those experienced in such manipulations—you will be free to make your choice of the following expedients, i. e., (a) tapping with the aspirator at proper intervals for a few days; or tapping with the old trocar and canule, either per rectum or over the pubes, and then renewing your efforts: (b) some form of external urethrotomy: (c) forced catheterization; or (d) puncturing the urethra behind the obstruction. I cannot now dwell on each of these expedients, for I am not discussing the subject of retention of urine, but will only say that I prefer them in the order in which I have named them.

Thirdly. Tortuosity in the passage can usually be overcome with patience and skill. It may be suspected if the size of the stream the patient is passing be much larger than you would expect to find in a case in which great difficulty is encountered in passing even a small instrument. I here show you a specimen illustrative of the great tortuosity sometimes present in these cases. You perceive that this wax bougie, about No. 5, is twisted at its extremity like a corkscrew. It was the first instrument I could succeed in passing, after an hour's patient and persistent efforts, through a stricture which many others, at various times, had tried in vain. The man passed a tolerable stream—I mean, of course, tolerable for a case of stricture—and yet no other instrument could pass. I succeeded in engaging the end of this one in the distal orifice of the contraction, and, feeling that it progressed slowly when a slight rotary movement was adopted, I gradually got it through. In taking it out I was obliged to give it the reverse turn. I here exhibit another wax bougie in which a somewhat similar condition is shown, but by no means to so marked a degree. In such cases as present the peculiar feature alluded to, i. e., where there is a tolerably good stream, and yet great difficulty in passing any instrument, indicative of probably great tortuosity without very great contraction, the old wax bougies well deserve a trial. They will often, under such circumstances, yield to the abnormal shape of the canal more readily than any other instrument. But for cases

of extreme diminution of calibre, combined with tortuosity, they do not answer at all; for beneath the size of about No. 3 they are too weak to be handled effectively. Again, you will observe that some of the whale-bone bougies have spirally twisted, and some angularly bent extremities. This is intended to enable them the better to find their way around any twists and turns in the canal. This turn or angle may be given to one of the straight instruments for the purpose, by dipping it in hot water, and twisting or bending the end while it is warm. If, however, this difficulty of urethral tortuosity should prove insurmountable, and the emergency be pressing, then we must resort to one or the other of the already mentioned expedients, as when we fail on account of extreme closure of the canal.

I shall say but little in regard to the after treatment for divulsion. If the bladder be distended, a catheter should be at once passed, for obvious purposes, and this as a rule should be of full size. If there has not been any retention, then a large, solid instrument may be inserted and at once removed, the object being to satisfy ourselves that the stricture has been thoroughly ruptured. It is a question whether the patient should be allowed to pass his urine naturally, or whether it should be drawn from him at proper intervals for a few days, until, in other words, plastic effusion may have glazed over the wound, and thus obviated the supposed danger of urinary infiltration. I have tried both plans; have seen no tendency to infiltration of urine in those cases in which no after catheterization was performed; but on the whole am inclined to advise that the water be drawn off at convenient intervals, with a medium sized gum catheter, for two or three days. After about three or four days the process of gradual dilatation should be invariably adopted, even, indeed, to the end of the patient's life, at lengthening intervals, as in all other cases of severe organic stricture.

As regards the constitutional after-treatment, I have only to say that it should be the same as after any other operation on a diseased urethra. It would be well to keep the patient quiet, though not rigidly recumbent, for three or four days; allow him plain, nutritious, but non-stimulating food in moderation, and keep him slightly under the influence of opium and quinine in combination.

In conclusion, I would say that the following summary may be said to indicate the chief points to which I desire to invite the attention of the profession in this paper.

1st. In all severe cases of organic stricture of the urethra, whether simple gradual dilatation alone, or assisted by divulsion, urethrotomy, or any other proceeding has been resorted to, the patient, before being discharged, should be cautioned against the danger of recontraction, and advised to continue to keep up the dilatation by passing a proper instrument, at gradually

increasing intervals, even to the end of his life—once a week, once a month, once in six months, according to circumstances.

2d. The plan of gradual dilatation will be found applicable to the vast majority of cases. Its use should be the rule.

3d. Where gradual dilatation alone is impracticable, from causes either inherent to the stricture itself, or referable to the circumstances of the patient, then divulsion is preferable to urethrotomy, either internal or external, except in strictures at or very near the meatus.

4th. That neither divulsion nor urethrotomy should be regarded in any other light than as mere aids to dilatation, which is the one essential indication never to be lost sight of in any case.

5th. It is of great importance in the performance of the operation of divulsion to choose an instrument whose mechanism is such as will the more certainly rupture, rather than merely stretch the stricture substance, and in which the rupturing force is applied centrifugally only. The instrument of Mr. Barnard Holt, and its modifications, best present these desiderata.

Report of Two Cases of Hepatic Abscess. By William R. Mandeville, M.D.

Case I.—H. W., aet. 31; occupation clerk; first called to see him Feb. 8, 1874; found him suffering with intense pain in the abdomen; very much constipated; informed me that he had had a very severe chill the night before; ordered him to take at once 12 grains Pil. Hydrarg., with 1 grain Ext. Belladonna, followed the next morning by a saline cathartic, and put him upon gtts. xx. of "Battley's Liquor Cinchona Cordifolia." The day after, was called to see him, and found him in great agony, referring the pain to the right hypochondriac and epigastric regions; he was then in a state of high fever. I ordered him a mixture containing the bromide of potassium and acetate of ammonia, for the relief of the pain and fever. The next day he was up walking about, but still complaining of a dull, sore pain over the hepatic region. Two days later he was again seized with another violent attack of pain, if anything more severe than the preceding, accompanied with slight jaundice, which lasted for a couple of days only. I put him upon broken doses of calomel for a couple of days, which treatment was followed by five-grain doses of the iodide of potassium in an infusion of gentian, applying at the same time a blister over the region of the liver. Under this treatment his improvement was considerable, so much so, that about two weeks from this last attack he was able to go out of the house, with still, however, some pain on assuming the erect posture. About a week previous to his going out I called in my friend Dr. Warren Stone; he seemed to be inclined to the opinion that the pains were perhaps due to a neuralgic element, and at his suggestion,

put him upon a mixture of arsenic, iodine and iodide of potassium, and he really seemed to have improved under this treatment; but one day on his return from down town, I was sent for, and found him with high fever and intense pain in the region of his liver. I applied blisters, and continued the above treatment for a few days longer, when I left it off, and put him upon twenty-grain doses of the ammoniæ hydrochlor., twice daily. I kept him upon this for ten or twelve days, and with marked benefit so far as the pains were concerned, but at the end of this time, and just about two months from date of first attack, I noticed a slight bulging of the ribs, with obliteration of the intercostal spaces between the tenth and eleventh ribs, but no distinct fluctuation. Four or five days later I made a careful examination, and detected a slight sense of fluctuation, and with but little enlargement of the liver. But "to make assurance doubly sure" I called in the following gentlemen, Drs. Holliday, Boyer, Smyth and Stone, who at once verified my diagnosis, pronouncing it an hepatic abscess.

The question then came up, whether to wait, or to open at once; and as this is a very important and delicate matter to decide, considering how professional opinion is divided on the subject, it may not be amiss to here say a few words. Dr. Budd, in his work on "Diseases of the Liver," considers the dangers of operating so many and so great that it is better to let matters alone and allow the abscess to open of itself.*

Maclean, of the Army Medical School, expressed a similar opinion.†

Frerichs‡ and Moreheads only advise opening the abscess in selected cases; while on the other hand, Dr. Murray, Dr. Cameron, and Sir Ranald Martin, maintain the opposite views entirely, particularly Sir Ranald Martin, who maintained that "when we have just grounds for believing that abscess of the liver exists, we ought not to lose a day in evacuating it by puncture, and that we are both justified and safe in endeavouring to hit upon it with a trocar."‖

Amidst such conflicting opinions, Murchison, in his late work on "Diseases of the Liver," very justly makes the following remarks: "After duly balancing, then, the danger of operation, such as pus being apt to escape into the peritoneum and exciting fatal peritonitis, the entrance of air into the abscess, exciting fresh inflammation &c, and the dangers of non-interference, such as the abscess becoming larger daily, and thus causing more destruction of hepatic tissue, the liability of the abscess bursting in various directions, the fact that the great majority of patients with abscess of the liver die of exhaustion from hectic

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* *Op. Cit.*, 3d ed., 1857, p. 124.
† *Lancet*, July 18, 1863.
‡ Dis. of Liver, Syd. Soc. Ed. II, p. 147.
§ Res. on Dis. in India, 2d ed., 1860, p. 410.
‖ *Lancet*, August 20 and 27, 1864.

fever, diarrhœa, either while the abscess is still confined to the liver or after it has burst, against the dangers of expediency, I do not hesitate to recommend to you the propri ty of evacuating the pus in a large amount of cases of tropical abscess of the liver.

The operation may not be free from danger, but to wait in these cases upon nature, as it is called, is to wait upon death, and I would suggest for your guidance the following rules, and which briefly condensed are as follows.

In all cases where there is a visible fluctuating tumor, operate at once.

In cases where the symptoms of abscess of the liver are present, with a distinct tumor projecting from the normal contour of the liver, or causing bulging of the ribs, even though there be no perceptible fluctuation, it will be well to operate.

When symptoms of abscess coexist with uniform enlargement of the liver, but with no distinct tumor or bulging, if there be any local œdema, or obliteration of an intercostal space, or acute pain, always localized to one particular spot when the patient takes a full inspiration, it will be well to operate."

After carefully considering the matter it was decided to evacuate the pus, I therefore thrust a medium sized trocar into the most dependent portion of the tumor, being about two inches from the median line, and an inch and a half below the ribs. Near five onnces of thick pus was evacuated, mixed with the detritus of hepatic tissue. The operation was performed Monday, the 17th day of April. The canula was left in, and secured by means of tape passed around the abdomen.

The abscess, on an average, has been discharging more or less about an ounce and a half of pus daily, till, within the last few days, the discharge has diminished considerably in quantity and quality, being much thinner than previously.

Previous to the operation the patient looked as if he was going to die, the prognosis being exceedingly grave. Since the operation the patient has rapidly gained in health and strength, hardly looking like the same individual. I am convinced that had the operation been deferred much longer, the patient would have surely died.

Case II.—J. M., aet. 42. Admitted, ward No. 24, March 27, 1874. Was attacked Feb. 18th; occupation gardener. He first noticed, shortly before entering the Hospital, that upon turning in his bed at night he would be attacked with a severe pain in his back and right hypochondriac region. He still, however, continued to pursue his regular avocation for two weeks longer. The pain continued to increase in intensity, and he was eventually compelled to keep his bed, it was then that he commenced to notice that at two points there was a slight bulging of the intercostal spaces, and by the advice of his friends applied for admission to the Hospital.

The case was diagnosed at once as an hepatic abscess, and

opened by means of a trocar, between the ninth and tenth ribs, and a pint of thick bloody pus was withdrawn; a poultice was applied; the next day the canula fell out through the carelessness of the nurse. An attempt was made to introduce a drainage tube, but gave the man so much pain that it was left alone.

Air entered the cavity of the abscess freely with a sucking kind of noise, as it also did upon one occasion in Case I.

For my part I had not that holy horror which some seem to have in regard to air entering a pus-secreting cavity. If the abscess should break into the bowel or bronchi, does not air come in contact with it? I am inclined to the opinion that a good, free opening, made in the beginning with a bistoury, is the safest and most rational plan to adopt.

The aspirator I would never use, for more reasons than one. 1st. That as the pus which comes from an hepatic abscess is very thick and often shreddy, it would be all the time blocking up the tube, and that to empty an abcess by this means, would take an unreasonable length of time. 2d. I think that the suction force thus employed would act injuriously upon the walls of the abscess, and thereby increasing the liability of breaking down tissue unnecessarily.

My objections may be based upon theoretical grounds, rather than an actual observation. Whether so or not, I certainly would prefer using a good size trocar, or if I was certain that adhesions had formed (and this at once is quite exceptional, for in 76 fatal cases analysed by Morehead in only 3 were they absent), I would make a few incisions with a bistoury as the readiest method for evacuating the pus, and as giving the patient the best chances for recovery.

I should mention that in this last patient, pain in the right shoulder was a prominent feature throughout the disease. The first case did not have this feature, except during the few days that he was taking the chloride of ammonia. In the first case, however, the bowels were inclined to run off the whole time, whilst in the last case they have been rather constipated. Both patients are doing well, and they may now be said to be out of danger.

Malarial Origin of Congestion of the Brain in Malarial districts.
By A. W. Perry M.D.

In this paper I propose to demonstrate that the greatest part (60–75 per cent.) of the cases and deaths reported as occurring in New Orleans from Congestion of the Brain, are of malarial origin, and are undeveloped cases of Congestive or Pernicious Fever, or Malarial Coma.

It is of the greatest importance in medicine and medical hygiene, to determine the true origin of diseases, or diseased conditions resembling each other,

Take for instance, Neuralgia, a diseased condition of the sen-

tient nerves or their centres: this may depend on malaria, constitutional syphilis, Rheumatic diathesis or debility:

Quinine is almost as certain to cure neuralgias of the first kind, iodide of potassium those of the second, colchicum those of the third. Tonics and restoratives those of the fourth kind: to know the origin is the next thing to cure, in the above, as it is, to a less extent, in many other diseases.

I will try to prove by statistics.

1st. That the number of deaths from reported congestion of the brain is in New Orleans three or four times greater than in New York, Providence, or Chicago.

2d. That the causes of true congestion of the brain are as strong or stronger in those cities than in New Orleans.

3d. That malarial diseases are far more frequent in New Orleans than in New York, Providence, or Chicago.

4th. That congestions of the brain in New Orleans occur chiefly at those seasons when other malarial diseases prevail.

5th. That congestions of the brain in New Orleans occur mostly in those parts of the city where malarious fevers prevail with greatest severity

First Proposition.—A glance at the subjoined Table, column 4, shows the relative greater proportion of deaths in New Orleans and other cities.

TABLE NO. I

1	2	3	4	5	6	7
CITY.	YEAR.	Deaths of Congestion of Brain.	Rate per 1000 of Conges'n of Brain.	Deaths of Malarial Fevers.	Rate per 10,000 of Malarial Fever Deaths.	Rate per 10,000 of Heart Disease.
New York, pop. 942,000.	1868	217	2.3	.....	1.4	5.3
	1869	208	2.2	....	1.3	4.
	1870	237	2.5	.....	2.2	....
Providence, pop. 69,000.	1868	7	1.	.....	0	7.4
"	1869	8	1.1	.....	0	8.8
"	1870	7	1.	.....	0	8.4
New Orleans	1869	108	5.6	334	17.4	9.0
	1870	99	5.	415	22.	9.1
	1871	141	7.4	350	17.	10.
	1872	186	9.6	292	15.3	12.
	1873	201	10.5	439	23.	11.7
Chicago.....	1866	52	2.5	.....	3.7	3.0
"	1867	48	2.1	.....	1.3	2.7
"	1868	52	2.	.....	1.1	3.6
"	1869	58	1.8	.....	1.1	3.3

May, June, July, August, September, Oct.....Malarial Fevers, 374. Total in Year, 439
" " " " " ....Cong'n of Brain. 121. " " 202

Second proposition.—Cerebral congestion is of two kinds: active, in which the amount of arterial blood circulating in the vessels of the brain is increased; and passive, the amount of venous blood being increased. The active is more common than the passive in the relation of 5 to 1. Wakefulness is the prominent early symptom of active congestion, with injection of eyes, photophobia, sensation and motion are diminished; a light coma and partial paralysis, and loss of intelligence follow, but none of these are complete as in apoplexy.

Passive congestion symptoms—Vertigo, stupor, greater loss of sensibility, and paralysis of sphincters.

Causes of active congestion—Extremes of heat or cold, particularly cold.

Andral found, 26 in summer to 50 in winter.
Hammond, 179 " " 206 "

Direct sun-heat, long and intense mental exertion, heart disease, are also causes.

Of all the above mentioned causes, none are predominant in New Orleans over the other cities whose statistics I have used. Extremes of heat and cold are far greater and more frequent in the northern part of this country. Mental exertion is probably somewhat more excessive in the northern cities than here. There appears to be, by column 7 of Table I., an excess of heart disease in New Orleans. What the cause is I do not know, but that it has no relation to congestion of the brain in New Orleans, is shown by the fact that heart disease is excessive in Providence, while congestion of the brain is very low there.

TALE NO II.

DISTRICTS OF NEW ORLEANS, YEAR 1873.

DISEASE—DEATHS.	1	2	3	4	5	6	C. H.	Other Places.
Congest'n of Brain,	45	38	31	23	2	13	....	.....
Rate per 10,000..	8	9	7.7	6.8	....	12	....	.....
Malarial Fever....	75	90	107	45	18	27	97	38
Rate per 10,000..	13.2	21	26	13.0	26	24.9	....	.....

The above table shows that those parts of the city where malarial fevers prevail most, are most subject to "congestions of the brain."

Third proposition.—That malarial diseases are far more frequent in New Orleans than in the above named northern cities, hardly needs proof: in columns 5 and 6 of Table I., it will be seen that deaths of these diseases are 8 to 12 times more frequent in New Orleans.

Fourth proposition.—That congestion of the brain and malarial

fever occur chiefly at the same seasons of the year: this is shown by the two lower lines of Table No. I.

Fifth proposition.—The rear parts of the city are far more subject to malarial fevers than the part lying along the river. I am satisfied from my study of the mortuary reports that ⅔ of the cases of congestion of the brain occur among that half of the population of each district who live nearest the swamps in the rear of the city—the exact figures I have not been able to determine, from there being so many certificates of death given which do not report the locality or age.

About ⅓ of the mortality of congestion of the brain is in children under 10 years of age, who rarely suffer from the causes of true congestion of the brain. I think that this is the class of deaths from congestion of brain which can be set down as entirely malarial coma.

These statistics and conclusions, although drawn from and in New Orleans, apply with equal force to all places where malaria and its obvious effects are preponderant.

On the subject of treatment I have nothing to say except that, where the quality of blood circulating through the brain causes coma, that it is absurd to cure this condition by lessening the quantity of the blood by any depleting measures, even in those cases (if they exist) where the composition of the blood being unaltered, the abnormal quantity of blood causes coma, depletion is at least of questionable advantage.

Report of a Case of Punctured Wound of the Kidney, resulting in Cure. By A. W. Perry, M.D.

Joseph Mazzet, aet. 36 years, residing corner Lapeyrouse and Derbigny streets, Third District, of a bilious nervous temperament and of dissipated habits, was wounded on Sunday, February 8th, 1874, at 2 o'clock in the morning, in the right renal region, the wound ranging from outwards, inwards, and obliquely downward, and producing severe external hemorrhage. From the width and extent of the slits and rents which the puncturing instrument seemed to have made upon his vest, shirt, undershirt and pants, it was evident that the thrust had been directed quite vigorously, and it was to be presumed that the wound was a deeply penetrating one, and therefore of a serious nature. My friend, Dr. Wiendahl, was called to see the case later in the morning, and reports that patient had voided darkened bloody urine at 6 o'clock, i. e., 4 hours after the morning, and continued to void similarly colored urine for three days. On the 4th day, having had castor oil administered to him to relieve constipation, which had been induced by opiates and styptics, patient had a recurrence of active, pure hemorrhage, of nearly a quart in quantity. This hemorrhage persisted, though in

smaller quantity for four or five days, after which another hemorrhage, not as copious as the former, set in. This persisted also, with some intermission, for several days. The date of last profuse hemorrhage was February 28th, i. e., 20 days after the infliction of the wound. Large clots were then voided, and prolonged syncope followed, so severe, indeed, as to lead us to surrender all hopes of recovery for our patient. This, however, was the last hemorrhage; patient continued for several days to void discolored urine, with now and then small coagula, which disturbed the patient exceedingly, inasmuch as he had a stricture of the urethra, and had been suffering for several years past from chronic cystitis and membranous urethritis.

This case I diagnosed punctured wound of the kidney, the knife having gone through both the cortical and tubular substances into the pelvis of the kidney, causing the severe hemorrhages, the flow of which the patient was sometimes made aware of by the gurgling and peculiar "opening of a stop-cock" sensation which he describes, as the blood poured from the right ureter into the bladder.

Upon reference to books, I find my case peculiar by the absence of two important symptoms, namely, external escape of urine from the wound, and retraction of corresponding testicle, the burden of explaining which I decline to assume, and leave it to those whose ingenuity, experience, or inclination would best qualify.

My case made a perfect recovery. I have followed it, in consultation with Dr. Wiendahl, since the fourth or fifth day of the infliction of the wound, and have made it a pleasure to accurately set down the facts I submit to-night to your enlightened consideration.

Congenital Club-Foot (Talipes Varus). By F. Loeber, M.D.

The question in regard to the cause of congenital pes varus has excited the medical world considerably, since the last fifty or sixty years. Some say it depends on spasmodic muscular action produced by cerebro-spinal irritation, others say it is produced by malposition and pressure of the fœtus in utero. The arguments pro and contra are with great care and diligence put together in Adams' work on club-foot, chapt. xii., page 195, but none of all the theories advanced give a satisfactory explanation. We see that the deformity developes itself in a very early state of gestation. Little, Adams, and many others have found it in fœtus of 3–4 months, and I think it is better to acknowledge our ignorance in regard to its cause, than to cover it with theories without any foundation whatsoever. In my opinion the real cause of congenital deformities will only then be found when we shall know a little more about the laws and rules which govern the early formation of bones and joints. The motto of the medical

world now is, "No belief; only facts!" and the adherence to it has advanced medical science in the last fifty years more than it ever did in the two thousand years before.

If we would know the cause of these deformities we might, or we might not, prevent them. We don't know it: the simple fact is only before us, that we are often called upon in our practice to correct the malposition of the foot called club-foot. To do this satisfactorily we must study first the normal or physiological anatomy, and then the pathological changes which have taken place in this special case.

But before we commence to study the anatomy and physiology of especial bones and joints, let me say something about the functions and mechanism of joints in general.

The function of a joint is motion. The different joints distinguish themselves from one another, first, by the mode, and, secondly by the extent of their motion. The mode of motion is always strictly dependent on the stereometrical form of the joint. The principal stereometrical forms we find in the human frame are, 1st, the plane; 2d, the globe; 3d, cylinder; 4th, cone. The joints are constantly so constructed that the articular facets of the two bones which form the joint belong to the same stereometrical form. Never do we find a plane moving on a globe, or a cylinder on a cone. The movements of plane surfaces may be twofold. One plane may make a turning motion on the other, or they may glide over one another. In the first case a certain line drawn perpendicular on the plane will be without motion. This line is called the axis of motion. A globe allows motion in all directions; because every radius of the globe may serve as axis. More simple are the movements of a cylinder; it only makes a rolling motion backwards and forwards around another cylinder—at the same time the cylinder might slide up and down, but this kind of motion we never find in the human joints—we often see it in machinery. A line connecting the fundamental circles of the cylinder is the axis. In the cone that line is the axis which connects its lower circle with its highest point, and therefore we have only one kind of motion. I wish not to be understood to say that every joint has only one of these stereometrical forms. Often we find a joint formed by different parts or bodies each having a certain stereometrical form, for example, the elbow joint, where we find a combination of the globe, cylinder and cone.

To have certain denominations for each movement of joint it is necessary to facilitate mutual understanding. We distinguish the different motions according to the direction of their axes, and we define the direction of these axes by lines we think drawn horizontally from left to right, one perpendicular from vertex down, and one from behind forward parallel to the septum of the nose. The first we call the frontal axis, the second, the perpendicular, and the third, the sagittal. Motions around a horizontal line are flexions and extensions. Around a perpendicular, rotation inward,

outward, and around the sagittal adduction or abduction. By those lines we are enabled to construct the axis in any movement of the joint. For example, we find in the hip joint that the axis does not run exactly horizontally, but on one extremity to somewhat backward, on the other forward, and, furthermore, on one side a little deeper, the other higher; we say the leg is flexed, extended, adducted or abducted, rotated inward or outward. If an axis does not correspond to one of the three fundamental lines, and very seldom it does, that axis which deviates the least is first put down, and then one or the other composing it is added. Seeing what importance the axis has on motion, it is natural to ask, How do we find the axis? In former times the axis of a joint was deduced from its form; it was simple, but not accurate. Henke has given two modes of finding it. The first is to drive a nail through the bone into the joint, so that its point just pierces the cartilage, and in moving the joint scratches the cartilage on the opposite bone, and so marks its course. Is the joint cylindrical or conical, the marks will be everywhere circles, or segments of a circle. Next we have to find the middle of the circle, and erect thereon a perpendicular, which perpendicular is the axis of the joint. Not so exact, but very apt for demonstration, is the second method. We pierce the joint with a long needle in the direction of the supposed axis (in young children this is done very easily) till we have found such a position for our needle that the end of it, by moving the joint, does not move; the needle then corresponds to the immovable line, and is the axis.

Let us now go back to the bones and joints affected by club-foot. These are principally the bones and joints of the tarsus. Looking at the principal and largest one of them, os calcis, which we find in the normal state, between the high body and the lower processus anterior, which articulates in front with the os cuboid, a peculiarly formed articulating surface. This surface, passing from a higher body to a lower one, must be naturally higher behind than in front. To compare its form with a certain stereometrical form would be difficult if we have the bone of an adult in our hands; but in the infant it corresponds to the segment of a cone. This cone would have its basis in a backward, its top in a forward direction; but if we look at it more closely we find that its base looks also downward and outward, and the top forward, and at the same time upward and inward. The articulating surface represents about one-third part of the arch of the cone, with its top cut off, and if we would lay it on a plane it would represent an irregular cuboid form. On this convex surface of the os calcis moves the concave articulating surface of the astragalus. In front of it we see a groove running from below inward and upward, which forms, with a corresponding one of the astragalus, a canal, sinus tarsi, filled in the recent state with ligamentous tissue, and separating effectively the posterior articulation just now described, called by Meckel, articulatio astra-

galo calcaneo propria from the anterior; articulatio astragalo, calcaneo scaphoidea. As its name indicates, the anterior articulation is composed by the astragalus, calcaneum and scaphoid. The head of the astragalus may be compared, without much stress, with the segment of a globe. The centre of it lies in the centre of the short neck of the astragalus. Neck we call the contracted portion of the bone situated between body and head. Looking at the articulating surfaces we find that they deviate from the globular in several points, principally the minor lower portion, which rests on the sustentaculum tali of the os calcis, is more flat than the outer and upper portion, which articulates with the os naviculare. The longest diameter of this last portion is from right to left; on the outer end it looks a little upward, and on the inner one a little downward. The concavity enclosing the convex articulating surface, or head of astragalus is formed by the scaphoid and sustentaculum tali of the os calcis. The form of the articulating surface of the scaphoid is concave and oval, the articulating surface of the sustentaculum is more like a strike, more or less concave, descending from behind forward. Between the inner border of the calcaneum and the inner border of the os naviculare extends the strong ligamentum calcaneo naviculare; it forms a concave disc on which that portion of the head of the astragalus rests, which is not supported by bones.

This superficial description is sufficient, I think, to understand the movements of these joints. The calcaneum has, as we have seen, two articulating surfaces, one belonging to the posterior, the other to the anterior joints; it is therefore necessary, that the movements of these two joints, if the os calcis moves on the astragalus, must be simultaneously, and in the same direction. The motions of the os naviculare might emancipate themselves from the motion of the os calcis, but the fibres of the ligament calcaneo naviculare are strong and powerful and attach this bone so firmly to the os calcis, that it can't move independently of the os calcis. The rest of the bones of the tarsus have to follow, because they are nearly immovably connected with these two bones, and through them with the rest of the foot. The anterior joint should, according to its globular form, permit motions in all directions, since every radius of the globe can serve as its axis. The posterior joint can permit only one kind of motion, it represents a cone, as we have seen, and has only one axis; therefore, all the motions in the anterior joint cease—and of all its radii, only the one radius serves as an axis, which is the direct continuation of the axis of the cone of the posterior joint. This can very easily be demonstrated by the needle method; hence, the physiological functions of these two joints, perfectly distinct, are the same. Hunter proposes to name them collectively, the talo tarsal joint; this is much shorter, and comprises the two joints.

Now let us examine the axis of this talo tarsal joint by introducing the needle; we find that in moving the joint, it will be perfectly quiet if it enters the os calcis at the external border of

insertion of the tendo achilles, going upwards and inwards it escapes from the os calcis in front of the sustentaculum tali in the region of the sinus tarsi; afterwards, it perforates the body and neck of the astragalus, and comes out near the inner border of the scaphoid. We see that the direction of the axis is not perfectly saggital, it has at the same time a somewhat perpendicular and horizontal course, further do we find that the long axis of the foot stands in its middle position, to the long axis of the leg, at right angle, and so the definition of motion in this joint is somewhat difficult. Hunter describes the motion in the talo tarsal joint analogical to the motions of the hand.

In the hand we call that motion by which the border of the little finger is depressed and the border of the thumb elevated "supination," the reverse, "pronation." The motion of the foot is principally elevation and depression of its sides. Supination would be, therefore, if its external border would be lowered and the inner one elevated; pronation, the inner border depressed and the outer one elevated. But, as we have seen, we have not only a sagittal axis, but at the same time, a perpendicular and frontal component, they prevent that the motion is alone a lowering and elevation of the borders, they produce at the same time certain movements of the toes and heel, viz: in supination the toes are lowered and turned inward, the heel is elevated and turned outward. In pronation the toes are elevated and turned outside; the heel is depressed and turned inward.

The muscles acting on the talo tarsal joint are either pronators or supinators. All of them which are riveted inside the axis are supinators, as tibialis posticus, flexor longus pollicis, flexor communis long, digitor; tendo achilles; all of them which are inserted outside are pronators, as tibialis anticus, extensor long. digitor, extensor longus pollicis; peroneus longus, brevis and tertius. The nervus peroneus innervates all the pronators, and the nerv. tibialis all the supinators. The effect produced on the motion of this joint, by the different muscles depends, as every where else, not only on the diameter of their contractile fibres, but also on their insertion—nearer or further from the axis. The tendo achilles lays so near the axis, that its action in supination is very little, in spite of the powerful soleus and gastrocnemius; exactly the same is it with the action of the tibialis anticus, it lays so near the anterior end of the axis that it cannot produce a considerable effect; but still it is a pronator. The furthest from the axis are the peronei, and by that they produce a powerful pronation, because they act on a long lever. The whole extent of motion in the talo tarsal joint, supination and pronation is about 40°. The internal and external border of the foot in its median position, are not horizontal with one another; the external border is lower than the inner one, and only in extreme pronation does the inner border become lower than the outer one.

The motion of pronation and supination is arrested in posterior part of the joint by the contact of two surfaces of bone. In

making pronation, the anterior lower border of the astragalus moves so far forward and downward, that it comes in contact with the superior border of the process, anterior calcanei. This contact, although very small, arrests pronatory motion at once. In supination comes the inner border of the astragalus nearer and nearer to the sustentaculum tali of os calcis; at last they come in contact, and supination cannot go any further. In the anterior part of the joint are no such bony impediments, and the os naviculare could move further and further over the oval articulating surface of the astragalus, if the short and strong ligamentum calcaneo naviculare would allow it. This pulls the os naviculare inward; but before, gentlemen, this ligament comes into play, the anterior lower border of the astragalus comes in contact with the processus anterior calcanei, and motion stops. The ligaments in the axis tarsi are only then put on a stretch, if the bony impediments are overcome by luxation.

Now, gentlemen, if you look at the foot of a new-born child, or one of several months old, you find that the external border stands much lower than the inner one; the sole of the foot is turned inward and the foot is in the position of supination; the child can easily bring the soles of his feet together in the same manner as the palm of the hands are brought together in the act of praying.

This, gentlemen, is the position of a congenital club-foot, pes varus. In the normal condition this position of supination does not get less till the child commences to stand and walk, when nature itself removes it; but in a club-foot it remains, nature is not powerful enough to cure it. We find that the extent of motion is the same in the child as in the adult, 40°, only that supination predominates. By examining the bones of an infant, we find that the impediments to motion, are the same bony points as in the adult. Their relative position is altered; by comparing the os calcis of an infant with one of an adult, we find that the line of contact or the processus anterior calcanei in an infant is much higher in proportion to the relative heights of the bone. In the infant the relative heights of the process anter calcan to the height of the body of the calcan is like 1:2, in the adult 1:3, according to Umle. If, in the child, the process anter calcan is half as high as the body, the astragalus must be much sooner arrested in its motion of pronation, than in the adult, where the process anter calcanei is only $\frac{1}{3}$ as high as the body. If we look at the inner side of an infantile os calcis we find that the sustentaculum tarsi is much lower than in the adult, and therefore, supination of the foot much later arrested than in the adult, where the sustentaculum comes up pretty near to the niveau of the body.

What is it, we ask, which transforms, during the time of growth, these bones? Is it the weight of the body, which rests in standing or walking on the os calcis. In every step we make by walking, the external border of the foot touches the ground

first, and by that the talo tarsal joint is forced in the position of pronation. The sharp border of the astragalus presses on the processus anterior calcanei and in standing it rests on it permanently. The osseous growth of the process anter calcan on the line of contact is impeded, and in the course of time, the relative height of the process gets less than in the infant. The growth of bone on the sustentaculum is not impeded, there is no pressure, and it develops itself after delivery, free and unobstructed. The walls of the uterus pressed the foot into the position of supination, and by that prevented the growth of the sustentaculum, but after birth, no pressure exists to bring the foot in this extreme position of supination. On the contrary, the habitual motion of the foot as walking or standing induces pronation, and only exceptionally, as in climbing, we force the foot into supination.

Examining the bones of a child, we see at once, according to the examinations of Hunter and Adams, the extreme height of the process anter calcan, and therefore pronation very soon arrested, much sooner than the external border, comes in a horizontal position with the internal one. The articulating surface of the calcaneum looks very little outward, the greater part of it looks inward and downward. The sustentaculum tarsi is not developed, and therefore supination not arrested. The neck of the astragalus, instead of being continued directly forward, is directed inward, towards the inner malleolus. The articular surface, instead of looking forward, looks forward and inwards. The os naviculare is not changed in form, but in position; it articulates with the lateral articulating surface of the head of the astragalus, and has instead of an anterior position a lateral one. The long axis of the astragalus and os naviculare are parallel instead of being at right angles with one another. The other bones have to follow the movements of these bones, as we have seen before. We find the inner border of the foot excessively high, the outer very low in supination; but we also have seen that in supination the toes are depressed and turned inside, the heel elevated and turned outside; so we find it in pes varus, only more intensive, depending on the altered condition of the neck of the astragalus.

The muscles, according to all writers on club-foot, do not show at the time of birth, any difference, neither in substance nor in direction. The only difference Hunter found was, that in very severe cases of pes varus, the tendon of the peroneus longus did not run to the sole of the foot over a groove on the cuboid bone, but on the os calcis. The muscles in congenital club-foot are not paralyzed. If they were we would find atrophy, as we do in paralytic club-foot. Nor is it a primary affection produced by nervous influence. In children which are otherwise perfectly healthy and well formed this is impossible; because the muscles are not in such a state that we could say they were in a state of contraction. Adams lays a great stress on the contraction of ligaments. They only can get shorter by an inflammatory pro-

cess; in all other cases they are dependent from the position of the joint; their shortening is only secondary. Henk, Hunter, and the author of it Volkman Lüike say that the pes varus depends on an excessive formation of bones and alteration of joints according to the same type which are formed physiologically inside of the uterus. The bones and joints in the posterior part of the foot develope themselves according to a certain type, which type corresponds to the position of supination during uterine life. The correction of these forms physiologically is produced by pressure of the body in walking and standing by which pronation is induced. An extreme formation of this type produces pathological forms of bones and joints which represent the pes varus, and these forms are only pathological for the reason that the correction of their forms cannot be effected physiologically by the act of walking or standing.

If a child with a pes varus tries to stand or walk it touches the ground with the outer border only; it cannot fulfil these acts correctly; it stands infirm and walks in a waddling manner; instead of the sole, the dorsal surface touches the ground. The consequence of this is, that instead of pronation being induced, as it should, supination is increased. The parts of bones which are not exposed to to pressure increase in size; the other parts which are exposed to pressure do not grow, but remain small. The muscles and tendons accommodate themselves to the growth of bones. The continued weight of the body presses also the cuboid and cuneiform bones together and so the malformation gets from day to day worse. Important are the secondary changes which take place in the muscular substance, which is intact and perfectly functionable at the time of birth. These changes generally commence in the second year, and depend principally on their deficient use in the act of walking. A club-foot performs the act of walking without the fine and regular motion of joints, and consequently that fine and regular action of muscles is not required. The consequence of a deficient muscular action is here, as everywhere, fatty degeneration and atrophy of the contractile muscular substance, and at last paralysis.

It is necessary to have these consequences before our eyes, to commence treatment at the proper time. Treatment should commence at the end of the first year, or if the child is very strong, at 8 or nine months. At this time the formation of bones and joints is the same at the time of birth, and the contractile power of muscles is intact; the skin has a greater power of resistance, and at last but not least, the child commences about this time to walk, and by this act finishes the process of correction which we have induced by our treatment.

In what manner shall we treat pes varus, and what means have we at our disposal to attain a cure? In light cases, passive motions producing pronation, daily methodically performed, are sufficient. These motions have to be made by the doctor himself, or by the parents of the child, if they are intelligent enough

to make them understand what he wants. These passive motions daily performed act like a drop of water falling for a length of time on the same spot of a stone. In more severe cases this mode will not do, and we must look for something else. Treat them by machines? Machines are expensive, need often repairing, and often cannot be had in smaller places at all. But suppose that everything stands at our disposal, I do not think that a machine can be made to fit the foot and leg of a child one year old accurately enough to do much good; but even in the case you get a really good machine, then I think a plaster of Paris bandage will do just as good and better than the best made machine. The plaster of Paris bandage has to be put on under chloroform, so that the muscles are relaxed, in the utmost pronation of the foot, commencing from the margin of the toes up to the spine of the tibia. Change the bandage every 14 days, and in the course of 1–2 months the pes varus of a lighter degree will be cured; if of a severer degree, it will take 2–4 months: a cure can be effected quicker if you change the bandage every 4–5 days, instead of every 14 days. The treatment by plaster of Paris may be discontinued when a considerable part of the sole of the foot touches the ground—then it is susceptible to the physiological correction by the act of walking and standing. The tendency to set the foot down on its external border is easily prevented by letting the child wear a Scarpa's shoe, and if that is not sufficient, put now and then another plaster of Paris bandage on, to correct it. The treatment has to follow and is to be founded on our anatomical studies. By pressure we have to correct bones and joints. By making pressure on the astragalus and calcaneum in a manner to pronate the foot, we produce atrophy of the parts of bones which are higher or longer than normal; that is, as we have seen, the processes anterior of os calcis and the external portion of the head of the astragalus, and the parts which were exposed to pressure in supination, we relieve; and by that permit them to grow unobstructed. The articulating surfaces follow the changes of the bones. We know that if we change the axis of two bones in relation to one another, the cartilaginous surface atrophies where there is no contact with another bone, and adds cartilage in the opposite direction.

The principle of treatment I have given above is not new, and it has been in use since club-foot was treated, but since the last 30–40 years it had to combat with an erroneous idea of treatment—tenotomic. Stromeyer was the first to introduce this operation practically, and now his followers are thousands. One tendon after another is cut through, and still they effect a cure only then when they follow up their tenotomie with a rational orthopœdic after treatment; if not, club-foot is not bettered, although all the tendons might have been cut. A little resistance, it is true, we find in the shortened supinator muscles, and at the time when the operation of tenotomie was first made by Stromeyer, Little, Dieffenbach and others, they did not know the

action of chloroform overcame the resistance; and furthermore, they had to operate mostly on old cases, and they gained from their tenotome without doubt great advantages. It is different to-day. We can commence our treatment at an early day, and by bringing our patient under the influence of chloroform, we relax the muscles so much that if the osseus impediment would not stop pronation, we could bring the foot at once in its normal position, and therefore tenotomie is unnecessary.

EXCERPTA.

It is now some twenty years since a very wise physician said in my presence, "You must not try to cure tetanus; you must try to make it chronic." What admirable philosophy this apparently paradoxical instruction inculcates! In your efforts to relieve the paroxysms, do not jeopardize the patient's safety by ignoring that physiology upon which his existence depends.

The therapeutic indication which is paramount in treating this disease, is to employ some agent which exerts sufficient control over the spasms to prevent the production of death by fixation of muscles, the integrity of whose functions is indispensable, or by the exhaustion which continual spasms occasion.

The pharmacopœia abounds with medicines capable of securing these results. The difficulty, however, consists in the fact that all these agents possess a greater or less toxic action, and as this effect is inseparable from their curative control of spasm, we cannot obtain the latter without incurring the danger of the former. Another important consideration is, that the period of time during which these agents must be employed to reach complete recovery, is too long for the system to endure continuously the influence of a decided poison. And just here idiosyncrasy comes in for a greater share of influence upon the ultimate issue of the case than in any other form of disease we are called upon to treat. I think there is a growing disposition on the part of those who devote much attention to the study of the physiological effects of drugs upon living creatures, to assume that the primal conditions are uniformity of action. If it should ever become susceptible of proof that those varied states of susceptibility to the influence of certain foods, or drugs, evinced by different constitutions, are acquired instead of being natal, it would

then remain to learn how these departures from the law were brought about. Assuming that all this knowledge has been obtained, we would then be in a position to justify us in postulating in each individual case of tetanus, what drug is best suited to its treatment. But until we are enabled to occupy some such stand-point, our selection of the anti-spasmodic to be used must, in so far as idiosyncrasy is concerned, be purely empirical.

If proper hygiene can be enforced, and more especially the complete isolation of the patient from every form of excitation, physical or corporeal, the amount of medication requisite to allay the spasm is in every case lessened. This is a very great desideratum, for if we must administer a poison, the first and highest consideration is, to give the minimum quantity consistent with the desired effect.

A careful study of a large number of cases of tetanus recovering under the use of bromide of potash, has convinced me that in a majority of them, the rigorous rules of the medical attendants in respect to the patient's surroundings and bedside management, constituted the paramount elements of success.

This statement detracts nothing from the actual efficiency of bromide potassa as a remedy calculated to quiet nerve excitability, but only shows that the *savoir faire* of the physician has all to do with the success of his therapeutics. Thus it is that we we are enabled to record the cure of so large a number of cases of tetanus by remedies widely different in the degree and nature of their therapeutic action. There is, however, scarcely a doubt that the particular remedy which has the widest range of usefulness in the treatment of tetanus, is chloral hydrate. This may therefore be regarded as a drug which combines greater control of spasm, with less interference with the physiology of the system than all others at present employed for this purpose.

Treatment of Tetanus by Calabar Bean.—By W. H. Jalland, F.R.C.S.

In reference to a "Case of Traumatic Tetanus successfully treated by Calabar Bean," reported by Dr. John Cunningham in the *British Medical Journal* of April 4th, I should like to ask one question. Dr. Cunningham gives us very careful notes of this interesting case, both as regards the symptoms, the dose of the bean given, and the general effects produced; but he unfortunately omits to mention whether the physiological effects of the drug were noticed, and if so, as soon as they were produced, whether the spasms were diminished both in frequency and

severity. I think that it would be interesting to know this; as, if contraction of the pupils were produced, and, when it was so, the spasms became less intensified or ceased altogether, this would tend to teach us that the drug should be administered so far; but when the contraction was produced, and the tetanic condition not lessened, then the bean should be laid aside as not suitable for that case.

I remember seeing a case of tranmatic tetanus successfully treated by Calabar bean in a provincial hospital (unfortunately I have not the notes of the case), in which this was strikingly manifest. The bean was administered in increasing doses until the pupils were contracted down to a pin's point; and, as soon as this was so, the spasms ceased. They, however, recurred slightly upon the drug being left off, and when the pupils regained their normal condition, but were speedily brought under control upon the drug being again continued, which was done for several days following, diminishing the dose gradually. The case ultimately recovered perfectly. I may add that I have seen the bean tried in two other cases unsuccessfully; but in neither of these were the physiological effects produced.

Any drug used in the treatment of this terrible malady, after the administration of which recovery ensues, is hailed with joy by the profession; but if, after further trials, the result should not be equally satisfactory, it is often thrown aside as useless; and this may occasionally, perhaps, be due to our own neglect in not watching the effects of the drug sufficiently. I make this suggestion, as perhaps, in some of the cases in which Calabar bean has been used unsuccessfully, the physiological effects were not produced.—*British Medical Journal.*

Chloral Hydrate Successfully Used in Tetanus.

Dr. Coryllos, of Patras, in Greece, published a case of this kind in the *Allgem. Weiner Med. Zeit.*, No. 2, 1873, and now the same author records two similar cases, one under his own care, and the other treated by Dr. Basilin. The latter case relates to a woman of forty, who had wounded her finger with a splinter, which she removed herself. Tetanus occurred one month after the accident, and she had more than ten general attacks in the twenty-four hours. Sixteen days after the first tetanic symptoms the patient removed from the wound a bit of splinter, the size of a pea, which had been left in it unobserved. The usual narcotics having failed, chloral was tried, and succeeded. Altogether three ounces and a half were taken in twenty days.

In Dr. Coryllos' case a man of forty had his left temple wounded by a pointed piece of reed. Tetanus supervened, and here, again, a portion of the foreign body was removed twelve days after the accident. He had at first fifteen-gram doses of chloral,

and improved much upon them. But the tetanus recurred with renewed severity, and the chloral was pushed as far as one hundred and twenty grains per diem. The patient completely recovered, and had taken, in about thirty days, six ounces of chloral.—*Philadelphia Medical Times.*

Injection of Chloral as a Remedy for Traumatic Tetanus.

M. Bouillard, on the part of M. Ore, a professor of the Bordeaux Medical School, related a case to the Academie des Sciences, February 16, in which chloral had been injected into the veins, as a remedy in traumatic tetanus. A man, after a wound of his finger, became the subject of tetanus, in consequence of which his mouth became so closed that no remedy could be administered. M. Ore threw an injection, containing ten grammes (about 154 grs.) of chloral, into the veins, which produced peaceful sleep; and this was followed by a second and third injection, with the effect of obtaining a sleep of eight hours.—*Chicago Medical Journal.*

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*Case of Poisoning by Codeia.** By A. S. Myrtle, M.D.

Last summer I had a run of cases of saccharine diabetes. One female and eight males consulted me, during August and September, suffering from this complaint. Every kind of treatment had been put in force, from skimmed milk to creosote; every system of diet had been adopted with apparently very little success. At last, one gentleman presented himself who had been ill for eighteen months. During that time, he had gone through the faculty at home and abroad; he had visited mineral springs innumerable, and finally arrived at the conclusion that the only remedy which gave him relief, and checked the progress of the disease, was codeia, prescribed for him by Dr. Pavy. He assured me that all other preparations of opium had been tried, and found useless. This was the first time I had heard of codeia being used therapeutically. I asked him how much he took. He did not know; his London chemist had his prescription, and supplied him with pills as required. Of these he took one night and morning. I thought I had got an useful hint, and determined that the first case which turned up should be put under the influence of this new remedy.

Soon, an old patient came from the North to see me; he had become alarmed, as a physician had told him his urine was wrong. On inquiry, I found he had been subjected to much anxiety, watching and grief—first, by the long-continued illness

* Read before the Leeds and West Riding Medico-Chirurgical Society

and death of his mother from epilepsy, and, secondly, from the death of his brother from diabetes, which had run its course with unusual rapidity. Since then, he had perceived little wrong with him, except that he had felt very much depressed in spirits, disinclined for active exercise, and had periodically been attacked with what he called "the strangest pissing fits." These came on suddenly when in church, whilst out walking or riding, and the amount which he passed at a time was very great. He had not observed any change in appetite; had no thirst, and was, he thought, of his average weight. He did not know under what medical treatment he had been, and ate and drank moderately of anything which came in his way. On examining his urine, I found it of a pale lemon color, having a fruity smell, and a specific gravity of 1038; it contained a large amount of oxalate of lime in octohedral crystals, and showed abundant evidence of the presence of sugar with the usual tests. I thought of a trial of codeia. I wrote a prescription for pills, each containing four grains of codeia and one-twentieth of a grain of sulphate of strychnia, the patient to take one night and morning. The diet and everything else were as usual. The chemist to whom the prescription was sent very properly called on me, saying that he feared I had made a mistake, as the dose seemed to him excessive; but, as Dr. Gregory (quoted in Squire's *Companion*) had not found it so, I asked if he obtained his codeia from a reliable chemist, and, learning that he got it from Messrs. Smith, of Edinburgh, I told him to make up the pills as ordered.

On the following morning, at 10 o'clock, I found my patient standing at his hotel door. He asked me if he might accompany me in my round, and away we went. I began to think codeia in four-grain doses must be a very mild agent indeed; but, on questioning my friend, I discovered that the pills had arrived too late for him to take one the previous evening, and he only had swallowed his first half an hour before I found him that morning. By and by he said, "I feel uncommonly jolly, as if I were up to anything: I have such a genial glow all over me." We walked together for two hours. He then left me to write letters. As soon as he began to write, he observed he did not see the letters distinctly. Suddenly his sight failed him; he felt the room going round with him, and he made for the door, got into the open air, feeling very bewildered, but directing his steps to my house; he knew he met two ladies, recognized their voices but could not speak to them. At 1.30 I found him in my study, just four hours after he had taken the pill. He was standing, holding on by the back of a chair; he had a terrified look; was exceedingly pale; sweat was standing in large beads on his forehead; his pupils were slightly contracted; the pulse at the wrist was scarcely perceptible; the surface of the body was cold and clammy, his voice was reduced to a whisper. I asked, "Have you vomited?" "Only a mouthful." "How do you feel?" "Sick as death." I at once gave him a tumbler of hot water with

brandy and ammonia; this he at once ejected. I repeated the dose in five minutes, and made him lie flat down, wrapping him in warm rugs. In half an hour he showed signs of improvement; said he was in no way sleepy—just terribly sick, as if he had smoked a lot of bad tobacco. He remained in this state till ten at night; during all this time, if he attempted to move he began to retch violently. I got him to bed, applied hot bottles to his feet, and covered him well over with blankets. He passed a wretched night, slept none, and was slightly delirious. He got up next morning at nine o'clock, feeling still very sick and ill. His pulse was 56; the surface was cool; the face pale; the pupils much contracted. He had passed about six ounces of urine. He could eat nothing all day. Late at night, he took a cup of tea and some dry toast, and went to bed, slept well, and rose feeling quite himself in the morning. I examined his urine of the previous night and morning; he had not passed much. Its specific gravity 1018; there was no sugar; oxalates were still plentiful.

I told him of the mistake I had made; that I wished him to give codeia a fair trial, and that I would reduce the dose to one grain; this proved too much, so I had to reduce it to half a grain. This he has taken night and morning for two months. There has been no return of glucosuria. About the end of December he wrote: "Since you nearly did for me I have only had one slight attack of diuresis, and that, I think, was brought on by exposure to cold. I consider myself quite well now." His medical attendant, Dr. Wilson, of Alnwick, has since written me, confirming the above report, and stating that, in his opinion, the case was one of those transient forms of glucosuria which we occasionally meet with, arising from some disturbance of the nervous system. In this I fully concur, at the same time as his brother had so recently died from diabetes. I thought it right to persevere with the treatment for a considerable time after the sugar had disappeared.

In the discussion which followed the reading of this case, I was struck with the general opinion held, that codeia depended solely on the presence of morphia for its effects. Is it so?

The symptoms I observed lead me to infer that, as a therapeutic agent, it forms a connecting link between morphia and nicotine. First, we had a short period of decided vascular excitement, with consequent increased vascular activity and power common to both; then followed a period of depression, with symptoms equally common to both—dimness of sight, nausea and sickness, cold clammy skin, muscular debility, and a manifest and persistent "slowing" of the vital functions. On the other hand we had no stupor, not even drowsiness; no headache, as with opium; no vomiting, no increased alvine, vesical, or salivary action, as with tobacco. In thirty-six hours the patient had completely lost all his disagreeable feelings, and was free

from all signs of having been poisoned by codeia.—*British Medical Journal.*

As tending to afford some explanation of the mode of action of the Calabar bean in the cure of tetanus, attention is directed to the following excerpt, from the *London Medical Record* of April 22d:

Martin-Damourette and Others on the Physiological Antagonism of Poisonous Substances. By Alex. B. McDowall.

Several memoirs on this subject have lately appeared in France and in Germany. For the following particulars we are indebted to a writer in the *Archives des Sciences* for March 15th. (An account of the researches of MM. Rossbach and Fröhlich on the action of atropia and physostigma on the pupil and heart was given in the *London Medical Record* for December 17, 1873.)

M. Martin-Damourette has studied the physiological properties of eserine, the active substance of the Calabar bean (*Physostigma venenosum*), named also physostigmine and calabarine. Having made experiments on rabbits, birds and frogs, he thus sums up its properties as affecting motor organs.

1. It excites the muscles, and produces muscular convulsions, shown in fibrillar contractions, convulsive movements, contractions of the intestine, and lastly, pupillary contraction.

2. It excites the cerebro-spinal motor centres.

3. It dimishes (like curare, but less rapidly), the excitability of nerve-terminations in the muscles.

These three elementary properties appear differently according to the dose given, and especially according to the mode of administration. While a large dose of the poison quickly produces convulsions, paralysis of the phrenic nerves, and death by asphyxia, in warm-blooded animals, it is possible, by using divided doses (*refractées*), to destroy muscular contractility, without producing the nervous excitation, which, in the former case, caused death. These two actions M. Martin-Damourette regards as a sort of antagonism established between the two principal physiological properties of the substance; the muscular paresis being opposed to the death by nervous excitation. It is in this way that he is led to explain the success which some authors have said they met with in using the Calabar bean for treatment of tetanus. He insists on the necessity of using divided doses in the treatment of tetanus, in order to obtain muscular relaxation. 'Weak and gradually increasing doses of sulphate of eserine,' he says, 'are a certain means of developing this antagonism to advantage of neuro-paralytic effect, and hence results a tolerance of doses which are more than toxical, capable of relaxing the tetanised muscles.' In the continuation of his memoir, M. Martin-

Damourette seeks to prove that an analogous influence is brought into play, when (as Dr. Fraser shows) the toxic action of eserine is neutralized by a small dose of atropine. Atropine, in small doses, has thus neuro-paralytic properties capable of arresting the convulsive action of eserine.

One of the important properties of eserine is that of making the pupil contract, either when applied locally to the ocular globe, or when, through absorption, it penetrates into the aqueous humor. This property, often utilized in treating mydriasis produced by atropine, has received very various interpretations; some explaining the phenomena by paralysis of the sympathetic and dilator of the pupil (M. Rosenthal, M. Bernstein-Dogiel); others (M. Grünhagen) by excitation of the terminations of the common oculo-motor in the sphincter of the iris; others, again, by a combined action of both these agents. This question has also occupied M. Martin-Damourette, and it has been one of the principal subjects of study by MM. Rossbach and Fröhlich (in a memoir named), who have limited their researches to the physiological action of physostigmine and of atropine on the iris and the heart.

Like MM. Rossbach and Fröhlich, M. Martin-Damourette remarks (a fact already pointed out by M. Grünhagen) that a slight excitation of the cervical sympathetic produces dilatation of the pupil contracted by eserine; the nerve is not then paralyzed.

M. Martin-Damourette thinks also that the irritation of the third pair (which furnishes the ciliary nerves governing the contraction of the sphincter) is not the cause of the myosis (a view which, on the other hand, MM. Fröhlich and Rossbach accept). The contraction of the pupil takes place even when the common ocular motor is paralyzed by atropine or otherwise; whence M. Martin-Damourette concludes that eserine acts directly on the constrictor muscle of the pupil, as on the other muscles.

According to Köhler (in a recent memoir), the antagonism of Calabar bean and of atropine only exists in warm-blooded animals, and is not observable in the frog. It is the same with saponine, which has an antagonistic action to Calabar bean. In the frog, as M. Roher has also shown, the Calabar bean paralyzes the accelerator fibres of movement of the heart, and does not act on the musculo-motor ganglia of that organ.

M. Köhler makes also a comparative study of various poisons which act on the heart and modify its rhythm in different ways. Analyzing the action of these poisons, he seeks to demonstrate that the phenomenon of arhythmia, studied by Heidenhain, is due to irritation of the muscles of the heart by blood containing an irritant poison, which brings on a momentary fatigue and relaxation, atony of the ventricular muscles.

According to this author, Calabar bean does not produce arhythmia, because it leaves intact till death the cardiac muscles and ganglia, and it may even stop the arhythmia produced by injections of saponine and of atropine. This interpretation of

arhythmia is different from the one adopted by Heidenhain.

We have next M. Luciani's researches made in the laboratory, and under the direction, of M. Ludwig. The author proposes to study, with the aid of the graphic method, the contractions of a frog's heart separated from the body. He gives a drawing of the registering apparatus used: it consisted of a transmission-manometer, connected with the heart, which was filled with serum, and in which a determinate pressure was produced. A style connected with the manometer registered on a cylinder the oscillations of the mercury. The author used in his experiments (following the example of Dr. Bowditch, who has experimented in the same direction), a heart stopped in diastole by a ligature applied at the level of the sinus of the vena cava; he investigates the influence of a change of temperature, of pressure, ligature, &c., on the heart. Subjecting the organ to the influence of various poisons (muscarine, atropine, nicotine) he examines into the causes governing the periodic action of the heart. From a study of the curves obtained, and analysis of his numerous experiments, the author concludes, it cannot be admitted, as a large number of authors suppose, (in particular Böhm, Schmeideberg, Köhler, &c.) that there are in the heart different centres, some of which produce arrest of the heart, the others excitation of its movements. According to him there is only one automatic centre, which can be modified in different ways by exterior and toxic agents.—*London Medical Record.*

Prof. Spiegelberg in the subjoined excerptum attempts to produce a philosophical formulation of our present knowledge in regard to "exudations in the neighborhood of the female genital tract." The subject is one of very great practical importance, and we are well satisfied that our readers will consider that the pages given to the translation of Spiegelberg's doctrines are well occupied.

*Spiegelberg on Exudations in the neighborhood of the Female Genital Tract.** By W. C. Grigg, M.D.

Professor Spiegelberg states that further experience has justified his former assertion that these exudations are the most important affections with which one has to deal in diseases of women. A knowledge of this complaint is entirely of modern date, and, thanks to French authors, we are now in a position to take a fair survey of it. Already there exists two opposing doctrines. One maintained especially by Bernutz and Gonpil, is that the peritoneum covering the female pelvic viscera and the

* *Sammlung Klinischer Vortrage*, No. 71, 1874.

neighboring pouches and folds is the more dominant factor, and that the disease is a pelvi-peritonitis; the other is, that the disease has its origin in the cellular tissue in which the genital tract is situated, and which forms the parenchyma of the broad ligament; and that a phlegmon of the pelvic cavity is the chief cause of these exudative swellings and nodules. Authors are not yet unanimous whether perimetritis or parametritis is the more frequent, and in some cases the distinction is exceedingly difficult. It is a wide field of contention amongst writers, and this arising principally from want of clearness in the anatomical representations, and from imperfect knowledge of the relations of the serous membrane to the genital organs and to the pelvic fascia, and of the latter to the uterus and vagina. Dr. Spiegelberg relates much that is already well known, for the purpose of making a few remarks on their connections, etc., which, he considers will have some influence on the etiology of this disease.

There are two plates: one a modified copy of Kohlrausch's vertical section of the pelvis and its organs; the other a transverse section, showing the vessels on the posterior surface of the uterus and cervix with the rectum and half of the peritoneum removed. He gives a brief anatomical sketch, and by aid of these plates attracts particular attention to the great amount of loose areolar tissue beneath the peritoneum at the point of its reflection from the anterior surface of the uterus (opposite the internal os), and where it covers the bladder. Between the bladder and cervix and upper portion of the vagina, it diminishes considerably. This tissue is exceedingly vascular and rich in lymphatics. On the posterior surface of the uterus, the serous membrane, instead of descending in a direct line, opposite the middle of the neck, is separated from it by a bulging mass of cellular tissue; thence it is reflected along the hinder part of the vagina, and up the anterior surface of the rectum, being for a space of one or two centimètres below this mass, in very close contact with the vagina. By this arrangement, a retro-uterine as well as a retro-cervical phlegmon becomes possible.

Laterally, the peritoneum, about the middle of the uterus, suddenly quits it, being reflected over the broad ligaments, whose roof it forms, leaving free a triangular space with its apex pointing upwards, full of loose areolar tissue. Thence the serous membrane spreads upwards over the sides of the pelvis, and over the iliac fossa. Between the peritoneum and the recto-vesical fascia there is a large quantity of cellular tissue and vessels. Beneath the recto-vesical fascia is the ischio-rectal fossa, containing vessels, a large amount of fat, and areolar tissue.

Luschka, from their relations, has named these three cavities, from above downwards, the cavum pelvis peritoneale, the cavum pelvis superitoneale, and the cavum pelvis subcutaneum (ischio-rectal fossa). Of these cavities, the superitoneal is the most important, as it contains not only the trunks of vessels, but also venous plexuses, besides abundance of veins and lymphatics,

whose roots are embedded in the uterus. It is here that the ante- and retrocervical subperitoneal tissues become united. The cervix is enveloped in cellular tissue, gradually diminishing in quantity from above downwards. This is particularly important:—1. Because this tissue is intimately connected with the parenchyma of the neck, and in a measure forms its capsule; 2. Because it not only conducts blood-vessels and lymphatics to and from the tissue of the cervix, but of itself represents a cavernous structure (Rouget); 3. Because it takes part in all irritations and tumefactions of the cervix. In lesions of the cervix, on account of the thinness of the cervical walls, the cellular tissue easily becomes effected. The slightest lesion may set up inflammatory swellings; and, from its very construction, the tissue is an extremely favorable place for the reception and spread of septic matter by the blood-vessels and lymphatics.

Virchow (*Archiv.* no. xxiii.) introduced the name of parametritis to denote inflammation of the cellular tissue surrounding the lower portion of the uterus and the vaginal substructure. This word has later come to mean inflammation, not only of the parenchyma of the broad ligaments, but also of the entire pelvic fascia (Matthews Duncan), and hence has arisen some confusion.

The pathological importance of the pelvic fascia in the different situations is very various. Dr. Spiegelberg proposes to give the name 'parametranal' to that layer, copiously supplied with blood-vessels and lymphatics, which surrounds the lower segment of the uterus and the base of the vagina for about two centimètres in width. Its inflammatory infiltration and subsequent induration would be parametritis; but, in order to prevent mistakes, it would be better called 'parametranal inflammation'—preserving the French nomenclature for inflammation of the parenchyma of the broad ligament and the pelvic fascia, viz., 'phlegmon of the broad ligament,' and 'phlegmon of the pelvic fascia.'

From this brief sketch, it is easily seen that intra-peritoneal exudation must very rarely occur in front, hardly ever laterally, but invariably behind in Douglas' pouch, consequently retro-uterine as well as retro-vaginal. As in the commencement all peritoneal exudations are fluid, they flow as a matter of course downwards, collecting at the bottom of Douglas' pouch, which is the most dependent portion of the abdominal serous membrane. They only appear as tumors, when encysted through gluing together of the intestine and pelvic viscera with the parietes by lymph. The process is generally gradual, but in some instances very rapid. Not a few of these cases are diagnosed as retro-uterine hæmatocles. The contour of the swellings is always smooth and uniformly arched. The tumor is lowest at its centre, sometimes reaching down as far as the external os, especially in women who have borne children, in whom Douglas' pouch extends lower down. Laterally, it never touches the sides of

the pelvis, but approaches nearer to them from below upwards. Its border is ovoid. The uterus is pushed forwards and upwards to an extent depending on the size of Douglas' pouch and the amount of fluid effused.

An intraperitoneal exudation has not a retro-uterine position except when it collects in occasional hollows, previously formed by false membranes and adhesions, either between the bladder and uterus, or laterally between it, the Fallopian tubes, and ovaries. Adherent loops of intestines may simulate tumors. The high situation of these tumors, and their being placed permanently laterally and in the posterior part of the cavum peritoneale, secures their correct diagnosis; still there are cases in which it is exceedingly difficult to distinguish them from phlegmonous nodules.

Professor Spiegelberg lays great stress on the fact that 'parametranal phlegmon forms a tumor from its very first commencement,' from being surrounded by a layer of cellular tissue. Its consistency varies; at first it is boggy, then fluctuating; later on, fixed, hard, knotty, and cord-like. The swelling may be on any side of the cervix; it is rarely large in front, from the small amount of areolar tissue between the bladder and neck, but may extend upwards along the abdominal walls for some distance, presenting a board-like structure, with a sharp well defined border closely attched to the abdominal integuments, reaching to various heights in the inguinal region, and losing itself in the iliac fossa. It may be situated posterially, forming a retro-cervical parametritis, and is frequently mistaken for an intraperitoneal exudation, with which it has a great tendency to combine; it is distinguished by its diffuse border gradually emerging into the parenchyma of the broad ligament, and its marked retro-vaginal position, pushing the cervix forwards and upwards. As a rule, parametranal exudations are situated laterally. If they be of large size they extend into the broad ligament, so that a phlegmon of the latter may be a continuation of a parametranal inflammation. Still, phlegmon of the broad ligament may be primary, and of various sizes and positions, implicating perhaps the whole of the pelvic cellular tissue. These nodular exudations project downwards, shortening the contiguous portion of the vagina, and can be only felt through the abdominal walls when very large. Their contour is irregular, lumpy, gradually passing into healthy tissue and, as a rule, fixing the uterus to the pelvis.

As previously stated, Dr. Spiegelberg considers the pathological importance of the parametranal tissue the greater; and in studying the etiology of the affection, the truth of this assertion is very apparent. Any injury to the lower segment of the uterus and upper portion of the vagina, however brought about, may set up inflammation of the tissue. It is no accidental coincidence, as no gynæcological affection is so frequent, not only as the result of child-bed, but also from any lesion. The limits of

the disease are entirely dependent upon the idiosyncrasy of the patients, and beyond the control of art. Septic parametranal phlegmon, and even septicæmia have followed the most trivial wounds. Its spontaneous origin from so called internal causes cannot, Spiegelberg thinks, be denied. Primary phlegmon of the broad ligaments chiefly originates spontaneously, proceeding from tumefaction of the membrane surrounding the situs of the ovary through some irritation of that organ, or from some small menstrual hæmorrhage into the broad ligament. Now and then they take their origin for external injuries.

He believes that pelvic peritonitis originates occasionally as a sequence of paracervical inflammation; from contusions and lacerations of old standing, false membrane, the results of a previous confinement; but generally from endometritis and salpingitis, or from some menstrual disturbance. In the former it is principally the result of a gonorrhœal inflammation, in the latter it comes through the retention of some small clots in the above organs, causing extravasations into the peritoneal cavity, and on the surfaces of the Fallopian tubes and ovaries.

He sums up by saying: 'Parametranal nodules proceed from some pathological condition of the lower section of the internal genital organs. Phlegmon of the broad ligaments follows parametranal phlegmon, or springs from the parovarian membrane. Pelviperitonitic exudations accompany affections of the uterus and the Fallopian tubes, particularly of its internal surface; the latter are less frequent than the former and are most generally secondary; they never progress so insidiously as inflammations of the cellular tissue tend to do. Only at an early date, by careful examination, can the course of the development of the disease be learned. Frequently enough, this knowledge is concealed. The history of parauterine and periuterine inflammation will long be a fertile field of clinical, as well as of anatomical and gynæcological observation.'—*London Medical Record.*

Acute dysentery is a disease which brings great pain and danger to the patient, as well as great anxiety to the physician. Its successful treatment, when severe, requires prompt as well as correct therapeutic advice. We feel sure that there is scarcely any other acute disorder of equal violence, whose course and ultimate issue are more favorably influenced by the early application of proper remedies. We have so often seen free purgation, induced preferably by a saline, give a desirable leverage in respect to the future control of cases of dysentery, that our solicitude for the patient's safety has come to be habitually lessened when this has been an early step of treatment.

We believe, as a rule, our cis-atlantic mode of treating dysentery is quite equal, both as it respects its philosophy and its success, to that of other countries. Indeed, the greater prevalence of the disease here, and the consequent greater experience in its management, should give us a superior skill in its cure. Nevertheless, it is frequently a matter of profit to us, to study the course pursued by those who have had to deal with this affection in latitudes where it exhibits its utmost degree of virulence. Some very admirable reports of cases by an English medical officer serving in the Ashantee expedition, afford us the opportunity to give our readers this advantage.

Dysentery. Case I.—Improvement.

Captain G., aged 34, service fifteen years, a volunteer for the Ashantee expedition, arrived at Cape Coast Castle in the latter end of September. Was attacked with dysentery early in December, while serving at the front. For this he was sent down country, and ordered on board H. M. troop-ship *Tamar* for a cruise. He improved considerably at sea, and the dysenteric symptoms quite disappeared, only, however, to recur on his return to the Coast. Transferred to hospital-ship on the evening of January 2d, in a very debilitated state, and suffering from dysentery in a sub-acute form. Skin bathed in sour perspiration; pulse 60, weak and jerking; respirations 22; temperature 98.5°; tongue flabby, and covered with a thick brown fur. On examination he was found to complain of pain over the cæcum, extending along the course of the colon, and increased on the slightest pressure; occasional attacks of tormina; evacuations frequent, of a greenish-brown color, offensive, and seeming to consist of mucus mixed with scybala. Ordered a small dose of castor oil with tincture of opium. January 4th: passed a very good night; bowels moved by the castor oil; alvine dejecta as before; no tenesmus, and only slight tormina; tongue very brown; pulse 54; respirations 22, temperature 98.4° at 8 a. m., and 99° at 5 p. m. Treatment exclusively dietetic, viz., sago pudding, eggs and milk, barley-water as a drink, six ounces of port wine daily, and after, a little beef-tea and chicken-broth. He improved steadily under this treatment, and the bowel affection had quite subsided on January 9th, when he left for England by the hired transport *Manitoban*, but his general health had suffered so much that he was recommended to spend the spring months at Lisbon, Mentone, or the south of France.

Case VI Acute Dysentery and Fever—Death—Post Mortem Examination.

Private W. S., 2d Battalion Rifle Brigade, aged 27; service

three years. Landed at Cape Coast Castle with his regiment on January 1st, and marched with it to the front. On the evening of January 21st he was attacked with remittent fever, sent down to Cape Coast Castle, and admitted to H. M. hospital-ship *Victor Emmanuel* on the evening of the 30th of the same month. On admission he was very pale and weak, but free from fever, and was able to walk about. He did not exhibit any signs of a scorbutic taint. During the night of the 31st he was attacked with diarrhœa, unaccompanied by abdominal pain, tenesmus, or pyrexia, which continued, however, so slight as not to occasion any uneasiness until the evening of February 3d, when suddenly high fever set in without any preliminary cold stage. This was characterised by great heat of skin, temperature rising from 98.2° to 105.4°; congestion of eyes, suffusion of face, and severe frontal headache. The bowels continued loose, but there was no abdominal pain, even on pressure. Towards the morning of the 4th the pungent heat had gone, the skin became moist, and the temperature fell to 104°; but the headache and congestion continued, as did the relaxed state of the bowels, which were moved four times during the night. Tongue was red at the edges, but furred down the centre; no gastric irritability, no tenesmus, and no evidence of pain on pressure being made over the abdomen. Quinine was now given in ten-grain doses, and thirty grains were administered between early morning and 4 p. m. The symptoms did not change during the day; he took a little beef-tea and wine; in the evening the temperature was still 104°, bowels were quiet, and skin moist, and he no longer complained of headache. Slept a little during the night. On the morning of the 5th, temperature had done up to 104.6°, and some irritability of the stomach was evinced, but this was not so great as to cause it to reject the quinine, of which he took twenty grains before noon. About 3 p. m. severe vomiting set in, a little bilous fluid alone coming up. At the same time the abdomen became very tender all over, and the bowels were moved every half-hour, the motions consisting of a little chocolate-colored fluid, which was passed without any straining. Severe pain was now felt on pressure being made over right iliac fossa. Counter irritation was applied, with the effect of relieving for a time the irritability of the stomach, but throughout the night the bowels were moved very frequently. On the morning of the 6th the temperature had fallen to 100°, there was less vomiting, and the pain in the abdomen appeared to be relieved. The looseness of the bowels, however, continued, and the same chocolate colored fluid was passed. He was able to retain beef-tea, iced drinks, and brandy. In the evening vomiting had quite ceased, the temperature had risen to 102.2°, and the patient appeared to be much prostrated and falling into a typhoid state. Became delirious during the night, and passed several motions in bed; matter passed was of the same color, and very offensive. On the morning of the 7th, temperature had fallen to 100.4°, the skin was cold and clammy,

features pinched, tongue red, dry, and pointed, and every sign was present of great general depression; no abdominal tenderness or swelling was to be noticed, but the involuntary stools continued in character as already described. Notwithstanding the liberal administration of beef-tea and stimulants, he never rallied after this; delirium set in towards evening, and at 2 a. m. of February 8th he expired.

Post mortem examination was made of the body eight hours after death. As the abdominal viscera alone presented signs of disease, no notice is here taken of the state of the other organs. The stomach was found to be congested in irregular patches of a bright pink tint. Duodenum and upper part of small bowel, down to within a foot of the ileo-cæcal valve, normal. Here the bowel presented a dark livid color; the mucous membrane was almost black, and granular-looking; the bowel was much increased in thickness. Glands not evident, except just above the valve, where one or two were prominent and had commenced to ulcerate. The cæcum and first eight inches of colon were of a dark color; mucous membrane thickened; glands dark and prominent, but not presenting signs of ulceration. The remaining portion of the colon was simply congested, the vascularity becoming less towards the rectum, which was healthy. Liver normal in size, pale in color, and very friable. Spleen small, of a deep purple color, and soft and pulpy consistence. Mesenteric glands enlarged and congested. The cut muscle presented the peculiar purplish color met with in cases of death from enteric fever.

Case VIII. Acute Dysentery—Death—Post-mortem Appearances.

Sergeant W. B., Royal Engineers, aged 30, service seven years, including four months and a half on the Gold Coast. Contracted diarrhœa up-country on January 24th, which shortly afterwards merged into dysentery, and he was sent down-country—a most fatiguing journey to a sick man under any circumstances, and especially so to a patient with dysentery. He had to be detained at several stations on the road, owing to his prostrate condition, and when taken on board the hospital-ship, on the afternoon of February 15th, he appeared to be in the very last stage of exhaustion. Stools very frequent, consisting of chocolate-colored fluid tinged with blood; and at times clotted blood was passed in considerable quantities, attended with much tenesmus. Great tenderness was evinced on pressure being made over the cæcum and colon. Stimulants, with beef-tea, chicken-broth, jelly, and milk and soda-water were freely administered and retained, hot fomentations and cataplasms were repeatedly applied, and ipecacuanha with opium prescribed; but he was too exhausted to derive more than temporary relief from any treatment, and was only kept alive by nourishment and stimulants. He died on February 20th, six days after admission.

Post mortem examination of the Body five hours after Death.—External appearances: body much emaciated. Head: great congestion of the vessels of the pia mater, with sub-arachnoid effusion at the upper and back part of the hemispheres, Chest: old and firm adhesions of the surfaces of both lungs to the pleuræ, and bases of both adherent to diaphragm; both lungs much reduced in size and collapsed; heart normal in size, valves healthy, left ventricle hypertrophied; small patches of atheroma were noticed in the descending aorta. Abdomen: liver very much enlarged, extending as high as fourth rib, and as low as three inches below the cartilages and across to left hypochondrium; substance smooth, and hepatic lobules distinctly marked; patches of white opacity were noticed over the surface, which was generally mottled in appearance; structure of the organ friable; slight reaction with tincture of iodine; capsule not adherent. Gall-bladder half full. Spleen not much enlarged; substance firm. Malpigian bodies well marked; slight reaction with iodine. Pigmentation of the mucous membrane of stomach, especially towards pyloric orifice. Small intestines healthy, except near the lower part of ileum, which was much congested. Cæcum distended with putrid bloody matter, and in a state of softening and ulceration. General thickening of the coats of the large intestine, with patches of yellowish deposit in places undergoing softening and ulceration, and extending from the ileo-cæcal valve to the rectum. The ulcers vary in size from a pin's head to one inch and a half in diameter, some being circular with a clean base and thin inverted edges, and others oval with a rough base and thickened irregular edges. The long diameter of the large oval ulcers in this, as in previous cases, was at right angles to the axis of the bowel. Coats of the bowel, especially near the rectum, were so soft as to break down under the pressure of the finger.

Case X. Acute Dysentery—Severe Symptoms—Recovery.

Surgeon L., aged 35, service fourteen years, including seven on West Coast of Africa, and eleven months on the present occasion. Ordered down from Sierra Leone last August to take part in the Ashantee expedition. Shortly after his arrival he was sent into "the bush," where he had to undergo much exposure and fatigue, with very often insufficient and badly cooked food. On his return he was attacked with fever and diarrhœa, and was admitted into hospital at Cape Coast Castle on December 15th. He was treated there for acute dysentery with ipecacuanha, pulv. ipecac. co., hyd. c. cretâ-fomentations and poultices to abdomen; port-wine, beef-tea, etc. Transferred on January to hospital-ship in a very prostrate and critical condition—pulse 86, respirations 16, temperature 102°; tongue very foul, coated with a dirty brown fur all over the surface, which was flabby and showed the indentations of the teeth. There was uneasiness complained of over

the abdomen, more especially in the right iliac region, which was intolerant of the slightest pressure; the inclination to go to stool was frequent, and attended with distressing tormina; the alvine dejections were very fetid, and seemed to consist of mucus mixed with scybala. A dose of castor oil and tincture of opium was prescribed, and a large linseed cataplasm applied over the entire abdomen. Great relief was occasioned by the latter, and the castor oil caused two motions from the bowels, which consisted of lumps of mucus and fæculent matter, with some shreds of lymph. On the night of the 5th he had a change for the worse; bowels were moved very often, the evacuations consisting of greenish lumpy mucoid matter. At 8 a. m. on the 6th, temperature was 99°, pulse 80, respirations 16. Passed a restless night, and suffered much from tormina; was purged three times; temperature at 8 a. m., 100.2°, pulse 68, respirations 18; at 5 p.m., temperature was 102·6°, pulse 88, and respirations 24. To have fifteen grains of hydrate of chloral at bedtime, and the same quantity after two hours, if required. Linseed poultice repeated, and kept on abdomen during the night, to be changed every third hour. ℞. Mist. cretæ co. ℥vj., tinct. catechu ℥ijss., and tinct. opii ℥jss.; an ounce three times a day. January 8th: Passed a very good night; bowels only moved once; temperature continues high, being this morning 101.8°, pulse 98, respirations 16. At 5 p. m., temperature 101.2°, pulse 94, respirations 18. On the 9th, had passed rather a restless night; bowels moved three times, and motions accompanied with much griping and tenesmus. Morning temperature 101°, pulse 100, respirations 16; at 5 p. m., temperature 103.4°, pulse 106, respirations 20. Astringent mixture and poultices continued, and five grains of quinine ordered once a day. On the 10th had passed a very restless night, and suffered from acute pain over the cæcum, tending to become diffused, with frequent attacks of tormina. Bowels were moved four times; motions consisted of mucus and scybala, offensive in the extreme, and were accompanied with great tenesmus; vomited frequently during the night; tongue very brown and dry; appearance anxious, and general condition low. Morning temperature 99.8°, pulse 90, respirations 18; at 5 p. m., temperature 100°, pulse 82, respirations 18. Astringent mixture, fomentations, quinine draught, and hydrate of chloral at bedtime. From the commencement brandy and port wine were freely administered, with milk, eggs, beef-tea in small quantities frequently repeated, and jelly. On the 11th, had spent a good night, almost free from pain; bowels not moved; tongue deeply furred but moister; temperature in the morning 98.8°, pulse 80, respirations 18; at 5 p. m., temperature 100.2°, pulse 84, respirations 16. Treatment continued, spongiopiline with hot water and opium being used occasionally instead of linseed cataplasms. Two pints of chicken broth, four ounces of brandy, six eggs, and two tins of preserved milk are consumed by him daily, and a special orderly has been in attendance

upon him throughout, relieved by a second during the night. On the 12th, temperature at 8 a. m. 99°, pulse 86, respirations 18; at 5 p. m., temperature 100.6°, pulse 72, respirations 18. Signs of improvement are for the first time noticeable. On the 13th, at 8 a. m., temperature 99.4°, pulse 86, respirations 16. At 5 p. m., temperature 100°6, pulse 86, respirations 16. Astringent mixture omitted, hydrate of chloral continued at bedtime, and quinine increased to ten grains daily (with dilute sulphuric acid and tincture of orange peel), five grains at noon and five grains at 4 p. m. From this date until the 18th he improved slowly but steadily. The severe attacks of tormina, which had hitherto rendered his existence miserable, gradually subsided, and finally left him; the tenesmus also disappeared; and the tenderness over the cæcum, where it had been most severe, became less. He lost that morbid craving for narcotics, which had up to this time characterized him, as also the fancy for strange and unsuitable articles of diet; and his tongue began to clean. The temperature, however, still kept above the normal standard (98.8° to 99.4°); nor did it fall to the standard until January 24th, by which time he had been up and going about for some days. On the 15th the quantity of quinine was increased to eight grains twice a day. Having suffered much in health from long service on this coast and from his late severe illness, he was recommended change of climate, and left for England by the hired transport *Elizabeth Martin* on February 6th.

Case XI. Dysentery—Death—Post Mortem Examination.

Assistant Commissary M., aged 40, but looking much older, with a total service of twenty-two years, of which a considerable portion was spent in the tropics, had been four months on the Gold Coast, and enjoyed good health until January 24th, notwithstanding very hard work and great exposure to solar heat and malarial influence. On that day he was attacked with dysentery, which would appear not to have been preceded, as it so often is out here, by "coast fever." He was sent down country to Connor's Hill Hospital at Cape Coast Castle, where he was treated by ipecacuanha, pulv. ipecac, co., fomentations, beef-tea, stimulants, etc.; but as he did not improve he was transferred to the hospital-ship on February 1st. He was carried on board in a very weak and semi-collapsed condition. Pulse small and compressible, 124; temperature 102°; skin moist and clammy. On examination the abdomen was found to be tympanitic, and tender on pressure over the cæcum and ascending and transverse colon. The bowels moved very frequently, but without much tormina or tenesmus; stools scanty, consisting of serum tinged with blood, with mucus and clots. He was ordered fomentations and Dover's powder, milk and soda water as a drink, and beef-tea and brandy in small quantities at frequent intervals. In the evening he became worse, and hiccough set in. Champagne and egg-flip

with brandy were freely administered, with a view to stimulate the flagging powers of life; but he never rallied after admission, although he took with avidity and retained everything that was given to him. Delirium set in during the night, followed in the morning by picking at the bed clothes, subsultus tendinum, and involuntary motions from the bowels; and at 12:45 p. m. on February 2d he expired.

Post Mortem Examination, five hours after death.—External appearances: height about five feet ten inches. Body well nourished; abdomen distended and tympanitic. Several livid patches of ulceration, probably scorbutic, on the legs, varying in size from a three-penny piece to a florin. Head not examined. Chest: beyond slight congestion of both lungs, nothing worthy of note was discovered. Abdomen: liver enlarged; right lobe thicker than normal; left lobe flattened and elongated, and extending across the epigastric region; hepatic substance pale and friable; no reaction with tincture of iodine; capsule adherent; gall-bladder distended. Spleen small, substance soft, and capsule adherent. Kidneys healthy. Small intestines healthy, and contained bilious matters and gas. The solitary glands of the ilienm, near ileo-cæcal valve, were large, hard, and elevated, and contained white exudation material. Large intestines from ileo-cæcal valve to the rectum were in a state passing into gangrene. The caput cæci was distended with putrid matter, and exhibited several large sloughs in the process of separation. In the colon there were large patches of separating sloughs, with occasional spots clear and free from ulceration. Oval and circular ulcers, varying from an inch to an inch and a half in diameter, were found throughout the colon, and down as far as the rectum, their long diameter being transverse to the bowel, and parallel to the muscular fibres. Coats of the gut were so thin and soft in places as to break down under the finger.

Case XIV. Dysentery—Death—Post Mortem Results: Very severe Ulceration and Perforation.

Private P. C., 42d Highlanders, aged 25; served five years and eight months, of which two months were on the Gold Coast. Admitted to hospital-ship on the evening of February 23d, for dysentery contracted twelve days before in the front. He was carried on board in a most prostrate condition, greatly emaciated, body covered with bullæ and livid patches of ulceration; eyes and cheeks so sunken as to give him a death-like expression; pulse 100, and extremely feeble; temperature 99°; mind wandering; bowels moved involuntarily; tongue dry, and coated with a yellow fur; stools frequent, and consisting of dark-colored bloody mucus. Ipecacuanha in small doses was prescribed, with turpentine fomentations, and a starch and opium enema; and beef-tea and brandy, with milk and eggs, were administered frequently. Little change was noticed on the 24th and 25th, except that

towards the morning of the latter day he became very restless and feverish, and was almost quite collapsed on the morning of the 26th. Ten grains of Dover's powder, with five grains of quinine, were ordered three times a day, with repeated fomentations and sinapisms to the abdomen; and his strength was kept up by constant supplies of nourishment and of stimulants in small quantities at a time. He complained of great heat and pain over the lower part of the abdomen, which evinced much tenderness on pressure. Stools very fetid, dark-colored, and containing bloody clots; stomach very irritable. February 27th: pulse 100; temperature 92.2° at 8 a. m., and at 5 p. m. 99.8°. He sank gradually throughout February 27th and 28th, and died at 5 a. m. on March 1st. The stools at the last were greenish, fetid, watery fluid, and the matter vomited was much the same in character.

Post Mortem Examination, six hours after Death.—External appearances: body much emaciated; livid circular spots and some superficial ulcers, about the size of a three-penny piece, were observed upon the back of the neck and on the arms and chest, probably scorbutic. Head and chest: nothing worthy of special note was discovered. Abdomen: liver normal in size, structure pale and friable, and capsule adherent in places. Gall-bladder full of thick inspissated bile. Spleen soft; capsule adherent. Kidneys congested. Congestion of the mucous membrane of the cardiac end of stomach. A large ulcer, three-quarters of an inch in diameter, was found just outside the pylorus, in the commencement of the duodenum, and almost perforating its coats. Contents of the large bowel were found effused around the caput cæci from perforation, and in the same locality was noticed recent exudation of lymph and dark gangrenous discoloration. Recent lymphy exudation existed about the bladder, gluing together the convolutions of the small intestines to one another, to the sigmoid flexure of the colon, and to the upper part of the bladder. Large and small intestines distended with gas; lower part of the ileum highly congested and thickened, with spots of commencing ulceration. Lower half of the caput cæci separated as a slough. Large oval ulcers—the large diameter transverse to that of the bowel, and varying in diameter from one inch to two—were common in the whole course of the intestine. A large perforating ulcer, one inch in diameter, with dark-looking and regular edges (as if punched out), was noticed at the lower part of the sigmoid flexure, surrounded by lymphy exudation. Peritoneum rough from patches of recently effused lymph. Large intestines, from caput cæci to rectum, much thickened and ulcerated, especially towards the latter, and soft and gangrenous in places.

Neither in this nor in any of the previous post mortem examinations have any parasites been detected.—*Medical Times and Gazette.*

In this connection we also call attention to some valuable hints in regard to the mode of action of Ipecacuanha, in the cure of dysentery, taken from a Madras letter to the London *Medical Times and Gazette* of April 11th, 1874.

Dysentery is a very common* malady here amongst natives and Europeans, and seems to me (as a looker on) to take here, partly, the place of bronchitis in colder climates. Anyhow, errors in diet and the influence of a low temperature are effective causes, especially if they often act in combination. The soldier who gets drunk and lies asleep in the open air, exposed to the night dews; the gentleman who eats fruit at a late dinner, and sleeps in a cold current of air; and the native who sleeps in his "pyal" or verandah, thinly covered with a strip of calico, may each suffer from dysentery. So may the man who goes suddenly out of the heated plains into a colder climate, and who neglects to put on warm clothing. The great remedy used against it by Dr. George Smith, at the General Hospital, was *ipecacuanha;* and the details of his practice were very instructive to me. If I may use an almost obsolete phraseology, the drug was so used as to be a revulsive, evacuant, anti-spasmodic, sedative, and febrifuge. Let us suppose a patient admitted; feverish, with griping pains, and frequent attempts at stool, which give issue to merely a little mucus, perhaps bloody. The first thing done usually is to administer a dose of twenty or twenty-five minims of laudanum, to check the immediate urgency of the symptoms and any subsequent tendency to vomit. Then, after an hour, a full dose of ipecacuanha—say twenty-five grains—is administered in the form of bolus or pills. In order to avoid vomiting, the patient is made to lie quite still, in the horizontal posture, and is forbidden to drink for a time. If he vomits the first dose, it does not matter; the "revulsion" and "evacuation" do good rather than harm. After six to twelve hours the dose is repeated, and it is soon found that the stomach tolerates it; and a large dose is less likely to create vomiting than a small one. Meanwhile the patient is kept quiet, and fed on milk. The dose is repeated at intervals, according to the urgency of the case, till at last there come away free, copious stools, of a peculiar character—"ipecacuanha stools" they call them; whether because produced by the drug, or because they are of a light brown, like ipecacuanha mixed with water, I cannot say. This evacuation is at once a cause and symptom of relief. The mucous membrane of the upper half of the alimentary canal is disgorged, its contents are discharged, and with this the spasm and irritation of the lower half, with the straining, and scanty and slimy stools, are relieved. Then the remedy must be repeated in smaller doses, at night and morning, or at

* Forty-nine Europeans, of whom four died, and twenty-one natives, of whom three died, were admitted in 1872.

night only, till the symptoms vanish, and the patient is gradually restored to his ordinary food and work.

This treatment is adapted for quite recent cases in robust persons; but for cases of longer duration, or less favorable it will require to be modified at the practitioner's discretion. The ipecacuanha (as I learned from Dr. Oswald and Dr. Young, of Bangalore) may also be used as a local application. Twenty grains rubbed up in water may be injected into the rectum, where it will act locally as a soothing remedy, and produce its constitutional effects as well.

Just in the same way, ipecacuanha is used by Dr. George Smith for spasmodic asthma, and for acute bronchitis with spasm, which seems to be not uncommon here. It will be seen that this drug takes the place which calomel, with or without opium, took in the practice of the generation now passing by, and that it does what calomel was believed to do, i. e., evacuate foul secretions, relax spasm, "equalise the circulation," promote perspiration, and so relieve local irritation and congestion. English practitioners are often enough called in the early night to children with "croup"—that is to say, a child has been exposed to one or both of the hostile elements, cold wind and indigestible food; it wakes out of its first sleep, hot and restless, and coughing with the peculiar sonorous inspiration, whence the name "croup." In such a case it is the practice to empty the stomach first with a full dose of ipecacuanha or antimony, after which the symptoms often yield; but they may go on, and become a truly inflammatory *cynanche trachealis* (neither *diphtheria* nor *laryngismus stridulus*). Perhaps ipecacuanha, pushed as above described, would be as efficient as the leeches, antimony, and opium which are the practitioner's trusty weapons at home.

The following remarks concerning Malaria are also taken from an account of the Ashantee expedition.

To begin with Netley, where one has an opportunity of comparing the results of the malaria on the Gold Coast with those of the most malarious districts in India, as seen in soldiers invalided for malarious disease from both regions: it seemed to us that the results of the African malaria were more severe than those of the Indian form of the malady, but then the long sea-voyage in fairly warm climates, which has such a revivifying effect, had to be taken into consideration in the Indian cases. The first thing that strikes the observer; with regard to the subject of malarial poisoning, is the intense anæmia and debility produced by a comparatively short febrile attack. Some of the men we saw were blanched to the uttermost (their tongues, gums, and lips quite white), yet they complained of nothing save weakness—in short, said they were quite well.

Malaria itself is a curious thing; the nature of it no man can tell, and its workings are as little capable of being reduced to

rule as are its effects. How it is that of several men, subjected apparently to exactly similar circumstances, one should be seized and another left, is hard to say; but of this we have some analogy in our English fevers; but why one man, who apparently had been not at all exposed to its influence, should be seized with fever, and others who had lived in swamps all the time they were on the Coast should escape. is not easy to make out. One man we saw, belonging to the Welsh Fusiliers, who said he had never been ashore, yet he was sufiering from fever. Another curious thing to be remarked was that several of the men had been all the way to Coomassie, been perfectly well during the whole expedition, till close upon Cape Coast Castle on their return, and there broke down of fever. In other instances, again, the men had left the Coast perfectly well, and fallen ill of fever on the voyage home. One man distinctly told the onset of his illness. He left Madeira in fairly warm weather, and in a few hours sailed into a cold region; then the attack of fever commenced. This is no isolated instance, but has been noted again and again, and furnishes some grounds for belief that in certain ways it would have been better to direct these regiments to some warm station before bringing them home to England. On the other hand, the prompt return from the Gold Coast has undoubtedly saved many valuable lives.

The malarial poison has in this expedition almost invariably given rise to either remittent or intermittent fever or to dysentery—sometimes to more than one of these. Thus it has been no uncommon thing for a soldier or sailor to be sent to hospital for remittent—if such a name could be given to the malady—and after a time to suffer either from intermittent or dysentery. One good example of the latter sequence we saw at Netley, where a man had been ill of fever ashore, but was fairly well when he embarked, yet after being some time at sea severe dysentery developed itself, of which the man was ill at Netley. We have said above that sometimes the malady could hardly be called remittent; it seemed to the sensations of the fever-stricken patient to be continued. It is, indeed, to these sensations we have as yet mainly to trust; for we fear we have hardly arrived at such a pitch of scientific accuracy as to take temperatures regularly while on a campaign. At all events, if such temperatures have been taken they have not yet come to hand, and the materials for a full medical history of the expedition are not yet available. No doubt the thermometer would have recorded some degree of diminution in the morning temperature; but to the feelings of the sick man there was none. This in a certain way indicates the virulence in the type of the malady. Ordinarily the fever lasted about five days, sometimes eight, and in some others fourteen days; but even the shortest period was enough to blanch and debilitate the strongest men.

The onset of the disease was generally marked by a severe chill or rigor, but not always. In some cases vomiting was

severe and persistent, but not in all. Some complained of pains in the back and loins, but all had headache of a most distressing kind, and often lasting long—even weeks or months—after all other symptoms had passed away. Most frequently the pain was situated in the forehead, sometimes at the back of the head, and in others still over the top and sides of the head, especially if the man had been exposed to the sun on sentry duty just before falling ill. The pain was agonizing, and described by some as if the top of the head was blown off. The bowels were, as a rule, constipated.

Most of the fatal cases ended with brain symptoms, which in many came on in the most insidious fashion, sometimes with very little sign of fever. Sometimes these symptoms would take the form of excitement; delusions would arise, sometimes ending in suicide, but in the end the symptoms were those of coma, and after death there was plentiful effusion into the cranial cavity. Fortunately, such cases have not been very numerous, and in a good number of instances judicious treatment, especially by the application of cold to the head and blisters to the nape of the neck, has sufficed to bring about recovery after unmistakable brain symptoms have appeared. Hepatic and splenic symptoms have not been very marked, though a minor degree of congestion of both organs has probably been present in all or nearly all.

The treatment adopted in most of the cases has been the ordinary remedy, quinine in full (i.e., fifteen to twenty-grain) doses. At Netley, however, as well as on the Coast, Warburg's tincture is largely used, and with great advantage. The dose is half a bottleful. This secret remedy is one of very great value, and it may be questioned whether it would not be cheaper for Government to purchase the secret than to continue buying it at the heavy price now paid for it, inasmuch as it is now very largely used in all malarious regions, military surgeons in this following the teaching of Dr. Maclean. When the fever has, left, and only debility and anæmia remain behind, citrate of iron and quinine in average doses are ordinarily prescribed.

The composition of "Warburg's Tincture" is supposed to be for each bottle, approximatively as follows:

℞—Socotrine Aloes,
Root of Zedoary, aa ℈iv.,
Angelica Root,
Camphor, aa grs. ij.,
Saffron, - - grs. iij.,
Alcohol, - - - - ℥iij.

Digest for eight days, filter, and add to the filtered liquor, Sulphate Quinine ℈ij.

Dose—℥ij to ℥j.

White Canella may be substituted for the Zedoary.

It is well known to the profession that several of those patented "Cholagogues" and "Ague-cures" owe their efficiency to a combination of cinchona salts with aloes. The writer has made a fair trial of "Warburg's Tincture," prepared by the above formula, and while he is not prepared to accord as unqualified praise to its efficacy as MacLean and other British practitioners do, his success in its use was sufficiently pronounced to justify reference to it in this connection. There can be no doubt that its utility is likely to be more clearly marked in those cases of malarious disease attended by saburral states of the system. The tendency to the occurrence of comatose conditions indicates quite positively that this was one of the leading characteristics of the cases reported. The almost universally profound anemia adverted to by the writer, shows that a clinical period of disease had existed in which eliminative measures were likely to have proved of service. This fact, however, does not make it evident that the purgation and diaphoresis induced by Warburg's Tincture constituted the only, or even the most desirable method of securing elimination. My own experience, now so large as to be entitled to a good deal of weight, is decidedly in favor of the superior benefits resulting from the free use of bitartrate potash given in lemonade as eliminative treatment. But while this may be true with regard to the great numerical majority of malarial attacks in this country, it is not by any means improbable that the energetic revulsive and depurative effects of aloes afford more proper indications in those states of overpowered nerve function observed in the intense toxæmic conditions occurring after exposure to malaria in tropical regions.

Treatment of Acute Rheumatism by Immovable Bandages.

Dr. Oehme reports, in the *Archiv der Heilkunde*, vol. v., 1873, the observations made by Heubner in the hospital at Leipzic, on the treatment of acute rheumatism by fixing the parts immovably. Similar experiments had already been made by Scutin and Gottschalk; and in more recent times by Concato, of Bologna; with good result. Heubner used pasteboard; but for small children and restless patients, the plaster of Paris bandage was necessary. In applying the pasteboard splints, the upper limb was bent at a right angle at the elbow, and the leg was straightened at the knee; the splints were well wadded, and fastened by bandages. The apparatus was generally applied immediately on the admission of the patients, and was allowed to

remain, not only until all pain and swelling had left the joint, but until the constitutional symptoms had disappeared, and especially until the temperature had returned to the normal standard. The results of this treatment were extremely favorable as regarded the pain, the fever, and the duration of the disease. Even when the pain had been most severe, the fixing of the part was followed in a few minutes by so great an amount of relief that the patients were almost free from pain. Of forty-five cases collated by Dr. Oehme, this result occurred in all without exception. After a time, varying generally from one to two days, all pain finally disappeared in the joints which were fixed. The period of final cessation of pain is later in those joints—the hip and shoulder—which cannot be so securely fixed as others. To show the effect on the duration of disease, Dr. Oehme gives tables, comparing forty-five cases treated by fixing the joints with forty-five similar ones treated otherwise. While in the latter the average duration of the pain was 21.75 days, in the former it was only 13.11 days. The treatment by fixing the joints had also a favorable influence on the duration of the fever, which was distinctly shortened. It seemed also as if the fixation of a joint acted as a prophylactic in preventing the other joints of the same limb from being affected. From the observations now made, it may with safety, Dr. Oehme thinks, be asserted that the treatment of acute rheumatism with the immovable bandage reduces the pain to a minimum, shortens the duration of the fever, and essentially cuts short the course of the whole disease.—*Medicin.-Chirurg. Rundschau*, March, 1874.

Hydrate of Chloral in Incontinence of Urine.

Dr. E. Vecchietti, in a communication made to the Medico-Physical Society of Florence, in December last, after citing the observations of Thompson, Bradbury, and others, on the use of hydrate of chloral in nocturnal incontinence of urine, relates five cases of the malady occurring in his own practice. All had been treated unsuccessfully; and all the patients recovered under the use of rather less than half a gramme (about 7½ grains) of hydrate of chloral, given in a little water in the evening; abstinence from drinks being at the same time enjoined. The effect was rapid, and in most cases permanent after taking the first dose. He explains the action of the chloral by supposing that the bladder is under the influence of two sets of nerves, one connected with the cerebro-spinal system, and the other with the sympathetic; that incontinence of urine depends on irritation of the bladder at the same time that the sphincter is withdrawn from the action of the will; and that the chloral reduces the exaggerated excitability of the organ.—*L'Imparziale*, March, 1874.

Medicinal Uses of Chloral.

Dr. Joseph Pollak, of Salzburg, concludes an elaborate paper on chloral hydrate and its uses in medicine, published in the *Wiener Medizinische Wochenschrift* (Nos. 4, 5, 6, 8, and 9, 1874), with the following summary. 1. Chloral is a good hypnotic. In all diseases consisting of or complicated with abnormal irritability of the brain, it tends to produce sleep and tranquilize the patient. 2. It allays pain by producing sleep; without sleep it has no power to allay pain, and when pain is very intense, it has little hypnotic power. In such cases, it is very useful to combine the chloral with morphia. 3. It relaxes the muscles, single or in combination, voluntary and involuntary; and is an excellent remedy against the most varied forms of convulsion. 4. In cases of diseases of the heart and lungs, with disorder of the digestive canal, it is sometimes inoperative, and sometimes produces unpleasant and even dangerous symptoms; hence it is here either contra-indicated, or should be given with caution. 5. In general it cannot be used as an anæsthetic for great operations. 6. Its continued use is generally not followed by any unpleasant after-symptoms; and if such appear, they are of no importance, as it does not produce congestion of the brain or disturbance of digestion and nutrition. 7. In most diseases in which it is used it is an excellent palliative, but it has almost no influence on the disease itself. It may be briefly said, that chloral hydrate is especially indicated where morphia would be indicated, except for its unpleasant after-effects. It is contra-indicated in diseases of the heart, lungs, and digestive canal.—*British Medical Journal.*

Subcutaneous Injection of Carbolic Acid in Erysipelas.—Dr. Anfrecht, of Magdeburg, says (*Centralblat für die Medicinischen Wissenchaften*, February 21st) that, having last year under his care four cases of erysipelas of the limbs in old persons, in all of which treatment was unsuccessful, he was led to try the effect of carbolic acid. If erysipelas were the result of the entrance of minute organisms into the subcutaneous connective tissue and of their multiplication, and if carbolic acid had the property of destroying such germs, then, he argued, it should arrest the spread of erysipelas. To ascertain that the hypodermic injection of carbolic acid was harmless, he injected into his own subcutaneous tissue six decigrammes (about ten minims) of a one per cent. solution. No local or general disturbance followed. In July of last year, he applied this treatment in the case of a woman aged 56, with erysipelas of the forearm and hand, following a slight abrasion; and in January he used it in the case of a man aged 82, who had erysipelas of the leg after the breaking out of a cicatrised ulcer. In the first case, he injected carbolic acid night

and morning for three consecutive days, making five injections in all; in the second, four injections in two days: they were made into the healthy subcutaneous tissue in the neighborhood of the erysipelatous portion lying nearest to the body. The erysipelas did not spread in the direction of the part where the injection was made, but some isolated patches above the points first injected necessitated the more frequent repetition of the remedy. Not only was the erysipelas arrested, but the fever and the frequency of the pulse were reduced, and the general condition of the patients was improved. The erysipelatous swelling and redness diminished perceptibly, remaining only two days after the injection.—*British Medical Journal.*

From the "Croonian Lectures on the Functional Derangements of the Liver," by Dr. Murchison, we extract the following opinions in respect to "Errors of Diet" as a cause of such functional derangements.

Functional derangements of the liver, when primary, may be due to a variety of causes. Of these, the principal are—

1. *Errors in Diet.*—There can be no doubt that the present system of living, and especially the consumption of even what are regarded as average quantities of rich food and stimulating drinks, contribute largely to derange the liver. It will be generally admitted, nor would it be difficult to prove, that most persons are in the habit of eating a quantity of food far greater than suffices to maintain the nutrition of the body. Much of this excess is fortunately never assimilated and is got rid of in the fæces; but very often much more is taken into the blood than can be converted into tissue, or pass through the ordinary processes of oxidation preparatory to elimination. The result is that the excess of food is thrown out in an imperfectly oxydized form by the kidneys, lungs, etc., or accumulates in the system; while more work is thrown upon the liver than it can readily perform, and functional derangement of the organ ultimately ensues. With regard to individual principles of food, speaking generally, it may be said that the liver is most apt to be deranged by saccharine and fatty substances. The derangement of the liver which leads to lithæmia or gout is more likely to be induced by even small quantities of these substances than by a moderate excess of purely nitrogenous food, such as meat. Cooked articles of diet containing a large proportion of both sugar and fatty matter are in many persons certain to derange the liver. The excess of carbon in these substances must either be deposited as fat, or must take away the oxygen, so as to leave little free to act on the nitrogenous matter passing out from the tissues or derived from the food; and hence, as Dr. Bence Jones has observed, in speaking of gout, "with carbonaceous diet in

excess, the whole of the uric acid from the tissues might pass off through the blood unoxydized."* There are also constitutional peculiarities with regard to many articles of food, which always derange the liver in certain individuals, though they are comparatively harmless in others; and some persons who are the subjects of lithæmia get on best with a vegetable diet and milk.

But of all ingesta the various alcoholic drinks are most apt to derange the liver. They do so in two ways. *a.* They may cause persistent congestion of the liver. Even small quantities of alcohol in healthy persons produce a temporary hepatic congestion; but if alcohol be taken in excess, or too frequently, the congestion of the liver becomes permanent, and the functions of the organ are deranged. Like results may ensue from comparatively small quantities in certain persons, who may be said to have a constitutional intolerance of alcohol. Of course, if the congestion be long maintained, structural disease may follow. *b.* But wines and other alcoholic drinks often cause derangement of the liver, which a corresponding quantity of pure alcohol would not produce, and which, in fact, cannot be accounted for by any one ingredient of the offending liquid—neither by the free acid, the ether, the salts, the gum, sugar, or extractive matter. This general rule, however, I believe holds good, that the injurious effect of alcoholic beverages upon the liver increases in a direct ratio with the amount of sugar plus alcohol which they contain. It would seem, indeed, that a mixture of alcohol and sugar produces injurious results, which would not be caused by the admixture of a much larger quantity of sugar, or of alcohol alone, with the food. In accordance with this view, the alcoholic drinks which are found from experience to be most apt to disagree with the liver are malt liquors of all sorts, but especially porter and the stronger forms of mild ale, port wine, madeira, tokay, malaga, sweet champagne, dark sherries, liqueurs, and brandy; whilst those which are least likely to derange the functions of the organs are claret, hock, moselle, dry sherry, and gin or whiskey largely diluted.

Derangement of the liver from excessive eating or from other errors in diet usually first shows itself in middle life—from 35 to 45. Young people, who take much exercise, and whose bodies are still undergoing development, require more food, and can often with impunity eat more than they require. But by the age of 40 the body is fully developed, and most persons take less exercise than before, while at the same time they often indulge more freely at table. At any age, errors in diet will be all the more likely to tell upon the liver, if there be any constitutional weakness in the functional power of the organ.—*British Medical Journal.*

Ergotin as a Hæmostatic.

Dr. Schwaighoffer, in the *Irish Hospital Gazette*, gives some

* *Op. cit.*, p. 142.

account of the experiments made by Dr. Drasche, of the Rudolf Hospital, on the use of ergotin as a hæmostatic:

"The experiments as to its hæmostatic powers were mostly made on phthisical subjects who had hæmoptysis, either after the usual means, such as ferri sesquichlor., alum, digitalis, plum. acet., etc., had been tried in vain, or especially in cases where the hæmoptysis began as an actual hæmorrhage. However, the ergotin was also tried in cases of epistaxis, hæmatemesis, hæmorrhage from the bowels in enteric fever, and especially in scorbutic hæmorrhages; and generally with success, even though the improvement was sometimes only transient. In cases of hæmoptysis the quantity of ergotin that was injected was from one grain to one grain and a half; in exceptional cases this was increased to seven grains (spread over several days), which was the maximum dose. The local inflammation was always proportionate to the strength of the solution. The injections were always made in the neighborhood of the greater pectoral muscle. Usually, shortly after the injection, the skin around the puncture became red and tender, with a sensation of burning and often of pain; sometimes there was swelling and persistent induration, with greenish yellow discoloration of the skin. In four very obstinate cases of hæmorrhage in advanced phthisis, where cavities had already formed, the hæmorhage was checked after from one to three injections; indeed, in one case a single injection proved sufficient. Even in those cases where the hæmorrhage was not at once checked, it at all events became much less copious, and frequently only remained as a painful expectoration of coagula, which ceased after a few more injections. The rapid effects produced by the subcutaneous injection of ergotin were all the more striking, as, shortly before, much larger doses of this drug had proved inert when administered by the mouth. In some cases where a copious hæmorrhage had its origin in a large cavity, of course we could not expect ergotin to work miracles, and still only in a single such case was its use absolutely without effect. One phthisical young man, who had been attacked every morning for a week with violent epistaxis, which had been treated unsuccessfully for four days with ice and perchloride of iron, was completely cured after two one-grain injections of ergotin. In scorbutus, where the cause of the hæmorrhage is want of tone and a liability to rupture in the walls of the vessels, ergotin is particularly useful, especially when other remedies have been tried and have failed. These results show that this remedy is general, practical, and effective. To the practitioner it cannot but be most comforting in dangerous cases to have at hand a remedy at once so certain and so easily applied, especially where sudden and profuse hæmorrhage calls for immediate action, and where, as too often happens, great difficulty is met with in administering internal remedies."—*Medical Times and Gazette.*

NOTICES OF NEW BOOKS.

A Treatise on Therapeutics, comprising Materia Medica and Toxicology, with especial reference to the Application of the Physiological Action of Drugs to Clinical Medicine. By H. C. Wood, Jr., M.D., Professor of Botany and Clinical Lecturer on Diseases of the Nervous system in the Medical Department of the University of Pennsylvania, &c. 8vo., pp. 578. Philadelphia: J. B. Lippincott & Co.; 1874.

This book is dedicated to "Dr. George B. Wood, L.L.D." * * * "by the author, his nephew." By "Dr. Wood," it may be inferred that he means the Emeritus Professor of the Theory and Practice of Medicine in the University of Pennsylvania, though the style quoted above conveys precisely no medical qualification.

The opening paragraph of the preface is presented to illustrate the author's apology for the offspring which he has begotten.

"At the present time, when the shelves of private and public libraries are groaning beneath their ever-increasing loads, when a thousand presses in every city send forth day and night their printed messages until the earth is filled with them, it seems almost presumptuous for any one to offer new volumes to the world. Indeed, art is so long, life is so short, that every student has the right to demand of an author by what authority he doeth these things, and to challenge every memoir for its *raison d'être*. This being so, it assuredly will not appear egotistical for the author to state that his voluntary task was first suggested by his own wants, and that to its performance he has brought the training, labor, and experience of years spent in the laboratory, the study, the class-room, and the hospital ward."

It is but fair to assure our readers that the first sentence is doubtless used in a figurative sense, and that the evident hyperbole must not be taken as a specimen of the prevailing style of the body of the work.

In a subsequent paragraph, speaking of the uncertainty of our therapeutic knowledge, we read: "What has clinical therapeutics established permanently and indisputably? Scarcely anything beyond the primary facts that quinia will arrest an intermittent, that salts will purge, and that opium will quiet pain and lull to sleep." Even his examples—quinia, salts, [what salts?] and opium—are not infallibly sure remedies, and we hardly venture to hope that medicine will ever become an exact science, even

under the physiological system of studying therapeutic agents—the one on which this work is based. The physiological action of remedies is claimed by the author as the one whose study will afford the most precise and useful results, though he admits that they are yet so imperfect that he is obliged to use the clinical method to complete his plan. There would be no doubt of the truth of his proposition, if the use of medicines were restricted to individuals in the condition of health; but, as sick people cannot be deprived of the benefits of physic, it seems to us that the action of remedies in pathological conditions should not be neglected; nay, more, that its practical utility is paramount in importance. With this averment, the classification of remedies according to physiological action is not denied its convenience for their consideration in a systematic treatise, nor is it intended that this action should be disregarded or depreciated.

An important feature in this work is announced in the following paragraph from the preface.

"I have added to the book a consideration of toxicology, so far as it is of interest to the physician. This has been done for several reasons. First, it was necessary to study the action of poisonous drugs upon man, in order to make out their physiological action; secondly, physicians are constantly required to diagnose and to treat cases of poisoning; thirdly, it is often of the greatest importance for a medical man in a court of law to be able to state what are the symptoms and post-mortem appearances produced by a given poison, what diseases they simulate, and how far and in what they differ from the phenomena of these diseases. That part of the science of toxicology which treats of the recognition of poisons in the cadaver, or in food and drink, belongs to the domain of the chemist, and I have avoided it altogether. For a similar reason, in the sections on materia medica, the chemical relations of mineral substances have not been discussed at all."

The classification of remedies is in some respects peculiar, and interesting enough to justify the use of the space needed to present it in full.

PART I.—SYSTEMIC REMEDIES.

DIVISION I.—GENERAL REMEDIES.

ASTRINGENTS.—*Vegetable Astringents.*—Tannic Acid—Gallic Acid—Gall—Catechu—Kino—Hæmatoxylon—Rhatany—Oak Barks—Roses—Geranium—Blackberry Roots—Tormentil—Persimmon—Pomegranate.

Mineral Astringents.—Alums—Sulphate of Aluminium—Lead and its Preparations—Bismuth and its Preparations—Zinc and its Preparations—Copper and its Preparations—Silver and its Preparations.

TONICS.—*Simple Bitters.*—Quasia—Simaruba—Gentian—Barberry—Columbo—Boneset—Goldthread—American Centaury—Dogwood—Salicin.

Peculiar Bitters.—Wild Cherry Bark—Cinchona—Sulphate of Quinine—Sulphate of Cinchonia—Chinoidine.

Aromatic Bitters.—Chamomile—Virginia Snakeroot—Cascarilla—Angustura.

True Aromatics.—Cinnamon—Cloves—Nutmeg—Allspice—Cardamon—Ginger—Black Pepper—Red Pepper—Oil of Cajeput, etc.

Mineral Tonics.—Iron and its Preparations—Sulphuric Acid—Muriatic Acid—Nitric Acid—Nitromuriatic Acid—Phosphorus.

CARDIAC STIMULANTS.—Ammonia, Carbonate and Nitrate—Alcohol—Brandy—Whisky—Wine—Turpentine—Digitalis.

CARDIAC SEDATIVES.—Antimony and its Preparations—Veratrum Viride—Veratrum Album—Veratria—Aconite Leaves and Root—Prussic Acid—Cyanide of Potassium—Vegetable Acids.

ANTISPASMODICS.—Musk—Castor—Valerian—Valerianic Acid—Assafetida—Camphor—Brominated Camphor—Amber—Oil of Amber—Hoffman's Anodyne—Hops—Black Snakeroot.

ANALGESICS.—Opium—Morphia—Narcein—Codeia—Narcotina—Thebaia—Papaverina—Porphyroxin—Meconia—Meconic Acid—Indian Hemp—American Hemp.

MYDRIATICS.—Belladonna—Atropia—Hyoscyamus—Stramonium.

ANÆSTHETICS.—Nitrous Oxide—Ether—Chloroform—Bichloride of Methyl.

EXCITO-MOTORS.—Strychnia and Drugs containing it.

DEPRESSO-MOTORS.—Calabar Bean—Bromide of Potassium—Bromide of Sodium—Bromide of Ammonium—Chloral—Nitrite of Amyl—Lobelia—Conium.

ALTERATIVES—Arsenic and its Preparations—Mercury and its Preparations—Iodine and its Preparations—Iodoform—Cod-Liver Oil—Phosphoric Acid and the Phosphates—Colchicum—Sarsaparilla—Guaiac—Mezereum—Sassafras—Dandelion.

DIVISION II. LOCAL REMEDIES.

EMETICS.—*Vegetable Emetics.*—Ipecacuanha—Bloodroot—Apomorphia—Gillenia—Mustard—Squill.

Mineral Emetics.—Antimony—Sulphate of Zinc—Sulphate of Copper—Alum.

CATHARTICS.—*Laxatives.*—Foods—Tamarinds—Cassia Fistula—Magnesia—Sulphur.

Purges.—Castor Oil—Mercury—Rhubarb—Butternut——Aloes—Senna—Saline Purgatives.

Drastics.—Scammony—Colocynth—Compound Cathartic Pills—Podophyllum—Elaterium—Gamboge—Croton Oil—Black Hellebore.

DIURETICS.—*Hydragogue Diuretics.*—Squill—Digitalis—Broom—Sweet Spirit of Nitre.

Refrigerant Diuretics.—Potash and its Preparations.

Stimulating Diuretics.—Buchu—Pareira—Uva Ursi—Pipsissewa—Juniper—Carota—Erigeron—Turpentine—Copaiba—Cubeb—Matico—Cantharides.

DIAPHORETICS.—*Nauseating Diaphoretics.*—*Refrigerant Diaphoretics.*—*Simple Diaphoretics.*—Spirit of Mindererus—Sweet Spirit of Nitre—Alcohol.

EXPECTORANTS.—Atomization.

Nauseating Expectorants.—Lobelia—Ipecacuanha—Tartar Emetic.

Stimulating Expectorants.—Muriate of Ammonia—Seneka—Ammoniac—Benzoin—Benzoic Acid—Balsams of Tolu and Peru—Garlic—Squill—Tar.

EMMENAGOGUES.—*Tonic Emmenagogues.*—*Purgative Emmenagogues.*—*Stimulating Emmenagogues.*—Savine—Rue—Parsley—Apiol—Cantharides—Guaiac.

OXITOCICS.—Ergot.

SIALAGOGUES.—Pellitory.

ERRHINES.

EPISPASTICS. Counter-irritation—Cantharides.

RUBEFACIENTS.—Mustard—Spices—Turpentine—Burgundy and Canada Pitch.

ESCHAROTICS.—Potash.—Vienna Paste—Arsenic—Chloride of Zinc—Canquoin's Paste, etc.

DEMULCENTS—Gum Arabic—Tragacanth—Slippery Elm—Iceland Moss—Irish Moss—Liquorice-Root—Sassafras—Tapioca—Arrowroot—Sago—Barley.

EMOLLIENTS—Glycerine.

DILUENTS.

PROTECTIVES.—Collodium—Solution of Gutta-Percha.

PART II. NON-SYSTEMIC REMEDIES.

ANTACIDS—Sodium and its Preparations—Lime.

ANTHELMINTICS.—Pinkroot—Wormseed—Koosso—Santonin—Male Fern—Pumpkin-Seed, etc.

DIGESTANTS.—Pepsine.

ASORBENTS.—Charcoal.

DISINFECTANTS.—*Oxidizing Disinfectants.*—Permanganate of Potassium—Chlorine and its Preparations—Iodine—Bromine.

Desulphurating Disinfectants.—Metallic Salts—Lime.

Antizymotics—Sulphurous Acid—Carbolic Acid.

Absorbing Disinfectants.

APPENDIX.—Tables—Preparations omitted by an oversight from the text—Errata.

The difficulty of contriving a satisfactory classification of remedies is one generally acknowledged, and Dr. Wood fully concurs in the prevailing complaint. Notwithstanding his decided predilection for the physiological plan of studying the effects of medicines, it is to be observed that he names two classes *Antispasmodic* and *Analgesics*, from their effects in morbid conditions, thus deviating from some eminent authorities. We have never seen the terms *Analgesics* and *Mydriatics* used before, but their meaning is quite obvious, and a *penchant* for novelty may easily be granted, especially as it is useless to hope for uniformity of either classification or nomenclature.

The term *Antispasmodic*, as designating a class of remedies, is, in our opinion, inappropriate. It describes rather a result than an action, and it is doubtful whether any of the remedies called antispasmodic produce that action directly; while it is certain that many agents not included by the author in this class are in general use to counteract spasmodic affections.

It would be easy to extend remarks at greater length upon the author's classification, but it is probably better to leave further criticism to our readers, as the scheme is before their eyes. Having subjected the work to a systematic examination, we shall now proceed to notice such points as seemed worthy of particular attention.

Among the Mineral Astringents brief notice is given to the nitrate of lead. The author remarks: "This salt is not used except as a disinfectant." This is certainly a mistake. It is prescribed occasionally in ointments or to be dissolved in glycerine for external use, while, as a disinfectant, it has been superseded by cheaper and more efficient agents. Most of the zinc preparations are considered under the same head. The emetic action of the sulphate is noticed, but no mention is made of its use as a tonic in nervous affections, in night sweats, and in gastric debility.

Speaking of Simple Bitters, he observes: "Probably all bitter vegetable substances possess tonic properties, but in many of them, as in morphia and strychnia, these properties are completely overshadowed by other inherent powers." This is undoubtedly true of morphia, but we believe our readers will share our surprise to find such a statement in regard to strychnia. In our hands it has proved an invaluable remedy for atonic dyspepsia, and in combination with iron and quinine, for general debility. The antiperiodic property of salicine is unduly depre-

ciated, and that of the cornus florida is scarcely noticed. Southern practitioners during the late war learned to set a high value on the willow and dogwood as indigenous remedies, when deprived of the imported cinchona preparations.

Cinchona and its alkaloids receive extended notice, as their importance demands. Mention is made of the discoveries announced by Prof. Binz, (1) that quinine in large doses arrests the amœboid movements of the white blood-corpuscles, or, in other words, destroys their vitality; (2) that, even in small quantities, quinine diminishes the ozonizing property of the blood, thereby lessening the oxidation of the tissues and the production of animal heat. The author believes that the first conclusion is rendered very doubtful from the results obtained by other experimenters, as well as by the fact that conia and camphor produce, in a still higher degree, the same effect upon the white corpuscles, with totally different therapeutic properties; but he inclines to the belief that the general antipyretic influence of quinine is due to its preventive influence on the process of oxidation. He acknowledges that no regular diminution of temperature can be obtained by quinine in healthy subjects, and it is well known that the advocates of the "contro-stimulant" use of this remedy never expect such an effect, except from very large doses. From the fact that a proportional fall of temperature does not follow smaller doses, it appears to us more reasonable that this effect results from a profound impression on the vasomotor system of nerves.

The action of quinine in arresting putrefaction and fermentation, and in destroying low forms of animal and vegetable life, even in weak solutions, is mentioned; but a deduction which might be drawn from these observations, in connection with certain others, is not made in this work, nor have we observed it elsewhere. Many years ago, Dr. J. K. Mitchell announced his theory of the fungoid nature of paludal miasms, and within the last ten years Prof. Salisbury, of Cleveland, Ohio, claims to have discovered and classified the ague plant, and to have verified its effects by actual experiment. Now it is known that quinine is absorbed into the circulation and eliminated unchanged by the kidneys. Moreover, Bence Jones claims to have discovered in the blood a substance whose fluorescent behavior is identical with that of quinine, and which he calls animal quinoidine. This substance he found to be diminished in subjects of malarial fevers, and

supposes that it is antagonistic to paludal miasms, is destroyed by them, and may be supplemented by medicinal doses of the cinchona alkaloid. Should the above propositions be confirmed by extended proofs, it would lead to the conclusion that quinine exerts the action of a true disinfectant within the organism, just as carbolic acid does on living germs outside. In support of this idea, we have the example of arsenic, which, like quinine, is destructive to low forms of life and curative of malarial affections. On the other hand, Binz is quoted to the effect that salicine in much stronger solution has no power over infusoria, while the effects of quinine and arsenic respectively on low forms of life and on malarial fevers by no means maintain similar ratios. Some recent experiments, indeed, have been made with carbolic acid, used hypodermically, in obstinate intermittents, and with alleged success. The dose is ¼ drop dissolved in 10–15 of glycerine, and two or three doses are considered sufficient to effect a cure. It is reasonable to anticipate that the homœopaths will soon catch the scent and experiment with carbolic acid, to prove that it will physiologically produce intermittent fever; and then the quarter-drop, triturated and diluted to a suitable potency, may figure in one of the inevitable alternate tumblers, to the admiration of old women of both sexes.

We have ventured to suggest the foregoing thoughts on the relation between quinine and the malarial poison; but the author, with greater prudence, remarks: "Various theories have been brought forward to account for the anti-periodic action of quinia; but, as we know nothing of the nature of malaria, or of the method of its action, and as we are very partially informed as to the effects of quinia on the healthy organism, it seems to me perfectly futile to endeavor to explain why it averts a paroxysm of intermittent."

With regard to the abortifacient and parturifacient properties of quinine, the author is discreetly incredulous.

In relation to mode of administration, he might have added that it is eligible by enema in case of nausea, and endermically in case of young children.

Phosphorus, which is classed by Prof. Geo. B. Wood as an Arterial Stimulant, and by Prof. Stillé as a General Stimulant, is here reckoned among the Tonics, with special reference to the nervous system. In this respect the author's views are conformable to those of most recent therapeutists. The oil of turpentine

has been proposed as an antidote to its poisonous effects, but he avers that the crude article only is efficacious. The soluble salts of copper are said by him to afford an innocuous phosphide of copper, and therefore to act as true chemical antidotes. If so, this is quite different from the reaction outside the body. The phosphorus deoxidizes and decomposes the copper salt, and receives a deposit or film of metallic copper.

Alcohol is discussed as fully as the limits of the work admit, and, we think, very judiciously. The conclusions of Böcker and Hammond are accepted—that it diminishes the excretion of urea and carbonic acid, and therefore checks retrograde metamorphosis or tissue waste. As to the disposition of alcohol in the economy, the author believes that only a small portion escapes unchanged, and that most of it is oxidized and exhaled from the lungs as carbonic acid and water. Thus it must contribute to the support of animal heat, and the demand upon other sources of combustion would be diminished. In this connection due credit is given to the researches of Prof. Ford, lately of this city, which lead to the conclusion that the hepatic sugar is converted into alcohol in the course of those changes by which it is finally consumed; and confirmation is thus gained for the view above mentioned of the alimentary use of alcohol. The nutritive value of alcoholic preparations receives prominent notice; their usefulness in dyspepsia is recognized, as well as their service in pulmonary consumption; and their moderate use, largely diluted, in a state of health is not disapproved. While the toxicology of other remedies receives prominent attention, but brief notice is made of acute alcoholic poisoning, and the remote deleterious effects of the abuse of stimulants are not even alluded to. This last point, from its relation to the vice of intemperance, is probably relegated to the doctors of the moral law.

In regard to the oil of turpentine, the points open to criticism are its recommendation as a vermifuge for the *round* worm, instead of the *tape-worm*, and the omission to mention its use in neuralgia, particularly sciatica, as recommended by Trousseau and C. H. Jones.

The views of the author on the therapeutic action of digitalis are in accordance with those of the most advanced school of therapeutists, and more or less at variance with those found in the systematic treatises. His theory is, that it operates through the vaso-motor centres upon the circulatory system, producing

increased tonicity and more forcible action of the heart, together with contraction of the arteries. The action of the heart therefore becomes stronger, and, from increased arterial resistance, slower. The indications given for its use in cardiac complaints are in accordance with these views, and it is forbidden in simple hypertrophy. No allusion is made to its use in delirium tremens.

The antagonism which was presumed to exist between opium and belladonna from their opposite effects upon the pupil of the eye, is regarded by the author as by no means universal in extent. While not denying its value in opium poisoning, he thinks it antidotal only in its effect on the respiratory process. It is known that over-doses of opium cause failure of respiration, and, in his opinion, atropia is directly stimulant to the nervous centres presiding over respiration. This view is in harmony with our own observations upon the use of atropia in opium poisoning. The contracted pupils always dilate, the respirations increase in frequency, but the narcotism persists. On the other hand, in poisoning by belladonna, he thinks opium of doubtful utility, as he is bound by his conception of their therapeutic and toxic properties.

Atropia, daturia, and hyoscyamia are regarded as identical chemically, physiologically and therapeutically. There can be little or no disagreement with regard to the first two; but the author stands alone, so far as we know, in the addition of the last to this category. Chemical identity is simply an accidental or incidental circumstance, and signifies nothing in regard to physical properties. Hyoscyamia is said by Stillé to operate more energetically upon the iris than atropia, and less so upon the other sphincters. Hyoscyamia has no recognized control over mammary engorgements, and but rarely produces cutaneous efflorescence; while it is accredited with some narcotic virtues, in distinction from atropia.

Chloroform is erroneously stated to be produced by the action of chlorine on alcohol. This would produce chloral. For ordinary use as an anæsthetic he regards it as too dangerous, being fatal in the ratio of one to three thousand administrations. In this issue the author stands opposed to one of our strongest American characteristics. In our country no ordinary danger has any weight where convenience is concerned, whether in relation to remedies or railroads. A little reflection will show that the preference given to ether at Boston forms no exception to the rule, but is simply the outgrowth of local pride, from the fact that

ether anæsthesia originated there and the use of chloroform elsewhere.

The internal use of liquid chloroform is recommended as an anodyne. We have also found it extremely useful in convulsions. It likewise seems to have great power in arresting paroxysms of intermittent, when administered in the earliest stage.

It is gratifying at length to have some authority upon the long mooted question, whether a person may be chloroformed during sleep. It is stated that experiments made at the Philadelphia Hospital prove that it may be done, provided the sleep be profound. Light sleepers are sure to be wakened.

The theory that chloral hydrate, when absorbed into the blood, is converted by its alkali into chloroform and formic acid, and thus exerts the real action of chloroform, is controverted on both chemical and physiological grounds. When this process is effected outside the body, a temperature considerably above animal heat is required. Moreover, after the ingestion of chloroform by enema, it can be detected in the breath and in the blood; while the same experiment with chloral fails to afford chloroform. The two remedies are far from being identical in their action, chloral being more hypnotic and less anæsthetic in its properties than chloroform. The appearance of bile-coloring matter in the urine after poisoning by chloroform, and its absence in case of chloral, afford another distinction. Thus it is satisfactory to be disabused even of a pleasing delusion.

As a remedy in tetanus, the author concludes, from evidence afforded, that its use should be limited to its hypnotic effect at night. At the same time he gives authority for its great success in trismus nascentum. In this city, unfortunately, we have frequent need of a remedy for this disease, and chloral has proved scarcely more valuable than the hundred other agents tried and abandoned. In our hands it has been the only successful remedy, and in a single case which was nearly two weeks old before the attack commenced.

The indications for treatment in poisoning by chloral are, in his opinion, the same as in opium poisoning. Atropia is regarded as most serviceable to support the failing respiratory powers.

The new remedy of Liebreich, croton-chloral hydrate, receives brief notice, and is recommended particularly for facial neuralgia.

The points of special interest in regard to the mercurials observed in this work are the debatable ones about the mode of absorption of the insoluble preparations and the action of mercurials on the bile-producing function of the liver. The theory adopted is that calomel is decomposed in the duodenum by the alkaline fluids, the mercury undergoing oxidation; then by aid of the fatty matters present, it is dissolved and absorbed. An equally ingenious and plausible theory is that of Headland, that a chemical reaction takes place in the same tract between calomel and the tauro-cholate of soda in the bile, resulting in the production of the soluble tauro-cholate of mercury and common salt. Both are more reasonable than the old theory of the production of the bichloride in the presence of free hydro-chloric acid in the stomach, from which we might apprehend poisonous effects, if the theory were true.

The greenish stools observed after the administration of calomel and blue pill are attributed by the author to bile-coloring, and on this he bases their cholagogue action. Physiologists are not agreed whether the pigment is produced in the liver or is separated from the blood. Virchow and others maintain that the bile-pigment is produced by destruction of the blood-corpuscles, probably through some change in their hæmatin; and the effect of mercurials in spoiling the blood-corpuscles is one generally admitted. It appears, therefore, that some better evidence than the production of greenish stools must be found for the chologogue action of mercury. Experiments on dogs have led to contradictory results in the hands of different observers, and left the relations of mercury and the liver in a very unsatisfactory condition to the inquisitive seeker after truth. Old practitioners, however, especially those flourishing in malarious localities, do not trouble their heads about the precise mode of action of calomel and blue mass, but rely on them to produce some sort of disgorgement of the abdominal viscera. They find satisfactory results, and are content to leave theories to those who have more time to spare.

The remedies classed as Diuretics afford a favorable field for the exercise of judgment or ingenuity in making subdivisions, and every writer is expected to display some degree of originality. The peculiar views of the author may best be expressed in his own words:

Diuretics are very naturally divisible into three sets—the

hydragogue diuretics, the refrigerant diuretics, and the stimulating diuretics. These classes, of course, run more or less into one another, but are sufficiently distinct for practical purposes. The drugs belonging to the first set simply increase the flow of water from the kidneys, and are therefore used chiefly for the relief of dropsy; those of the second division exert a marked sedative action upon the system—very generally do not increase to a great extent the water of the urine, but mostly modify the secretion in one way or the other. They are used in dropsy to alter the urinary secretion, and for their sedative and eliminative action in acute disease. Diuretics belonging to the third division are of such nature that their active principles are eliminated by the kidneys, and act upon the mucous surfaces over which they pass; for which purpose they are chiefly employed.

According to the above explanation, we fail to observe any essential distinction between the first and third divisions. All, with the exception of digitalis, probably owe their effect on the kidneys to their passage through these organs, and the local effect caused by their contact with the mucous secreting surface. The second division, comprising the saline diuretics, are credited by Stillé with a very important property, in distinction from other diuretics—a property recognized by this author, and attributed to increased oxidation of tissues—that of augmenting the soluble constituents of the urine. They are consequently styled by Stillé *depurative* diuretics, and the term appears to us more appropriate.

The chlorate of potassa, which is classed as a sialagogue by Headland, and by most other writers as an alterative, is placed by this author among the diuretics. We fail to comprehend the precise reason for this choice, unless it may be to show his independence of therapeutic applications in classifying remedies.

We observe with some surprise that gossyppium is ignored, though its oxytocic and emmenagogue properties are recognized by the most prominent therapeutists, and it is justly regarded as a valuable remedy.

Only one variety of cantharides is mentioned, the cantharis vesicatoria, though several indigenous species, possessing the same vesicant properties, are mentioned by other authors.

The author's views on Disinfection are mainly correct and judicious, and this section forms a valuable portion of the work. Some statements, however, need qualification. The following is an instance: "It is a law which holds everywhere, that the more imperfectly developed animal organisms are, the more tenacious

are they of life; and it seems to me also a general law that highly specialized germinal matter is more readily destroyed than that of lower life-grade."

This is quite true of certain destructive processes, for instance, mutilation and deprivation of air, moisture and nutrition; while resistance to extremes of temperature and to certain destructive agents is quite variable. Mankind possess greater adaptability for living in a wide range of climate than most other forms of animal life, and endure greater extremes of temperature for limited periods, varying from 400–600° F. of artificial heat to—60° of natural cold. The destruction of entozoa affords an example of greater tolerance of injurious drugs; and the practice of smoking out mosquitoes and other plans for riddance of insects are too familiar to need mention.

Again we read: "The nature of miasmata is not known at all. Over many of them, malaria for example, we have no control." The first proposition will not be disputed, as it cannot be certainly controverted; but the second is quite too broad a statement. The production of paludal miasms, or malaria, has been reduced to a minimum in cities where periodic fevers were formerly prevalent, by a judicious system of drainage. Abundant vegetation seems either to check the production of malaria or to absorb it, as shown by the increase of malarial fevers after the destruction of forests; and there is reason to believe that advantage may be derived in newly settled districts, from cultivating vegetable growths of ample foliage.

The author's plan for disinfecting a room with chlorine gas is different from the one practiced by the Board of Health in New Orleans. He directs the sulphuric acid and water to be mixed beforehand, and added *cold* to the mixture of common salt and peroxide of manganese. In this way the chlorine is generated very slowly and imperfectly, unless the whole be subsequently heated. The plan here is to produce rapidly a volume of gas sufficient to saturate the room. This is effected by making a mixture of the salt and manganese with water, and then pouring on strong sulphuric acid. The elevation of temperature resulting from the union of the acid and water promotes the chemical reactions between the several agents, so that they are completed with no further trouble. It is not probable that chlorine is of any use as a disinfectant in such degree as can be

tolerated by human beings; hence, a rapid and complete action with the above materials is the proper one.

The author concurs in what is generally observed of the effect of carbolic acid in arresting fermentation and putrefaction. Of its internal therapeutic use, he remarks:

"In the doses in which it is usually given, carbolic acid exerts no perceptible effect upon the system. Upon the idea that the so-called zymotic diseases are due to a sort of fermentation in the blood, that they are the result of changes set up in the vital fluid by living organisms similar to the yeast-plant or to the vibrio of putrefaction, carbolic acid has been quite largely used in such diseases The zymotic theory is, however, at the very best merely a plausible speculation; and clinical experience with the acid in these diseases has certainly demonstrated its uselessness. It has been extensively employed in *typhus fever*, in *pyæmia*, in *smallpox*, and in other allied affections, but at present is rarely used and is of no value. The study of its physiological action has failed to show the possession of any property which should render the medicine valuable in constitutional diseases, and clinical experience has borne this out: so that it is employed directly in medicine only for its local effects."

In this he is doubtless correct; any dose which could be effective as an antizymotic would be destructive to the patient.

An appendix contains tables converting the French decimal system of weights and measures to the denominations used in this country; a table showing the alcoholic strength of different wines; and a supplement of a few remedies omitted previously. A list of Errata follows, to which some additions should be made. The volume closes with a good index of Materia Medica, and an imperfect one of Diseases and Appropriate Remedies.

Having now noticed the work in detail, it may be expected of us to make some remarks upon its general merits as a whole. The unfavorable prepossession created by the preface has greatly diminished in the course of extended examination of the contents. The tone of confidence there assumed is not carried out to the extent which might be expected, and the author is sometimes perhaps over-cautious in venturing an opinion. A large amount of information of recent date is presented, as authorities are freely quoted; and they are accredited with their views. The book is sufficiently systematic for a text-book, and easy of reference for a mere consultation. It may be presumed that we think it capable of many improvements, but we know of no better work of this scope, and therefore recommend it to those who desire

one on a more limited scale than the two-volume treatises of Pereira, Wood and Stillé. S. S. H.

Lectures on the Diseases of Infancy and Childhood. By Charles West, M.D., Fellow of the Royal College of Physicians; Physician to the Hospital for Sick Children. Fifth American from the Sixth revised and enlarged English Edition. Philadelphia: Henry C. Lea; 1874.

A publication of a new edition of Dr. West's excellent book, which should include his latest investigations and observations, again places the work in the front rank of authorities on diseases of children. The new edition came to hand too late to be announced in the annual circulars as one of the text books of the Medical Department of the University of Louisiana, but we have authority from Professor Hawthorn to advertise it as one of his text-books.

Ligation of Arteries. By Dr. L. H. Farabeuf. Translated by John D. Jackson, M.D., Danville Kentucky. Philadelphia: J. B. Lippincott & Co; 1874.

It is our pleasure to welcome the excellent operative manual with the above title.

The work is divided into two parts. The first treats of generalties in the process of obliterating arteries, and describes with remarkable clearness the *discovery* of the sheath of the artery; the *isolation* of the vessel, and the *ligation* proper. Torsion and compression as means of obliterating arteries also receive attention in this part of the work.

Part second is devoted to special ligations, and treats of the ligation of each artery as a special operation.

A systematic treatise upon this important department of operative surgery has long been a desideratum; and in our opinion this one will be of great value to every medical man who may be under the necessity of ligating an artery.

Every step in the various proccdures, as well as the course of the results, and their relations, is illustrated by engravings. These engravings are worthy of special attention, both on account of the excellence of execution, and for their number.

An admirable feature in the second part of the work is the

very excellent anatomical resumé accompanying the description of each ligation.

When one first attempts the various ligations upon the cadaver, it will be evident that the mere knowledge of anatomical facts is not the only requisite for finding and securing the vessel with facility. In fact the ligation of each artery is a distinct and substantive operation, and it is necessary that the landmarks, which are to guide the finger and eye to the artery, be familiar. This fact has been carefully observed by the author of the work under consideration.

The writer of this review has studied the manual with care, performing each ligation upon the dead body in connection with the same, and after such a test of its merits feels warranted in recommending it to the special attention of the profession.

For young men just closing their studies in the anatomical rooms it will be an invaluable aid; and to practitioners so situated as to be without the facilities for operating upon the cadaver it will be of immense value.

The translator has performed his work well, the language being clear, concise, and elegant.

Many young men in the United States, wishing to avail themselves of a special course on operative surgery, as well as the practitioner, will appreciate the service rendered by "the draper" in placing within their reach this excellent operative manual.

M.

A Treatise on Pharmacy: designed as a Text-Book for the Student, and as a Guide for the Physician and Pharmacist; Containing the Officinal and many Unofficinal Formulas, and numerous examples of Extemporaneous Prescriptions. By Edward Parrish, late Professor of Theory and Practice of Pharmacy in the Philadelphia College of Pharmacy, etc., etc. Fourth Edition, enlarged and thoroughly revised by Thos. S. Wiegand, Graduate of the Philadelphia College of Pharmacy. With two hundred and eighty illustrations. Philadelphia: Henry C. Lea; 1874.

To the pharmacist, or physician who does his own dispensing, this is an invaluable work. Its "working formulas" are explicit and direct, and every page is replete with practical information of the kind the practitioner is most in want of.

Pamphlets Received.

1. *Medical Problems of the Day: The Annual Discourse before the Massachusetts Medical Society, June 3rd*, 1874. By Nathan Allen, M.D., LL.D. Lowell, Massachusetts.

This able paper is from one of the most vigorous and consistent thinkers of our profession. Among the questions discussed are "State Medicine," "Public Hygiene," "Physical Development," and "Position of Woman."

2. *On the Origin and Development of the Colored Blood Corpuscles in Man.* By Dr. H. D. Schmidt, New Orleans.

3. *On the Construction of the Dark, or Double Bordered Nerve Fibre.* By Dr. H. D. Schmidt, New Orleans.

These two papers are the productions of our laborious fellow-townsman, Dr. Schmidt, whose pen and pencil have already achieved much in the enlargement of our store of facts. They were read before the Microscopical Society of London, and have been complimented for the care and accuracy with which the drawings illustrating them had been done.

4. *On Intra-Uterine Fibroids.* By J. Marion Sim, M.D.

5. *Fibroid Tumors of the Uterus.* By Ely McClellan, M.D., Assistant Surgeon, U. S. A.

6. *Retention of Urine.* By Alexander W. Stein, M.D.

7. *Syphilitic Membranoid Occlusion of the Rima Glottidis.* By Louis Elsberg, M.D,

8. *Mutual Relations of Druggists and Physicians.* Charles E. Buckingham, M.D.

9. *Rupture of the Perineum, its Causes and Cure.* By A. K. Gardner, M.D.

10. *Ectrolysis in the Treatment of Stricture of the Urethra.* By Robert Newman, M.D.

11. *Address Delivered before the Kentucky State Medical Society at its Nineteenth Regular Annual Meeting.* By Joseph W. Thompson, M.D.

An earnest and able paper from one of the most zealous and dignified of our co-laborers.

12. *Boylston Medical Society of Harvard University. Catalogue*, March, 1874.

13. *A Stricture on the Peculiarities of the Physical Geography of*

the Mississippi and its Delta. By Edward Fontaine, Professor of Theology and Natural Science, etc.

14. *A Treatise on the Hot Springs of Arkansas.* By Dr. A. S. Garnett.

15. *De l'Immunité Phthisique. Par le Docteur N. C. Lombard. (Aer papubum vitæ.)*

16. *Etude sur le Goitre et le Crétinism Endémique, et sur leur Cause atmospherique. Par le Docteur H. C. Lombard* (from author).

17. *Transactions of the Medical Society of the District of Columbia. April*, 1874.

18. *Transactions of the Minnesota State Medical Society.* 1874.

CORRESPONDENCE.

CHICAGO, April 20th, 1874.

Mr. Editor:—Several months since I gave you a brief account of the medical societies and institutions of this city, and promised to keep you informed of such part of their doings as might be of interest to your readers, or to the profession generally. The urgency of other professional engagements has prevented me from doing this as promptly as I had intended. At a full meeting of the Chicago Society of Physicians and Surgeons, held on the evening of November 10th, 1873, the time was chiefly occupied in listening to a paper read by Dr. John Bartlett, of this city, "On a Marsh Plant from the Mississippi Ague-Bottoms, with a consideration of its Genetic Relations to Malarial Diseases." Dr. Bartlett is a careful and candid investigator, an experienced practitioner, and has devoted much time to the study of malarial diseases and their causes. We think he has proved, by numerous personal observations in different seasons and localities, that there is a direct coincidence and connection between the appearance of a vegetable germ called the "ague-plant," and the prevalence of malarial fevers. He has, consequently, taken much pains to cultivate the plant, and to study microscopically its growth, nativity and propagation, and has embodied the results of his observation and study in this paper. After referring to the observations of Dr. Salisbury, of Cleveland, Ohio, in 1866, concerning the Germiasma or Palmetto Plant as a cause of ague,

Dr. Bartlett describes his "ague-plant" as corresponding, in the first stage of its growth, with the *hygrogastrum* of Rabenhart, but as developing in its progress changes not described by that botanist. The globular portion of the plant, at a certain stage of its progress, undergoes collapse, extruding spores from the cells composing its walls. From these spores were developed two forms of organic matter, namely, crystalloid bodies and crystalline mycelium threads. From these crystalloid bodies and threads there came to be developed the original or mother plant, showing that the successive transformations were complete. On examining more closely, the spores were found to contain a protoplasmic fluid, in which appeared germinal particles and minute cells, which multiplied rapidly and developed into the crystalloid bodies and threads. These germinal atoms were exceedingly small, being less than one fifteen thousandth part of an inch. While cultivating the "ague-plant," and studying its habits, the doctor had some paroxysms of ague, and detected what he regarded as the same germinal atoms in his blood, specimens of which were submitted for examination by members of the society. How far the observations of Dr. Bartlett will contribute to the settlement of the question as to the efficient cause of malarial fevers, I am not prepared to say. But he certainly deserves much credit for having given us the results of much patient study and careful investigation of an obscure subject.

In the same society, at the meeting on the evening of December 6th, Dr. A. R. Jackson read a lengthy report from the Section of Obstetrics and Diseases of Women and Children, in which he relates two cases of fibrous tumors of the uterus treated by hypodermic injections of Ergotine. One of them was cured, and the other was still under treatment with benefit. The solution used was one part of ergotine in three of water. In one of the cases, the injection over the seat of the tumor resulted in the formation of abscesses, and it was afterwards introduced over the region of the deltoid, causing it to penetrate deeply into the muscular fibres, and with the same beneficial effect as when injected nearer to the tumor.

At the meeting of the same society on February 9th, 1874, Dr. Y. N. Danforth presented a paper on the Morbid Anatomy of Cholera, as illustrated both by post-mortem dissection and microscopic examinations. The report was accompanied by sections of healthy mucous membrane of the intestine in comparison with

sections from cholera patients who died in the cholera hospital the previous summer. The microscopic field in both cases was projected upon the screen by means of a solar microscope, and made a most interesting exhibition. The report, however, developed no new facts in relation to the pathology of cholera. More anon. GLEANER.

RICHMOND, VA., June 5th, 1874.

Mr. Editor:—Richmond is rapidly growing in size, beauty, and importance as a commercial and medical centre. Its population, though insignificant as compared with that of the "Crescent City," has increased more than 20 per cent. within the past seven years; so that, according to the census taken last February, by order of the City Council, the population is 60,665, of whom 27,213 are blacks. While the total population is believed to be somewhat under-estimated, the ratio of blacks to whites is thought to be about correct.

As to the number of physicians, there is about one to every 550 or 575 inhabitants—quite a sufficient number as compared with Paris, where, according to the *Almanach de Médicine* for 1874, the proportion is one to every 923 inhabitants. But be it said to the honor of the profession of Richmond, there are comparatively few "irregular" doctors. There are not more than half a dozen graduates of medicine who are known to be guilty of practices unbecoming the profession; there are but three homœpaths, and there is no eclectic or other "nonsensical exclusionist." True there are one or two self-styled "doctors" among the whites; but their practice, like that of their very fit negro associates, of whom there are some half dozen or more, is confined almost exclusively to the lowest of the low negroes—"the poor white trash" not even submitting themselves to their treatment. As for "abortionists," whose presence is so much complained of in many other cities, especially north of Virginia, even of less size than this, I know of no well grounded reason to believe that there is any in our midst. *En passant*, I may remark that while our State Penitentiary is well filled by about 800 *delegates* from every section of the State, among whom are represented whites of almost every trade and profession—even ministerial—there is yet not a doctor among them.

The common talk just now among the doctors is in relation to

the proceedings of the late session of the *Association of Medical Officers of the late Confederate Army and Navy*, which are, in general, highly approved; and a special pleasure is anticipated in that the next session is to be held in this city. A better selection of officers to represent Virginia could scarcely have been made. The unfortunate selection, as Vice President, from Louisiana, however, is not generally known. That such a choice should have been made as the representative officer from your gallant but sorely afflicted State, was due, I am quite sure, to want of information as to his *post bellum* history. Even had his present political associations been known to those who nominated him, I feel safe in affirming that his name would never have been mentioned in connection with the Association itself. With this assurance, which is not altogether personal, it is to be hoped that the profession of Louisiana will regard this unfortunate selection as being due to a total misapprehension of the facts in the case, and will therefore have none the less interest in the success of the organization, but will rather lend it the support of their influence and presence at the next meeting in this city. Since there can be no affiliation between the party thus ignorantly and unfortunately elected, and since his attendance upon the next session will be exceedingly unwelcome, as even his presence would be to the good citizens of this place, it is to be prayed for that he himself will recognize that he has been unwittingly invited into the wrong pew, ask for his hat, and retire as decently as he may be able—thus ridding the Association of an incubus by his own voluntary act which *it* cannot remove until next July. Such an act would do more to entitle him to the thanks of the profession than almost any other he is now capable of performing. *So mote it be.*

The proceedings of the present session of the American Medical Association in session at Detroit, are also looked upon with a great degree of interest. That a Virginian is not present is due simply to want of means to travel. Under the administration of Dr. J. M. Toner, the policy has been to use every means to reconcile any conflict in the interests of either section of the country. It happens to fall within a part of my personal history to know that, whatever may be his political predilections, he has been friendly to the wants of the profession of the South as well as North, and as such he is to be respected. But as all our journals will contain full reports of the important proceedings of this ses-

sion, it is unnecessary to detail any of the minutes which have come to us. In fact there has been no time, nor is the data yet in hand sufficient to examine them with an eye to criticism. That the next session is to be in Louisville is, however, a pleasure, since there is more hope of seeing a fuller representation from the Southern States.

I find that my letter grows too lengthy, especially as it is proposed that this correspondence shall continue indefinitely. Before closing, I must be allowed to make this public acknowledgment of thanks for the kindly manner in which you referred in your May issue to the *Virginia Medical Monthly.* It will be my study and effort as its editor to make it worthy of that patronage it has already obtained through the influence of warm friends, which patronage is, however, by no means limited to Virginia and the neighboring States. In return for your generous wishes in its behalf, allow me to express the hope that the *New Orleans Medical and Surgical Journal* will continue daily to grow in favor and patronage, and in every way meet with that success to which its value to the profession eminently entitles it.

Yours very truly,

LANDON B. EDWARDS, M.D.

Mr. Editor.—By your solicitation I submit this relation of a case of Trismus Nascentium which recently occurred in my practice, and was successfully treated by the chloral hydrate. I make more free to do so, inasmuch as all the trials made of this medicine, both in Europe and America, in the treatment of infantile lock-jaw are highly calculated to inspire the hope that we have in it a remedy which will prove, when we better understand how to use it, as efficacious in the treatment of this most formidable disease as is mercury in syphilis, or iodine in scrofula, or quinia in malaria. Is it not to be hoped that every practitioner will give chloral a thorough trial in every case of tetanus which may come under his treatment, as all other modes of treatment have failed to give any satisfactory results.

The case which I have to relate occured in the babe of Mrs. D., on D. street, New Orleans; was born March 1st, 1874; was a male; the third child of Mrs. D. Her two first died of tris nascentium, when five days old; the second one of which I treated in 1872,

but did not use chloral. As precautionary measures I ordered the lying-in room to be kept dry and clean, and at a temperature of about 70.8° Farh., and moderately darkened; the windows thrown up once daily, and the room well aired. The babe dry and clean, its bowels well cleansed of meconium by administering sweet oil. The child did remarkably well till the cord dropped on the seventh day.

On my visit on the morning of the eighth day I learned the babe had passed a wretched night; suffered intensely with what the mother took to be colic; had nursed badly. On examination I found it uttering a most distressing moan at every expiration; the mouth partly open, the corners drawn slightly downwards and backwards; eyes closed, a twitching of the lids; breathed rapidly through the mouth; skin dry and hot; paroxysms of clonic convulsions recurring every few minutes, were ushered in by an apparent straining at stool, followed by a curving backwards of the trunk in a condition of opisthotonos; the eyebrows drawn upwards enormously; corners of the mouth drawn still more downwards and backwards; the whole face bearing the expression of intense pain. The hands firmly clinched, with thumbs in palms; uttering the most pitiable screeches; the breathing impaired; the face livid. It nursed badly; took the breast with great difficulty, and soon quit it uttering a whimpering cry.

I ordered castor oil ʒj, and began the administration of chloral in half-grain doses, using the following prescription.:

℞—Chloral hydrate.....gr. xij.
Syr. flor. aurantii.....ʒj, ɱ.
S. Twenty drops every half hour.

6 p. m.—Sleeps profoundly. Symptoms somewhat modified. Chloral continued, but less frequently.

9th, 9 a. m.—passed a quiet night; mouth closed, but little trismus; nurses little. Chloral continued.

1 p. m.—Slight improvement. Chloral continued, less frequently, and in less doses.

7 p. m.—Improvement in all the symptoms. Chloral discontinued.

18th, 9 a. m.—Passed a wretched night. Paroxysms frequent and severe. Mouth fixed and drawn down at the corners. Screeches most pitiably; flesh looks shrunken; surface dry;

lips livid during paroxysms; constipated. Gave syr. rhei ʒss, and renewed chloral.

1 p. m.—Slight improvement; sleeps; bowels moved. Continued chloral.

9 p. m.—Marked improvement. Not much trismus. Chloral continued.

11th, 8 a. m.—Passed a quiet night. Nurses little. Stop chloral.

1 p. m.—Most of the symptoms return, but in a milder form. Eyes twitch; increased trismus. Renew chloral.

12th, 8 a. m.—Much better. Skin smooth; perspires; no trismus. Continue small doses chloral.

6 p. m.—Marked improvement. Continued chloral at longer intervals.

13th, 8 a. m.—Slept but little; moans, but no trismus. Ordered an enema containing castor oil ʒj. Continued chloral enough to keep the little thing sleeping.

5. p. m.—Better. Slight twitching of the muscles of the face. Continued a little chloral.

14th, 8. a. m.—Free from all symptoms. Chloral discontinued.

5 p. m.—Quiet and sleeps well.

15th, 9 a. m.—Passed a good night; nurses well. Patient dismissed.

At the time of this writing, June 2d, there has been no recurrence of tetanus, and although the babe was in a condition of great debility for several weeks it is now remarkably well and stout.

Most respectfully, S. E. Hale, M.D.

New Orleans, May 1st, 1874.

Mr. Editor:—I propose to write for each number of your Journal a letter over the signature of "Medical Tattler." Now I am free to acknowledge that this is not a very dignified *nom de plume*, but yet it expresses better than other terms the nature of the character in which I wish to appear, which is that of prating in a most colloquial manner of medical matters and experiences.

Please do not understand me as intending to appear in any other character than that of a plain, practical doctor, who has had opportunities to see much that relates to his profession, and

who, like "Barney," wishes to tell some of the "ins and outs of it," that others may compare his experiences and conclusions with their own. Again, please do not, in advance, judge me to be a "gossipping doctor." They are despicable characters, and whatever the dictionaries say, there is an appreciable difference in the distinction to be drawn between the verbs "tattle" and "gossip." We may tattle that which is instructive, but we gossip that which is personal, and which in our profession should remain wholly in the back-ground. Now, if my coveted position is understood and granted to me, I will write you letters upon such subjects as at the moment chance to occur. I will furthermore promise not to ask remuneration in cash or kind, for my tattle, and will also promise to desist upon the first intimation that this kind of tattle is not acceptable to your readers.

Last night I was much awake with a female patient who was suffering with hemoptysis. This is the fourth or fifth attack she has suffered. The first took place during the heat of summer in 1871, when the lady had been employed during the whole day preceding the hemorrhage in preserving figs. It came on first, as all the subsequent hemorrhages have done, during the night, and simultaneously with the catamenia. The hemoptysis has, therefore, in no previous instance been vicarious, in the sense of being substituted for the natural flux, but has simply been critical in its character. Thus far, however, the present attack is entirely vicarious. The patient is now forty-five years of age, and of course the period of menopause may be supposed to have been nearly reached.

What is the mechanism of the hemorrhage in such cases? Is there a plethora of the vascular system, because of an obstructed discharge and a "slopping over" upon the pulmonary surfaces simply because the weak points chanced to be there? Or is there some special nerve influence awakened which disturbs the vaso-motor nerves of the pulmonal vessels, very much as œsophageal spasm is produced in hysteria? Does the blood escape from the bronchial, or pulmonary arteries? I should infer from the former, as we see that in all other instances these vicarious hemorrhages take place from true systemic vessels.

I think it is Flint who states that in his whole experience he has not met with a case of hemoptysis vicarious of menstruation. Barnes seems also to have found it infrequent. From memory alone I can sum up three cases which have occurred under my

care. One of these occurred in the summer of 1844, and the woman is still living. In truth, so far as my knowledge goes, none of these patients are dead.

It is wonderful that, while it is true, that hemoptysis is, as a rule, a symptom of very dangerous import, these critical hemorrhages, from whatever surface they may take place, are not only devoid of danger, but are actually safety valves. In all of these cases to which attention is cited, my treatment has been for the most part expectant and hygienic. Absolute quietude, warmth to the feet and pelvic regions, leeches to the vulva or thighs, eating a little salt, soothing doses of an opiate, and encouraging moral influences, are quite as much as the doctor's duty demands, unless some plain indication of a morbid state pertaining to the genito-urinary functions calls for special interference. However important it may be to arrive at a satisfactory diagnosis as to the cause and source of the hemorrhage, it is by no means so easy as some would have us believe. In two of my cases the blood was so little admixed with air, and the disturbance of respiratory function so slight, that I felt entitled to conclude that the hemorrhage was a bronchorrhagia, and this having taken place coincidently with the catamenial period, the diagnosis seemed obvious. In one other case, the respirations were 70 to the minute, the blood frothy, and the whole contour of the case so threatening that I was in doubt as to a proper diagnosis. A future recurrence resolved the doubt.

If we feel necessitated to call in the question of "probabilities" to aid us, let us not fall into error. A recent writer says: "The chances are—perhaps a thousand to one—that a case of hemoptysis is bronchorrhagia. It is best to assume this until a certain diagnosis proves the contrary—which turns on the presence of cavities and consolidation of pulmonary tissue." My experience is precisely opposed to this; for, in the many hundreds of cases of hemoptysis which I have seen, the instances in which I felt justified in ascribing the hemorrhage to the bronchial arterial supply, have been so few in proportion to the aggregate that the difficulty consists in excluding those lesions of the pulmonary arteries which so generally determine hemoptysis. We have tattled quite enough upon this subject. Let us conclude it by referring to the citation in Barnes' work of Trousseau's* observa-

* Dr. Gregory, in Tweedie's Library of Practical Medicine, published in 1842, writes of vicarious menstruation: "It is probable that it is always, or nearly always, determined to a mucous surface, as giving an outlet."

tion in regard to that conservative provision of nature which causes all physiological hemorrhages to take place upon mucous surfaces, to which natural outlets always exist.*

May 2d, 1874.

There is nothing which strikes the busy practitioner as being more strange than that certain examples of disease known to have no laws or agencies to connect them may yet appear to occur in groups. This seems also to be strangely true of traumatic maladies, or even of accidents. I once heard a venerable physician say, that if he was called to one case of tetanus he expected, within a day or two, to see another. The only two instances in which I have ever treated lacerations of the vulva from falling upon the upright shafts of chair backs, occurred within the period of one week.

On yesterday I jotted down a few thoughts suggested by a case of hemoptysis occurring at the menstrual epoch; to-day I have been asked to prescribe for a patient in regard to whose case I elicited the following facts. Eugenie T., of French extraction, completed her fifteenth year during the month just past. Her catamenia appeared for the first time in February, 1872, and continued to recur at regular periods until February, 1873. At this time a complete arrest of the discharge took place, and was followed by some leucorrhœa and a general state of anemia. These symptoms were not considered sufficiently grave to require medical interference, and no physician was consulted. In February, 1874, the young girl states that upon experiencing a sense of burning and fulness in her right ear, she put her finger to it and was surprised to find it covered with blood. After its first manifestation, the otorrhagia has returned at intervals of from two to four days until the present time. In the meantime the young lady's health is decidedly improved. She states that the quantity of blood exuding at each paroxysm is enough to saturate one and sometimes two handkerchiefs. There has never been any previous disease or known imperfection of the hemorr-

* Notwithstanding the correctness of this observation when considered as the rule, the exceptions are far from being unfrequent. A large number of cases of pelvic hæmatocele are undoubtedly as clear instances of ectopic menstruation as the ordinary phenomena of vicarious epistaxis. Some curious cases of interstitial sanguineous effusion are recorded, which by their periodicity and substitutive character, appeared to be ectopic menstrual fluxes. We may suppose that Sir Astley Cooper's "menstrual ulcers" are produced and rendered chronic because of the local sanguineous determinations in the vessels which afford their blood supply.

hagic ear. There are no evidences of organic disease, either thoracic or abdominal. The patient was ordered to be taken from school and have abundant exercise in the open air, with varied and plentiful diet, cheerful associations, and to take twice daily one of the following pills:

℞. Reduced Iron ʒj,
Sulphate Quinia,
Extr. Nux Vomica,
Pulv. Aloes aa grs. x,
Gum Myrrh ℈j.
℔—Ft. Pil. No. xx. S. One twice daily.

Many authors allude to hemorrhages from the ear as instances of ectopic menstruation, but this is the first instance in which any evidence has been exhibited under my observation in support of such statements.

May 5th, 1874.

The *Journal de Médecine et de Chirurgie pratiques*, of April, publishes a short notice of a book lately issued in France bearing the following title—"Le Parnasse medical francais ou Dictionnaire des médecines pöetes de la France, par le docteur Achille Chereau."

It is not strange that the medical profession should be less addicted to the poetic inspiration than the theological, and probably less than the legal. For the former, the contemplation of the supernatural must awaken whatever poetic emotions a man possesses, while the very varied studies of the lawyer lead him to the altar of the muses more often than does the physician's unromantic art lead its votaries thitherward. Whoever has read the "Temple of Nature" and "Botanic Garden" must admit that philosophic poetry is a dead failure. In truth, we cannot do otherwise than commend the good taste of the sacred nine in refusing to be chained to the grim car of philosophy. In scientific polemics they belong to the class of

"Sweeter logicians in bliss,
Who argue their point with a soul-telling eye,
And convince us at once with a kiss."

It is not then a matter of wonderment, that the metrical essays of our profession are the more successful exactly in inverse proportion to their ambitious pretensions. Like half-fledged birds, they are safer when they do not attempt to soar too high.

Nothing seems more natural than that physicians should be

often required to expend their poetic talent in writing epitaphs. The friends of the deceased—and with full knowledge of his character, who is more likely to be asked to commemorate his virtues in verse, which the intent and the occasion render acceptable, however miserable intrinsically? Perhaps the force of some unfortunate circumstances of this kind caused the following witty lines to be perpetrated in regard to Dr. Valère, which appear in the review under discussion.

Heureux qui recoit la mort
Des mains du docteur Valere,
Car avant qu'il nous enterre
Par ses vers il nous endort.

It is certainly to be desired that some similar work be performed for this country, which now affords the rare instance of a combination of medical and poetic talent of the highest order in the same person. There are very few readers of the *New Orleans Medical and Surgical Journal* who need to be informed that in this statement reference is made to Oliver Wendell Holmes, of Boston, the physician, poet and scholar. Indeed, so far as my information is correct, New England enjoys, for the present and the past, almost entire monopolization of the honor of uniting medicine and poetry. The first Warren was a poet, whose verses are said to have been held in high esteem. I have seen but one of his effusions, and should infer that its merits consisted chiefly in the patriotic sentiment inculcated.

While my mind has been occupied with this interesting subject, I have striven to recal some of the recreative studies of my boyhood, consisting, in so far as the present subject is concerned, in reading the quaint effusions of the pilgrim fathers and their collateral authorities. Among these we find many curious, and even meritorious, attempts at versification. In looking over these it sometimes occurs that a peculiarly technical mode of expression betrays the medical authorship of these generally anonymous contributions. In 1657 William Bradford, Governor of Plymouth jurisdiction, "expir'd his breath."

"The ninth of May, about nine of the clock,
A precious one God out of Plymouth took:
Governor Bradford then expir'd his breath—
Was called away by force of cruel death."*

I do not know that any records now preserved indicate the authorship of these lines, but to my mind the expression "ex-

* New England's Memorial, page 158.

pir'd his breath" is sufficient to compel our profession to father it. In this connection I also recal Fessenden's "Terrible Tractoration," by Christopher Caustick, M.D., A.S.S., Fellow of the Royal College of Physicians, Aberdeen, &c. This poem attracted a great deal of attention in England as a severe diatribe against that most audacious of "Yankee tricks," Perkins' metallic tractors. The *Gentleman's Magazine* for January, 1804, says: "Who the author can be, we have not the least conception; but from the intimate acquaintance he discovers with the different branches of medical science, we should imagine him to be some jolly son of Galen, who, not choosing to bestow all his arts upon his patients, has humanely applied a few escharotics for the benefit of his brethren." The "Terrible Tractoration" was written in a style imitating that of Hudibras, and was unquestionably a striking success. It has been over a good score of years since I have seen the book, and only two lines remain unforgotten:

"With bolus, pill, and squirt-gun,
There'll be a monstrous deal of hurt done."

After all, the physician who assumes to become a poet must, like Bavolet, be prepared for ridicule.

"Honneur a Monsieur Bavolet,
Qui dansun genereux delire
A change sa seringue en lyre
Et sa canule en flageolet."

TATTLER.

EDITORIAL.

The American Medical Association.

We give as much space as it is possible to spare for this purpose, to extracts from the published reports of the proceedings of the American Medical Association.

The American Medical Association convened at Houghs' Detroit Theatre at half-past eleven o'clock, the President in the chair.

Upon the stage were J. M. Toner, of the District of Columbia, President; W. Y. Gadberry, of Mississippi; J. M. Keller, of Kentucky; N. C. Husted, of New York; and L. F. Warner, of Massachusetts, Vice-Presidents; William B. Atkinson, of Pennsylvania, Secretary, and Theodore A. McGraw, of Michigan, Assistant Secretary. Besides these gentlemen the Rt. Rev. Samuel A. McCoskry, D.D, LL.D., D.C.L., Oxon, Bishop of

Michigan; N. S. Davis, M.D., of Illinois, ex-President of the Association, and Samuel Gross, LL.D., D.D.L., Oxon, of Pennsylvania. The exercises were opened with the following prayer by Bishop McCoskry:

PRAYER.

Almighty and merciful God, we come before Thee as children to a father. We come before Thee with confidence, for Thou hast invited us to come, with deep humility, for Thou art holy, and we are prone to sin against Thee—with hope, for Thou hast promised to pardon and bless us.

May our hearts be warmed with love to Thee for Thy unspeakable mercies to us. May we fear to offend Thee, and enable us to make Thy law the rule and guide of our life. Enable us by Thy grace to give our hearts and lives to Thee—the only return which Thou requirest for all the blessings we have received.

Blessed Father, look upon us with mercy, give Thy strength to us, teach us Thy laws. Make us to walk in Thy truth, give us the fear of Thy name, and knit our hearts unto Thee with indissoluble bonds of love and obedience, that we may truly worship and glorify Thy holy name. Holy Father, we need the continued aid of Thy blessed spirit to guide us. Our hearts are prone to wander from and to forget Thee. The temptations of the world, the flesh, and the devil are often too strong for us. O, keep us safe by Thy guiding and sustaining hand; keep us from the snares of the ungodly, and from ourselves—from the dangers of our unholy desires. Leave not our souls in our own hands, but keep them under Thy protection and care, lest we swerve from Thy commandments. Heavenly Father, fill our hearts with Thy holy sorrow and compunction whenever we sin against Thee. Teach us to deny ourselves and take up our cross and follow our blessed Lord through good and evil report. May we catch his spirit of compassion and love for the poor and the needy, the sick and the distressed, the sinful and the dying. We bless Thee that Thou hast revealed the way by which many of the evils and sorrows of life can be lessened and removed. O, teach us that way! Take away the selfishness of our hearts, and enlarge them with true sympathy for all who need our aid. We bless Thee for all the human agencies which have been so signally used to restore health to the sick, to strengthen the weak, and to prevent the inroads of disease and death. Especially we ask Thy richest blessing upon this Association. May they in all their labors to understand and explain the mysteries of our being see and acknowledge that we are "fearfully and wonderfully made?" May they be deeply impressed with Thy wisdom in so forming every part of our mysterious frame to give unity and harmony of action, and energy and power to perform the various duties of life. May they never forget that the body is but a part of our being; that it was once the residence of a spirit that can never die; that it has been purchased and redeemed; that we will be restored to ourselves and placed

beyond corruption and death. May they never use the gifts Thou hast given them to promote and encourage vice and sin. Enable them to imitate and follow St. Luke, the beloved physician, who not only labored to heal the body, but (with the wholesome medicine which Thou alone can give) to cure the diseased and sinful soul. Be with them in all their deliberations. May peace and harmony be found here. Let Thy holy spirit ever be with them, and so lead them in the knowledge and obedience of Thy love that in the end they may obtain everlasting life through our Lord and Savior Jesus Christ, who with Thee and the Holy Ghost liveth and reigneth ever one God, world without end. Amen.

Wm. Brodie, M.D, then read a graceful and appropriate address of welcome.

We believe the President's address to be among the very best papers which have been read by presiding officers of the Association. We only regret that it is not in our power to place it before our readers in its entirety. We however, publish a few extracts which will show the spirit and elevated vein of thought which marked the whole address.

This Association is a true medical republic, national and representative, catholic and paternal. It admits to membership delegates from all respectable medical institutions throughout the United States.

What a grand and imposing spectacle it is to see in this beautiful city by the great American lakes, near the northern boundary of our vast country, assembling for the second time hundreds of leading physicians, brought together as a national medical council prompted only by a desire to promote the general good of the profession, a motive which speaks directly to the heart and head of every true physician.

Note 2.—The territory of the United States extends from near the twenty-fifth to the forty-ninth degree of latitude (or, including Alaska, to within the Arctic circle), and from the sixty-seventh to near the one hundred and twenty-fifth degree of longitude, embracing nearly 4,000,000 square miles, and over 40,000,000 population.

Note 3.—The American Medical Association met in Detroit in 1856.

Here are assembled the Nestors of the profession, who have won their laurels, and wear them with dignity; the middle-aged, who have just entered upon the enjoyment of their recently recognized but well earned fame; and the young physicians, fresh from college, full of enthusiasm, and well instructed in the latest discoveries and improvements in the art and science of medicine, eager for the contest in which to win fame, all seated together with an equality of rights and privileges – the retiring,

the busy, and the expectant practitioner forming a trinity of links in the great chain of professional life, connecting the past, the present, and the future; experience, science, and enthusiasm fraternizing and forming one of the grandest pictures of advanced civilization. I am aware that there has been from the first meeting of this Association a contrariety of opinion as to its true province and best policy. Some would have it limited in number, exclusive and dictatorial, if not coercive, in its relation to the profession at large. There are those who would have it abandon all semblance of a convention, and devote its time to discussion of scientific facts and the recent discoveries in medicine, to the exclusion of all other questions and purposes.

A few would have it an arena where any and every subject that can enter the mind of a medical man might be introduced without order or even definite purposes.

Others would be content and feel rewarded for the expense and loss of time in the enjoyment of the social elements so lavishly evoked on all these occasions.

But the arrangement for scientific discussions and the exchange of professional views, with the social reunions which the average judgment of the gentlemen who are in the habit of attending the meeting from year to year has approved, come as near meeting the requirements and securing due consideration to every factor, social, ethical and professional, that enters into and serves to advance the true interests of the whole profession, and bind it in one harmonious brotherhood, as any general policy that could at present be adopted.

Aggregations of individuals from communities and intercourse establishes society. Common interest and enlightened sentiment, with frequent meetings and exchange of views, evolve dominant ideas, and dominant ideas rule not only societies like ours but the world.

Twenty-eight years ago this Society was organized, in the city of New York, on a basis to represent the regular profession of the United States. Whether it has always acted wisely, or done the best and all that was possible under the circumstances for the elevation and advancement of the profession, must be impartially judged by our successors.

Note 4.—The first meeting was held in the city of New York, May 5, 1846, on a call of the Medical Society of the State of New York for a national medical convention. This action was brought about chiefly through the far-seeing wisdom and active exertions of N. S. Davis, M.D., then of Binghampton, New York.

All medical societies and institutions throughout the United States were solicited to join in the measure. A convention assembled, which led to our present organization. The institutions represented in the first meeting were: State medical societies, 7; local medical societies, 13; colleges, 14; hospitals, 2. Total, 36 institutions from seventeen different States, sending in all 129 delegates.

Whatever it has done it will be conceded has been done with disinterestedness and a sincere desire for the elevation of our noble calling, and for the more speedy and certain relief of human suffering.

Of those who find fault I might ask: What organizations or institutions are perfect or give even general satisfaction? They are not to be found among religious creeds, nor are they to be found in any known code of laws. They do not exist. But the great improvement taking place throughout the world in all the conditions that regulate society and protect rights of the individual is marvelous. And though absolute perfection is not attainable, we are striving with encouraging success to elevate and perfect as far as possible the art and science of medicine. If I possessed the ability to draw a picture that could correctly represent the status of the profession and its organizations throughout our country at the period when this Association came into existence, and contrast it with one showing our present prosperous condition and numerous well organized societies, the younger members would scarcely recognize them as representing the same profession.

Note 5.—In 1846, when the American Medical Association was formed, there were but about 125 medical societies and institutions of all classes in the United States. At the present time there are over 1200. The first medical organization in the United States that I have seen any reference to was one in Boston in 1741. Another was formed in Philadelphia, Pennsylvania, in 1765, of which John Morgan was a promoter and an active member. In 1766 the New Jersey Medical Society was formed. In 1773 the American Medical Society was formed in Philadelphia, Dr. William Shippen, President. In 1776 the Delawere Medical Society was formed. In 1781 the Massachusetts Medical Society. In 1784 the Connecticut Medical Society. In 1787 the College of Physicians, Philadelphia. In 1789 the Medical Society of South Carolina. In 1791 the New Hampshire Medical Society. In 1799 the Medical and Chirurgical Faculty of Maryland.

It is not too much to claim that this Association, by the mere moral support it gives in encouraging the formation of local medical ooganizations, has done immeasurable good, and added largely to this result.

It fully recognizes the unity of the regular profession throughout the United States. At the same time it has erected a standard of medical excellence and ethnical *esprit du corps* never before attained in America, and has persistently and publicly held them up for admiration and adoption. It has drawn with distinctness a line that separates the regular from the irregular practitioner, which will in the future as in the past be firmly maintained, and it is desirable, as far as practicable, to encourage uniformity in the plan and scope of all medical societies which in the sphere of their influence correspond to our civil political divisions; and further, I think we should continue our efforts to encourage

these local medical organizations until every State and county in the United States shall have its medical society, holding proper relations with each other and with this body. The Association is now and always has been anxious to bring into its fold the leading physicians from every State, and from every city and village and rural district in the land, and has solicited and still invites original papers on any subject that can advance medical science. It has published annually from its organization a volume of contributions and regular and special reports of great value in the different departments of medicine on questions of immediate interest to the profession.

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Note 8.—The several States are gradually enacting laws to secure registration of births, deaths and marriages, and establishing State Boards of Health. But, indeed, it is rather surprising that so few (California, Massachusetts, Michigan, Minnesota and Rhode Island) have any laws upon this subject in successful operation.

Maryland and Virginia have recently established boards. Massachusetts deserves to be followed as a model in her registration and health laws, as well as in their intelligent execution.

It is within the observation of all, that while the profession is advancing in knowledge and influence, so is the general intelligence of the people, who are yearly demanding greater protection to their health, and greater skill and ab lity on the part of their physicians in the various branches of medicine.

Education is synonymous with elevation. Knowledge in medicine, as everywhere else, is power and ability. It is a source of sincere congratulation that our medical educational institutions are rapidly enlarging and perfecting their curriculums, and becoming more thorough and efficient in teaching the science of medicine.

Note 9.—In 1859 the Chicago Medical College (now the Medical Department of the Northwestern University of Illinois) adopted the three years course, and a system of graduated classes and a lecture term of six months. In 1872 Harvard University adopted practically a similar course. I am not aware that any other college has yet adopted this plan. The system is working well in both the institutions named. It has so much to recommend it that it is to be hoped all our first-class colleges will adopt it within a very few years.

In 1870–71 returns from 51 medical colleges show 5,876 matriculants and 1,698 graduates. In 1871–2 returns from 36 of the 60 medical colleges show 4,956 matriculants and nearly 1,400 graduates. In 1873, in 59 regular medical colleges there were 780 instructors employed. There were 6,491 matriculant students, of whom 1,941 received the degree of Doctor of Medicine. (See Report of Educational Department for 1873.)

The successful medical man must be fully up with the age and the times, conversant with the latest means of diagnosis, the

theory of diseases and their cures. He must have tested the newest remedy, examined the most recent invention—he must have read the latest telegram. And, indeed, the genius of the profession, responding to these demands, has devised physical tests for an exact diagnosis in almost every important disease. There is scarcely a vital function the normal or diseased action of which cannot be determined with accuracy through the aid of some chemical test or mechanical device. The improvement in this direction is still actively progressing.

Note 10.—We shall enumerate but a few of the many appliances used in diagnosis, as they will readily occur to the mind of the physician: The dynamometer, the microscope, the opithalmoscope, the laryngroscope, the rhunscope, the stethescope, pleximeter, endoscope, specula, the sphygmograph, the spirometer, the thermometer, the anæsthesometer, anæsthetics, chemical examination of excretions, etc., etc.

Perhaps there is no branch of medicine brought so prominently before the public as that of surgery, or that has been so much improved within a few years.

American surgeons can justly claim to have done their part to advance its efficiency.

Their daring and successful operations, as well as ingenious devices and numerous mechanical appliances, are the admiration of the profession everywhere.

In our cities centre not only dense populations, with great wealth, but all the conditions and institutions most favorable for prosecuting special studies in the most thorough and satisfactory manner. It is them, as a general rule, that society fosters, and so frequently rewards with princely fortunes the pre-eminently capable and skillful physicians, as it does men of genius in other callings.

It is true that in great cities pretenders and charlatans also prosper, but where this is a particularly noticeable fact it may, as a rule, be inferred that there is abundant room for physicians of first-class ability. Indeed, physicians possessing the latter qualifications are sure of employment, and are at home and welcome everywhere. Even the best educated neighborhoods and most highly enlightened communities, if not supplied with acceptable physicians, possessed of adequate professional knowledge, and well acquainted with the latest improvements in medicine, will patronize the most plausible pretenders to such acquirements. The physician is a necessity. He is employed neither as a luxury nor from friendship, but for his actual or presumed skill and ability to prevent, relieve or cure disease.

Progress is the order of the day—a law of the universe. He who does not constantly keep adding to his knowledge and increasing his resources, must soon fall behind the more enterprising and the better informed of his contemporaries. The physician who does not know that the community in which he lives is keeping a constant watch upon him, and contrasting his

knowledge, skill, and success in his profession with those of the best and most successful medical men within the range of their reading or acquaintance, shuts his eyes to an important fact of great interest to himself. The judicious and observing know that time regulates many apparently conflicting professional interests, and sets them working in a manner that in the end promises no discredit to our calling.

It is the judgment of those who watch closely the workings of this Association that questions of a hypothetical, local or transient character ought not to be intruded here. The time of the Association should be fully employed in scientific discussion, and in the consideration of questions of necessary and proper legislation.

* * * * *

I wish, further, to suggest or to inquire whether our influence ought not to be given to the end that all medical institutions created by law or acting under charters should be required to make and publish annual reports of their facilities, their labors, discoveries, and the extent of their operations to the legislative body that created them.

The principle should obtain, that when a charter issues the institution assumes a public character and should be held responsible by the public for the manner in which it exercises its privileges, raises and expends its funds and conducts its business. The public has a right to claim this much, that it may profit by experience, correct abuses and encourage only the deserving. It has been estimated that there is about 2,400 separate and distinct diseases. The number will probably be reduced as our knowledge of physiology and pathology and of the natural history of diseases becomes more general and accurate.

What seems at first in many cases to be a specific difference frequently disappears as we become more familiar with the range of circumstances and variations that enter into the producing causes, as well as those that may prolong and modify prominent symptoms. Hospitals for the cure and medical treatment of the insane have been provided for most liberally by the general government and by nearly all the States. Being public institutions, and yet bearing the character of specialties, and possessing unequaled advantages, it seems but natural that the profession should expect reports from them with records of carefully studied cases and detailed accounts of autopsies and microscopic examinations of diseased brain and nerve tissue of practical value to the general practitioner. A most important step has been taken in this direction by Dr. J. P. Gray, Superintendent of the New York State Lunatic Asylum at Utica, who requested that a special pathologist be appointed as his assistant in that institution. The doctor's recommendation to this end was unanimously passed by the Legislature, and an accomplished microscopist was appointed to the position. A complete outfit for careful chemical and microscopical examinations, and for autopsies, and

for taking photo micrographs of diseased organs and tissues was provided. The late reports already attest the wisdom of the measure.

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There are some hundreds of young physicians of first-class ability, noted for their originality of thought and boldness of investigation, residing in different parts of our country, who are working with the scalpel and microscope, and the test within the spirit of true philosophers. But where we have ten such we ought to have a hundred. It is from among such that must come the leading medical men of the future.

Young physicians are peculiarly adapted for experimental investigations by having the time, the enthusiasm and the daring imagination to be original. They are also free from undue reverence for old ideas, and are without preconceived theories—conditions so essential for original experiment and discovery, for which age is found to be largely disqualified.

It is therefore chiefly to the young that the profession must look for its original work and new facts. And while we are deeply gratified and ever ready to award due praise to the retired or deceased physicians who have fairly won, by earnest study and devotion to medicine, the highest honors of the profession, I feel it to be a duty to encourage by honorable commendation young men who, by virtue of genius, industry and force of character, are conquering obstacles to increase the knowledge of our profession and gain recognition and success. Such young men became the teachers of their teachers, the masters of the colleges.

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Our country has much reason to be grateful to a kind and overruling Providence for the prosperity and general good health that has been vouchsafed to her citizens during the past year. A few cities in the South, however, have been visited by severe epidemics. You will immediately recall afflicted Shreveport and Memphis, where yellow fever of a most malignant type prevailed for over two months, carrying off thousands of valuable citizens.

Note 14.—In Memphis, in 1873, about 2,000 died of yellow fever. In Shreveport, the same year, 759 died of yellow fever.

The devotion of physicians to their professional duties in those places furnishes additional evidence of the heroism of medical men, even to the extent of sacrificing their lives in the line of duty.

In Memphis seven physicians fell at their posts.

Note 15.—The physicians dying of yellow fever in Memphis were Drs. Crone, Minor, Kennon, Hatch, Brount, Freeman and Williams.

And at Shreveport, four, three of whom were resident physicians and one, J. A. Richardson, of Jefferson, Texas, who had volunteered his services to these people in their hour of distress.

Note 16.—The physicians who died in Shreveport of yellow

fever were Drs. John B. Wise, T. P. Hotchkiss, J. L. Hebbette and J. A. Richardson.

At a most alarming period of the epidemic at Shreveport, in response to a call for professional aid, physicians from New Orleans, sixteen, went there to assist the overburdened and thinned ranks of the profession of that city.

The departments of the national goverment in which exist an organized medical corps, the army and the navy, need no special mention or encomiums from me. Their well conducted bureaus and the highly accomplished personnel of the medical staff corps are the pride of the profession of the whole country. Their works speak more fittingly of their praise than could any words of mine. The great army pathological museum and the magnificent medical library, which their thoughtful considerations for the profession have created, fully attest their devotion to their high trust, and their love for advancement of medical science.

Within a few years a third medical department, known as the Marine Hospital Service, under the immediate direction of the Secretary of the Treasury, has come into existence. It is under the supervision of an able medical officer, and the purpose is to provide hospital accommodations and medical treatment for sick seamen belonging to the merchant marine of the United States. At most of the ports contracts are made with the local hospitals to receive and treat the sailors.

The Government owning but ten hospitals, it consequently makes but few medical appointments. The service is therefore small, and may, from its importance, be considered in a formative state.

The medical officers of the army have not yet received that consideration and relief from Congress which you recommended at your last meeting. The committee appointed by you drew up a memorial, which was duly presented to Congress; they also called in person upon the members of the Military Committee of the Senate and House of Representatives, and prepared a bill entitled "A bill to increase the efficiency of the medical department of the army," which, if passed, would place the medical staff upon a footing of equality with the other staff corps. This bill was introduced into the House of Representatives, January 6th, 1874, read twice and referred to the Military Committee, who have not yet taken any definite action on the subject.

Meanwhile, the action of the American Medical Association and your committee has been heartily endorsed by the general, State and county medical societies, almost every one of which has passed resolutions on behalf of their brethren in the army, and forwarded them to their representatives in Congress.

The medical profession of the United States can never be content that the medical officers of the army shall continue to be treated with less consideration in the matter of rank and pay than the officers of other staff corps, or of the medical corps of the navy, and will never cease their appeals to the representa-

tives of the people until justice is done to this deserving body of medical men. I recommend, therefore, that a large committee be appointed to plead their cause anew; that we may, if possible, secure favorable legislation before the present Congress adjourns, or if this prove impracticable, that we may bring the matter to the attention of the next Congress so soon as it convenes.

The systematic observations in meteorology that are being made throughout the country, under the direction of the United States Signal Office and the Smithsonian Institution, promise in their results to be of great value in the study of climatology and the causation of disease. I hope to see that system widely extended, and its stations multiplied throughout the whole country.

* * * * * *

Note 18.—The combined ages of the twelve Presidents of the Association, now deceased, gives an average of seventy-five years, the oldest having reached the age of eighty-six and the youngest dying at the age of fifty-two. The average age of the thirteen now living is nearly sixty-four years, the oldest being seventy-seven and the youngest forty-eight. From records in my possession I find the average age at which 2,000 American physicians died was 58 years. I find also the average age at which 1,000 United States Congressmen have died is 62 years. The average age at which 1,200 Presbyterian clergymen of the United States died was 57 years. The latter fact has been furnished by J. M. Wilson, of Washington, D. C.

* * * * * *

In conclusion, I must beg the privilege of thanking you for your patient attention, and again reiterate my confidence in the belief of a steady increase and accumulation of knowledge in the world, and the undoubted progressive and permanent elevation of the profession in the United States. My conviction is decided that in no period in the world's history have the rights of individuals and the wants of humanity been so well secured and so invariably accorded to the mass of the people as at the present time.

And further, I affirm that the United States by her liberal institutions, the customs and intelligence of her people, conspicuously leads the nations of the earth for comprehensive benevolence and Christian charity. The heart and mind of the American people are educated from childhood to enjoy freedom of thought and liberty of action, and to exercise all their talents as their judgment dictates, at the same time they recognize a brotherhood in the whole human race, and award an equal opportunity to all in the struggle for fame and fortune.

"Do unto others as ye would that they should do unto you," expresses the spirit and logic of the age, and renders invincible the crusade that is revolutionizing the world.

With Tennyson, I exclaim:

"Ring out the old,
Ring in the new,
Ring out the false,
Ring in the true."

The inspired promises and hopes of mankind demand a kindlier heart, a large brain and a commune of sympathy between all classes, the ruler and the ruled.

Finally, I beg your earnest support and wise counsel in conducting the business of the Association. The eyes of the profession of the United States are upon us. It is in our power to make this meeting one of marked success, both in the amount and character of the scientific work we may do.

Whatever ability I possess is at your command. It is with me an earnest desire to win, by fealty to my trust, the approbation of this Association and of my brother practitioners through the country. I hope to deserve to have it written of me that I love my profession and my fellow man.

We will be obliged to postpone further notice of the proceedings until our next issue, when we propose to discuss, as searchingly as possible, certain scientific propositions and "new departures" which have been advanced. The next meeting will be held at Louisville, Ky., on the first Tuesday in May, 1875.

The Association paid Dr. W. K. Bowling, of Nashville, the well merited compliment to make him their President.

Cyclopædia of the Practice of Medicine.

We call the attention of our readers to the subjoined announcement. We must admit that the science of medicine has reached a degree of perfected cultivation in Germany scarcely attained to in other countries. The names of the authors who are to assist Dr. von Ziemssen include the leading medical celebrities of Europe; we are therefore able to assure our readers that those who obtain the work will have cause to congratulate themselves upon their outlay. If readers in the South desire, their names and addresses will be furnished the publishing house through the Editor of this JOURNAL.

At the request of Dr. H. von Ziemssen, Professor of Clinical Medicine at Erlangen, a number of the most eminent clinical instructors of Germany have undertaken to prepare, in a series of independent treatises, a complete Cyclopædia of the Practice of Medicine; the incentive to this labor is the great need which has been felt the past year or two of a work which fully corresponded to the present stand-point of clinical medicine. This Cyclopædia will embrace the entire range of Special Pathology and Therapeutics, and will be completed in fifteen volumes, large octavo, of from 500 to 700 pages each. The list of contents of each volume, herewith appended, gives the names of the authors and

the special departments which they have undertaken. While the work of each writer will bear the stamp of individuality, there will be an effort made to give to each subject the prominence and space due to it only—that the harmony of the entire work may be preserved. It is designed that the Cyclopædia shall be, par excellence, a Practical Hand-book for Physicians; and for this reason especial attention has been given to clear and systematic arrangement.

For the value of the whole work, as well as the separate departments, the names of the writers are a sufficient guarantee. Each volume will have a full and carefully prepared Index.

Messrs. Wm. Wood & Co. have the honor to announce that they propose to publish by *subscription* a translation of this work. The translating will be done by professional gentlemen, many of them former students of the writers of the different treatises, under the supervision of a responsible chief. Great care will be taken with the mechanical execution of the volume. The type will be large and clear, the paper fine, and the engravings electrotypes of the originals. It is proposed to publish three to four volumes a year, at, as nearly as possible, regular intervals, in order to distribute the cost of subscription equally over about four years.

Among the most beneficent of duties which a good government is capable of discharging is that of instituting scientific inquiries in regard to the causes and prevalence of epidemic diseases, in order that measures may be adopted for their arrest or mitigation. Therefore, whenever governments do undertake the discharge of such a duty, all good men should contribute their energies and observations in its aid. Inviting our readers, both in this city and in all other parts of the South where cholera appeared in 1873, to aid Dr. McClellan in carrying out the objects of his detail, we can assure them that the designs of the act commissioning him with this work will be most energetically and faithfully completed if proper aid is afforded him. We trust that none of our profession will prove delinquent in furnishing their observations. The following is a copy of the circular issued by Dr. McClellan.

NEW ORLEANS, LA., June 1st, 1874.

Doctor.....

DEAR SIR.

Having been detailed to carry out the provisions of an act of

Congress in relation to the epidemic of cholera of 1873, I am desirous of obtaining a complete record of all cases which occurred in the city of New Orleans. I have obtained from the office of the Board of Health a complete record of the fatal cases; but I am very desirous of obtaining a record of the cases which recovered. For this purpose Blank Forms are enclosed to your address, and if you will fill them up with the required data, and address them to me in the care of Prof. S. M. Bemiss, University Building, at your earliest convenience, I will be under the greatest obligations.

Very Respectfully,
Your Obedient Servant,
ELY MCCLELLAN,
Assistant Surgeon U. S. Army.

Obituary.

At a called meeting of the Zanesville Academy of Medicine, held Saturday, May 2d, to take action in relation to the death of its late Fellow, Dr. John G. F. Holston, Sr., which took place at Washington, D. C., May 1, 1874, the following resolutions were adopted.

That we, whose occupation has been to relieve human suffering, are reminded that the time must come when our places on earth shall be vacated, therefore

Resolved, That in the death of Dr. Holston, the Zanesville Academy of Medicine loses one of its prominent members, and the profession at large an eminent physician and surgeon of extensive professional and literary culture, ripe experience and accurate judgment, and society a warm-hearted, genial and generous member, whose life has been mainly devoted to the good of his fellow beings.

Resolved, That we attend the obsequies of our deceased Fellow in a body.

Resolved, That we deeply sympathise with the family and relatives of the deceased.

Resolved, That the Corresponding Secretary transmit a copy of these resolutions to the family, the City Press, and the Medical Journals.

C. C. HILDRETH, *Chairman.*
A. E. BELL, *Secretary pro. tem.*

HUGHES—At his residence, No. 24 West Walnut street, Louisville, at 5.30 p. m., May 8th, 1874, Dr. JAMES N. HUGHES, aged 69 years.

Dr. Hughes was a zealous and skillful physician, a conscientious and upright man.

METEOROLOGICAL REPORT FOR NEW ORLEANS.

Table I---May.

Day of Month.	Temperature.			Mean Barometer Daily.	Relative Humidity—Mean.	Rain fall—inches
	Maximum.	Minimum.	Range.			
1	74	59.5	14.5	30.04	66	.00
2	81.5	63.5	18	29.83	71	.00
3	86	73	13	29.81	74	.00
4	72	67.5	4.5	29.89	48	.00
5	70.5	58	12.5	29.99	57	.00
6	73	57	16	30.08	78	.00
7	78	59	19	30.18	61	.00
8	80 5	63	17.5	30.24	56	.00
9	82.5	63.5	19	30.19	65	.00
10	83	65	18	30.10	73	.00
11	82	64.5	17.5	30.11	68	.00
12	87	67	20	30.10	63	.00
13	85	69.5	15.5	30.04	63	.00
14	84	70	14	30.01	66	.00
15	88.5	70	18.5	30.00	65	.00
16	86	69.5	16.5	30.00	69	.00
17	84.5	69	15.5	30.03	58	.00
18	88.5	66.5	22	30.02	62	.00
19	84	69	15	30.05	54	.00
20	88.5	69.5	19	30.03	70	.00
21	92.5	71	21.5	30.02	65	.00
22	92	71	21	30.03	63	.00
23	92	74	18	30.03	64	.00
24	92	74	18	30.03	71	.00
25	89	75	14	30.04	66	.00
26	88	73.5	14.5	30.03	71	.00
27	89	76	13	30.02	67	.00
28	90.5	75	15.5	30.02	70	.00
29	91.5	75	16.5	30.10	65	.00
30	92	76.5	15.5	30.11	66	.00
31	90.5	76	14.5	30.01	78	.12
Mean..	85.097	68 726	16.371	30.037	65.58	Total. .12

Table II---June.

Day of Month.	Temperature.			Mean Barometer Daily.	Relative Humidity—Mean.	Rain fall—inches.
	Maximum.	Minimum.	Range.			
1	93	74	19	29.89	70	.00
2	90.5	74	16.5	29.91	66	.00
3	89.5	74	15.5	29.98	62	.00
4	91	74	17	30.00	60	.09
5	93.5	74	19.5	29.96	66	.00
6	92	75	17	29.95	76	.00
7	92	74.5	18.5	30.00	72	.00
8	92	76	16	30.02	67	.00
9	91	77.5	13.5	30.03	68	.28
10	92	77	15	30.02	72	.00
11	90	77.5	12.5	30.04	80	.70
12	92	76.5	15.5	30.05	71	.00
13	87	74.5	12.5	30.07	83	1.60
14	90	75	15	30.06	79	.37
15	88	76.5	11.5	30.04	91	2.25
16	84.5	72	12 5	30.06	—	.00
17	82	74	8	30.07	—	1.14
18	86.5	74	12	30.10	—	.20
19	87	73 5	13.5	30.11	—	.00
20	88	75.5	12.5	30.09	—	.54
21	87	74.5	12.5	30 01	—	.00
22	87	74	13.	30.02	—	.80
23	89.5	75	14.5	30.10	—	.00
24	90.5	76.5	14	30.15	—	.00
25	88	78	10	30.16	—	1.09
26	92	76.5	15.5	30.11	—	.00
27	92.5	76.5	16	30.03	—	.00
28	92	76.5	15.5	29.97	—	.00
29	93	75.5	17.5	29.98	—	.00
30	94	78.5	15.5	30.01	—	.00
Mean..	89.883	73.350	14.533	30.033	00	Total. 8 97

Mortality in New Orleans from May 1st, 1874, to June 30th, 1874, inclusive.

Week Ending	Small-pox.	Malarial Fevers.	Typhoid Fevers.	Dysentery.	Choleraic Diseases.	Total Mortality.
May 10.....	16	5	3	1	2	98
May 17.....	20	2	0	3	3	113
May 24.....	23	8	0	5	10	148
May 31.....	20	9	1	6	23	189
June 7.....	18	12	2	6	12	163
June 14.....	12	11	3	7	14	172
June 21.....	13	11	4	6	6	131
June 28.....	19	12	9	4	5	154
Total.....	141	70	22	38	75	1168

THE

NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

SEPTEMBER, 1874.

ORIGINAL COMMUNICATIONS.

ARTICLE I. *Variola with Black Vomit.* By J. C. FAGET, M.D.

A FEW NOTES ON AN EPIDEMIC OF VARIOLA COMPLICATED WITH HEMORRHAGES OF VARIOUS DESCRIPTIONS, OBSERVED IN THE ORPHANS' ASYLUM OF NEW ORLEANS, FROM THE LATTER PART OF DECEMBER, 1873, TO THE END OF MARCH, 1874.

"*Jusqu'à preuve du contraire, la Peste d'Athènes fut une petite vérole compliquée de Typhus.*"
—Daremberg, cite par le Professeur Anglada de Montpellier.

"The disease known as *Plague of Athens* must be looked upon as Variola complicated with Typhus, provided further evidence should not prove the contrary to be true."—*Daremberg, quoted by Professor Anglada of Montpelier.*

The inmates of the Orphans' Asylum number on an average 300 souls. I have been in charge of the institution, as attending physician, for the last twenty years or more. During all that time the condition of the place has never been more healthy than it was last year (1873). Nothing has been changed in the regulations of the establishment; the same careful attendance to hygiene has been kept up; the fare is poor but quite sufficient, with a larger supply of meat than vegetables, no salt provisions or pork meat being used. In spite of hard times, everything went on thus smoothly up to the latter part of the year 1873, when a few cases of variola, more or less modified by vaccine, began putting in an appearance. These first cases I considered ample warning, and thought of going to work immediately revaccinating

the whole house; we were then suddenly shocked by several strange and sudden deaths occurring among the inmates: within a few days one of the sisters and two children had died. The sister died on the third day, from *metrorrhagia* (menses not being then due), during a very high fever; *pustules* began showing an appearance at the moment of death. The two children likewise died within three days with *epistaxis*, *black vomit*, pain in the abdomen and head, but they showed *no sign of any eruption.*

I could gather no further information concerning these first cases; I regret to say, I did not see them. The first child fell sick on the 17th day of January, and died on the 20th; the other child was taken ill on the 24th and died on the 27th (in both cases black vomit was observed, with violent headache and abdominal pain): such is the sister's narrative.

On the 28th day of January, I resumed my regular visits to the Asylum. A boy 9 years old had been transferred to the infirmary the day previous to my visit; the disease in his case was to run the same course as in the two first cases above mentioned, and undoubtedly to have the same result. The following are the notes I wrote down in the infirmary and in the dead-house.

Case I.—J. J. Daly, 9 years. January 26th, evening; headache and fever, thirst, sick stomach. 27th; fever has increased (sister's statement); *vomiting* is *characteristic*, i. e., *grayish*, *heavy mucous* fluid (sinking in water), containing *black lumps, no hemorrhage from the nose having as yet occurred.* Ten grains of quinine were administered. 28th, or second day: high fever all night and *epistaxis* (first occurrence); pulse 120, very compressible; temperature in the mouth 104° F., in spite of the skin being apparently cool to the hand feeling it; eyes very red; patient restless, pushing away the blankets, etc. Evening—During the day the patient had been throwing up bloody mucus from the stomach; epistaxis; black vomit stains the linen of his bed; pulse 108; temperature 104°; skin cool, even on the head. During my visit I am called back to the child, who is said to be dying. He is actually going through the latter stage of a violent general *convulsion.* Pulse hardly perceptible; strong and tumultuous beating of the heart, no abnormal sound being present; no eruption on the skin; blisters.

January 29th—third day—morning. patient moribund; death rattle; pulse hardly perceptible, 120; temperature 105° F.; spon-

taneous micturition. Evening—patient breathes his last at 6 o'clock, as I enter the infirmary; the heart has stopped beating; thermometer in mouth shows 103°, and a few minutes later 102°; so far *no signs of variola.* Post mortem January 30th: black blood filling the sinuses of the dura mater; intense congestion of pia mater; gray substance of brain well marked; white substance normal as far as concerns color and consistency, shows no dotted appearance of congestion on the surface of sections; the congestion is only in the meninges. Lungs and heart normal; the left ventricle of the heart contains a discolored yellowish white fibrinous clot, which goes to explain the signs during the agony of death. Liver enlarged, of a deep red color, left lobe reaching as far as the spleen; the latter organ has preserved its normal color and consistency, but appears twice as large as in physiological conditions at that age. Contents of stomach watery and mucous, turbid; the mucous membrane in the region of the large cul-de-sac of the stomach is congested to such an extent that it seems as if on the verge of bleeding. Its appearance recalls to my mind the condition shown in a specimen of a stomach containing *black vomit* from a case of yellow fever in Prof. J. Jones' practice; there we merely have *grayish mucous* fluid, which grows thicker as we approach the pylorus. After removing the fluid contents of the stomach, no muciparous glands are visible in that region. In the duodenum the mucous membrane is found to be intensely congested in the upper portion of the bowel; it also shows pimples resembling *a discrete eruption of variola.* These are mucous glands of the duodenum which are hard, round and as large as a pin's head. As we proceed towards the lower part of the duodenum these corpuscles are more scarce, and the mucous membrane of the intestine is less red. The above changes could only be seen after removing with a scalpel a thick yellowish red mucous fluid, which would sink in water. Bladder the size of a pigeon's egg, contains a few drops of non-albuminous urine.

The above post-mortem examination was performed with the assistance of my son, a resident student in the Charity Hospital. It took us several hours to go through the work, and yet the investigation remained incomplete. I especially regret that we had not examined the lower portion of the small intestine; my previous experience in *similar* cases leaves me under the impression that we should have found Peyer's patches merely marked, or more marked, than in normal conditions.

Case II.—McGuire; eleven years old; January 26th, evening; uneasiness, thirst, fever, nausea, headache. 27th, colics and vomiting. 28th, has been vomiting during previous night; the matter thrown from the stomach is a *brown fluid*, at the bottom of which appears a *magma of heavy and black mucus*, which to me is characteristic. Pulse 120; temperature 102° F. Breathing difficult, with sighs; patient has passed his water; fluid passages from bowels, yellowish color; must have felt the influence of 20 grains quinine the day previous, for hearing is dull; spasmodic movements in the face, threatening convulsions; constant nausea; ice, mustard poultice, quinine.

Evening—end of 2d day. Has taken twice ten grains of quinine, and kept it with the exception of first dose. Urine pale, transparent, watery, no albumen by the acid test. Matter thrown from stomach always the same, viz: brown fluid, over which are spread some dark flocculent corpuscles ("flies wings"); at the bottom lies a magma of *black mucus*, *as yet no epistaxis.* Pulse 120; temperature 102.5° F. Suspirious breathing.

January 29th—3d day—morning. Pulse, 120; temperature, 104.5° F. Was delirious during the night, small quantity of same fluid from stomach; *epistaxis*, first occurrence. Tongue coated and damp; *herpes* labialis; two stools, solid, brought by injections with quinine; suspirious breathing; grimaces. Blister on epigastrium; subcutaneous injections of quinine, (5 grains).

Evening.—During the day the patient has been bleeding profusely from the nose. Pulse, 108, very compressible; temperature 105° F. Patient has passed water, has had no vomiting, no passage from bowels. Has taken and kept some wine of cinchona, and a little brandy. Shows no mark of vaccine.

January 30th—4th day—*Eruption of variola*, plainly visible on the face, Pulse, 100; temperature, 103° F. During the night had *genuine black vomit* very abundantly, together with bleeding from the nose, black vomit in this case being partially due to the blood running from the nares into the stomach. Evening—the eruption continues its course; patient quiet; a few more drops of blood from the nose; has had no more vomiting; passes water freely. Pulse, 100; temperature, 102.5°. Has taken 5 grains sulph. quinine.

January 31st—5th day. Night quiet; confluent eruption on the face; hardly any pimples on the body; a few on the legs and

arms. Pulse, 120; temperature, 102.5°. Palate and pharynx covered with confluent pustules.

February 1st—6th day. The eruption progresses and spreads downwards; the face of the patient is largely swollen; bowels in good order; passes water abundantly. Pulse, 120; temperature, 103°. A few pustules on the tongue; throat swollen and white (coated); no further bleeding or vomiting.

February 3d—8th day. Face immensely swollen; eyes closed from the swelling; abundant salivation. Pustules confluent to such an extent as to form *large flat white patches* on a very red integument. Pulse 144, miserable; temperature 102.

9th day. Dessication of the pustules begins to show on the forehead, where scabs are to be seen. Pulse 144; temperature 104. Profuse salivation. In front of the left ear the pustules have grown together in such a way as to show a flattened vesicle as if a blister had been placed on the spot. Broth and whisky are prescribed, together with milk, eggs (raw) mixed up with milk. Bowels and bladder normal.

10th day. Pulse over 144; temperature 105. Very profuse salivation. Breathing difficult and accompanied with a *rattling* sound from the fluids accumulating in the nose and throat. Dessication progresses on the forehead. Stools and urine normal. Patient refuses to take any more milk; still takes some wine of cinchona.

11th day. Very troublesome all night. Pulse 144; temperature 104°.

12th day. A black and offensive discharge is thrown from the pustules. The face looks horrible; the body appears as if scalded. Pulse miserable, beats over 144; temperature 102°. Incessant complaint. Drinks with difficulty and throws up everything. Death occurs on the 13th day. The body had been buried before I reached the Asylum, on account of offensive smell.

Case III.—Peter Crane, aged 8 years. Enters the Infirmary Jan. 28th, after experiencing uneasiness for several days. Instead of the *ataxia* shown by the other patients, we have in this case a deep *sopor*. Passes water unconsciously; skin cool; temperature 106° F. in mouth; pulse 108; small quantity of *brown mucus* from stomach; no epistaxis. Shows no signs of previous vaccination. Ten grains of quinine in the morning. Ten more grains in the evening are thrown off.

2d day. Sopor. Pulse 115; temperature 107° F. Skin not warm to the hand; injection (10 grains quinine) into rectum promotes healthy passage from bowel. Another similar injection is absorbed.

Evening. Less soporosè; complains of pain in the head. Pulse 115; temperature 107°. Slight nausea. Three or four ounces of fluid from stomach. It is a transparent fluid on top of which may be seen thousands of "*black wings of fly*" (small, dark flocculi); at the bottom of the basin is a magma of *heavy and black mucus.* Epistaxis has never occurred.

3d day. The eruption shows itself; it is a discrete eruption and is seen on the forehead and cheeks. Pulse 100; temperature 103.5°. Temperature in the evening 105° F.

4th day. Few pustules are to be seen on the face, none on the body, legs and arms; a confluent eruption exists on the breech which is constantly bathed in urine; pulse 96; temperature 102°.

5th day. All quiet. The eruption on the face consists of *round vesicles* which are not umbilicated; none are to be seen on the body and extremities; on the breech, large red pimples irritated by the urine. Pulse 96; temperature 102°.

7th day. Pulse 104; temperature 100. Scabs begin to show on breech.

8th day. Offensive smell; remarkable shaking of the hands. Pulse 108; temperature 99.

Immediate convalescence.

Case IV.—Victor Bello, age 9 years. Taken ill Jan. 28th. Severe headache, with nausea. Evening; pulse 120; temperature 104°; skin hot.

2d day. Pulse 120; temperature 103.5°. Morning; has taken 10 grains quinine. Evening; profuse and repeated vomiting during the day of a bilious, yellowish and greenish matter; some of it remaining in the basin is of a *yellowish* color, with an enormous magma of *grayish mucus* which can only be discovered after removing the fluid yellow matter. Headache and pain in the epigastrium. No passage from bowels. Pulse 130; temperature 106°. Great thirst. Mustard plaster to epigastrium; camphorated oil and laudanum on abdomen. Carbonate of magnesia (purgative dose), quinine, 10 grains, morning and evening.

3d day. Face, and especially ears, very red. Watery and turbid fluid from stomach; at the bottom of the basin, when

emptied, there remains a quantity of *grayish mucus, spotted with black dots*, and which adheres to the basin. No blood from the nose so far. Pulse 120; temperature 104.5°. Evening—same condition; has not vomited.

4th day. Pulse 96; temperature 100° F. Three or four pimples appear on the face.

5th day. Pulse 96; temperature 100°; same appearance of the eruption. Convalescence.

Case V.—Castello, age 4 years. Taken ill Jan. 29; headache and rachialgia; nausea, thirst. Pulse 120; temperature 106.5° F. No signs of previous vaccination.

2d day. Delirious all night, had to be kept in bed by force; quiet in the morning; pulse 108; temperature 104°; eruption already visible. Evening—cheeks red, skin moderately warm; temperature 105°; pulse 104; good passage from bowels after injection.

3d day. Confluent umbilicated pustules on the face, none on the body, few on the limbs. Slight epistaxis. Pulse 84; temperature 101° F. Wants to leave the bed.

4th day. Patient still restless, slightly delirious; face intensely red and covered with a confluent eruption; discrete eruption on limbs; nothing on body (anterior part). Pulse 108; temperature 101°. Urine and stools normal. Pustules are quite numerous on the back, as also in the pharynx and on the palate; a few pimples are seen on the tongue.

6th day. Where the eruption exists the parts all swollen and red, though temperature proves normal.

7th day. The confluence of the pustules on the chin has produced blisters; hands swollen, red and pimpled. Pulse 120; temperature 98°.

9th day. Dessication around mouth, on forehead and nose. Appetite and sleep good. Pulse 100; temperature 99°. Convalescence.

Case VIII.—Weber, age 8 years. The eruption in this case appeared only on the *sixth day* (a very rare occurrence), the patient having had no fever for two days previous. The fever began in the usual way, on the 3d day of February; pulse 120; temperature 105° F. No vomiting, except to a small extent after a second dose of quinine. 2d day—pulse 120; temperature

106.5°; vomiting; yellowish fluid with *heavy and dark magma* at the bottom and a few *black lumps* on the surface; no blood from the nose at any time. Quinine is again administered.

3d day. Pulse 84; temperature 100°. No eruption.

4th and 5th days. Normal condition.

6th day. Child is allowed to leave the room; in the evening of this *sixth day*, some *very few pimples* are seen *on the face.*

Case IX.—John Chine, age 9 years. This it a very severe and abnormal case. Feb. 4th; diarrhœa, with colic; on the 5th enters the infirmary with colics and headache; face and arms intensely red; pulse 144; temperature 108°. No signs of vaccine being discovered, I revaccinate the patient.

2d day. Diarrhœa and colics have subsided; patient quiet, but red and hot; pulse 144; temperature 107°.

3d day. Night previous patient was very cold, and grew delirious. Pulse 144; temperature 100°. Quinine should now be given, but vomiting had set in (clear water, with a small amount of grayish mucus at the bottom of the basin). Quinine is administered per rectum.

4th day. Pustules begin to show on forehead; vaccine absorbed. The patient does not improve as might be expected from the eruption making its appearance; on the contrary, he shows signs of fluid in the ventricles, such as the "*cris encephaliques*," pupils dilated and immovable, etc.; hands cold and damp; pulse 120, miserable; temperature hardly 97° F.

The case seemed hopeless; however, stimulants and tonics (brandy, and especially wine of cinchona) were freely used; so also were the skin stimulants. Pulse 120. 5th and 6th days, temperature 100°, during which time a discrete eruption made its appearance. About the middle of February dessication had fully established itself, but patient was still weak and sleepy; he was extremely thin; in a condition of marasmus with gangrene of coverings of sacrum; stomach in good state. Good diet and cod-liver oil did handsomely; recovery was complete, but very slow.

Case XVIII.—William Chine, elder brother, 12 years, fell sick in the latter part of February. *Hemorrhage from mouth*, and *fever* during *suppurative stage* (which occurrence has been rare during our epidemic at the Asylum), were the remarkable fea-

tures in this case. Fever had set in the day previous to his entry into the infirmary on the 23d day of February, when a few pustules could be seen on face and neck. Pulse 120; temperature 103°.

3d day. Eruption progresses, fever subsiding. Pulse 120; temperature 100°.

5th day. A few pustules on lids; confluent eruption on face; disseminated pustules on trunk and limbs. Pulse 130; temperature 103°.

7th day. Pulse 140 and 150; temperature 105° F. Teeth and lips fuliginous; excessive perspiration (weather hot and patient covered with blankets).

9th day. Full suppuration. Temperature 105° F. Following days, lips swollen, black and offensive, from the presence of hemorrhagic scabs. Convalescence long and laborious.

Case XXI.—James Trall, 9 years. February 18th (first day), headache and *rachialgia.* Pulse 120; temperature 107°. Has taken ten grains sulph. quinine No signs of vaccine, in spite of repeated previous revaccinations performed in the Asylum.

3d day. Confluent eruption on face; a few pustules on limbs. Pulse 104; temperature 104° F.

4th day. Face intensely red and largely swollen, covered with confluent pustules. Pulse 96; temperature 102°. Pharynx spotted with pimples. Patient coughs.

6th day. Pulse 120; temperature 103°. Suppuration on face.

7th day. Pulse 120; temperature 103°. No signs of vaccine.

9th day. Dessication begins to show on the face, which is largely swollen. Pulse 145; temperature 106. Eruption rather confluent on limbs, discrete on rest of body; few pustules in throat; patient coughs much, with no signs of croup (in voice or coughing). Tongue clear, shows no pustules.

11th day. Pulse 130; temperature 105° F. Face covered with a brown crust; the body has presented hardly any change.

13th day. Dessication progresses; convalescence.

Case XXIV.—John Molton, 14 years. March 18, complains of pain in the head and in epigastrium; fever. Pulse 120; temperature 103°. No pain in back. Slight mark of vaccine on left arm; has been vaccinated to no effect.

3d day. Pulse 90; temperature 103°. A few pustules on head and arms.

4th day. Pulse 72; temperature 100°. Eruption in statu quo.

6th day. Pulse 72; temperature 100° F.

7th day. Pulse 84; temperature 99° F.

9th day. Dessication without actual suppuration. Pulse 108; temperature 104° (patient eating heartily).

11th day. Pulse 96; temperature 100. Convalescence.

After this 24th case, several other cases of varioloid occurred which were more and more simple; it was the end of our small epidemic in the Asylum, which had lasted three months, showing complications only in February, the middle month. From the latter part of February hemorrhages were of rare occurrence.

Remarks.

Three hundred children constitute the average population of the "Orphan Boys' Asylum of New Orleans;" sixty in these three hundred entered the infimary from the latter part of January, 1873, to the latter part of March, 1874; most of all these have suffered from a fever with *varioloïc eruption*, and the majority have had *hemorrhages* with very peculiar *mucous vomiting*, from the latter part of January to the end of February. Twelve deaths have occurred, all during the period of hemorrhages and mucous vomiting, from the latter part of January to the latter part of February.

The occurrence of hemorrhages and mucous discharges *complicating* our epidemic, which was rather one of *varioloïd*, such accidents making an appearance in the middle of the progress of the epidemic, proving benign before they subsided, and also after they had subsided, constitutes in my opinion an interesting feature for discussion. When the epidemic first broke out in the Asylum, months had already elapsed during which true variola had been raging all around the institution, especially among the colored people, who are very numerous in that part of the city. In the latter part of December, 1873, a few cases of varioloid occurred in the asylum, most of the children having been repeatedly revaccinated. Among the negroes, who died in large numbers outside of the asylum, no hemorrhage or other unusual symptom has occurred to my knowledge.

What then is the meaning of the *hemorrhagic* and *mucous* acci-

dents complicating the varioloid in the asylum from the latter part of January to end of February, 1874? Nothing occurred that should suggest the idea of a benign form of *varioloid* changing suddenly, for a few weeks only, to a *hemorrhagic* or *"black variola,"* always proving a fatal disease; not a single case occurred in which it would be justifiable to say that the pustules contained blood and pus at one time; we have not even seen petechiæ or echymoses in any of the cases.

Epistaxis, and in several cases *black lumps* and thousands of "flies wings," in the fluids thrown up from the stomach, even *without any epistaxis having previously occurred*, true black vomit with abundant mucous fluid; altered gastro-intestinal mucus, heavy and thick, hidden at the bottom of the fluids from stomach, and only discovered after emptying off the more watery fluids, then appearing as a *magma* of grayish, chocolate, or black matter evidently consisting of altered blood combined with mucus; such are the signs observed, and to which we wish to call attention.

Is there any ground to the idea that our epidemic has taken a *mucous form* with *hemorrhagic tendencies* in the middle of its course, but neither before nor after that period? Such an opinion might be entertained; it is not ours.

Such events as have occurred in the Orphans' Asylum for several weeks in January and February, 1874, regardless of the coexisting variola or varioloid, I have observed several times in twenty years: I mean, very sudden deaths, with mucous and bloody discharges from stomach; at the autopsy, the liver and spleen were found enlarged to a great extent, stomach and duodenum lined with a thick mucous coating, which was often black, the mucous membrane showing congestion, sometimes bleeding, the muciparous glands being enlarged so as to appear like small pimples.

In the present circumstances the same things occurred with only this difference, that such occurrences took place during an epidemic of variola.

My impression is that in the earlier part of 1874, we had to deal in the asylum with an epidemic of *variola* modified by vaccine and *complicated*. for a few weeks, with a *hemorrhagic mucous gastro-intestinal* trouble. Now what is the nature of this last trouble? An answer to this question would take too much space in these remarks; let me hope that I shall take another oppor-

tunity for it in the NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

For the present let me acknowledge that the other opinion is very reasonable: the *mucous form of true variola* may truly be said to exist, and is proven by post mortem examinations. Some physicians have discovered, in cases of variola, a number of *pimples* appearing as an *eruption* more especially in the *duodenum*, and resembling variolar papules. Such cases led to the belief that the special eruption of variola had extended into the pharynx, and also into the bowel. Some other investigators, who were more of anatomists, made out that these so-called *variolar papules* were the *enlarged mucous glands* of the duodenum. It follows that the existence of a *mucous form* of variola is proven by *anatomical* characters. This form is not likely to be rare in New Orleans. Besides, we have to admit that various hemorrhages, more especially epistaxis, are not of unfrequent ocurrence in cases of variola.

Be that as it may, I think I have good reasons to believe that the unusual accidents which have occurred during our small epidemic of variola in the Asylum constitute something more than a simple *mucus* and *hemorrhagic* form of the disease, existing for a short space of time.

The above mentioned mucous, hemorrhagic, gastro-intestinal trouble, which I have considered as an intercurrent disease and an addition to our variola in the Asylum in 1874, during a few weeks, is no new thing to our profession: it has even been partially described in writings that are now somewhat ancient. Similar cases may be found in one of my pamphlets (1864), such having been observed in the same asylum. The following case is from 1860: "On the 7th day of February, 1860, I was requested to call at the Asylum to see a young boy six years old, who seemed to be in good health the day previous. I saw the patient a few hours after he had been taken ill. In the latter part of the night he had been *vomiting*, had complained of *colics*, and had *glairy discharges from bowels*. After throwing up what food he had previously taken, he began vomiting a kind of grayish, heavy, and muco-purulent matter, looking very much like what we see in cases of tuberculosis of lungs when cavities exist. Face discolored, lips blue, eyes extravasated, convulsions of muscles of face. In spite of all our efforts the patient died two hours later in convulsion fits. Autopsy: Liver enlarged to such

an extent as to reach the umbilicus and spleen; the latter is of a chocolate color, and measures 13 centimetres by 9; stomach filled with mucous secretions resembling bronchial catarrh; grand cul-de-sac congested; pylorus discolored and blocked up by mucous secretions from duodenum; no visible follicles in stomach; in duodenum *a confluent eruption of follicles* exists, similar to an eruption of variola; these are covered with a *thick coating of grayish and heavy mucus, looking like cream*, the specific gravity of which is greater than that of water" (Hemorrhagic Paludal Fever, page 38. 1864). In the same pamphlet I have reported another case, of which the following is a brief resumé: "August 27th, 1863, a boy seven years old, in the Asylum, goes to bed after supper. At three in the morning the nurse is awakened by the child's efforts at vomiting; he is very soon taken with *cramps* and *colics*, etc.; at half past six he had expired in convulsions. The liver, very much enlarged, of a dark vinous color, reaches the left hypochondrium; on its outer surface a few spots can be seen which look like *discoloration of liver* to be met with in cases of yellow fever. A brown fluid with black lumps fills the cavity of stomach; the mucous folds are coated with a *thick* grayish black *mucus*, heavier than water; mucous membrane shows *large reddish plates or patches* in several places; no follicles are visible; these begin to show in the pylorus. In the duodenum thousands of follicles are to be seen, appearing as hard, round corpuscles, resembling an eruption of variola. Beyond the duodenum the enlarged Bruner's follicles were still quite numerous, growing, however, scarce as we went towards the ileum. Two feet above the ileo-cœal valve, before reaching it, *elliptical Peyer's patches* were visible; mucus secretion was there quite abundant, thick and dark: Spleen larger than both of my hands can cover, and of a slate color."

How and where shall we locate such cases?

Assuredly I have all due respect for our text-books; but everything is not found in them. Not long ago, the *hematuric form* of malarial poisoning was unknown to science; the hematemesic form was with difficulty acknowledged. I would like to undertake the study of the *mucous form* of "paludal fever or telluric fever" in Louisiana; but I have no spare time, and will only make a few remarks. I would like to see the term *mucous fever* used only to designate the *forms* of those "*fevers*" whose *localisation* is more particularly in the *follicles* of the gastro-intestinal

mucous membrane, the term *catarrhal fever* being then used to designate such fevers as seem to carry fluxion and congestion more especially to the mucous membrane of the respiratory passages. Now, in intra-tropical countries, the *bilious form* is the most frequent in "fevers," whilst in Louisiana (Lower Louisiana) we have more especially the *mucous* and *catarrhal* forms.

The symptoms differ greatly in the *mucous form* of "fevers," according to the various *locations* of the *congestion* in the different varieties of cases, which fact renders the diagnosis very difficult. The difficulties are still more increased when the various *forms* and *varieties* rest on a *paludal* or *malarial* basis, which constitutes them essentially and forces the use of *the specific* (cinchona). Physicians of the North who have not to deal with malarial influences might consider that the practice of their confrères in hot malarial climates is really extravagant; such an impression is quite natural. We all recollect the scientific contest between Laroche, of Philadelphia, and Merrill, of Natchez, "On Pneumonia and Malaria." (1854.)

I contend that Merrill has actually denied the existence of pneumonia or of broncho-pneumonia, especially when connected with "malarial fever;" I understand rather that he has sometimes observed in Natchez cases of "pneumonic form of malarial fever," which is quite a different thing, and could have been considered, nay, has actually been looked upon in Philadelphia as a monstrosity.

So also in New Orleans such physicians are to be found, who will readily acknowledge that such diseases as *cholera*, *dysentery*, *typhus*, &c., really exist; they have seen cases indeed; they have seen them with no malarial complication; but they also know, or at least they are satisfied, that "malarial fever" can not only be combined with these various diseases, but that it shows sometimes a *choleraic form*, *a dysenteric form*, *a typhic form*, and even *a "typhoïdic" form*. We have heard a great deal about cholera, dysentery, and especially "typhoïd fever," in this city for the last few months; are we satisfied that we have had to deal with genuine cases in all instances?

Following in the footsteps of our masters is assuredly a very modest and especially a very easy task. However, when it happens that our masters have had no experience in the country where we are doing practice, when such country proves to be very different from that in which we have gone through

our medical studies, are we not necessarily led to search into things on our own hook? The "*magister dixit*" simply deserves our respect, but calls for our own investigation.

The above digression has been called forth by what is going on here for the present; let us now go back to our small epidemic in the Asylum, which is especially interesting on account of its *complication*. Notwithstanding that complication, I have been able to fully test a *supposition* which was very likely to prove true, viz.: the *march of the pulse* in variola follows that of the temperature, as in all fevers in general. In our article in the NEW ORLEANS MEDICAL AND SURGICAL JOURNAL, No. of September, 1873, while comparing the course of the temperature in yellow fever with that of variola, I wrote as follows: "In the incipient stage of variola we have the same *ascending direction* of the line of temperature even for four days; the probability is that in the meantime the direction of the line of the pulse is *not descending*. It would be more satisfactory if we could *affirm* what we suggest."

The following tableaux from our epidemic in the Asylum are conclusive. I take 11 in a lot of 25 or 30, the remaining others not being as complete; these eleven, though incomplete, are yet quite sufficient, and will very well answer our purpose.

The tableaux also show, 1st, that in general the pulse has followed the temperature; the number of pulsations has never shown a decrease at the start; 2d, that the febrile course of variola, even when modified by vaccine, is hardly influenced by intercurrent morbid accidents, which constitutes one more point in the resemblance of variola with yellow fever. Such fevers hardly suffer themselves to be disturbed in their natural course.

In our struggle against the double morbid factors, the double poison which we had to deal with in the circumstances present, we were fortunately assisted by two powerful special modifiers of the organism, not to say specific modifiers, i. e., vaccine and quinine. Were it not for anterior vaccinations and *revaccinations*, which were practised as soon as feasible, and for quinine largely and skilfully administered with promptitude by the sisters in the institution, it is likely that our losses would have been much greater, and then our Orphans' Asylum would have suffered the ravages of a *small plague*, similar to the *great Plague of Athens*, which latter disease, according to the teaching of the learned Daremberg, was "variola complicated with typhus."

Tableaux.

DAYS OF DISEASE.

Number of Cases.	Temperature and Pulse.	1	2	3	4	5	6	7	8	9	10	Remarks and Results.
1....	Temp....	.....	104°		.....	.....	.....	.....	.....	.....	.....	Death the 3d day, without any eruption.
	Pulse....	.....	120	105° 120	.....	.....	.....	.....	.....	.....	.....	
2....	Temp.....	.....	102°	104 5	104°	102° 5'	103°	.....	104° 5'	104° 3'	104° 3'	Eruption the 3d day. Epistaxis. Death the 12th day. Hemorrhages.
	Pulse....	.... ..	120	120	100	120	120	.....	120	140	150	
3....	Temp....	106°	107	103 5	102	102	.....	.....	.. ....	.....	.....	Eruption 3d day. Patient recovered.
	Pulse ...	108	115	100	96	96	.....	.....	.....	.....	.....	
4....	Temp ..	103 5	106	104	100	100	.....	.....	.....	.....	.....	Eruption the 4th day. Recovered.
	Pulse ...	120	130	120	96	96	.....	.....	.....	... ...	.....	
5....	Temp....	106	105	101	101	.....	.....	.. ....	.....	.....	.....	Eruption 2d day. Epistaxis 3d day. Recovered.
	Pulse ...	120	104	84	100	.....	.....	.....	.....	.....	.....	
8....	Temp....	105	106 5	100	.....	.....	.....	.....	.. ....	.....	.	Eruption only the 6th day. Recovered.
	Pulse ..	120	120	84	.....	....	.....	.....	.....	.....	..	
9....	Temp ...	108	107	100	97	.....	.....	....	.....	.....	.....	First day vaccination. Eruption 4th day. No result from vaccination. Recovered.
	Pulse ...	144	144	144	120	.....	.....	.....	.....	.....	.....	
10....	Temp ...	108	107	100	97	98	101	102	.....	.....	.....	Vaccination without result. Delirium. Eruption 4th day. Recovered.
	Pulse.....	144	144	144	120	120	120	120	.....	.. ..	.....	
18....	Temp....	.....	103	100	.....	103	.....	105	.....	.....	.....	Eruption 2d day. Hemorrhage from gums. Recovered.
	Pulse....	.....	120	120	.....	170	.....	150	.....	.....	.....	
21.....	Temp....	107	.....	104	102	.....	103	.....	.....	107	105	Revaccination with no result. Eruption 3d day. Suppuration, sixth day, begins.
	Pulse ...	120	.. ....	104	96	.....	120	.....	....	144	130	
24....	Temp....	107	.....	103	100	.....	100	99	.....	.....	.....	Eruption 3d day. Recovered.
	Pulse....	120	.....	90	72	....	72	82	.....	... .	.....	

ARTICLE II. *Contributions to the Natural History of Yellow Fever; Constitution and Changes of the Blood in Yellow Fever.* By Joseph Jones, M.D., Professor of Chemistry and Clinical Medicine, Medical Department, University of Louisiana, New Orleans.

That the blood undergoes profound changes during the period of febrile excitement of yellow fever is manifest even to the casual observer, in the impeded capillary circulation, purplish, jaundiced and dusky hue of the surface, livid blotches, passive hæmorrhages from slight abrasions, leech bites, blistered surfaces, and from the ears, eyes, mouth, gums, and gastro-intestinal mucous membrane, which in some cases are characteristic of the succeeding period of calm or exhaustion.

Although after the subsidence of the fever, at the end of from two to five days, the skin becomes cool and pleasant, the tongue shows a disposition to clean, and the tips and edges are less red, the thirst abates, and appetite for food returns, and the anxiety and morbid fear of death, which may have been great, subside, and both patient and bystander may regard convalescence as established; nevertheless, a careful examination, will show that the circulating fluid has been altered during the preceding stage of febrile excitement: the eye loses its glistening appearance. and assumes a condition of chronic vascularity, of a dull orange red; the flushed countenance gives way to a bloated appearance and dusky, dirty complexion; the sclerotic of the eye is jaundiced, the forehead presents a dusky appearance, which extends also over the neck and chest, the langour of the capillary circulation is indicated in the purple and lobster red condition of the skin, and by the pale marks left by pressure over the forehead, cheek, abdomen, or surface of the extremities; the matters vomited, which at first may be tinged with bile, change to a clear, acid, mucous fluid, and then become discolored by small dark specks and flocculi of blood. The further changes of the blood are indicated by an increase in the purplish or yellow suffusion of the surface, and by such a loss of vitality and of the fibrinous elements as manifests itself in the raw claret-colored surface of blisters, in epistaxis, ecchymosis, bloody oozings from the mouth, ears or anus, excoriation of the scrotum, the copious ejection of dark altered blood from the stomach, with little or no

apparent effort, copious stools of black altered blood, and by the foul fœtid alkaline breath containing ammonia.

It appears to be an error to treat of the changes of the blood as confined to the latter stages of yellow fever, thus regarding the disease as manifesting only two grand stages, viz: that of reaction, irritation and fever, and that of unhealthy subsidence or contamination, characterized most prominently by exhaustion of the nervous system, slow pulse and passive hæmorrhages.

The changes of the blood appear to be continuous, from the time of the introduction of the poison to the fatal termination; these changes being increased, and their character being modified as the disease advances, not only by the direct action upon the constituents of the blood by the poison, but also by the addition of certain noxious substances, as bile, urea, carbonate of ammonia, sulphates and phosphates and extractive matters.

Certain constituents of the blood, as the albumen and fibrin, are not only altered physically and chemically in the early stages of yellow fever, but as the disease advances, from the causes just specified, certain excrementitious matters, which in a state of health are continually eliminated, accumulate in the circulating fluid, and by their direct action upon the elements of the blood, and upon the nervous system, and by their disturbing action upon the processes of digestion and nutrition still further alter the physical chemical and vital properties of this fluid.

As far as my observations extend, the alterations of the blood in yellow fever consist chiefly in—

1st. Such an alteration of the chemical and physical properties of the fibrin and albumen as leads to the transudation of the latter through the excreting structures of the kidney.

2d. Various degrees of alteration and diminution of the fibrinous element.

In some cases there is an almost entire disappearance of the fibrinous element. This disappearance of the fibrinous element appears to be due not so much to the action of ammonia, which is so often present in abnormal amount in the blood of yellow fever, but to the direct action upon this element of the fibrile poison. From this alteration in the amount and character of the fibrinous element it results that the blood coagulates imperfectly in most cases, and the clot is voluminous and soft. The amount of serum formed is small, and upon standing, the clot frequently dissolves, leaving a thick, non-coagulable gru-

mous blood. The blood taken from the cavities of the heart and large blood vessels after death is frequently black and fluid; and if, as is sometimes the case, fibrinous concretions are formed in the cavities of the heart, they are small, soft, and of a bright golden yellow color, and much smaller in size and less firm than is usual in diseases attendant near the fatal issue, with similar retardation of the circulation in malarial fever. In the latter disease the formation of firm light colored, blood-clots is, as I have shown, by a large number of observations, not only frequent, but also a cause of death in certain cases of pernicious malarial fever.

3d. Whilst the colored blood corpuscles are very slightly diminished in yellow fever, they present, under the microscope, certain peculiar appearances, which appear to be referrable to the action of certain extraneous matters in the blood.

4th. Increase of the extractive matters of the blood.

5th. Increase of the fatty matters.

6th. Accumulation of bile in the blood, in consequence of the profound lesions of the liver induced by the febrile poison, and in consequence of the failure of the excretory function of the kidneys. Many of the changes of the blood, as well as certain cerebral symptoms, may be dependent upon the presence and action of the biliary constituents. Even the nausea and vomiting, as well as the depression of the pulse, and the nervous agitation, delirium and coma, may to a certain extent be referred to the same cause.

7th. Accumulation of the urinary constituents, and especially of the urea and phosphoric acid, sulphuric acid, chloride of sodium and carbonate of ammonia, in the blood, consequent upon the profound lesions induced by the febrile poison and its products upon the kidneys.

8th. Rapid dissolution of the colored blood corpuscles, after the blood is abstracted from the body, either during life or after death. The rapid alteration of the investing membrane, of the colored blood corpuscles, in the blood of yellow fever, after the abstraction of the blood from the vessels, appears to be immediately related to, if not dependent upon, the physical and chemical action of the biliary and urinary constituents retained in the blood.

9th. Rapid putrefaction of the blood of yellow fever, after its

abstraction from the living body, or from the large vessels after death.

These conclusions have been established by careful and laborious observations at the bedside and in the laboratory, the nature and method of which we will illustrate in this paper, by the following observations.

The following analysis of the blood, in a severe case of yellow fever which terminated fatally, on the 6th day, with black vomit and urinary suppression, presents a correct view of its composition on the 4th and 5th days, in severe cases of this disease.

The fibrin in 1000 parts of blood extracted from the arm of this patient being 0.271, or not more than one-tenth of that of healthy blood. This diminution of the fibrinous element was attended by passive hemorrhages, black vomit, &c.

Specific gravity of blood, 1055.6; of serum, - -	1027
Water in 1000 parts of blood - - - - -	802.12
" " " " serum - - - -	922.90
Solid matter in 1000 parts of blood - - - -	197.88
" " " " " serum - - - -	77.10
" " " serum of 1000 parts of blood - -	67.03
Saline matters in 1000 parts of blood - - - -	8 48
" " " blood corpuscles of 1000 parts of blood	1.78
" " " 1000 parts of serum . - -	7.71
" " " serum of 1000 parts of blood - -	6.69

1000 *parts of blood contained:*

- Water 802 12
- Solid Residue, 197.88
  - Organic Matters, 189.40
    - Dried Blood Corpuscles, 130.57
      - Organic Matters 128.79
      - Saline Matters 1.78
  - Fixed Saline Constituents. 8.48
    - Solid Matters in Serum. 67.03
      - Albumen 53.40
        - Organic Matters 51.59
        - Saline Masters.. 1.81
      - Extractive Matters..13.72
        - Urea, Bile, Carbonate Ammonia, &c... ... 8.84
        - Inorganic Matt's 4.88
  - Fibrin 0.271

1000 *parts of blood contains:*

- Moist Blood Corpuscles..... 522,28
  - Water of Moist Blood Corpuscles..... 392 71
  - Organic Matter..... 128.79
  - Saline Matter..... 1.78
- Liquor Sanguini..... 477.72
  - Albumen..... 53.40
    - Organic Matters..... 51.59
    - Saline Matters..... 1.81
  - Extractive Matters, 13.72
    - Urea, Bile, Fat, and Carbonate of Ammonia.... 8.84
    - Inorganic Matters..... 4.88
  - Fibrin..... 0.271

Urea and bile acids were detected, after death, in considerable amounts in the brain, liver, heart and spleen of this patient; the black vomit also, ejected during life and remaining in the stomach after death, contained urea.

CASE ILLUSTRATING THE CONSTITUTION OF THE BLOOD IN YELLOW FEVER.

Charles Cellenberg, native of Iowa, attacked with yellow fever July 30th, 1871, died in Charity Hospital, after the supervention of black vomit, August 4th.

The blood from the cavities of the heart, as well as the black vomit, and the structures of the heart, liver and kidneys, were subjected to careful microscopical examination a few hours after death.

The blood from the heart presented the characteristic golden-colored areola when dropped upon bibulous paper, and under the microscope the colored blood corpuscles presented a crenated and stellate appearance. No animalculæ, or sporules, or vegetable organisms were observed, even under the highest powers of the microscope. Numerous vibrios were observed in the black vomit taken from the stomach after death.

The muscular structures of the heart presented a pale, yellowish brown flabby appearance, and contained much oil in the form of globules

The liver presented the characteristic yellow color, and contained much oil, deposited in the form of oil globules, in and around the secretory cells of this organ.

The kidneys presented a yellowish brown color, and the excretory tubes were filled with yellow granular matter.

CASE ILLUSTRATING THE CONSTITUTION OF THE BLOOD IN YELLOW FEVER.

In the case of Newton Simpson, who was attacked on the 11th of October, 1871, and died with urinary suppression and profuse black vomit, on the 9th day of the disease, the textures of the body divided by the knife presented the bloody appearance usual in such cases of yellow fever: UREA *was detected in the blood, brain and liver.* The heart contained in all its cavities

dark fluid blood, which gave an acid reaction at the time of the post mortem examination, 10 hours after death.

Examination of Fluid Blood, from Cavities of Heart, Three Hours After Death.

The blood after its abstraction coagulated, forming a loose clot; the fibrin, however, dissolved, and no serum was separated.

Under the microscope, the blood corpuscles presented no special alteration. When spread upon the glass slide, and also during the coagulation of the blood, they rapidly adhered together, forming rolleaux, as in inflammation, and in the blood of the horse. The running together of the colored corpuscles, was as rapid and as complete as in cases of well marked inflammation.

It was impossible to determine the weight of the fibrin, or to collect pure serum, free from colored blood corpuscles and the coloring matter of the red corpuscles, as the fibrin rapidly dissolved after its partial separation.

The fibrin was in small amount, and apparently not one-thirtieth of the usual proportion,

Specific gravity of blood from cavities of heart, 1047.

1000 *parts of blood from the cavities of the heart contained,*

Water		821.57
Solid Residue 178.43	Organic matters, colored	
	Blood corpuscles, albumen	
	Urea, extractive, and fatty matters	170.59
	Fibrin	Trace.
	Fixed saline constituents	7.84

Examination of Fluid Blood from Vena Cava, Three Hours After Death.

Specific gravity of blood from vena cava, 1062. Blood fluid and warm when drawn. Coagulated forming a very loose coagulum, which did not enclose the whole amount of colored corpuscles.

This coagulum gradually dissolved, and it was impossible to determine the amount of fibrine, or to obtain clear serum, free from colored corpuscles.

Reaction of blood slightly alkaline.

1000 *parts of blood from the vena cava contained—*

Water		775.00
Solid Residue 225.00	Organic matter, colored	
	Blood corpuscles, albumen	
	Urea, ammonia, extractive, and fatty matters	217.30
	Fibrin	Trace
	Saline constituents	7.69

It will be observed that the blood in the vena cava contained more solid matter than that of the cavities of the heart. It would appear that the returning current of blood from the capillaries and venous system, owing to coagulation and congestion, contained more solid matter than the blood in the cavities of the heart.

No vegetable or animal forms were discovered in the blood, even with the highest powers of the microscope.

Yellow fever blood exposed upon my table, in capsules, beakers and bottles, for days and weeks, and examined microscopically from time to time, developed no special forms which could be referred exclusively to yellow fever. Only the simpler forms of animal and vegetable life made their appearance, which might be developed in any other similarly constituted albuminous fluid.

In a fatal case of yellow fever occurring in one of the wards of the Charity Hospital, during the month of October, 1871, in a stout young man, aged 25, who, from the result of the post mortem examination, had evidently suffered with malarial fever, incipient cirrhosis of the liver, and granular degeneration of the kidney before the supervention of yellow fever: death occurred after an illness of ten days: the disease was complicated during its progress by the supervention of pericarditis, pleuritis and pneumonia.

The urinary secretion was abundant, until near the close of life, and the urine contained blood corpuscles and the coloring matters of the blood. The cavities of the heart were filled with loosely coagulated blood. Portions of the lungs were infiltrated with blood, as if hæmorrhage had taken place in and around the air cells and bronchial tubes.

Louis, in his Anatomical, Pathological, and Therapeutical Researches, in the yellow fever of Gibraltar, of 1828, has shown that the exhalation of blood into the pulmonary tissue was frequent; while the inflammation of that tissue was rare in subjects who had died of yellow fever.

Louis found the lungs entirely natural in three subjects only.

Black spots and masses of the same color, more or less impermeable to the air, were found in nine subjects. Usually of a brown black, rarely of a crimson hue. They were more or less concentrated, and occupied a variable space at the exterior or in the interior of the lung, and in some cases they were found only in the lower lobe. The black or blackish masses which existed in the lungs of six individuals contained no air, had no granular aspect, as in hepatized pneumonia, and presented no traces of organization. Usually they could be easily broken down, in some cases yielding by pressure. The blood, of which they were almost entirely composed, and the parenchyma remained apparently of its natural consistence.

In this case, complicated by the supervention of inflammatory disease, the fibrin of the blood has not diminished to the extent usual in yellow fever.

The pre-existing effects of malarial fever were evident in the masses of black pigmentary matter derived from the hæmatin of the blood scattered through the lobuli of the liver, but chiefly accumulated in the perepheral portions, in and around the portal vein, and portal capillaries.

In uncomplicated yellow fever these altered masses of hæmatin are uniformly absent from the structures of the liver, their place being supplied by oil globules; and it would appear that in this disease the albuminoid constituent of the blood and tissues is chiefly altered, the altered albumen appearing in abundance in the urine, whilst in malarial fever the colored blood corpuscles are chiefly attacked, the coloring matter being deposited in various organs, whilst in the acute stages of uncomplicated malarial fever albumen rarely appears in the urine.

The results of my investigations on the occurrence of pigment in the blood, and in certain organs, as the liver, spleen and kidneys, and in the capillaries of the brain, in persistent intermittent and remittent malarial fever, and in chronic malarial poisoning, correspond with those of Meckel, Virchow, Heschl and Platner, and more especially with the researches of Frerichs.

Not only is the spleen enlarged and the color of the liver altered to a steel gray, blackish slate and chocolate and bronze color in individuals who die from the affects of marsh poison, but the change of color may extend to other organs, as the kindeys and brain.

Careful microscopical examinations of sections of the various organs have shown that the changes of color are clearly referable to the formation and accumulation of pigment matter resulting from destruction and abnormal changes of the colored blood corpuscles.

Whilst from the anatomical structure of the spleen, as well as from the great congestion which it undergoes during the cold stage of malarial fever, it is without doubt true that a large portion of the pigment is formed in the sinuses of the spleen, and passes from this organ into the portal vein, and that part of it remains impacted in the capillaries of the liver, while the rest passes through these capillaries and is carried into the general circulation; at the same time it must be admitted that under the action of the malarial poison, the transformation of the red matter of the blood into black pigments may take place everywhere throughout the vascular system, and also external to it, but more especially in the liver and kidneys.

If fine sections of the liver be made, with Valentine's knife, and examined under comparatively low magnifying powers ($\frac{1}{2}$ to 1 inch), as I have done in a large number of subjects, accumulations of pigment will be observed in the capillary network of the portal and hepatic veins, either uniformly distributed, or limited to certain regions; sometimes deposited chiefly in the interlobular veins, forming black margins to the lobuli of the liver, or more uniformly extending from the circumference of the lobules half-way to their centre, or penetrating as far as the commencement of the hepatic veins.

That portion of the pigment originating in the spleen, and passing through the capillaries of the liver enters the general circulation and may be deposited in various organs, as the brain, kidneys and lungs, inducing marked symptoms, varying in character and degree with the organ, when capillary circulation may be especially retarded by the arrest of the pigment particles.

The larger particles remain impacted in the capillaries of the portal vein and obstruct the circulation of the blood, through these vessels, causing various derangements in the secretion of the liver, and in some cases extensive capillary stagnation of the blood in the roots of the portal vein, attended with exhausting hemorrhages from the gastro-intestinal mucous membrane, of an intermittent character, profuse diarrhœa, vomiting severe effusions into the peritoneal sac, and finally from the

destruction of many of the smaller branches of the portal vein, chronic atrophy of the liver, with its distressing and fatal consequences.

Even by simple inspection of the brain we may form some idea not only as to the cause of the alteration from the color of health, but also of the amount of pigment which has passed unarrested through the vessels of the liver and lungs. The mechanical interruption to the circulation of the brain thus induced not only gives rise to a peculiar train of symptoms, but may also cause the rupture of the small vessels, and the formation of numerous capillary apoplexies, as has been shown so clearly by Meckel, Platner and Frerichs.

Frerichs has shown, and I have upon many occasions confirmed the accuracy of his investigations, that the large pigment granules and cells which enter the kidneys along with the arterial blood not unfrequently become impacted in the capillary coils of the malphigian bodies, and by altering the pressure of the blood, give rise to derangements in the secretion of urine, which exercise a powerful influence over the further progress of the disease. Albumenuria, fibrinous casts, and pigment masses and granules, and blood corpuscles characteristic of the urine in malarial hæmaturia, should to a large extent be referred to the effects of the pigment particles arrested in the capillaries of the kidneys. Hæmorrhage, congestion, exfoliation of the excretory cells of the tubuli-uriniferi, fibrinous exudation into the excretory tubes, and even complete suppression of the urinary secretion, may occur in such cases. Destruction of the colored blood corpuscles may in like manner occur directly in the capillaries of the kidneys in malarial fever; and the pigments thus formed may give rise to a similar train of symptoms.

As far as my observations have extended no such changes take place in the colored blood corpuscles and various organs in yellow fever. *The colored blood corpuscles are not destroyed in the various organs, neither is there any special diminution of their important elements, and all the organs and tissues are free from the presence of pigmentary granules in uncomplicated yellow fever.*

If, however, as in the preceding case, the yellow fever occurs in one who has previously suffered with malarial fever, or if the disease supervenes upon yellow fever, then the pigment matter may be present to a greater or less extent, especially in the liver and spleen.

The result obtained by actual chemical analysis, and by careful microscopic research, viz.: that the colored blood corpuscles are not specially destroyed in yellow fever, as is the case in malarial fever, is still further sustained by the well established fact that in general convalescence is rapid, complete, and attended with a speedy restoration to health. Such a rapid and complete restoration would be impossible if the colored blood corpuscles were destroyed to any great extent in yellow fever.

CASE ILLUSTRATING THE CONSTITUTION OF THE BLOOD IN YELLOW FEVER.

In the case of Jacob Siegarest (age 26, native of Germany), who was attacked on the 12th of October, 1871, and who suffered with well marked yellow fever, characterized by suppression of urine and alkaline black vomit, and who died on the 7th day of the disease, urea and bile were found in the blood and in all the organs, brain, liver, spleen, and heart.

At the autopsy, performed three hours after death, both cavities of the heart contained warm fluid blood, which gave a slightly acid reaction to litmus blue paper.

Under the microscope many of the blood corpuscles presented a crenated wrinkled appearance; others were swollen, whilst others again presented the usual appearance.

I could, after careful examination with high powers, detect no animalculæ, or simple vegetable or animal forms, although the search was conducted with great care, with powers of various degrees, from 1-4 to 1-18th of an inch objectives, and the highest oculars.

The blood was fluid, with little or no fibrin, and no fibrinous clots.

The fibrin was in so small an amount, and in such a soft and unstable a condition, that it was impossible to determine the quantity.

Specific gravity of blood, 1046.

1000 *parts of blood from the cavities of the heart contained:*

Water	852.70
Solid residue	147.30
Fibrin	Traces.

From experiments which I have conducted, it appears that the specific gravity of the blood in the cavities of the heart, after

death, from yellow fever, is less than that contained in the large blood vessels. This appears to be due to the fact that the heart continues to circulate the blood, or to force it through its cavities, during the last moments of life, the red blood corpuscles are arrested in the capillaries, and only the more tenacious constituents of the blood are returned to its cavities, during the last moments of life.

In this case the spleen was somewhat enlarged, and its "*mud*" consisted of numerous colored corpuscles, and oil globules and granular masses; but no living animalculæ or vegetable forms were observed. The blood corpuscles of the spleen were not specially altered in their appearance, oil globules were abundant in the contents of the splenic cells.

The reaction of the blood from the liver was acid, and at the end of 24 hours numerous prismatic crystals, of the triple phosphates of lime, magnesia and ammonia formed in the fluid, oozing from the cut surface of the liver.

The blood of the liver contained numerous oil globules, but no specific animalculæ or vegetable organisms.

During the few last months of 1870, when 587 deaths from yellow fever were officially reported by the authorities of New Orleans, although the number was probably greater, as 445 deaths were during the same period (August, September, October, November and December) referred to the various forms of malarial fever, I instituted in like manner, careful chemical and microscopical examinations of the urine, blood, black vomit, and of the various organs in yellow fever, and the results were similar in all respects to these recorded at length in the preceding pages, and we shall, for the sake of brevity, simply allude to some of the more general results.

The type of the disease was severe, and I saw cases in which the defibrinated blood oozed from leech bites, from the ears, and from the gums and mouth; and some cases suffered with profuse hæmorrhage from the bowels before death. The bodies underwent putrefaction a short time after death, having emitted during life a most disagreeable, foul, yellow fever odor.

The black vomit contained uniformly colored blood corpuscles, variously altered; also mucus epithelium from the gastric mucous membrane; sometimes various matters, as medicine, food, and drink taken by the patient; oftimes numerous vibrios and plants resembling the torula and saracena were present, but I was

drink taken by the patient; ofttimes numerous vibrios and plants resembling the torula and saracena were present, but I was unable to detect any specific vegetable forms characteristic of yellow fever.

The urine contained albumen, and in the fatal cases was almost uniformly suppressed. In the worst cases the urine consisted only of a yellowish albuminoid fluid, discolored by bile, containing little or no urea, but loaded with excretory cells and granular fibroid casts of the tubuli uriniferi. The lightest colored urine was frequently characteristic of the severest cases.

As a general rule, the blood changed to the arterial hue upon exposure to the atmosphere, and the cut and exposed muscles presented a beautiful scarlet color.

In some cases, golden colored heart clots, of small size and slight consistency, composed of yellow laminated fibrin, were observed in the cavities of the heart, and in those cases which had suffered with black vomit. Blood drawn from the cavities of the heart, after death from yellow fever, and carefully examined under the microscope, presented swollen and stellate blood corpuscles. This stellate condition of the blood corpuscles appears to be very common in the blood of yellow fever, and even in that drawn from the veins during life. When a drop of yellow fever blood is allowed to fall upon white bibulous paper, the red corpuscles occupy the original area of the drop, and around spreads a golden circle, of the serum colored by bile. In every fatal case, occurring during the fall of 1870, which fell under my observation, the serum was of a bright golden color, from the presence of bile.

The nervous symptoms characteristic of this disease were referred, in great measure at least, to the retention of the biliary matters in the blood, and to the suppression of the function of the kidney, and the retention in the blood of the urinary constituents, and especially of the urea of the blood.

The chief causes of death in these cases of yellow fever appeared to be: the direct action of the fibrile poison upon the nervous system and blood, depressing and deranging the one, and rendering the other unfit for the proper nutrition of the tissues; the suppression of the functions of certain organs, as the kidneys and liver, and the retention in the blood of the excretions normally eliminated by the organs. Blood loaded with bile

and urea is evidently unfit for the maintenance of healthy nutrition and of the vital acts.

The hemorrhage from the gastric mucous membrane appeared to have been due to several causes, as the direct irritant effect of the poison upon the gastric mucous membrane, the destruction of the fibrin of the blood, the physical and chemical alterations of the albumen, and the irritant and dissolvent and "*excoriating*" effect of the carbonate of ammonia, resulting from the decomposition of the urea.

The black vomit in yellow fever, as shown by the observations upon numerous specimens obtained during life, and taken from the stomach after death, consists of altered blood corpuscles, the epithelial cells of the stomach, mucus corpuscles, various matters received into the stomach, as food, medicine, water, &c., serous exudations, acids of the gastric juice (acetic, hydrochloric and phosphoric), urea, carbonate of ammonia, and various forms of animal and vegetable life of simple organization.

After careful examinations of numerous specimens of black vomit, we failed to detect any forms which could be considered as characteristic of this fluid; which may not be developed in albuminous fluids exposed to the action of the atmosphere, at all times, whether yellow fever be present or absent.

The careful microscopic examination of the blood with the highest powers, was equally fruitless in disclosing any forms of animal or vegetable life which could be considered as active in the production of yellow fever, or as invariably accompanying its manifestations.

We have established by the preceding observations—

1st. The blood in yellow fever often if not always contains abnormal amounts of UREA and BILE.

2d. The presence of the urea and bile in the blood is attended with certain nervous disturbances.

3d. It is probable that the extreme slowness of the pulse which characterizes many cases of yellow fever, after the subsidence of the first stage of fibrile excitement, is due to the presence of the bile and urinary constituents in the blood, as well as to the anatomical lesions of the heart.

4th. The fibrin is greatly diminished in the blood of yellow fever.

5th. The passive hæmorrhages and capillary congestion in yel-

low fever are without doubt largely dependent upon the diminution of the fibrin.

6th. The colored blood corpuscles are not specially diminished in yellow fever; and in this respect the disease differs widely from malarial fever.

ALTERATIONS OF THE BLOOD IN YELLOW FEVER INDUCED BY THE SUPPRESSION OF THE FUNCTION OF THE KIDNEYS AND LIVER, AND THE RETENTION IN THE BLOOD OF THE CONSTITUENTS OF THE URINE AND BILE.

The constitution of the blood is more or less altered in every case of yellow fever, but the changes are observed in greatest intensity in those cases in which there is partial or complete suppression of the action of the kidneys.

By careful experiments I have determined, that during the active stage of febrile excitement in yellow fever, not less than 600 grains of urea are excreted by an adult during 24 hours. As the action of the kidneys is in many cases wholly arrested for various periods, ranging from one to four days, before death, the whole amount of urea accumulating in the blood and remaining in the system may range from 600 to 2400 grains. The gastro-intestinal mucous membrane, in such cases, eliminates the urea, both as urea and as carbonate of ammonia, and both these substances frequently enter into the constitution of black vomit, which in many cases is intensely alkaline from the presence of carbonate of ammonia, in sufficient quantities not only to neutralize the acid of the gastric juice, but also to give forth such strong fumes as to be visible as a dense white cloud, when a rod dipped in hydrochloric acid is held over the black vomit, freshly discharged from the stomach or removed from the viscus, immediately after death.

The presence of ammonia in the fluids of the stomach in such cases is clearly not due to subsequent changes, but is referrable to the decomposition of the urea in the stomach after the manner which has been pointed out by Frerichs, Bernard, and other observers.

In such cases, the blood in the cavities of the heart and in the large vessels is black, and does not form a clot, and the solvent properties of the ammonia is manifest not only in the dissolution of the fibrin, but also in the altered appearance of the colored

blood corpuscles, and in the rapidity with which they disappear when the blood is allowed to stand.

One of the most striking facts which arrests the attention of the observer in post mortem examinations in this disease, is the apparent abundance of the blood in the various textures, its fluidity, and also the bright arterial hue which it rapidly assumes upon exposure to the atmosphere. This condition is referrable to the distention of the blood-vessel system, in consequence of the failure of the kidneys to eliminate the watery elements, as well as to the action upon the blood of the excrementitious materials.

Whilst it is true that urea is present in healthy blood in small amount as may be determined not only directly by chemical means, but also by a simple calculation, as has been done by Dr. Goodfellow (Lectures on the Diseases of the Kidney, generally known as Bright's Disease and Dropsy, London, 1861, pp. 67–69), I have at the same time, by careful analysis, shown that the urea is greatly increased in the blood of yellow fever, amounting in some cases to near two per cent. of the whole mass of blood.

The results of these investigations on the condition of the blood, and upon the chemical constitution of black vomit in yellow fever, sustain the view held by M.M. Prevost and Dumas, Claude Bernard, Ricord, and other physiologists, that urea after having been generated in the blood and tissues, is simply excreted by the kidneys, and do not confirm the doctrine taught by M. Herman, Dr. Beale and others, that urea and uric acid are not only eliminated but are formed by the renal tissue. And careful physiological experiments which I have performed have been confirmed by the results of the inquiries of M. Grehaut (Archives de Physiologie Normale et Pathologique Sep et Novembre, 1870—*Am. Jour. Med. Sci*, October, 1871, p. 530), which extended over several years. According to this careful observer, immediately after nephrotomy in the fasting dog, urea begins to accumulate in the blood, its increase being manifest within three hours after the operation; the increase of weight of the urea in the blood and in the lymph, twenty-four hours after the ablation of the kidneys, is equal to the weight of it that would have been excreted by the healthy fasting animal in the same space of time; the accumulation of urea in the blood, in the hours that succeed nephrotomy, follows the same march as after ligature of the ureter. After ligature of the ureter, the quantity of blood circulating through the kidney of the side tied diminishes; under

normal conditions the blood of the renal vein always contains less urea than the corresponding artery; in an animal in which the ureters have been ligatured, the renal venous blood obtained twenty-four hours after the operation contains as much urea as the renal arterial blood, so that the tissue of the kidney neither excretes nor secretes any more.

Finally, M. Gréhaut has shown, that ligature of the ureter and nephrotomy are two operations, that are identical in their results; they both suppress the eliminative function of the kidneys, *whilst they form no obstacle to the formation of urea, which takes place outside of the kidneys.*

Whatever theory may be held, as to the cause of the peculiar phenomena denominated uremia, whether that of the poisoning of the blood by the urinary constituents generally, or by urea especially, or by the product of its decomposition, viz., carbonate of ammonia, or by the œdema of the brain from the accumulation of the watery constituents of the blood, attended by serous apoplexy and irritation of the brain and its membranes; it is evident that in many cases of *yellow fever, the fatal issue is determined chiefly by the retention in the blood of the constituents of the urine.*

To this cause must be attributed, to a great extent, the restlessness, nervous agitation, intoxication, delirium, convulsions and coma, characterizing the stage of calm or depression in many cases of yellow fever.

To the action of the same cause, also, may certain changes of the blood be referred, as the dissolution of the fibrin, and the rapid changes which the colored blood corpuscles undergo after death and after removal from the living body.

During the healthy action of the kidneys, as has been shown by Simon, of Berlin, and Bernard, of Paris, fibrin disappears from the blood passing through this organ, and is most probably converted into albumen, and the increase of the fibrin in Bright's Disease has been referred to the cessation of this change of the fibrin in the diseased kidney; but in yellow fever, notwithstanding that the function of the kidneys may be wholly arrested, there is an almost complete disappearance of the fibrin of the blood, so profound is the action of the defibrinating cause.

That very important changes take place in the blood during the healthy action of the kidneys, from the separation of the urea and other constituents of the urine, is evident from the fact observed by Bernard, that when the kidney is active, the blood

issuing from the renal vein has a bright sparkling red color, but if the secretion is more or less diminished, or altogether suppressed, not only is the blood issuing by the vein dark colored, but the whole organ assumes a purplish tint, more or less deep. This change of color has, with some show of reason, been referred chiefly to the non-separation of water during the suppression of the function of the kidney.

Nasse has shown, that if blood be diluted with water it assumes a dark red color; if the blood be previously dark colored, it becomes still darker on the addition of water; and if the blood corpuscles be examined under the microscope, they are found to be distended and to have lost their discoid form, and to have become spherical; the blood collectively therefore must appear darker, since each individual corpuscle has become converted into a spherical mirror, from which the red rays are scattered and reflected.

It has been shown by the experiments of several physiologists that when the red blood corpuscles are in normal amount, a certain quantity of urea added to the blood scarcely produces any effect upon them; but on adding the same quantity to the same amount of blood, from which a considerable number of blood corpuscles had been withdrawn, a very striking effect was at once observed—the corpuscles rapidly assumed a vermillion tint, and sank to the bottom, presenting the appearance to the naked eye of fine vermillion; and on examining this sediment with the microscope it was found to consist entirely of red corpuscles, reduced to about a sixth of their natural size. The effect of the urea seemed to be in exact relation to the quantity of red corpuscles in the dependent portions of the blood, resembling fine vermillion, as in the preceding experiments, and this appearance must not only be referred to the loss of fibrin, but also to the action of the urea.

The blood of yellow fever containing more or less urinous excrement, and with a physical alteration and actual diminution of both the albumen and fibrin, is not only ill adapted to the nutrition of the organs, and actually conveys to the stomach materials which when eliminated alter so completely the properties of the gastric juice, as not only to arrest digestion, but also to irritate and corrode the mucous membrane; but it is also by its physical and chemical constitution unsuited to the maintenance of the general capillary circulation.

Both Bernard and Poisenile have observed that fibrin when in intimate mixture with a due normal proportion of albumen, facilitates the movement of the blood; but if the fibrin be withdrawn the blood globules fall to the most dependent part, and obstruct the capillary circulation.

Bernard has recorded the observation, that when the capillary circulation in the web of the frog's foot is examined under the microscope, the globules are seen suspended nearly uniformly in the liquor sanguinus; but if we examine this circulation in an animal, whose blood has been defibrinated, we see the globules fall to the most dependent part, whilst at the superior parts pure liquor sanguinis alone circulates. If a horizontal vascular trunk bifurcates in two divisions, not situated on the same horizontal plane, the lower branch will be plugged by the accumulation of the globules, while the upper branch will be full of serum.

Not only are similar causes active in producing the capillary congestions which are so characteristic of yellow fever, but the altered albumen and fibrin coagulate in the secreting and excreting structures of the liver and kidneys after its transudation through the walls of the capillaries and biliary and urinary tubes.

To these changes of the blood, therefore, must be referred, to a great extent, the suppression of the functions of the liver and kidney; for it is very evident that in the case of the liver the obstruction does not exist in the biliary or hepatic ducts, but in the biliary tubes.

It is evident that certain changes in the blood, as well as certain nervous symptoms in yellow fever, are due to the retention of bile in the blood.

Next to black vomit, jaundice occurring in the febrile stage of yellow fever is the most fatal symptom. Many observers regard jaundice as unfavorable in proportion to the earliness of its development; some writers considering jaundice as surely indicative of a fatal termination, when appearing upon the first or second day, or up to the third, fourth, or fifth day.

Mr. Maher says that the period at which jaundice makes its appearance during the course of the disease is, according to the observations of Dr. Belot, a prognostic sign of great certainty. When it appears towards the third or fourth day death will infallibly ensue; if it occurs towards the fifth or seventh day, the probability of a fatal issue is greater than that of recovery;

finally, when it manifests itself only after the seventh day from the period of attack, recovery is almost certain (Rapport Médicale sur l'Epidémic de Fiévre, Jaune). The most valuable statistics with reference to this symptom have been furnished by Daniel Blair (Some Account of the Last Yellow Fever Epidemic of British Guiana. Third Edition; London, 1852: pp. 79–83).

According to this accurate observer, "Yellow skin was always a sign of great intensity of disease. Among the 2071 milder and graver cases, 385 had *yellow skin;* and of these 385, 178 died. Thus the proportion of cases in which the symptom appeared was 18.54 per cent., and the rate of mortality of the symptom was 46.23 per cent.

The following table will show the number of cases in which yellow skin was observed on different days of the disease, the number of deaths, and rate of mortality for each day, and rate per cent. of symptoms.

TABLE—Showing the Number of Cases in which Yellow Skin was observed on different days of Yellow Fever, the Number of Deaths, and Rate of Mortality for each day.

Day of Disease	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	Unascertained.
Total number of cases..	8	16	46	89	86	66	30	18	7	8	5	..	..	3	1	2
Number of deaths.....	1	4	18	44	53	27	13	10	1	3	2	...	...	....	1	1
Rate of Mortality	12.5	25.0	39.01	49.45	61.62	40.90	43.33	55.55	14 28	37.5	40.	..	..	....	..	.....
Rate per cent. of Symptoms.....	0.38	0.76	2.22	4.20	4.15	3.14	1.45	0.87	0.34	0.38	0.23	..	..	0.14	..	...

Black vomit, although a very unfavorable symptom, and more so than yellow skin, being as 75.68 to 46.23, is still not necessarily fatal. Out of the 366 cases of it, 277 only died, giving the centesimal mortality just stated, viz., 75.68. Yellow skin and black vomit were closely associated as to the time of their appearance, but the former was generally the antecedent (fortunate for the patient when otherwise). Thus in 139 ascertained cases, the former preceded the latter in 51 instances, appeared simultaneously in 46, and succeeded it in 42 instances. The double symptom of yellow skin and black vomit was of all others the most dangerous, and especially when the latter succeeded the former, and within twenty-four hours. The co-existence of these symptoms was observed in 144 cases, being 6.95 per cent. of the

2071 mitior and gravior cases admitted to the Seaman's Hospital. Out of these 144 cases, 122 died, making the mortality of the conjoined symptom 84.72 per cent.

The following table shows the day of the disease on which yellow skin and black vomit occurred simultaneously, also the number of cases in which the former preceded or succeeded the latter, and the number of days it so preceded or succeeded.

YELLOW SKIN AND BLACK VOMIT.

122 DEATHS.						22 RECOVERIES.					
40 Cases Simultaneous.		45 Cases Preceded.		22 Cases Succeeded.		6 Cases Simultaneous.		6 Cases Preceded.		10 Cases Succeeded.	
Day of Disease.	No. of Cases.	Number of days preceded	Number of Cases.	Number of Days succeeded	Number of Cases.	Day of Disease.	Number of Cases.	Number of Days preceded	Number of Cases.	Number of Days.	Number of Cases.
3d ...	3	1	28	1	25	3	1	1	3	1	5
4th....	14	2	10	2	4	4	1	2	2	2	2
5th...	9	3	5	3	2	5	2	4	1	3	2
6th...	10	4	1	7	1	6	2	.....	.....	4	1
7th....	3	5	1	.....	.....	.....	.....	.....	.....	.....	.....
8th...	1	0	0	.....	.....	.....	.....	.....	.....	.....	.....

I have confirmed, by careful chemical analysis of the blood, and of the nervous structure and organs, the view held by some pathologists, that the yellow hue in jaundice observed in many grave cases of yellow fever is due to the presence of bile in the blood.

By careful post mortem examinations, as well as by attentive observation of the amount and chemical constitution of the urine in this disease, I have been able to refer the accumulation of the bile in the blood in this disease to two causes, viz.: structural alterations of the liver, attended with desquamation of the excretory cells, obstruction of the biliary ducts, with altered fibrin and albumen and oil, and the accumulation of free oil in the excretory cells, and structural alterations of the kidney, resulting in impairment if not total suppression of its function.

Bile is a very common ingredient of the urine in yellow fever, even in those cases which terminate favorably, and as long as the function of the kidneys are fully and freely performed, the blood may be relieved from any great accumulation of bile, even

when the lesions of the liver are well marked, and from this cause the urine may be loaded with bile.

When the kidneys cease acting, the bile as well as the urinary constituents rapidly accumulate in the blood, and certain changes in the physical and chemical characters of this fluid are referrable to the bile, as well as to the urinary constituents.

In order to ascertain the effects upon the various functions resulting from the absorption of a large quantity of bile into the blood, but particularly with the object of tracing what becomes of the bile in the blood—whether it is transformed, or is excreted as bile, and, if transformed, what may be the nature of the changes it undergoes—Frerichs performed a series of experiments the general results of which were as follows. Some of the animals died under symptoms of violent dyspnœa (obstruction of the capillaries of the lungs), owing to the entrance of air into the lungs, or to the too great consistency of the injected fluid; twenty-nine of the experiments, however, succeeded in such a way that their results could be made use of.

In no case did any remarkable derangements of the nervous functions follow the injection; in no case was stupor, convulsions, or retardation of the pulse observable; vomiting, however, occurred repeatedly in about one-fourth of the experiments, and in some of these cases also the injection of the bile was followed by some drowsiness. The character of the urine which was voided after the injection varied; sometimes it contained a larger or smaller quantity ot coloring matter, and at other times it contained none; the former was the case in 19, and the latter in 10, of the 29 experiments. The urine containing coloring matter was always passed in small quantity; it was of a greenish brown color, became turbid upon cooling, and then appeared green, rapidly depositing flakes which, under the microscope, presented a finely granular appearance. When collected upon a filter, this deposit formed a dark, grass-green layer, which dried readily, and which, when recently dried, exhibited the characteristic properties of the coloring matter of bile, becoming decomposed by the action of impure nitric acid, or by a mixture of sulphuric and nitric acids, with a lively play of colors of green, blue, violet and red. The reaction of the urine was, in most cases, neutral or alkaline, the removal of the flakes of coloring matter was speedily followed by an abundant deposit of triple phosphates; the specific gravity of the urine, varied from 1012

to 1019. But what is of especial interest in it bearing upon yellow fever, as developed by the experiments of Frerichs, is that in 17 *of the* 19 *cases the urine was albuminous*, and after the sepation of the green flakes by filtration, its *color was blood red apparently owing to dissolved blood pigment; no blood corpuscles could be detected in the sediment.* Of the ten cases in which the urine was passed clear and pale, there was invariably a remarkable increase of the secretion; on the other hand, when the pigment appeared, the urine was scant, and albuminuria and hæmaturia present; and this latter condition of the urine appeared to be favored by whatever interferes with the respiration. The appearance of the albumen and blood in these cases was due to one or both of these causes: either the bile induced such alterations in the constitution of the blood as led to the transudation of the altered albumen and coloring matters of the colored corpuscles through the excretory structures of the kidney, or it acted as a direct irritant upon this organ, causing congestion and suppression of secretion, and consequently transudation of albumen and the coloring matter of the corpuscles.

Dr. Johnston has adduced the condition of the kidney in jaundice, as an illustration of the effect upon the secretory cells of an effort to eliminate new materials. He assumes, that when from any cause, the functions of the liver are so imperfectly performed as to allow the bile to accumulate in the blood, the urine is found to be deeply tinged with some at least of the constituents of the biliary secretion; and on a microscopical examination of the urine, we find the renal secreting cells, in variable numbers, some being scattered, while others are entangled in moulds of the kidney tubes, and all of them colored by the bile contained within them. When the kidney is examined after death, in these cases, the tubes are deeply tinged by the bile contained in their secreting cells, and some tubes are nearly or quite filled with cells which have been thrown off, while others have been formed upon the basement membrane beneath them.

Dr. Johnston concludes from these facts, that when the blood circulating through the kidneys contains an excess of bile, the renal cells, in striving to eliminate these materials, become deeply tinged by it, and many of them are so far modified as to be shed by a process of desquamation.

Without doubt the bile exerts a direct effect upon the kidneys, inducing congestion and desquamation of the excretory cells, in

yellow fever, but the albuminuria, hæmaturia, and even total suppression of the function of these organs, cannot be referred wholly to the direct irritant action of the biliary products, but must, as in the albuminuria and desquamative nephritis of scarlatina, be referred to other causes also, as the irritant action of the poisons exciting the diseases, and the morbid condition of the blood induced by the action of specific poisons, as well as by the altered bile.

That the retention of bile in the blood is capable of inducing profound alterations in its constitution, is evident from the fact that in all cases of jaundice which last for a length of time, the blood becomes impoverished by a diminution in the proportion of red globules and fibrin, and a tendency is developed to hæmorrhages from the various mucous membranes. Whilst this tendency to hæmorrhage is practically observed in conjunction with cerebral symptoms and other indications of blood poisoning in cases of jaundice, when there is no obstruction to the bile ducts, it also occurs in cases of mechanical jaundice of long standing, in which the immediate cause of death is not unfrequently copious hæmorrhage from the stomach and bowels.

Whilst Frerichs observed in his experiments, no marked effects upon the action of the heart or nervous system, it is well established by clinical observation that the presence of bile in the blood frequently retards the action of the heart, and diminishes arterial tension, the pulse becoming irregular, and falling to 50, 40, or even 20, and presenting the same characters as the pulse in yellow fever; and it is equally well established that acute delirium, stupor, coma, muscular tremors and convulsions, may supervene in cases of jaundice, where there is no obstruction of the ducts, as well as in cases of long standing mechanical obstruction. The fact that slowness of the pulse is not an invariable symptom of jaundice has been explained by supposing that it is due to one particular ingredient of the bile, which does not exist in the blood in all cases of jaundice. Thus, Röhring has shown by experiments upon animals that the biliary acid salts exercise a specific paralyzing action upon the heart and retard its action, while bile pigment has no such effect: slowness of the pulse, therefore, in jaundice has been referred to the presence in the blood of unchanged biliary acids. Notwithstanding that the blood of the human subject may be saturated with bile, in jaundice for long periods of time, without the manifestation of any

serious cerebral symptoms, at the same time, there are certain cases of suppression of the function of the liver, attended with cerebral symptoms of the most violent character, hæmorrhage from the bowels, and profound alterations in the blood and urine, which can only be referred to the presence in the blood of the altered secretion of the liver.

The theory advanced by Dr. Austin Flint, that the cerebral symptoms in jaundice are due to the retention of cholesterine in the blood, does not solve the question; for if the retention of all the elements of the bile in the blood does not give rise to cerebral symptoms, it is impossible to understand how the symptoms can result from the retention of cholesterine alone, which is one of the constituents of bile.

Cholæmic eclampsia, attended with jaundice, and rapidly fatal tonic and clonic convulsions and coma, as well as that peculiar form of jaundice which is characterized by vomiting of blood, bloody stools, convulsions, coma and a rapid progress and fatal issue, evidently arise from the alterations induced in the blood by the constituents of the bile, and the products of their decomposition, resulting from *acute atrophy of the liver;* and present, not only in some of the symptoms, but also in the acute fatty degeneration of the liver, and infiltration of the organ with oil, and the alterations of the heart and kidneys, a striking analogy to the grave cases of yellow fever.

It is still a matter of dispute and doubt which element of the bile, or what products of the decomposition exercise the baleful influence on the nervous system in acute yellow atrophy of the liver. According to Frerichs, the formation of leucine and tyrosine, crystaline products of the decomposition of albuminous substances, may produce the nervous disturbances. Virchow, however, feels himself unable to grant this, because leucine and tyrosine are also found in typhus and exanthematous diseases, and may possibly be formed not until after death. Bamberger, on the other hand, thinks it more probable that they are produced by the acids of bile, and by the possible products of their decomposition; and Dr. Budd contends that they are due to some peculiarly noxious matter evolved, *in consequence of decomposition*, in the lobular substance of the liver; while Dr. Charles Murchison holds that the poison producing the cerebral symptoms is more probably generated in the blood and throughout the body generally than in the liver particularly.

The investigations of physiologists have shown that the liver not only excretes bile, but it forms grape sugar, and elaborates certain constituents of the blood, and exercises a continuous and important influence on the metamorphosis of matter constantly taking place in the blood and tissues.

That the arrest of the function of the liver checks or modifies the normal healthy metamorphoses of the blood and tissues is manifest not only in the physical alterations of the blood in *acute atrophy* of the organ, but also in the great diminution of urea, and the appearance in large quantities of leucine and tyrosine, which possess a composition intermediate between it and the albuminoid components from which urea and uric acid are derived.

In acute atrophy of the liver, in acute phosphorus poisoning and in yellow fever, diseased states which have many symptoms and pathological lesions in common, it appears that the albuminoid substances of the blood and organs are split up into nitrogenous and non-nitrogenous combinations; but the combinations of the oxygen of the blood are not carried far enough to produce the final result of the normal oxidation in the healthy organism, and in addition to the formation of leucine and tyrosine, and of various nitrogenous substances, the composition of which is not perfectly known, *fat* results from these changes, which not only imparts an oily appearance to the blood, but also from the arrest of the oil globules in the capillaries infiltrate the textures of the liver, heart and spleen, and induce in these organs fatty degeneration.

Experiments upon Living Animals, with the Blood from the Heart and the Black Vomit from the Stomach of Yellow Fever Cases.

Experiment First.—With a small "subcutaneous" syringe I injected beneath the skin of a healthy puppy about thirty drops of blood taken from the heart of a yellow fever patient three hours after death.

No ill effects were observed.

Experiment Second.—In like manner I injected beneath the skin of an active Guinea pig about the same quantity of blood from the *yellow fever heart.*

The next day, October 16th, 1873, the Guinea pig appeared

lively, and ate its food. October 17th, the animal appears sluggish and refuses food. October 18th—Animal feeble; moves with difficulty, and is evidently ill from the effects of the injection. October 19th, 5 a. m.—I found the Guinea pig dead, cold and stiff.

Post Mortem Examination.—Body emits a disagreeable odor; cellular tissue of skin and surface discolored and greatly congested around the point of the injection of the yellow fever blood. Cellular tissue and skin softened in those portions around the area of injection. Under the microscope the fluid from these portions of the cellular tissue was found to contain bacteria and revolving animalculæ. Cavities of the heart distended with dark, almost black, loosely coagulated blood. Blood changed rapidly to the arterial hue upon exposure to the atmosphere. Under the microscope the blood corpuscles presented no peculiar alteration. Liver congested. The microscope revealed no accumulation of oil in the textures of the liver. Liver cells distinct but pale. No bacteria, or animal or vegetable organisms, were observed in the blood of the heart, or in the blood and structures of the liver.

Experiment Third.—Into the subcutaneous tissue of a large, healthy and active male guinea-pig, I injected about thirty drops of black vomit, taken from the stomach of the yellow fever patient three hours after death.

The black vomit thus carefully injected into the cellular tissue caused the death of this animal in six hours.

Post Mortem Examination.—Diffused redness, and great capillary congestion of cellular tissue beneath the skin. Congestion greatest in the immediate vicinity of the injection. Body emits a foul putrid odor. Immediately around the point where the the black vomit had been injected dark blood had been effused, and the textures presented precisely the appearance of those wounded and poisoned by the fangs of the rattlesnake or copperhead.

The cavities of the heart were distended with dark, almost black, loosely coagulated blood. No animalculæ, bacteria or fungi, or algæ discovered in the blood. Decomposition rapid; and although the animal was examined almost immediately after death, the odor was disagreeable and resembled that of the black vomit.

Experiment Fourth.—I injected into the subcutaneous tissue

of a large, healthy guinea-pig, about thirty drops of black vomit which I had preserved from a case of yellow fever, ejected shortly before death on the 9th of October, 1873, six days before the present experiment.

Death caused by the black vomit in six hours.

Post Mortem Examination.—Results similar in all respects to those recorded in the preceding experiment: intense congestion of cellular tissue around locality of injection of black vomit; no animalculæ or vegetable organisms in blood; putrefaction rapid and marked by foul odor.

Experiment Fifth.—I injected into the subcutaneous tissue of a guinea-pig about thirty drops of putrid blood, which I had extracted from the cavities of the heart of a subject who had died six days before, on the 9th of October, in a "*congestive*" malarial chill.

Death caused by the putrid malarial blood in eight hours.

Post mortem changes similar to those recorded in experiments third and fourth.

The preceding experiments are of importance in establishing the fact that *black vomit, taken from the stomach immediately after death, or ejected during life in yellow fever, will, when injected into the subcutaneous tissue, produce as deadly and as rapidly fatal results as putrid blood.*

We deduce the following practical conclusions:

First. BLACK VOMIT, *when absorbed into the circulatory system, may act as a deadly* SEPTIC POISON.

Second. As the capillaries of the stomach in yellow fever are often ruptured, and the epithelium of the mucous membrane denuded, it is possible that in some cases the absorption of black vomit, especially after it has undergone putrefactive changes in the stomach, may be an important cause of the fatal issue.

Third. If remedies could be used which would prevent putrefactive changes in the black vomit and render it comparatively inert, a certain proportion of cases might be rescued, after the appearance of *black vomit.*

The preceding experiments and reasoning led me to employ the SULPHO CARBOLATE OF SODIUM, in doses of 20 grains, every 4 or 6 hours in the treatment of yellow fever.

I have used the *sulpho carbolate of sodium in about forty* cases of yellow fever, with satisfactory results.

I find that it is readily borne by the stomach at all stages of

the disease; that so far from exciting nausea or vomiting, it often modifies and arrests these disturbing symptoms.

It appeared in some cases to arrest the decomposition of the black vomit. One case accompanied by a temperature of 107° 1′, and attended with black vomit, recovered under its use, combined with ice water injections into the rectum.

At some future time we hope to present statements of these cases.

Davaine claims to have made the following discoveries: After injecting beneath the skin of an animal's neck a single drop of putrid blood, the surrounding tissue became extensively infiltrated, and death soon followed with symptoms of septicæmia. The blood of this animal was then employed to inject other animals in a similar way, and was found to be more poisonous than the original putrid blood. A third and fourth animal was then injected, each with the blood of the preceding one, and in this way 24 in all were experimented on. The results seemed to show that the toxic power of the septic material increases by dilution.

Stricker, in a series of experiments undertaken to test these statements, inoculated 25 animals with healthy, 23 with putrid, and 73 with blood diluted by transmission. Of the first series 4 died. Of the second series 11 died. In experiments with diluted blood, when it had passed through 13 different animals, 53 of the 73 died. From this it appears that even the very small amount of $\frac{1}{1250}$ of a cubic centimeter of poisonous material could cause death when injected hypodermically. Davaine's statement, that organisms form in the blood and increase by every transmission, was not sustained by investigation. Stricker found numerous colorless bodies in the blood of these animals, but he did not regard them as organisms, but rather as proto-plasmic bodies.

The following facts he regards as ascertained:

1. That transmission greatly increases the injurious and fatal action of the putrid matter.

2. That the original disease was infectious, but that through inoculation it became contagious.

3. That though it be not proven, yet it is probable that the special poison is a living contagion (*contagium animatum*), for such rapid proliferation is only possible in organized material.

4. That the poison is diffusible and is not destroyed by boiling

(*Allg. Wien. Med. Zeitung*, 20, 1873; *New York Medical Record*, July 1st, 1873, p. 311).

If it be true that the poison of yellow fever may be generated in human beings under certain conditions of the constitution, and more especially of the chemical and physical constitution of the blood and of the nervous and muscular structures, when subjected to the combined influences of heat and crowding in an impure atmosphere; if it be still farther true that decomposition, both before and after death, is more rapid than in any other form of disease, and if this decomposition forms the most favorable condition for the rapid multiplication of bacteria and fungi, and other simple organisms; it is reasonable, in the light of the preceding experiments of Davaine and Stricker, to suppose that these organisms may become *carriers* of the poison, and may constitute an important medium of its dissemination from the original foci.

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ARTICLE III. *Double Strangulated Femoral Hernia; Operation Five Days after Strangulation.* By ROBERT E. RICHARDSON, M.D., Fayette, Miss.

June 6th.—Called to the residence of Mr. G. T. B., four miles in the country, to see a Swedish woman who had been received and lodged for the night by Mrs. R. C. H., an aunt of Mr. B. The woman, Mary George, had walked through the hot sun and dust 16 or 18 miles, and in addition, had eaten imprudently of green apples, plums and blackberries. The following morning, June 2d, she got up and dressed, but complained of sick stomach and pains in the bowels of a severe griping character. To relieve these symptoms, Mr. B. gave her a dose of calomel, and in a few hours a full dose of castor oil, believing that colic was the trouble. The treatment only served to aggravate her condition. Nausea increased to vomiting of the contents of the stomach and mouthfuls of yellow bile. Pain around the umbilicus excruciating. In the afternoon of the 2d a single discharge from the lower bowels occurred, accompanied with much blood. The pain and vomiting grew from bad to worse, with considerable swelling of the abdomen. The poor woman's wailings induced Mr. B. to have me called on the evening of the 5th of June to see and prescribe for her. I was absent 18 miles distant, and did not reach

home until 12, noon, of the 6th, and did not see the case until 4 o'clock, p. m., of the 6th, when her condition was critical in the extreme: pulse 115; abdomen enormously swollen; pains so great that screams of the poor creature could be heard for a distance around. Vomiting bile and fœces took place every few minutes. Swelling in the right groin, extending to the vulva; very hard and sore. Strangulated Hernia was evident, and of so long duration that relief by taxis, or even an operation, was deemed extremely uncertain. Two grains sulphate morphia were injected hypodermically near umbilicus, and soon afforded marked relief from the severe pains and constant vomiting. The warm bath was freely resorted to, and warm water pumped into the lower bowels. Taxis gently and cautiously tried, but the tumor was hard and unyielding.

The woman was candidly informed of her real condition, and the necessity of operative interference, as the only hope, fully explained, also the fact that I had never operated for hernia, which, of itself, however well performed, was not devoid of danger; but that I was willing, under the circumstances, to assume the responsibility, death being certain without it.

The morphia had afforded so much relief that she was not willing to accept my diagnosis as correct, declaring that the attack was bilious colic, and that the hypodermic injection of morphia, or the "little squirt," as she called it, had cured her. Finding her stubborn in declining the chances of an operation, I left six grains of morphine to be given in six doses, as required to relieve the pains, and left her, expecting in the morning to find her dead or nearly so.

June 7th.—Pulse 125; abdomen swollen almost to bursting; pain excruciating, though all the morphine had been swallowed; vomiting of bile and fœces every few minutes; none of the morphine swallowed seemed to have the slightest effect. After much persuasion she consented that an operation be attempted, and notwithstanding the unfavorable condition of the case, I deemed it imperatively my duty to operate, even though the patient died under the knife. My friend, Dr. E. W. Harding, a veteran of many years in professional harness, was asked for advice and assistance. Concurring in opinion that the knife offered the only hope of relief, Dr. Harding administered a mixture of ether two parts, chloroform one part. A stout assistant grasped the legs above the knees and held them firmly, while I made an incision of three and a half inches in length in a line with and over the inner margin of the tumor, through skin, cellular tissue and fat, down to the hernial sac, which was cautiously opened, disclosing a knuckle of intestine as large as a hen's egg, deeply congested, but not gangrenous. By gently pulling the protruded bowel and using the index finger, the stricture was easily found, and with the aid of a grooved director and blunt pointed bistoury quickly nicked, the bowels gently compressed to empty them, when they slipped back without trouble. One small cutaneous artery was divided and secured by torsion; not exceeding a half ounce of blood was lost; wound closed with ten silk stitches evenly put in, and compress of lint wet with cold water laid over the cut. Gave half a grain morphia hypodermically; absolute quiet ordered. Milk and lime water for diet; quarter grain morphia in six hours, or sooner if required.

June 8th, 7 a. m.—Pulse 120; vomiting occasionally of a mouthful of yellow bile up to 12, noon; abdomen still swollen but less tender; complained of heat

and burning in the rectum, which a little cold water from a Davidson syringe promptly relieved. 6 p m.—Pulse 110; patient more comfortable in every respect; a small action from the bowels at 4 p m. Lime water and milk for diet in quantities of a wineglassful not oftener than once an hour; one grain opium at 8 p. m.

June 9th, 10 a. m.—Rested well last night; pulse 126; abdomen more tender; urine scant but clear; vomited four times since 6 o'clock. A benevolent lady had given a pint or more of sweet milk which the patient drank at one draught. It produced indigestion, causing the excitement of pulse, vomiting, and a free discharge from the bowels. Ordered warm fomentations to the abdomen, with ten grains bicarb. soda, and opium one grain, at 11 o'clock and at 6 p. m.

June 11th.—Rested well last night but not much sleep; bowels moved four times up to 12, noon, bilious and offensive; 12 grains Dover's powder, one scruple subnitrate bismuth, to be repeated if necessary, at 8 p. m. 9 p. m.—No further motions from the bowels; urine clear and increased in quantity; pulse 115; no pain; perspires freely, and in every way comfortable; wound not inclined to heal; stitches broke out; discharging moderately of a thin, yellow and offensive matter; dressed with a weak solution of carbolic acid on lint. 9½ p. m.—pulse 120; tongue inclined to dryness; bowels moved three times—offensive; skin and kidneys acting well. Abdomen soft and but little swollen or tender; wound inclined to gape, and discharging, less and less offensive; opium at 8 p. m.

June 12th 6 a. m.—Rested badly; bowels moved three times during the night—bilious, watery and offensive; sweating less; tongue slightly furred; pulse 115. Grain opium with 5 grains tannin once in six hours Beef tea and milk punch, with a little fresh buttermilk added to diet. 9. p m—Wound disposed to slough and discharging offensive matter with fœcal odor; soreness hardly perceptible.

June 13th.—Rested well, but did not sleep; bowels acted twice, fœces passing through the wound from a point above the upper extremity of the cut, evidently from a small strangulated hernia that had escaped detection during the operation on the 7th, having been masked by the smallness of its size and the great abdominal swelling Appetite good; pulse 108; two actions from the bowels since daylight, now 10 a. m; fœces passing freely through the wound; stuffed the wound lightly with cotton lint saturated with weak solution carbolic acid, and covered with light compress wet with same solution. Ordered eggs, soft boiled, milk and rice, milk punch, fresh buttermilk; opium as before.

June 14th, 9 a. m.—Slept well last night; stools more consistent, and passing out the natural way; the cotton wadding answered admirably, very little fœces passing through the wound; pulse 108. Improvement gradual but constant up to the 17th. Pulse 97; but little fœcal matter passing through the wound, which is throwing out granulations slightly.

June 18th, 7 a. m.—Pulse 100; fretful; tongue coated; fevers; stools passing natural and solid.

June 19th.—Pulse 110; bilious diarrhœa; fever; tongue heavily coated. Opium 1 grain, prepared chalk i, quinine iii grains, once in four hours.

June 20th.—Pulse 105; fever less; skin soft. Continued treatment.

June 21st.—Pulse 100; tongue cleaning off; but little fœces passing through the wound. Treatment continued, with the addition of finely powdered clay gently packed into the wound, and covered with compress of lint as before.

July 3d.—Artificial anus closed by granulations; clay dressing continued; free diet.

July 11th.—Wound almost healed; patient sitting up: clay dressing continued.

July 15th.—Wound entirely closed; patient sitting up and walking about the room; stools passing freely. Patient directed to exercise caution in her movements, and to wear a close-fitting truss.

The length of time from the beginning of the stricture, June 2d, to the date of operation, June 7th, gives encouragement to operate, with a promise of success, even in the most apparently hopeless cases. The formation of an artificial anus 11 days after the stricture and 5 days after the operation, is unusual, and difficult to account for except on the supposition of a small concealed hernia. I cannot close without mention of the beautiful christian benevolence of the two ladies (Mrs. Hunters), in caring for every want of the unfortunate woman thus accidentally, but providentially for her, thrown upon them.

PROCEEDINGS OF THE NEW ORLEANS MEDICAL AND SURGICAL ASSOCIATION.

Rinorrhœa or Ozœna. By Wm. R. Mandeville, M.D.

Of the diseases affecting the nasal organs, there is none more harassing or mortifying to the patient, disgusting to others, or more annoying and difficult of treatment by the surgeon.

Ozæna may be defined to be a purulent or sanious discharge from one or both nostrils, characterized by fœtor, and which has been compared to that of crushed bed-bugs, and called by the French *punaisie.* The word literally means—I smell.

Like Otorrhœa, it is generally but a symptom, and it will be necessary to consider the morbid conditions which may give rise to it, or upon which it may depend.

For convenience of description I shall divide it into two forms: 1st, simple ozæna, due, it may be, to several causes, but not dependant upon any constitutional vice or taint of the system; 2d, that which has for its cause some hereditary or acquired taint, as, for example, scrotula and syphilis, or the two combined, as is sometimes the case.

In simple ozæna the chief efficient cause is generally due to a long-continued catarrh or coryza, combined with a low condition of the system at large, and which often follows as a secondary affection some of the exanthemata, as variola, measles and scarlatina in children. If the coryza be not speedily checked, the secretion of mucus is liable to take on an unhealthy character, which soon becomes chronic in its nature, causing the Schneiderean mucous membrane to become hypertrophied or swollen, thereby diminishing the area of the nasal cavities, which is often attended with difficulty of breathing and a flow of tears. This is generally the forerunner of ozæna. The discharge continues to increase in thickness, but is not as abundant as before: this soon becomes extremely offensive and a great source of annoyance to the child's parents.

All the treatment necessary in these cases is the administration of tonics, such as iron and quiniæ, and cleansing out the nose morning and evening by means of Thudicum's Nasal Douche, using a pint or more of tepid water holding in solution about an ounce of common salt. It will be found that generally under this treatment the disease will be soon arrested, rarely taking longer than from three to four months. When practicable a change of air will readily accomplish the cure.

One of the most important causes of simple ozæna—and one which is very generally overlooked—is the presence of foreign bodies in the nasal cavities, introduced either by accident or intentionally, as by children in the act of playing, inserting such articles that may come across their way, as, for instance, pieces of ribbons, beans, peas, buttons, grains of corn, &c. I may here

suggest that one reason why some children are so fond of poking things up their noses may no doubt be in part due to intestinal worms, which, as we know, cause irritation and itching of this organ.

The presence of these foreign bodies within the nasal cavities gives rise to irritation and inflammation, which is at first characterized by an increased secretion of mucus from the pituitary membrane, but which is soon changed, however, into a mucopurulent discharge of an offensive odor, provided the substance be not speedily removed by the surgeon, or by the acts of coughing, sneezing, or by a forcible expiration. Unless the foreign body be very small, the acts of inspiration rather tend to increase the difficulty than otherwise, by causing it to be more firmly lodged in the posterior nares.

If this irritation still continues to be kept up, the pressure of the foreign substance may eventually give rise to abrasion of the mucous membrane and ulceration. This is more apt to occur in the intervals between the spongy bones and on the floor of the nasal fossæ, the membrane being very thin at these places. The inner wall or septum which separates the nasal fossæ from one another being but a thin partition, may also by the pressure exerted by the foreign body be perforated, so that the fossæ communicate. Caries of the ethmoid and sphenoid bones is apt to take place, and then the discharge becomes excessively offensive, disgusting to the patient, and a nuisance to all about him. The eyes also become watery, due in some cases to the fact that the membrane of the nose is continuous with that of the lachrymal sac, and also with the conjunctiva or mucous membrane of the eyelids, and to which the irritation may be communicated.

The treatment of these cases is generally of a simple character, the removal of the cause of the irritation and inflammation, and which may be conducted as follows: by the use of the nasal douche, bringing to bear a strong current of water, to flow from one nostril to the other, first discovered by Weber.

"That if one side of the nasal cavity is entirely filled through one nostril with fluid by hydrostatic pressure, while the patient is breathing through the mouth, the soft palate completely closes the choanæ, and does not permit any fluid to pass into the pharynx, while the fluid easily passes into the other cavity, mostly round and over the posterior edge of the septum narium, in some persons also through the frontal sinuses, and escapes from the other open nostril, after having touched every part of the first half of the nose and a great part, certainly the lower median canal, of the second half."

By applying this principle to the nasal cavity, we may often be able to dislodge the foreign body, and also to easily disinfect and deodorize by appropriate washes the whole surface of the nasal cavity, and, as Thudicum remarks, to remove the sordes which accumulate so easily in it, and to apply to its surface a great number of beneficial medicinal substances, so as to pre-

vent acute affections from extending, and to incline them towards a speedy recovery, to stop hemorrhages, allay irritation, and subdue in a remarkable manner chronic affections of the Schneiderian membrane, so as to establish a perfectly healthy surface and normal condition of the organ of smell.

I have alluded to the nasal douche at some length, because in simple cases of ozæna our treatment is almost confined to that of a local nature, and this method is surely the easiest of application, and by far the most efficacious. Foreign bodies may also be removed by means of a pair of curved dressing forceps, but this is often a rather difficult procedure, and is apt to be followed by considerable hemorrhage.

In the majority of cases, however, the method I have just advocated will be found to answer satisfactorily.

I may also mention what has been called by our English brethren, "the Yankee method," for getting rid of such foreign bodies, "though inelegant is still very ingenious." It is as follows: An emetic is first administered to the patient, a pocket handkerchief is then tied tightly over the mouth, and held there until the emetic makes itself felt, and the usual effect takes place. It is not difficult to calculate the result. Though I have never tried this plan, yet I dare say that in some cases it might prove useful, and I would suggest its trial in case of an emergency.

I know a family in which three of the children, their ages ranging from two to six years, were affected with ozæna, due to the introduction of foreign bodies into the nasal cavities. One especially, whose case I will briefly sketch. The child was a girl of five years. An offensive odor was first noticed by her mother, accompanied by a discharge of a purulent fluid; there was also some tumefaction between the eyes. After the use of various injections the child was seized with violent fits of sneezing which resulted in the ejection of over a yard of very narrow green ribbon. Two brothers of the girl were affected in a similar manner, in one the offending substance was a small china button, and in the other a grain of corn. After a rather tedious treatment they recovered entirely. It is here worthy to note that all three of these children were the subjects of intestinal worms. Another case I will mention of a lady who shortly after her marriage was rallied by her family for her change of voice, they declaring she had begun to speak through her nose. As time passed on an offensive discharge made its appearance, with ulceration and necrosis of the bones of the nose. Healthy children were born to her, and it was only after the lapse of some years that a brass button was discovered as the cause of all her troubles.

Some members of the family and she herself remembered that when a child there was some alarm and anxiety about her having thrust a button up her nose, but as no uneasiness was felt it passed from the remembrance of all, even the subject of these remarks. After the removal of this button the parts rapidly

healed, and the lady was supplied with a gutta percha nose attached to a light pair of spectacles. When her veil was down no one could have told that she was not wearing her own features as originally fashioned.

When pure water is used for rinsing the nose it causes in many persons very disagreeable sensations, such as a copious discharge of watery mucus from the nose, with considerable lachrymation, tears and sneezing, and if a large quantity of fluid be used, there is often experienced a sensation of cold in the head, which may last for several hours, accompanied with a modification or change of the voice. This difficulty may be avoided by employing a solution of chloride of sodium, of about one ounce of the salt to the quart of tepid water. The phosphates of ammonia and soda and chlorate of potass may also be used for this purpose, and exert a beneficial effect upon the mucous membrane, by loosening and dissolving the mucus and pus which frequently become hardened and dry upon the surfaces of the nasal cavity.

For deodorizing the nasal cavity, as standing first in importance, is a dilute solution of the permanganate of potassa, using from one to fifteen grains of this salt to the pint of tepid water, according to the severity of the case. I have often noticed that when the margin of the nostrils is excoriated the wash stains the excoriated part brown. This answers a good purpose; for, as the excoriated part does not fall off, the parts beneath are left of a healthy appearance and color. Permanganate of potash acts as an escharotic upon healthy, but more particularly upon erythematous and vascular parts.

Chlorinated soda also answers admirably for this purpose, using it in the proportion of one part to ten of water. Carbolic acid may also be used; but in my experience it seems to irritate the mucous membrane, even when very much diluted. I therefore give the preference to the two I have just mentioned.

It is always best to allow the fluid to pass in a small stream, and in small quantities at a time at first, so as to allow the sordes which have accumulated within the nose to become loosened, and the dry, hard crusts to soften down. This in some cases is attended with considerable irritation. The current should always be reversed; for, as Thudicum remarks: "This reversal has sometimes the effect of throwing large lumps of inspissated mucus and pus upon the upper side of the soft palate; and as they are too large to be carried round the septum narium into and through the nasal canal by which the fluid leads, they are taken into the pharynx, and immediately ejected by the patient through the mouth. It is really surprising what an amount of sordes will by this means be removed from the nose by the rinsing process. I have seen large chunks removed in this manner, and accompanied with intense relief to the patient."

As pointed out by Dr. Thudicum: The inspection of these discharges is of some importance in a diagnostic point of view; for

the nature and amount of the epithelium which they contain show us the special part of the nasal cavity from which they come. The upper regions of the nasal cavity are covered by a cylindrical epithelium, which mostly shows two or three layers of cells upon each other. The lower regions of the nasal cavity are lined with a ciliated cylindrical epithelium, the action of which removes particles of dust and fluid towards the nares. When the discharges are purulent, they consist of pus-corpuscles only, with hardly any admixture of cylindrical epithelium. This characterizes chronic ulcerations. When chronic ulceration becomes extended, the pus is mixed with cylindrical epithelium which is thus being shed. When the affection of the nose is quite recent no pus, but only cylindric epithelia are perceived in the secretion. In case of old follicular ozæna, the crusts have on their soft surface a villous appearance, being condensed mucous, indicating in some slight manner the distribution, size and shape of the mucous follicles of the Schneiderian membrane. In cases of ulceration with rapid loss of substance, as in specific disease, elements of the deeper tissues, particularly fibrous structures, can be perceived in the discharges. When the nasal cavity is the seat of villous or other descriptions of ulcerated cancer, the elements of these tumors are found in the blackish brown, thick, and extremely fetid discharges.

Of the medicinal solutions to be employed for the cure of this disease, they are numerous, and I will mention but a few of the most important. Of the solutions already mentioned, they should be used preparatory to those of a more active and energetic nature, though these act in a measure as alteratives, resolvents and escharotics, but in themselves seldom constitute a sufficient medical application. As the best astringents, belong the solutions of alum, sulphate of copper and sulphate of zinc. As the most suitable alteratives, the solutions of bichloride of mercury and nitrate of silver, and the solutions of chloride of calcium, in which suboxide of mercury is suspended in a finely subdivided state, together with the bichloride solution. As the best specifics, of stimulating solutions, a mixture of eau de Cologne with soft water is sometimes useful.

Here let me enter a caveat against employing our solutions too strong in the commencement of our treatment, individuals differing largely as to their capacity in this respect. In some the irritability of the nasal cavity is but slight, whilst in others the sensibility is very acute. The same principle involved in gonorrhœa as regards the treatment by injections is also applicable in this disease, and that is, it is always best to commence with weak solutions gradually increased; and in most instances it is beneficial to vary the injections; the mucous membrane appearing to become accustomed to the same stimulant after a time, and thus not being impressed by it in a proper manner, I therefore reiterate that we should use the greatest caution as

regards the strength of our solutions employed, whether the case be of long standing or of but recent date.

A healthy surface can readily defend itself against irritating chemically-impinging substances by means of a copious flow of mucus, but not so with excoriated or ulcerated parts, for they lack this power of rapid secretion, and are consequently much more easily affected by medicinal solutions than the healthy surface of the nasal cavity. Therefore it is always best in the beginning of our treatment to use dilute solutions of medicinal substances, and their strength to be gradually increased as we proceed in our treatment--as the mucous membrane becomes accustomed to its action.

As an alterative, and standing at the head of the list, is a solution of the nitrate of silver, not using more than a grain to the ounce of water, therefore to a quart of water we may add from twenty to thirty grains of this salt; to this we may add about half an ounce of the nitrate of soda. I have received more satisfaction from this solution than from any of the others. Sulphate of zinc, using from a scruple to a drachm to the quart of water, adding half an ounce of the sulphate of magnesia, forms a good wash; also acetate of lead, in about the same proportion as the former, using in conjunction the acetate of soda. The bichloride should be used with caution, employing not more than five grains to the quart of water, to which should be added an ounce of common salt.

The phagedenic waters – black and yellow wash, to which common salt has been added—are occasionally very useful.

As a sedative solution the following has been advised—of prussic acid forty minims to the quart of warm salt water, of tincture of opium two drachms, may be taken; this goes well with the alum and common salt solutions, but is incompatible with the metallic salts.

A solution of alum is spoken very highly of by Dr. Thudicum. He uses a solution of an ounce of roughly powdered crystalized alum dissolved in a small quantity of hot water, and the solution made up to one quart of cold and tepid water in such a manner as to insure that the temperature of the solution should be below but near to blood heat. "In superficial ulceration or blenorrhagic conditions this solution is well borne. Ulcerated parts, which before its application were red, mostly appear as white patches after its application, thus showing that the effect of the alum on the ulcerated surface has been considerable." The permanganate of potassa may be used in conjunction with the above, when smaller quantities of the solution are desired to be used.

As a hæmostatic or styptic solution, ice cold salt water often answers most admirably; but when this fails I give the preference to Monsel's solution, the Liquor Ferri Subsulphatis, using half an ounce to a pint of cold water. I prefer this to all the other preparations of iron, for the reason that it is less irritating.

This concludes what I have to say about Rhino-therapeutic

pharmacy, and I would like some of the members to give their experience in this department of therapeutics.

Scrofula, as we know, is a blood disease manifesting itself in a great variety of organs. The skin is unusually delicate and irritable; the mucous membranes partake of the same delicacy and irritability as the skin. The edges of the eyelids are often red and swollen, the eye is apt to be attacked by a peculiar inflammation, the tonsils are frequently enlarged, cold in the head, hemorrhage from the nose are frequent—in fact, the whole system is deficient in stamina. The lining membrane of the external ear frequently becomes swollen and painful, giving rise to purulent discharges which, when chronic, are apt to be exceedingly offensive; so it is with the mucous membrane of the nose, which is commonly thickened and frequently inflamed, so that strumous patients ordinarily breathe with the mouth open. Sometimes ulcerations of the mucous membrane takes place, and the bones beneath become carious, causing a horribly fetid odor. This constitutes scrofulous ozæna.

Scrofulous ozæna generally begins in early life. It may be a consequence of the catarrhal variety, or may follow some fever, or may begin spontaneously. Scrofula is often called into action by the debility induced by previous diseases, such as measles, scarlatina, hooping-cough, &c., and hence we not unfrequently see ozæna developed after these affections. This disease is generally insidious in its approach, and is but seldom attended with acute pain or symptoms of inflammation, and is very difficult of control by medicines, tending to the destruction of tissue by slow ulceration and defective in the process of repair.

As the most powerful occasioning cause of scrofula seems to be due to mal-nutrition and mal-assimilation, and a disregard of those hygienic laws so common amongst the poorer classes, such as improper food, deficient nutrition, &c., and to over-feeding and over-stimulating of the digestive organs amongst the children of the wealthier orders of society, which lead to a chronic irritation of the mucus membrane of the stomach and interference with the digestive powers, and consequently with nutrition. Philips, in his Treatise on Scrofula, shows that the immediate cause of this disease, when conjoined with the injurious effects of a confined impure air, may be considered as sufficient to occasion the disease in those cases in which no predisposition to it exists, and greatly to develope any hereditary tendency to it in the system. Therefore, as of first importance in scrofulous ozæna—for we generally find that the disease is most frequent amongst children, particularly half-grown girls—is to place them in as proper á hygienic condition as possible. The food should be simple and unstimulating in its nature, but at the same time nutritious and abundant; the child should be in the open air as much as possible, and be made to take a reasonable amount of exercise.

Remedial agents are to be sought in the preparations of iron, quinia, and the tonics generally, which act by improving the

tone of the system at large. Our reliance, however, as curative agents, is to be placed in arsenic, and iodine, and iodide of potassium. The iodide of potassium may be given in some bitter infusion, but for all purposes—and this seems to answer all indications so far as medication is concerned—in the following combination, from which the most satisfactory results have been obtained; viz.: ℞—Iodine grs. viij., potassii iodidi ʒiss., liq, potassæ arsenitis ʒss., syrp. simp. ℥viiss. Misce et signa. Teaspoonful two times daily. I would here state that this prescription was used to a great extent by the late Dr. Stone as an alterative and tonic. I am prepared to state positively that arsenic, given for a considerable length of time, is one of the most reliable preparations that we have for the treatment of scrofulous ozæna, combined as in the above prescription. Arsenic improves the nutrition in a remarkable manner, but as the processes of renovation which depend upon nutrition are slow, we must continue its administration in small doses for a considerable time.

Along with this treatment we are to apply our local remedies, as have already been described; viz., to cleanse the nasal cavity out well morning and evening, using some disinfectant and astringent for that purpose, and here I prefer the chlorinated soda and permanganate of potassa. When the disease has lasted for years, attended with superficial erosion of the mucous membrane, the following liniment pencilled over the part once a day answers well, viz., fifteen grains of chloride zinc to one ounce of olive oil. The citrine ointment may also be used for this purpose, applied occasionally. The insufflation of calomel, conjoined with an injection of bichloride of mercury, is extolled by M. Trousseau.

I will relate two cases of scrofulous ozæna which I have lately had under treatment—the first, a girl of 17 years of age, whose case is as follows. The disease was first noticed on her when she was but three years of age. I may here state that her mother had been afflicted the same way for many years. She first had an attack of ordinary catarrh, but this in a short time ran into ozæna, and had continued more or less constant up to the time I first saw her, which was about five months ago. She had been using the nasal douche for some months before she came to me, and with but varying success. I immediately put her upon the iodine and arsenic mixture already alluded to, using as a local application the permanganate solution and nitrate of silver, alternating between the two as one seemed to lose its effects. She has now been upon this treatment steadily, and with the most marked benefit, the offensive discharge and odor from the nose having entirely ceased.

The second case, a girl 14 years of age, came under my treatment a little over two years ago. The girl's mother first noticed in her room a smell which she thought was that of a dead mouse, and looked in all the bureau drawers and under the bed, expecting to find this as the cause of the vile effluvia. But one morn-

ing, upon awakening her daughter, she discovered that the smell proceeded from her daughter's nose. Some five or six months previous, her daughter had a severe attack of diphtheria, which was no doubt the developing cause of the ozæna, combined with a marked scrofulous diathesis. I commenced using the permanganate solution at once as a wash, and put her upon the iodide of potassium and the syrup of the iodide of iron. She seemed to improve under this treatment, which was of six weeks duration, when I left the city and lost sight of her until seven months ago, when her mother brought her to me again for treatment. The case was then in its worst form. Large casts of inspissated mucus of a grayish yellow color, and of a granular appearance, and highly offensive, were being discharged every morning. The cervical glands of the neck were swollen, and her face was covered with acne. She was also suffering from intense pain in the forehead, with some lacrymation. Her health was generally bad, she being rather anæmic, and her skin of a dusky hue; her menstrual flow was also very irregular. I put her on the mixture of arsenic and iodine already alluded to, this being the first case in which I thought of trying the combination.

I resumed the use of the permanganate, but after awhile, finding that it failed to answer as well as when first used, in its stead I used the nitrate of silver, of the strength of three grains to an ounce of water. The first application burnt her rather severely, causing considerable pain in the head and lacrymation. I then reduced it to the strength of a grain to the ounce of water, and used this solution for about a month, when I would alternate with some of the other washes. Under this treatment she has improved wonderfully, gaining in health and strength, the acne and swelling of the glands have entirely disappeared, her skin has lost its dusky hue, and she is now perfectly regular in her catamenia. At the time of writing this there is not the least discharge or smell perceivable.

These are the only two cases of scrofulous ozæna I have had, but I think they well illustrate the principle to be carried out in their treatment. An unusual feature in this last case is; that the sense of smell has been entirely destroyed, due, probably, to destruction of the olfactory cells, the strongest solution of ammonia being barely perceptible when held near the nose.

It is not unfrequent that after the cure of chancre, and when it is supposed that all is ended, and that the disease has been mastered, we find that the patient experiences a kind of stuffing of the nose, which if not soon attended to, runs into a coryza. Chronic catarrh occurring in an individual laboring under a syphilitic taint, should receive our earnest attention; for the discharge when continued for some time is likely to become opaque, profuse and ropy, assuming a greenish yellow color, accompanied generally by a sense of obstruction or stuffing, as if a tumor had formed within, and is not unfrequently

attended by a change of voice, headache, and impairment or abolition of the sense of smell.

The affection appears to extend to the lining membrane throughout all the windings of the nasal cavities, thus by the superior meatus it is propagated to the posterior ethmoidal cells, and by the middle meatus to the anterior ethmoidal cells, as also to the maxillary and frontal sinuses. If this be persistent for any length of time it is liable to terminate in suppuration, with caries and necrosis of the nasal bones and cartilages. Now, in patients of a delicate or strumous constitution we are very likely to have ozæna as a consequence, with caries and necrosis of the vomer and perpendicular plate of the ethmoid and nasal bones, with destruction of cartilage. As a consequence of this we sometimes find sinking of the bridge of the nose, when the bony lamellæ of the ethmoid cells are destroyed the nose and mouth become transformed into one cavity. According to M. Trousseau, one of the most characteristic and earliest signs of the appearance of syphilis in the infant is coryza, the first symptom noticeable being a discharge of mucous, followed eventually by a serous and purulent discharge terminating in caries and deformity of the nasal bones. The mucus membrane will be found to have assumed a fungous character, with considerable redness, and the discharge of a thick, yellow, purulent matter, at times mixed with erythematous crusts of a brownish black appearance. Patients laboring under this disease generally have what the French call the *nez crasè*, or flattened nose.

The treatment consists as follows: Local measures are to be adopted as in the other forms of this affection, for the purpose of removing crusts of inspissated pus and mucus, and as a promoter of healthy action in the diseased parts. For this purpose some of the phagædenic washes are to be used, or a solution of the zinci chloridi, not using more than from half a grain to one grain to the ounce of water. Some of the washes already alluded to may occasionally be used with benefit. Pencilling the parts with the ungt. hyd. acid. nit. answers as a good stimulant. The inhalations of mercurial vapor and the vapor of iodine are recommended by some, but I prefer the means aforementioned.

Our chief reliance is, however, to be placed in the preparations of mercury and iodine as curative agents. It has been asserted by some that caries and destruction of the bones seldom or never occur except where mercury has been administered. This I do not believe to be the case, for destruction of bones may and does occur even where no mercury has been given. Of course I do not doubt but that where it has been used indiscriminately, and therefore *abused* it has done *harm*, and aggravated the complaint. But where it is used with judgment and discrimination it is capable of being of the highest value, both as an alterative and curative agent. Mercury is of especial value in those cases where we have nodes (periostitis and ostitis), with wandering pains

about the bones generally. Its administration in these cases, in small doses, till the system becomes gently impressed by the mineral, short of ptyalism, and then followed by small doses of the iodide of potassium in some bitter infusion or tincture of bark, constitutes the most appropriate treatment. When the powers of the system are prostrated, and the patient is anæmic and exhausted by the severity of the poison, or when conjoined with a scrofulous state of the system, our treatment must then consist in building up our patient by the administration of tonics, such as the chalybeates, the potassio-tartrate of iron and the iodide, the preparations of bark, chlorate of potassa, hypophosphites and cod liver oil. After we have succeeded in improving the general tone of the system, we may resume the treatment mentioned above (i. e., mercurials).

But I will state that in all cases of ozæna, syphilitic or otherwise, *patience* and *persevering* treatment are essential to success.

On the Use of Chloride of Ammonium in Chronic Hepatitis. By L. F. Salomon, M.D.

Gentlemen—In bringing before you the chloride of ammonium as a remedy in the treatment of affections of the liver, I merely wish to state my experience in its use, for, as you are well aware, it is a remedy which has long been recommended, but which, in my opinion has not received the attention which it merits. I was led to its use by an article in the *Lancet* for August, 1870, by Dr. W. Stuart, of the East India service.

But, before proceeding farther, let us speak of the disease under consideration.

We find in our text-books very little said in regard to chronic hepatitis. It is either treated of as cirrhosis, or passed over with very little mention, as something demanding no consideration. I am unaware whether this is due to negligence (which is hardly possible), or whether it is because authors living in climates different from ours have never had their attention directed to the disease which I am about to describe, and which happens very frequently in our climate and demands energetic treatment

It may depend on two causes. Dr. Stuart, in his able article, says: "When acute hepatitis occurs in an individual of good diathesis, and is seen early and met by judicious treatment, the symptoms, both local and general, will for the most part disappear and the patient be restored to health. It is found, however, in actual practice, that in consequence of bad diathesis, advanced age or other causes, recovery by resolution does not take place, suppuration occurs, and hepatic abscess is formed." This is in India; but let us see what takes place in this climate, where we know that, although hepatic abscess does sometimes occur, it is not by far as frequent as in that tropical country. We know

from observation that hot climates are favorable for the production of abscess of the liver.

Although in our more temperate climate hepatic disease is not so common, still it is my opinion (and I give it for what it is worth) that here many cases of acute hepatis (especially during our last mild winter), instead of terminating in suppuration, take on a chronic form of inflammation which gives rise to all the symptoms which determine chronic hepatitis. Of these symptoms I shall speak further on.

Another cause of chronic hepatitis is malarial fever. Of this the authors whom I have been able to consult make no mention except, I believe, Niemeyer, who merely states it to be a fact without giving any explanation of it; and as I have treated lately two cases of undoubted chronic hepatitis, attributable to no other cause, I do not think I am far wrong in stating that intermittent fever, by the continual recurrence of internal congestion and hepatic engorgement, is one of the most fertile sources of chronic hepatitis in our climate.

I wish it to be distinctly understood that I am not speaking of cirrhosis, or granular liver, where the size of the organ is very seldom if at all affected, and in which no symptoms occur pointing to hepatic disease until dropsy is manifested, but to an affection of the liver presenting the following symptoms, which when taken as a whole leave no doubt of our diagnosis.

You are called to see a patient who has been or is the subject of malarial fever, or who will tell you that some months or even years ago he had an attack of acute hepatitis which did not terminate in suppuration. The following symptoms will be found most prominent. Patient complains of a dull transitory pain in the right hypochondrium, which is aggravated on pressure, coupled with a dragging sensation in the right side. The bowels are as a rule constipated, evacuations, when they do occur, being clay-colored, tongue furred and heavily coated, constant feeling of nausea with loss of taste, sometimes to such an extent that the patient does not relish the most palatable food, everything that is put in the mouth having a bitter taste, want of appetite, and occasionally jaundice. Always a sallow complexion. Whenever you see an individual presenting all these symptoms, especially after a long series of malarial paroxysms, you cannot fail to diagnose the case as one of chronic hepatitis.

As I have had no opportunity of making an autopsy in a case of the above type, I am unable to state the pathological appearance of the liver in such cases.

Having described in as short space as possible the disease, I shall now quote from Dr. Stuart's article what he says in regard to the muriate of ammonia in suppurative hepatitis, and which led me to use it in the chronic form of hepatic inflammation. The medicine was given in twenty-grain doses morning and evening. He says:

"As a general rule, about fifteen minutes after taking the

medicine the patient experiences a sense of warmth in the epigastrium, which by and by extends over the whole cutaneous surface. The nervous system is exhilarated * * * for the patient now feels light-headed. The acute pain previously experienced in the right hypochondrium * * * is either entirely removed or in its stead pain is referred to a point higher up and toward the base of the axillary region, where before none was complained of. * * * At this stage of the operation the patient sometimes falls asleep, relieved of all distressing symptoms. After the lapse of another quarter of an hour, a free and equable perspiration takes place over the entire surface, which lasts for a period varying from one to two hours, and gradually subsides. In the meantime the pain * * * will again manifest itself at or near its original position, or may be referred to one totally different, as the lumbar region, or even the right hip * * *

"With the evening dose similar effects will be observed to take place with like regularity and certainty; and with each succeeding one relief from pyrexia, pain, etc., will gradually become longer, till at length, in favorable cases, the relief becomes complete and constant.

"After several doses of the medicine the urine is much increased in quantity, is limpid, and is passed without uneasiness.

"After a few days the appetite is much improved, the patient craving for more food, which may be given provided it be light and nutritious; but solid food should on no account be permitted, as its ingestion would in all probability provoke a return of the acute symptoms."

Having given at length the effects of the medicine, as described by Dr. S, I shall now proceed to report two or three cases of chronic hepatitis which I treated with this remedy.

Case I.—Had been treating this patient for over a year for obstinate intermittent fever, until January last, when patient called my attention to a pain and dragging sensation in the right side. Examination elicited the following symptoms. Liver enlarged to such an extent as to encroach upon and occupy a portion of the epigastric region, with an almost equal amount of enlargement in a downward direction. The left and lower borders of the liver were hard to the touch. Skin and conjunctiva very yellow (jaundice). Patient complained of increase of pain on pressure. The bowels were constipated. She was unable to take much food at a time, on account of vomiting following the ingestion of even a moderate quantity, due, no doubt, to the mechanical pressure of the enlarged organ on the stomach. There were no symptoms pointing to gastric disease. Tongue heavily coated and taste lost, everything which was put in the mouth having a bitter taste. Patient was unable to lie on her side, owing to the dragging sensation and increase of pain whenever that position was assumed.

I administered mercurial purgatives (and I will here say that

it is astonishing what an amount of purging a patient in this condition can tolerate), and placed her on tonics (steel and bark). This treatment was continued for nearly two months, during which time a blister was applied, and the skin over the liver kept irritated with tartar emetic and croton oil, without any marked benefit except that under the continued purgation the skin and conjunctiva cleared up, and the heavy coating on the tongue partially disappeared, when I began the administration of the muriate of ammonia.

February 21st, at 7 p. m., gave 20 grains. Patient experienced the train of effects recorded above.

February 22d, at 8 a. m., took another dose of 20 grain, which was again repeated at 4 p. m. After each one of these doses patient complained of excessive heat, telling me that she had had a fever.

Patient being very weak, I now stopped its administration until February 25th, when I directed the medicine to be given in ten-grain doses thrice daily. From this time she continued to improve under its use until March 1st, when I stopped the muriate altogether. The pain, etc., had entirely disappeared, bowels were acting regularly, and tongue clean.

During the administration of the first three or four doses the urine was not increased in quantity, but was loaded with bile; but under the continued use of the remedy it gradually became clearer.

This patient died of a congestive chill two months afterwards, up to which time there had been no return of the liver trouble, she being perfectly free from all pain in the hypochondrium and able to lie on either side with comfort, whereas, before the administration of the chloride of ammonium she was not a day, scarcely an hour, without pain.

Without entering into a repetition of details, I will state that I treated, about the same time, another patient suffering from chronic hepatitis, who presented the same history (chronic malarial fever), and in whom the muriate of ammonia, administered in doses of ten grains three times daily, was attended with good results.

I have now under treatment a lady aged 50, who states that several years ago she had an attack of inflammation of the liver, which subsided under the use of calomel pushed to ptyalism. She suffered no more with her side until about four months ago, when the pain in her right side returned, but instead of being a sharp pain as she experienced before, it was a "dull, heavy" pain, accompanied with a dragging sensation.

Examination elicited the signs and symptoms related in case 1, which it is unnecessary to repeat here. Instead, however, of marked jaundice, the skin was of a dusky hue.

My diagnosis, as in the previous cases, was chronic hepatitis.

She has taken, up to the time of this writing, six doses of the muriate of ammonia of 20 grains each, and has experienced

marked relief under its use. The bowels, which before were constipated, are now acting freely; the sallow complexion is fast disappearing, and the pain, instead of being referred to the hypochondrium, is sometimes felt in the lumbar region and again at the angle of the scapula. The patient, without any leading question being put, has stated this to me, and moreover, has expressed to me in her own language the train of symptoms produced by the medicine—heat of surface, perspiration, etc.—as described above.

I have not entered into an elaborate description of these last two cases, because the effects of the remedy have been similar in all three cases, and it would only be trespassing on your time to repeat what has been detailed before.

As to the mode of action of the remedy, I am not prepared to say much. I can only suggest that it acts as an alterative and resolvent, and tonic on account of the chlorine it contains, and being a laxative, by causing a freer flow of bile relieves the liver of its surplus.

It is claimed, however, to be a specific in affections of the liver, for Dr. Stuart, in the article referred to, after describing the train of effects following its use in hepatic affections, says: "In some cases which I have known it tried either as an experiment in health or used as a remedy in diseases other than hepatitis, it has either been without appreciable effects or, if any, these have been but slight and not characteristic," and this "seems to point to the inference that the medicine is not only a specific in certain hepatic affections, but that its peculiar action being manifested in any given obscure case may be considered diagnostic of hepatic disease."

This Dr. S. puts forth merely as a suggestion, but if it be a fact that the train of symptoms above mentioned follow the use of the chloride of ammonium only in hepatic affections, and in all my cases they did not vary. I think I may claim for it a specific action.

As for myself I can only say that the relief was so prompt in every case in which I have used it, that I shall always resort to it in future before using other remedies.

If the experience of others will sustain what I have said here, the remedy will be rescued from the oblivion in which it has been for years.

At any rate I think it merits a further trial.

Purpural Small-Pox. By E. S. Lewis, M.D.

Mr. President and Gentlemen: I have selected as the subject of this communication that form of variola which is sometimes designated as purple, black, or hemorrhagic; a disease not surpassed in malignancy and in shortness of duration by any of the

exanthemata. It is ushered in by rigors, pain in the head and back, of varying intensity, thirst, irritable stomach, restlessness, insomnia and febrile reaction. In a few hours there appears on the surface of the body an eruption of a bright red color, sometimes like measles, but more frequently resembling scarlet fever, which disappears on pressure, and returns again slowly; whilst the mucous lining of the tongue, roof of mouth and gums become covered with a uniform whitish exudation, which gives it the appearance of being painted. On the third or fourth day violet colored spots like purpura are seen, due to cutaneous hemorrhage, more numerous on the dependent portions of the body, and in some instances confluent; presenting the remarkable spectacle of a generally diffused violet or purplish color of the integument, which contrasts strikingly with the red hue it supplants. At the same time hemorrhages are apt to occur from the mucous membrane of the mouth, nose, lungs, alimentary canal, and kidneys rapidly exhausting the patient. The character of the pulse is soft and compressible, not frequent at first, from 100 to 110 pulsations to the minute, but increasing as the disease progresses. The temperature is never high, attaining its utmost elevation on the second or third day, when it ranges between 102° and 103° F., after which it steadily falls until the termination of the case. The mind is unimpaired at first, and may remain so throughout, though occasionally delirium, coma, anxiety, difficulty of breathing, without any appreciable thoracic lesions, supervene. Its invariable termination is death, and, as in most of the malignant fevers, this takes place between the fourth and fifth days, or on the fifth day, sometimes occurring suddenly from cerebral congestion or hemorrhage, or from one of the nervous accidents mentioned. This disease is of the adynamic type, and chiefly attacks those whose vital powers are feeble or below par; therefore, old age, the puerperal state, a syphilitic or scrofulous taint, are the most powerful predisposing causes.

As an early recognition of this affection is of the utmost importance for promoting measures to prevent its extension, and as it is frequently mistaken for scarlatina or rubeola, a description of the points of difference between them would not be out of place, nor of the essential features by which it is marked.

Of all the exanthematous diseases, observes Trousseau, not one presents a more general elevation of temperature than scarlet fever, in which it ranges from 106° to 106°5′ F, this elevation, according to Wunderlich, being parallel to the intensity of the exanthem, and its high ruling course distinguishing it from the other eruptive affections. In variola nigra, as was mentioned before, there is a want of correspondence between the elevation of temperature and the intensity of the exanthem; and the surface heat, which in scarlet fever is dry and burning, does not appear in this form of variola to be much greater than is normal. The pulse also differs, being in the one compressible and not fre-

quent, whilst in the other (scarlet fever) its extreme frequency is regarded by Troussean as a diagnostic feature even in the absence of any eruption; but notwithstanding the wide differences in the symptoms enumerated, there is another which is pathognomic of purple small-pox. I refer to the whitish exudation on the mucous membrane of the mouth, of which no mention is made in any works on this subject; the accounts given of this disease being exceedingly meagre and unsatisfactory. The tongue in scarlet fever is at first coated yellowish white, with its edges and tip red, but after a few days, the whole organ is red and tumefied, with its papillæ prominent: besides, in malignant scarlet fever delirium is rarely absent, the mental disturbance being as great as in the most severe forms of typhoid fever, whilst in this disease, when present, it is the harbinger of death. In concluding, I would state that a patient presenting on the body a rash like measles or scarlet fever, with a pulse full, soft and not rapid, a temperature not exceeding 103° F., and a uniformly whitish coat covering the entire mucous membrane of the mouth, is laboring under unmistakable hemorrhagic small-pox, and the invariable result is death. I would add that regarding its resemblance to rubeola, the absence of the catarrhal element would still further aid the diagnosis.

The following cases here reported were typical cases, and presented most of the symptoms which have been described.

Observation I.

On the evening of May 16th, I was called upon to visit Mr. F., aged fifty-seven years, and a resident of the Third District of this city. From his statement he fell sick on Saturday night, May 14th, with headache, pain in the back, fever, restlessness and frequent vomiting. On the following morning he found his skin of a bright scarlet color and his face swollen, which led him to think he had erysipelas, conjoined with rheumatism from which he was seldom free. Being constipated, he took a dose of colchicum, and the next morning, not feeling better from the purging and the sleepless night he had passed, concluded to send for me.

When seen at about 5 p. m., his skin was of an intense red color and his face somewhat tumefied. His symptoms had not changed from the account already given, except that the prostration was greater and an occasional moisture bedewed his skin. The febrile reaction was not very great as indicated by the pulse, which was extremely compressible and numbering 110 pulsations to the minute, nor by the thermometer placed in the axilla, which showed a temperature of 102° F. Upon examining his tongue I found a white exudation looking like white paint, covering it and the mucous membrane of the mouth generally. He was perfectly rational, showing no nervous disturbance excepting great restlessness and insomnia. I prescribed sulphuric acid and

cinchona, and directed that stimulants and nourishment be freely administered to sustain him.

May 17th. Found him much worse, pulse extremely frequent and feeble; a nervous delirium had set in from which he could be roused when questioned; tongue becoming dry and brown, and on his back, breast and arms, purpuric spots had commenced to appear. His kidneys performed their function and the urine voided was scant and high-colored. This patient gradually sank, the purple spots became more numerous; blebs had formed on different parts of the body filled with a bloody serum, and at about 9 a. m., May 18th, he died.

Case II.

F. B., aged 22; of scrofulous diathesis; fell sick January 11th with fever, pains in the limbs and back, headache and thirst. I had treated him some months previous for pneumonia with typhoid symptoms, from the effects of which his health remained impaired. Saw him for the first time January 13th, and immediately suspected the nature of his disease, from the intense erythema of the skin and the prevalence of variola in the adjoining house. His eyes were blood-shot; his pulse 100 to the minute; temperature 102° 5′ F., and covering the mucous membrane of the mouth, the characteristic white exudation, which resolved all doubts as to the diagnosis.

January 14th. Hardly recognized my patient, for the color of his skin had changed to a dark violet, the result of hemorrhage in the tissue of the skin. His mouth was bloody, and a bucket by the side of his bed contained a large amount of blood which he had discharged from his bladder and bowels. His pulse was very frequent and weak; his respiration hurried and difficult. This continued until death occurred early in the next day, May 15th.

Case III.

Mrs. C., aged 55 years, residing in a house adjoining that of a family where, two weeks previously, I had treated five cases of variola, fell sick April 2d, but thinking it was a cold, only sent for me the next evening, April 3d. As regarded the temperature, character of pulse and tongue, there is nothing I could add to the description given in the cases just related. The only difference was in the mode of dying. In conversing with her daughter, who had a few moments before informed her of her approaching end, and in full possession of her faculties, she was suddenly seized with a slight convulsion and in a few seconds was dead.

In this communication I have endeavored to confine myself to a simple relation of facts as presented to my observation, deeming that such would prove of more interest to the Association than any speculative dissertation in regard to the causes inducing such a deviation from the natural course of variola.

EXCERPTA.

[In the *New York Medical Journal* for July, is published a paper by Thomas Addis Emmet, M.D., entitled "*The Philosophy of Uterine Disease, with the Treatment Applicable to Displacements and Flexures.*" This paper is so filled with sound philosophy and judicious principles of practice, that we regret our inability to lay it before our readers in its entirety. We cannot, however, afford to give the space which so long an article would require, and must therefore cull from it such extracts as exhibit the salient points both of philosophy and treatment.]

It would be difficult to present a subject on which a greater diversity of opinion exists in the profession than the one I now have the honor of offering for your consideration.

This difference is not only as to cause and effect, but to as great an extent in relation to the proper means of treatment. Indeed, at first sight, this great difference is incomprehensible, and it seems impossible to reconcile the extreme views held by men of honest purpose and equally based on personal observation. But experience has long since taught me that a medium course is the most successful one, as from this stand-point we can utilize and better appreciate the views based on the practice of either extreme. In years past I have honestly overcome all obstructions, and by the aid of the knife I have opened up the uterine canal to such an extent that it was impossible for any mechanical obstacle to exist; and I did not cure my patients. In turn I have devoted no little mechanical skill to overcome every displacement, and have succeeded beyond my expectations; yet the results were not satisfactory. At length I became confident that local congestion and inflammation were the causes of the evil, producing hypertrophy, hyperæsthesia, versions and flexures. I directed my treatment now exclusively to relieving the congestion, as I will describe hereafter. My results were better, for I found that as I diminished the congestion, the hyperæsthesia disappeared, the flexures lessened in degree, and there was, with less hypertrophy, a great improvement in the versions; but my patients frequently relapsed, and the results were not such as I had anticipated. I ultimately recognized the fact that there was a stage in the treatment of almost every case when mechanical support was indispensable as an adjunct, and under certain circumstances the use of the knife could not be ignored. The fact also presented itself to me that in a large number of cases the local condition was secondary to that of the general system, and in no case could we conscientiously overlook the connection. I became convinced that, as a rule, the local difficulty in the beginning was the effect, but a point was reached

ultimately when the uterine condition became the most prominent, and exercised a direct influence in reducing still further the tone of the general system. In addition, experience taught that we cannot restore the female to health by local treatment alone, nor by devoting our attention exclusively to the general condition can we relieve the local one. I regard some degree of anteversion as a normal position of the uterus, while retroversion is always incident to, and a flexure is an exaggeration due to causes to be considered hereafter.

Any conceivable amount of deviation from a normal standard may and does frequently exist without discomfort, so long as the circulation remains unobstructed and the proper functions are performed. Deviations may result from congenital causes, or from accident, and a degree of tolerance may become established which ultimately seems to be a normal condition for the individual. Yet there is in each case a limit to the immunity, as sooner or later Nature exacts the penalty. A retroversion may exist for years without producing inconvenience, but from some accidental cause the general health may become impaired; the vaginal walls will gradually lose their integrity, a miscarriage, or some other cause, will produce an undue congestion, with enlargement of the uterus, and we have at length the most urgent symptoms presenting themselves for relief. There is less immunity from flexures of the body of the uterus, yet a moderate amount of disease may be borne so long as the general health remains good. A flexure of the cervix below the vaginal junction, at a right angle even to the body, or a partially constricted os, may produce but little discomfort for an indefinite time, beyond a slight dysmenorrhœa in the unmarried female; but in the wife, if the condition has produced sterility, we are at length forced by a train of nervous symptoms to recognize a local cause of irritation. In other words, anteversion may exist without causing irritation of the bladder, retroversion without symptoms of prolapse or obstruction to the rectum, and flexure without dysmenorrhœa, so long as the nutritive functions maintain their integrity.

Impaired nutrition, as the cause and not the effect, depreciates the nervous force, and without this needed stimulus functional derangement naturally follows, a loss of balance results, and we have congestion or the opposite condition. Congestion produces enlargement of the tissues, and it may remain passive or result in inflammation, while a want of nutrition causes atrophy. Congestion is always the result of some local irritation, a condition which is but temporary in duration if the reparative powers are in a state of integrity. Congestion, however, does not imply inflammation, although the latter cannot have a beginning without it. If the congestion reaches a degree sufficient to establish inflammation, we have at once instituted a distinct train of symptoms which is accidental and secondary to the primary condition. The terms congestion and inflammation are synonymous

with many in application to uterine disease, and it leads to confusion. Inflammation cannot exist without molecular death, and its products are easily recognized until the injury has been repaired. We may look in vain, *post mortem*, for any evidence of a previously-existing endometritis, so called, or ulceration of the cervix, as it is termed. We find the tissues blanched, the blood from the capillaries having passed into the larger vessels as the heart failed in keeping up the supply, but there is neither loss of tissue on the surface of the mucous membrane beyond the epithelium, if so much, nor are products of inflammation to be found in the tissues of the organ itself. Inflammation can only exist in an acute form, although its products may remain for an indefinite period. Therefore the term chronic inflammation is a misnomer.

From congestion and increased weight the uterus will naturally settle into the pelvis, and a version mechanically results toward the heavier side. The neck of the uterus soon becomes, as it were, a fixed point, and can move but forward in the axis of the vagina, while the body above becomes bent on itself in the opposite direction. Hence a flexure of the body, a chordee, as it were, to be augmented by any increase or obstruction to the circulation. Violence or an accidental cause may produce partial retroversion, and, if the fundus advances into the hollow of the sacrum beyond a certain point, we have at once two forces operating, in opposite directions, to produce a retroflexion. The cervix becomes pressed upward against the anterior wall of the vagina, which can only yield to a certain degree, and, with the weight and downward pressure in the opposite direction, the body of the uterus is gradually bent upon itself. We have two causes of flexure which may be termed congenital, or at least having an origin previous to puberty. As the uterus becomes developed, the growth of the cervix is, in length, out of proportion to the body. Gradually, from a want of room, the cervix slides along the posterior wall, in the direction of the least resistance, until it presents in the axis of the vagina, causing a sharp flexure just at the vaginal junction. In a narrow vagina, with a deficiency or absence of the posterior *cul-de-sac*, a degree of retroversion must exist. At puberty, with increased weight of the uterus and from other causes, the fundus gradually settles into the hollow of the sacrum, and we have a flexure produced in the same manner as I have shown to follow this displacement when resulting from accident.

Congestion of the mucous follicles, limited to the cervix or extending through the uterine canal, with increased secretion and some enlargement of the organ, is the most common form of uterine disease. In my experience, some degree of flexure of the body, with an increase of anteversion, has been more frequently met with than in the displacement backward. Partial obstruction of the uterine canal, at a given point above the vaginal juncture, is caused by flexure, and is increased by congestion,

with passive œdema of the mucous membrane and possibly of the submucous tissues. A resort to surgical means for the relief of this condition has been a favorite procedure with many in the profession. For years I have not divided the cervix laterally except for the removal of fibroids At an early date in my experience, I satisfied myself that a flexure of the body could not be relieved by a lateral division, even if extended to the vaginal junction, and that the practice was based on unscientific principles. I have never seen a case permanently benefited by the operation, except in rare instances, where pregnancy fortunately took place during a slight remission of symptoms, due to the revulsive action attending the process of reparation. I can, moreover, state that I have never known the mal-practice of any other surgical procedure followed at times by such evil consequences. When practised by skillful hands, under proper surroundings, and with the requisite after-treatment, the operation is attended with comparatively little risk, but without benefit. It has been regarded by the profession at large as a simple operation; and it is certainly one in execution, but it requires no little experience to decide when it can be practised with safety, even when it is advisable. I believe that there are but few of us who have not in years past seen the most deplorable results following the indiscriminate practice of this operation, through reckless inexperience and neglect afterward.

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When a flexure of the body has long existed, the tissues, at the point of greatest constriction, gradually undergo fatty degeneration from pressure, and absorption takes place, causing a permanent deformity, as after caries of the spine. When a point has been reached, after careful treatment of such a case, at which all tenderness on pressure has been removed, it is often judicious to divide the cervix backward, and to incise forward the seat of flexure above, sufficiently to open the canal. This will facilitate the application of any after-treatment which may be found necessary to the canal above, and guard against a relapse from any mechanical obstruction afterward. But, if done too soon, without the proper preparation and the requisite subsequent care, all previous gain will be lost by pelvic cellulitis, and even general peritonitis may result. The patient's life may be saved for the time, but there is seldom vitality enough remaining even to regain the condition existing previous to the operation.

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We will now consider briefly the mechanical means to be resorted to for the relief of displacements. I am ignorant of any instrumental means, safe or reliable, for correcting the position of an anteverted uterus. Great relief may sometimes be obtained, on increasing the degree of anteversion, by the use of a pessary with a long enough curve in the posterior *cul de sac* to lift the neck of the organ from the floor of the pelvis. On thus

slinging the organ, as it were, with the fundus resting against the pubis, and the cervix elevated, the circulation will be improved, and the irritability of the bladder lessened. We gain time by this means, and enable the patient to take more exercise, since we break the force or jar which would be otherwise transmitted to the organ so long as the cervix rested on the floor of the pelvis. The various devices for forcing the uterus into an upright position to a point which the organ likely never occupied even when in a healthy state, are faulty in theory and wrong in practice. If we can lift, by any appliance, the uterus to a point where the obstructed venous circulation can be relieved through the neighboring tissues, which have been put on the stretch by the sagging organ, it is all that can be accomplished by such means, and the mere anteversion is of no consequence. Any instrument making direct pressure on the anterior wall, the chief seat of disease and the point of greatest tenderness, must prove a source of irritation. I deprecate even more the intra-uterine stem-pessary, for had this instrument been the device of the Evil One himself, its use could not be productive of more danger. Its use in a flexure seems as rational as would be the introduction of a straight steel sound into the urethra for the relief of an existing chordee; the penis might be straightened by force. but the cause of the difficulty would certainly not be removed. The treatment of retroversion of the uterus is more satisfactory, mechanical means can be better applied, and the good resulting from relieving the obstructed circulation is well marked on restoring the organ to its natural position. A recent case of retroversion can be reduced with comparative ease, and an instrument can readily be adjusted which will keep the organ so far anteverted as to render it difficult for it to return to its former position. If, however, the displacement has been of long duration and the uterus has become flexed, the condition will, in all probability, have acted as a source of irritation in causing cellulitis to a greater or less extent. Even should adhesions not have formed, a degree of congestion will have been kept up so as to require but a slight provocation to establish a fresh attack of inflammation. It is therefore wise to proceed with the greatest caution in any attempt at reduction until we have fully appreciated the condition. Should we find the uterus firmly bound down by adhesions, it can be replaced in time, for with care, patience, and good judgment, in not attempting too much in a single effort, these bands will gradually become so stretched and attenuated as to offer no longer any resistance. The utero-sacral ligaments, in a state of health, are scarcely worthy of note, being formed but of a reduplication of the peritonæum and a little cellular tissue. These, however, become frequently thickened, and, having closed partially over an enlarged and retroverted uterus, can be readily mistaken for adhesions, in consequence of the obstacle they sometimes present in an attempt to restore the organ to its normal position. I have long accus-

tomed myself to rely on the index finger for the reduction of this displacement, and with a little practice it becomes the most reliable means we can employ. It is one certainly attended with the least risk, as we are able to appreciate at once the point and extent of resistance. When we have once ascertained the fact that there are no adhesions nor lurking inflammation in the neighboring cellular tissue, an experienced operator may, with comparative safety, use the sound or any other means to which he has been accustomed. But the method which I will describe is attended with less pain, and I believe with the least danger, under all circumstances. The patient is to be placed on the back, with the knees flexed, and the hips drawn down to the edge of the operating table or chair. Introduce then the index-finger into the vagina, and direct the point of a tenaculum, which is to be hooked into the posterior lip, just within the os. This instrument is to be used for the purpose of gently drawing forward the organ, sufficiently toward the vaginal outlet, that we may be satisfied the fundus is distant enough from the hollow of the sacrum to pass the promontory when elevated. At the first attempt this must be done with care, and if a point is reached at which great pain is caused, we must then desist. By this manœuvre the uterus has, of course, become more retroverted than before. To correct this, the perinæum should be pressed firmly back, that the finger in the vagina may be passed as far up behind the uterus as possible, and made at the same time to lift up the organ. When the uterus has been thus elevated, and while it is being held up by the finger, the cervix is suddenly carried in an arc of a circle, downward and backward, by means of the tenaculum held in the other hand. By aid of the finger in the vagina, the fundus has been pressed up against the utero-sacral ligaments. These ligaments, having been put slightly on the stretch, gape as the tension is suddenly relaxed by carrying the cervix backward, and the fundus slips between them. The finger must be then placed against the anterior lip, the tenaculum withdrawn, and the organ anteverted by passing the finger repeatedly down the anterior face of the uterus, so as to press the cervix downward and backward into the hollow of the sacrum. If an unusual degree of pain is experienced at any point, we must use our judgment as to how far it may be safe to proceed, or desist entirely for the time being, until all active symptoms have subsided under the proper treatment. When successful, I frequently make no attempt, by mechanical means, to hold the uterus in position, until I have again replaced it and have satisfied myself that no tenderness on pressure exists at any point which would come in contact with the pessary to be used. The form of the instrument should be adapted to carry the cervix well back, and with a sufficient curve in the posterior *cul-de-sac* to keep it elevated, so that the organ must remain anteverted. I have been consulted, more than on any other point, as to the best form of pessary to be used in practice. A difficult question to answer,

as there is some individual peculiarity about nearly every case, on the appreciation of which to a great extent success will depend. Some modification of Hodge's closed lever pessary, however, will be found applicable to the largest number of cases, as it conforms more than any other to the natural shape of the vagina. A pessary, to do no harm, should be small enough to admit of the passage of the finger between it and the vaginal wall at every point, while the patient lies on the back. It must be just large enough to give the needed support to the uterus, and be at the same time small enough for the vagina to regain gradually its natural size. The elasticity of the canal is sufficient to admit of a dilatation to the extent of the pelvic excavation; but it will prove an exception to the rule if a pessary, properly curved, need ever be over three inches in length and an inch and a half in width. Whenever it is possible to avoid making the pubis the chief point of support, I do so. But it is often unavoidable in cases of long-standing retroversion, where the anterior wall of the vagina has become shortened in consequence, and in cases of prolapse of the posterior wall, from laceration of the perinæum. But where the vaginal outlet is not too large, and the posterior *cul-de-sac* is of a natural depth, the principle of the lever-pessary is applicable to nearly all cases. The fulcrum of this double lever rests on the posterior wall of the vagina at the bottom of the *cul-de-sac*. It should be so curved in reference to this *cul-de-sac* and posterior wall at one extremity, and at the other end bent with a lesser curve in the opposite direction, so that the instrument may be balanced. As the patient stands on her feet, the weight of the uterus will cause the other end of the instrument to rest against the anterior wall of the vagina, near the neck of the bladder. On assuming the horizontal position, the instrument will present in the axis of the vagina near the outlet. It will thus compensate itself by a change of position, so that it cannot, from continued pressure at any one point, cut into the vaginal tissues. A longer curve will be needed in the *cul-de-sac* where retroversion has existed, than with prolapse from hypertrophy, where the object is simply to lift the organ from the floor of the pelvis. In the latter condition, the upper portion of the vagina will be more dilated, as a rule, than the lower part, and the instrument must be made to correspond. The closing in of the vaginal walls around an instrument, made larger above, has the effect of crowding it upward in the canal. When even the outlet is larger than natural, and dilated from a prolapse of the vaginal walls, we must restore the canal to a natural size and close the laceration through the perinæum, by a surgical operation, before an instrument can be worn with advantage for correcting the retroversion. An instrument, under the circumstances, to be used as a temporary means of relief, must be made wider below, with the greater curve also at this point, so as to get the needed support from behind the pubis, and with a depression to guard the neck of the bladder from pressure. We find occasionally a

difference in the curve on each side of the symphysis, so that, if an instrument is made symmetrical, it will bury and cut into the soft parts covering the lesser curve. On the corners of the instrument there should be no sharp angles, but a gradual curve; and frequently it is necessary to bend the corners downward, to correspond with the roof of the vagina at this point. In the posterior *cul-de-sac* the instrument should never be so abruptly curved as to make pressure directly against the uterus at its junction with the vagina, but at some little distance beyond. The circulation in the neck is easily obstructed by pressure at this point, so that it will soon cause an erosion about the os; and frequently an intolerance to the presence of any instrument in the *cul-de-sac* becomes established, in consequence of irritation or inflammation of the lymphatic glands found in this neighborhood. The shorter the vagina, the straighter must the instrument be made, for if curved too much it will rotate and remain across the axis of the canal. A straight instrument has to be made wider in the middle, in proportion to its length, than a curved one. The widest part of the vagina is from one sulcus to the other, while the lateral walls and posterior surface of the canal form a concavity; consequently a curved instrument should be made rather smaller in the middle, as its support is chiefly derived from the posterior wall. It is a very common occurrence to find an instrument, when too wide, cutting its way along the lateral walls of the vagina, at the bottom of a deep fold formed as the pessary is carried downward from the pressure above. It may be accepted as a rule that, so long as a patient can recognize from her feelings that she is wearing an instrument, it either does not fit, or she is in no condition to wear one; and in either case it will do her harm. So soon as an instrument has been properly adjusted, and there is no tenderness on pressure at any point in the vagina coming in contact with it, the patient will be unconcious of its presence. I prefer at first the use of block-tin rings on account of their greater malleability. After modelling one of a proper size to the case, and having fairly tested its use, I then have the instrument reproduced in aluminium, silver gilt, or hard rubber. These are, in brief, the main points to be observed in adjusting a pessary properly, but in each case there will be a necessity for some modification in consequence of individual peculiarities. Success will depend entirely on an accurate appreciation of these differences, and on the mechanical skill innate to the operator. To a want of both or of either gift, must be attributed the unsatisfactory results so often complained of.

The uterus, an erectile organ, and surrounded by a mass of blood-vessels passing in every direction through loose cellular tissue, is directly affected by any increase or diminution in the neighboring circulation. We must appreciate that in no other part of the body have we such a matted net-work of vessels in the same space. In consequence of the erectile character of all

the tissues, these vessels become varicose from any continued obstruction to the circulation, and have an almost incredible venous capacity. As a stream of water will saturate the ground and lose itself in a marsh, so will the circulation through the pelvic cellular tissue, and become in disease equally sluggish. On theoretical grounds the difficulty could be easily overcome by local bloodletting; but the chief objection to the treatment, if there were no other, would be the already reduced general system of the patient. In this over-distended condition of the veins, the balance has been lost, and they are no longer able to return to the general circulation the same quantity of blood received by them from the arterial capillaries. Local depletion would, therefore, act rather as a source of irritation, to increase the congestion, where this loss of tone existed in the coats of the vessels. There are certain conditions where a few leeches applied to the anus, or free scarification of the cervix, may be resorted to with great benefit. We will take for an example, a sudden suppression of menstruation, from exposure or any other accidental cause, where the congestion is almost entirely arterial, and above the secreting point, as it were. Under such circumstances, by temporarily lessening the congestion we assist the vessels, which have not yet lost their tone, to regain their normal size; the circulation is stimulated, soon the organ is able to resume its function, and the equilibrium is restored. If we apply leeches to the uterus of a female who has been long suffering from local disease, we will find, although a momentary sense of relief may have been experienced, that the organ has increased in weight and is lower in the pelvis than before. As her general vitality had been previously lowered, even a slight additional loss of blood will be found sufficient to greatly increase the previous hyperæsthesia.

There can be no restoration to health, in either the local or general condition, so long as anæmia exists, since the blood has lost those elements by which organic life is properly stimulated. Throughout the menstrual life of the female, the organs of generation exercise a dominant influence over the nervous system; in health, through the reflex system, they act as the fly-wheel to the mechanism. Therefore, so soon as any serious local difficulty of a chronic character is established, the nervous force becomes lowered, general functional derangement supervenes, and impaired nutrition follows as a sequence. Although we are unable to cure the local difficulty until we have improved the general condition, yet we can set the ball in motion by lessening the local source of irritation. Unless we can control the pelvic circulation, and at least impart a temporary tone to these vessels, it will be found in the end that little has been accomplished. We have our remedies for local application within the uterine canal, and much can be accomplished by mechanical means when appropriate. But in the simple remedy, hot-water vaginal injections, we possess a most valuable means of relief when properly administered. Although it has now been many years since this reme-

dy was first introduced into practice, but a small portion of the profession appreciate its use or understand its action. It is generally conceived that the application of heat by this method relaxes the vessels and increases the congestion. This it does at first, but, if prolonged, the capillaries are excited to increased action; as they contract, the tonic stimulus extends to the coats of the larger vessels, their calibre becomes lessened, and, with an approach to healthy action, the congestion diminishes. No one applies a hot poultice with the view of increasing the congestion of the parts, but, as any old woman would explain it, "to draw the inflammation out," that is, to lessen the congestion by causing contraction of the vessels. That such is the effect of the continued use of a poultice is shown by the bleached and wrinkled appearance of the tissues after its removal. We can cause capillary contraction, also by the use of cold, and the effect is even more prompt, but when reaction comes on, the tissues will become more congested than before. In brief, the immediate effect of cold is contraction, and with reaction we always have dilatation; heat, on the contrary, causes dilatation at first, and its action is followed by contraction afterward.

If a woman be placed on her back, with the hips elevated by a properly shaped bed-pan under her, and a gallon or more of hot water at 98° or of a higher temperature be slowly injected into the vagina by means of a Davidson's syringe, the mucous membrane will become blanched in appearance, and the canal as diminished in size as if a strong astringent had been administered. While the hips are elevated, the vagina will retain, during the injection, a large quantity of water, which by its weight will distend every portion of the canal, so that it will come in direct contact with the whole mucous membrane under which the capillaries lie. The vessels of the neck and body of the uterus pass along the sulcus on each side of the vagina, and their branches encircle the canal in a most complex network. The vessels of the fundus, through the veins of which the blood passes by the liver back into the general circulation, communicate with those below by anastomosis. We can thus, through the vagina, influence directly or indirectly the whole pelvic circulation. We can so diminish the supply as not only to check congestion, but we can literally, by the use of hot water, starve out an inflammation. I know from my own personal observation that several of these injections a day, at 100° to 106°, will abort an attack of cellulitis if resorted to early enough, and their use persevered in, with the aid of rest and anodynes. These injections exercise a most beneficial effect on the reflex system by allaying the local irritation. I know of no better means for removing the nervousness and sleeplessness of an hysterical woman than a prolonged hot-water vaginal injection, when administered by an experienced hand. These injections will frequently soothe a patient to sleep in less time than could be done by any anodyne in the pharmacopœia. To receive permanent

benefit from their use, they must be continued until the patient is restored to health. They should be given at least once a day, and the best time is on retiring at night. The only position in which the patient can receive any benefit from them is on the back, with the hips elevated, as I have described. She cannot administer them properly to herself, and I know of no arrangement, by siphon or other means, which can take the place of an intelligent nurse. As the patient improves in health, the quantity of water can be diminished, and the temperature lowered until the injections are discontinued from daily use, but for some time they should be employed for a few days after each period.

In 1859 I first used tepid and then hot-water injections, in the treatment of a member of my own family; at that time and for years afterward cold-water injections, at a low temperature, were used by every one in the treatment of uterine disease. I continued to employ hot injections in my private practice until the autumn of 1862, when I was appointed to the charge of the Woman's Hospital. From that date to the present, in this institution, and in my private practice, nearly every patient coming under my care has been treated by this method, merely varying the quantity of water and the temperature according to the circumstances of the case.*

The patient will be in a better condition for getting out into the open air after we have lifted up a uterus with version, or prolapsed, by an instrument to aid in restoring the circulation through the organ. The general condition will, however, in all probability, yet admit of little local treatment. Beyond the vaginal injections, to which a little chlorate of potash, or any other remedy indicated, may be added, with a daily pledget of cotton saturated with glycerine, and introduced into the vagina, our treatment will be limited until we have directed our attention somewhat to the general system. In a case of long standing we will scarcely find an organ in the body which is not suffering from functional derangement. The connection of one function with another is so intimate in the organic circle of nutrition that the derangement of any one soon jeopardizes the integrity of the whole.

The result will be enfeebled digestion, a sluggish portal circulation, and imperfect respiration; so the blood is no longer oxygenized properly, and with but partial elimination it is returned

* The action of hot water in surgery, as a means of preventing hemorrhage, was first brought to my notice by the late Dr. Pitcher, of Detroit. He stated that for many years he had been in the habit, when operating, of applying to a bleeding surface sponges taken from water as hot as could be borne. His explanation was, that, after a clot had formed in the mouth of a dilated vessel, the continued application of heat caused it, on reaction, to contract so firmly on the clot that secondary hemorrhage could not occur. With his views of the action of heat, when continued, on the coats of vessels, and my own with regard to the condition of the circulation in the pelvis, came the first suggestion to my mind of its application in the treatment of uterine disease. I have been so thoroughly identified with this mode of practice, that it seems scarcely necessary to claim the priority. Certainly, no one in this country is on record as an advocate for the practice previous to myself; and, as far as I have been able to ascertain, the same is true in regard to the practice of gynæcologists abroad.

to the general circulation in a condition not unlike that of a cold-blooded animal. The kidneys are over-worked, and the skin is inactive; repair to a great extent has ceased, and a general waste is the rule. In addition, we often have, combined in the same subject, the pernicious effects from the habitual use of alcohol in some form, anodynes, and coffee. The first step must be at once, without a compromise, to break up the dependence upon either of these now active poisons to the nervous system. Their indiscriminate use having in the beginning aided not a little in bringing about the general wreck, a continuance would but defeat the best-devised efforts for a restoration. We can aid digestion but little at first by the use of medicines; our chief dependence must be simple and nutritious food, small in bulk and often administered.

An attempt must be made to bring about a healthy action of the skin by means of hot-air baths, general friction, and exposure of the body to the direct action of sunlight. With any improvement in the condition of the skin we will relieve, through the circulation, the over-taxed kidneys, the portal system, and indirectly assist digestion. The action of sunlight is beneficial in relieving the anæmia, by creating a rapid tolerance of the stomach to the administration of iron. The use of iron, in any form, and sunlight, must go together, for, without the aid of the latter, ferruginous preparations are not properly taken up by the stomach, and must only act as an irritant. As we lessen the anæmia and improve the condition of the blood, the capillary action will become more vigorous, and the power of assimilation and elimination must increase; we can then do more by medicinal means to assist digestion. We can relieve the portal system by the use of mild salines and other remedies, and by doing so we remove the chief obstacle to the proper return of the venous blood from the pelvis into the general circulation. As we continue the use of baths and friction to the whole surface of the body, the patient gains strength, and is able to exercise and remain more in the open air. With increased action of the skin, we will find that the bowels can be better regulated, and constipation relieved by less aid from artificial means. Having advanced so far in the general treatment of the case, the physician will have already thoroughly tested his knowledge of the practice of medicine, and by the least resort to drugs his success will have been in proportion to his resources.

After we have appreciated any existing displacement of the uterus, or simple prolapse from enlargement of the organ, due probably to imperfect involution, the most common cause, our attention will be directed to the existence of an erosion or excoriation of the epithelium; but so-called ulceration of the cervix is a condition in itself of but little moment, as it will heal by attention to cleanliness, and on lessening the discharge from the uterine canal. In practice we cannot appreciate the full extent of disease in the mucous membrane, whether confined exclusively

to the neck, body, or fundus, and it is but a question of time before the whole canal may become involved. We learn, however, from experience, that recovery is more rapid when the disease exists near the os, than when situated in the upper portion of the canal. The most common point of origin has yet to be determined; but my impression is that, in females who have not borne children, the disease generally has a beginning in the cervix, while the seat of the placenta is the point with others, and from which the disease extends toward the cervix, or in the opposite direction. The uterine discharge is more profuse when from the cervical canal, clearer and of a more gleety character from the neighborhood of the internal os, and diminishes in quantity and consistency in approaching the fundus. The selection of remedies varies somewhat with the seat of disease, but my present knowledge is not sufficient to be explicit. The truth is, that our practice here becomes somewhat empirical, for we can neither map out the boundary nor direct with accuracy our applications to the diseased surface alone: so that a remedy, which was apparently most efficacious in a previous case, may prove inert under a like condition, so far as we yet possess the knowledge of appreciating a difference. We know, however, that remedies of a more stimulating character, with astringents as adjuvants, are useful as applications to disease about the cervix, and alteratives for the upper portion of the canal. In our selection we must use those calculated to do the least harm to the mucous membrane which may still be in a normal condition. Rare indeed is the necessity for applying, within the uterine canal, caustics, the cautery, or the strong mineral acids. It is true that these remedies act promptly, so far as to heal an erosion and to check all uterine discharge. But we cannot restore the patient to health by so far changing the character of the mucous membrane as to leave it a mere cicatricial surface. Our ultimate success will be directly in proportion to the condition in which we leave this membrane, for we will need its healthy action in the after-treatment of the case. That individual cases escape with but little damage is only due to protection afforded by the secretions; yet the practice, as a rule, is disastrous enough to deprecate their use. We have no remedy which will act with more promptness than the nitrate of silver, when applied to the mucous membrane of the cervix, yet it has done more damage than any other. From being in common use it is the more dangerous, for its repeated action will ultimately destroy the mucous follicles, harden the tissues, and close the os as certainly as the application of the actual cautery. The evil effects of its application on the mucous membrane of other parts of the body are so well recognized, that its continued use for the uterine is remarkable.

I have found most useful for applications to the cervix, Squibb's impure carbolic acid, or creosote tar. Its action is very different from that of pure carbolic acid; it exerts a local anæsthetic effect,

and is not a caustic. This may be applied at intervals of ten days, with the intermediate use of tannin and glycerine, or the pinus canadensis. It is advisable to add to the last pint of the hot-water vaginal injection a certain quantity of chlorate of potash, chloride of sodium, borax, carbonate of soda, or alum, as may seem indicated. When the disease is above the cervix, and the patient is in a fit condition for their use, sponge-tents may be employed for their alterative effect.

They may be used, of a small size, merely to set up a new action by their presence, and by moderate pressure may be of benefit in relieving congestion, on remaining long enough to excite a free discharge; or of a larger size, with the view of dilating the canal fully and exciting the uterus to contraction, in cases of hypertrophy. After removing a large tent, and while the canal is still dilated, it is beneficial to wash out the cavity with a continued but gentle stream of hot water from the nozzle of a Davidson syringe introduced well up to the fundus. Before the uterus has contracted, it is well to make a thorough application of the strong tincture of iodine throughout the canal. I sometimes alternate with the use of iodine by introducing within the canal either the dry persulphate of iron, the oxide of zinc, or powdered alum. This is done by means of a roll of moist cotton twisted around the applicator and well covered with the powder. After wrapping the cotton firmly around the applicator and giving it the proper curve, the twist is reversed with the fingers so as to loosen the cotton sufficiently, so that when introduced to the fundus it will remain behind on withdrawing the instrument. The presence of the cotton can produce no irritation, as it occupies so little space, and it will be thrown out from the uterus in a few hours if a portion is left projecting from the os. The profuse white-of-egg-like discharge does not seem to be a product so much of congestion of the mucous membrane as from disease of individual Nabothian glands, which project and can be felt with the probe. In addition to the treatment given, I frequently remove these little projecting bodies by means of scissors when within reach. Disease at the fundus is a more serious condition to overcome, and one but little influenced by any special application. The lining membrane of the upper portion of the canal is so different in character, that I doubt if disease is ever confined exclusively to it, but at the same time the uterine tissues beneath are always more or less involved. The frequent use of iodine, with the view of lessening the size of the whole organ, must be our main reliance, with more care for improving the general system than where the disease seems to be confined to mucous membrane itself.

I feel that I cannot, in justice to the subject or myself, enter more into detail. The views of treatment which I have already advanced are equally applicable in some respects to any form of uterine disease. But, from the fact that displacements and versions cannot be treated by mechanical or surgical means alone,

the propriety of entering in addition on a subject of such scope might be questioned. It is but just that I should state that my views have been based to a great extent on a hospital practice, yet I hope they may prove no less useful as an experience in the treatment of an exaggerated form of disease which must always be opprobious in private practice. Imperfectly as these views have been given, they have been gathered from a most extended field of observation. In consequence of a continuous service of some twenty years in the Woman's Hospital, when it was so long the only institution for the treatment of these diseases in the country, I have been favored with advantages which could scarcely occur again; while, in addition, from holding the position of surgeon-in-chief to the institution during so great a portion of the time since its foundation, I have had a rare opportunity for observing the practice of others and for correcting my own mistakes. A record has been preserved of every case which has been under treatment in the Woman's Hospital, and in my own practice, so that I have been able to keep for years a large number of former patients under observation. By the aid of these records, and by personal observation on the return of the patients at stated intervals, I have been able from time to time to fairly test the value of different modes of treatment.

[We can most emphatically add our testimony to that of Dr. Emmet in regard to the curative action of warm irrigations in uterine and vaginal inflammatory affections. We are also able to state that in a considerable number of cases we have extemporized a siphon arrangement which has answered a better purpose than Davidson's syringe, in the hand of the patient herself. Of course, whenever a nurse or intelligent second person can be had to administer the hot-water injection, it is better, but in private practice all physicians know that insuperable objections often exist against the employment of a nurse, or indeed against imparting any information respecting cases to other parties. This siphon consists of a section, six or seven feet in length, of gutta percha tubing, to one end of which the vaginal nozzle of a syringe is attached. The fluid to be used is poured in a basin upon a table placed near the bed upon which the patient may assume the position directed by Dr. Emmet. The siphon should be carefully sunk in the water so as to exclude air. Nothing now remains to render the mode of irrigation successful except to grasp the tube tightly, as near as may be convenient to the nozzle, introduce this into the vagina and then relax the pressure upon the tube, which pressure must, to insure success, be sufficiently strong to prevent admission of air to the tube. Patients can be readily instructed

in this manipulation, so that failure in its use seldom or never occurs. It is not only a simple but an economical apparatus.]

How Purgatives Act.

[We presume that the allegation will go uncontradicted, that physicians use purgatives more indiscriminately, and with less consideration for the indications present, than is held to be proper in regard to other forms of medication. The universality of truth in respect to this statement is so well acknowledged, that vulgar people apply very ugly words to denote the physician's office. Now, if this be true, is there any other point in therapeutics which should interest us more than questions relating to the effects of purgatives upon the human economy? Fortunately it is not common to the profession now to follow in the footsteps of Hamilton and Cook, whose intentions and hopes of curing their patients bore a direct relation to the amount of avoirdupois fœcal matter they were able to expel. Yet it may be still a matter of question, whether we do not often misuse these powerful measures of treatment.

The practitioner seldom or never opens the treatment of an acute case without the "preliminary purge," and the teacher advises the student accordingly. We will not say that, as a rule, this practice is bad; for certainly it must be an important advantage to patients entering upon acute attacks, to have the alimentary canal cleansed of accumulations and impurities. For three reasons, this rule may be held to be correct. First, the presence of these intestinal accumulations may, by exciting reflex disturbances, complicate the case. Second, reabsorption may occur, and produce, or increase blood inquination. Third, absorption of medicines, or nutriments, may be hindered by their presence so as to act prejudicially upon the case. Then, on the other hand, purgation weakens the patient, and if the disease is to be of long duration we wish to husband his strength. It also interferes with absorption of either medicines or foods, and to this fact important attention should be directed.

Undoubtedly the practitioner should here, as in other departments of his profession, endeavor to cultivate and follow that happy mean which avoids either extreme. After long experience and observation we believe the postulate may be laid down, that purgatives are therapeutic agents more powerful for good, in structural inflammatory attacks and local congestions, than in

the essential fevers. In pneumonitis, pleuritis and meningitis, purgation is more uniformly curative than in malarial or yellow fevers, in which diseases we use them more as preparatory medication than for any direct effects in arresting the maladies. In truth we are prepared to admit, that the greater our experience becomes in treating essential fevers, the less our reliance is on purgatives as means of cure. In 1868 we treated a paraplegic patient during an attack of yellow fever. In consequence of his nervous paresis the patient was not able to control his sphincter, and consequently was greatly worried by purgation. When attacked by yellow fever his bowels had not been voided for three days. Notwithstanding this fact, we chose what we supposed to be the lesser of two evils, and gave no purgative. Never did a case of yellow fever pursue a more uncomplicated course, and the patient made a good recovery, although no measures were employed to evacuate the bowels until the eleventh day of the disease.

No doctrine is now more completely exploded than that which taught that malarial fevers could be cured by purgatives. We may say in truth that the diseases curable in toto, by purgative treatment are small in number, and consist almost entirely of functional derangements resulting in blood impurities, congestion, or reflex disorders.

These remarks are merely introductory to some extracts from an article in the *Practitioner* for June, by Dr. T. Lander Brunton.]

Having now come to a conclusion regarding the manner in which purgatives act, let us consider some of their effects upon the body. It is evident that the increased peristaltic action of the bowels will hurry along the food and cause its expulsion before the nutritive matters it contains have been fully absorbed.

If a purgative be taken immediately before or shortly after a meal, the result will be much the same as if less food had been taken or the meal entirely omitted. Many persons who are accustomed systematically to eat more than they require will regularly take a "dinner pill" or a course of Seidlitz or Pullna waters, although they cannot be persuaded to deprive themselves of a single opportunity of enjoying the pleasures of the table or to put the least restraint upon their appetites.

Increased peristaltic action will also remove fæcal matters as well as food from the intestine, and it will be greatly assisted in this by the increased secretion from the intestinal wall which purgatives induce.

I have already mentioned that mechanical irritation, such as

tickling with a feather or rubbing with a glass rod, will cause secretion from the *cul-de-sac* of intestine in Thiry's fistula, and hardened fæces seem to have a similar action. Thus diarrhœa is not unfrequently caused by the presence of scybalous masses or other irritating matters in the intestine, and nothing cures this like a dose of castor-oil. At first sight it seems odd that the scybala are not washed away by the fluid which they cause to be secreted, but this secretion will probably be poured out only at or below the point where they lie, and thus it will have little effect on them, though it may wash out the lower part of the bowel thoroughly enough. A dose of castor oil, on the contrary, will induce secretion in the bowel above the scybala, and the fluid in its downward rush will carry the fæcal masses along with it.

Irritating substances in the intestine, besides acting locally upon the bowel in the manner just indicated, may exercise an influence upon distant organs through the medium of the nervous system. Sir Charles Bell* observed a case in which ulceration of the ileum was found in a man who had suffered severely from tic, but there was nothing wrong whatever with the fifth nerve, in which the pain was felt. He therefore felt convinced that although the pain was felt in the cheek, its true source was irritation in the ileum. Acting on this belief, he administered croton oil ($\frac{1}{2}$ of a drop in combination) in tic douloureux for the purpose of removing any morbid condition of the bowel, and obtained the happiest results from its employment; and Newbigging† has found it equally efficacious in sciatica.

It is difficult to say whether the pain felt in the cheek is simply due to the irritation of the intestinal nerves being reflected, as it is termed, along the fifth nerve, or whether the irritation induces such a change through the vaso motor nerves in the blood-vessels of the cheek as actually to set up a new irritation in the course of the fifth nerve itself. At any rate, the vessels of the face and head are very easily affected by any irritation of the stomach or intestines, as is easily seen from the extraordinary pallor which at once overspreads the face when a state of sickness and nausea has been induced. The effect of constipation in causing a feeling of fulness in the head is well known, and Ludwig and Dogiel‡ found that when the intestines of an animal were moved by the finger the rapidity with which the blood flowed through its carotid arteries was greatly increased. The frontal headache which so frequently accompanies gastric or intestinal derangement may possibly be due to some of the intestinal contents which ought to be evacuated being absorbed and acting as poisons on the vessels of the head themselves. I am inclined to think, however, that although this may have much to do with it, yet the headache very often depends to a

* Bell, "Practical Essays," p. 85.
† Newbigging, *Edin. Med. and Surg. Journ.*, Jan. 1st, 1841.
‡ Ludwig's Arbieten aus der physiologischen Anstalt zu Leipzig, 1867, p. 253.

great extent on some alteration in the cerebral circulation caused reflexly by the condition of the abdominal organs; for I have myself had a headache, though not a frontal one, which alternated with nausea. The nausea would last for a few minutes, during which the headache would entirely disappear; then the nausea would leave me, and the headache instantly took its place. After evacuation of the stomach, both the headache and nausea disappeared, showing that in this instance at least they were due to irritation in the stomach. But in many instances, no doubt, not only headache but much more serious symptoms may be due to the decomposition of food in the intestinal canal and the absorption of its products. Thus Senator* relates a case where a simple gastric catarrh without fever was brought on by eating something which disagreed with the patient. This was followed on the second day by great belching of gas, smelling like sulphuretted hydrogen or rotten eggs. The urine also contained sulphuretted hydrogen. As soon as this occurred the patient collapsed suddenly, and became pale and giddy, with a small, frequent, and compressible pulse. The patient remained conscious, and in a minute and a half or two minutes the collapse passed away. A similar attack came on again during the same day, but after the bowels which had been constipated were opened, the patient rapidly recovered. Senator considers that the collapse was due to poisoning by the sulphuretted hydrogen absorbed from the intestine, and it certainly seems probable that this was one cause of the attack, even if it were not the only one.

Other poisons besides sulphuretted hydrogen may be formed in the alimentary canal and absorbed into the blood, where they exert their deleterious action. Among these may be mentioned butyric acid, which has frequently been found in the stomach in considerable quantities.† According to O. Weber‡ it is very poisonous, exerting its action chiefly on the nerve centres. The nervous symptoms which frequently accompany gastric derangement or disease of the intestines may therefore be frequently occasioned by poisons formed in the alimentary canal in consequence of imperfect digestion.

The administration of a brisk purgative or small doses of Epsom salts thrice a day is a most effectual remedy for frontal headache when combined with constipation; but if the bowels are regular, the morbid processes on which it depends seem to be checked and the headache removed even more effectually by nitro-hydrochloric acid or alkalies given before meals. If the headache is immediately above the eyebrows, the acid is best; but if it is a little higher up, just where the hair begins, the alkalies appear to me to be more effectual. At the same time that the headache is removed, the feelings of sleepiness and

* Senator "Berlinger Klinische Wochenschrift," 1868, No. 24, p. 254.
† Kuhne, "Physiologische Chemie," p. 58.
‡ O. Weber, "Deutsche Klinik," 1864, p. 488.

weariness which frequently lead the patients to complain that they rise up more tired than they lay down, generally disappear.

Somewhat analogous to the neuralgia of the fifth nerve in Sir Charles Bell's case, or to frontal headache, is the pain which we frequently meet with in persons having decayed teeth. The pain may be felt in the offending tooth itself, but very often it seems to give little or no uneasiness. The patients complain of neuralgic pains above the ear or along the jaw, and will often deny that they have any decayed teeth at all. It would almost seem that neither the irritation in the tooth nor irritation in the intestine alone is sufficient to produce pain, though they do so when acting conjointly; for extraction of the tooth, or stoppage of the cavity with cotton-wool steeped in melted carbolic acid, will often remove the pain although no medicine is given internally, while on the other hand a brisk purgative may also afford relief though the tooth be left untouched. It is best, however, to combine both methods of treatment, and if the tooth is not extracted or stopped, the pain is very apt to return; and it seems to me probable, though I am by no means certain of it, that this recurrence is connected with the renewal of gastric or intestinal irritation. According to Heincken,* otalgia may also depend on the presence of irritating matters in the intestine; and Sir Charles Bell observes that accumulations in the colon will give rise to pains in the loins, spermatic cord, or groin. Pain at the lower angle of the scapula is referred by him to disorder and distension of the duodenum. This pain is very often accompanied by flatulence, and is described by patients as a "pain in the pit of the stomach, shooting through between the blade-bones", and it is not unfrequently termed by them "windy spasms." It is relieved by rhubarb and alkalies given before meals.

Having said so much regarding the fæcal contents of the intestine and their local and remote actions, we must now consider a matter of no less importance, viz., the effect of purgatives upon the secretions which are poured into the intestinal tube by the various glands connected with it. The saliva which flows into the mouth from the submaxillary and parotid glands is swallowed and aids the digestion of starchy food in the stomach, and probably the intestine. A part of its active principle, ptyalin, is reabsorbed, and some of it is excreted in the urine;† but as we shall here afterwards see, it is probable that another part is excreted again by the salivary glands, and thus does its work twice over. This is at present only a probability as regards ptyalin, but it is a certainty in the case of several substances which are excreted by the salivary glands, such as iodide of potassium, for example, which can be detected with great ease. When this substance is swallowed, it is absorbed from the stomach, passes

* Heincken, "De Morbis Nervorum ex Abdomine," quoted by Sir Charles Bell' op. cit, p. 93.

† Colnheim, "Virchow's Archiv," xxviii. p. 250.

in the blood to the salivary glands, and is excreted by them much more readily than by the kidneys. It again passes down with the saliva to the stomach, is reabsorbed, and again excreted. Thus it may go round and round for a long time without getting entirely out of the body. If we wish to remove it quickly and completely, we must give a purgative so as to prevent its reabsorption from the intestinal canal by causing its speedy expulsion. The same is the case with other iodides, such as those of lead or iron. Iodine has been shown by Bernard to possess the power of making iron pass readily through the salivary glands, the iodide of iron being found in the saliva soon after it has been injected into the blood, while other salts of iron such as lactate, never make their appearance in it at all.* Several years ago iodide of potassium was proposed by MM Guillot and Melsens as a remedy in cases of lead-poisoning. The lead, they consider, is present in the body in the form of an insoluble-compound† which it makes with the tissues, but by the administration of iodide of potassium it is rendered soluble. It then finds its way into the circulation, and is excreted by the kidneys and other emunctories. But the iodide of lead is partly excreted by the salivary glands, for M. Malherbe, of Nantes, and Dr. Sieveking have found it in the saliva of persons suffering from lead-poisoning, and who were being treated by iodide of potassium. The lead salt being swallowed with the saliva, is again reabsorbed, and thus the cure is comparatively slow when patients are treated with iodide of potassium alone. I frequently see patients suffering from lead-poisoning brought on by working in white lead, and for some time I have been accustomed to treat them with five grains of iodide of potassium, three times a day, and a sufficient quantity of sulphate of magnesia or other purgative either thrice or once a day, to keep the bowels very freely open, and cause the expulsion of the lead from the alimentary canal as quickly as it is secreted into it. I have not made comparative experiments on the effect of this treatment and of that by iodide of potassium alone, or by purgatives alone, but from what I remember of cases treated by the late Professor Syme with castor oil, I am fully satisfied with the treatment I now adopt. The same plan would probably prove equally useful in chronic poisoning by copper or mercury.

But the gastro-salivary circle, as we may term it, from stomach to salivary glands, and from salivary glands to stomach again, is not the only one in which those metals move. Their circulation in the portal system, or entero-hepatic, as it is termed by Lussana,‡ is still more important. Iron is eliminated in great part by the bile: copper and manganese appear in it also, according to Albini and Moser,§ and it seems probable that manganese,

* "Bernard, "Physiologie Experimentale," tom. ii. p. 99.
† Guillot and Melsens, "Archives Generales de Medecine," 4th ser. iv. p. 517; and Melsens, "Annales de Chimie," June 1849.
‡ Lussana, "Lo Sperimentale," tom. xxix. 1872.
§ Quevenne, Albini and Moser, quoted by Lussana, "Lo Sperimentale," tom. xxix. 1872, pp. 340, 343.

lead, and all the heavy metals pass out of the body by this channel. From the liver they pass into the intestine, are reabsorbed from it, and again pass to the liver and recommence their course, They may be present in considerable quantities in the blood of the portal system without reaching the general circulation or getting a chance of passing out in the urine. They are therefore much more closely locked up in the entero-hepatic circulation than in the gastro-salivary one, for the salivary glands are supplied by the systemic circulation, and any blood which brings lead or any other substance to them must also carry it to the kidneys. The power of the entero-hepatic circulation to retain metals within the body being much greater than that of the gastro-salivary one, it is evident that the beneficial effects of purgatives in lead-poisoning are due to their removing the metal from the portal circulation still more than their action on the gastro-salivary one which has already been discussed. Other poisons, such as curare and probably serpent's venom, may also circulate in considerable quantity in the portal system without reaching the systemic circulation, and probably this is one of the causes, though by no means the only one, which renders these substances to a great extent inocuous when swallowed.*

But the circulation of iron, lead, curare, &c., in the portal system, important though it may be, is of far less interest than the circulation of the bile itself. For the sake of convenience I have merely stated that lead, mercury, &c., are excreted in the bile, and have hitherto assumed that bile circulates in a similar way in the portal system, without giving any reason for doing so.

It used to be thought by many that bile was formed in various parts of the body, and was simply excreted by the liver. This view is now given up by most physiologists, who believe that bile is formed by the liver only. But in altering their views regarding the function of this organ they went too far, and supposed that it only formed bile, which, when it had once found its way into the intestine and mixed with the intestinal contents, became decomposed and finally expelled with the fæces. A year or two ago, however, Schiff† found that this view of the hepatic functions was too limited, and that the liver removed bile from the blood or *excreted* it as well as formed or *secreted* it.‡ He observed that when all the bile was drawn away from the liver by means of a fistulous opening in the gall-bladder after ligature of the ductus choledochus, the quantity which flowed from the liver rapidly diminished after the fistula had been established, but could again be quickly increased by the simple process of putting bile into the duodenum. The bile was at once absorbed and again excreted by the liver, and it did not make much difference

* Lussana, op. cit.

† Schiff, "Pfluger's Archiv," 1870, p. 568.

‡ Although it is not correct to do so, I use the term "secreted," here as synonymous with "formed" for the sake of conveniently distinguishing between the formation of bile in the liver and its removal from the blood.

whether the bile just removed from the fistula in a dog was again injected into its duodenum, or whether ox bile was used instead. In the normal state of the animal the liver is always doing two things: it is *forming* new bile, and it is *excreting* old bile which it has received from the intestine by means of the portal vessels. When a biliary fistula is made and the bile is drawn away as fast as it is secreted, none gets into the intestine, and therefore no old bile reaches the liver; consequently, the quantity collected represents only the new bile formed in the liver, and is of course much less than that which would normally pass through the ductus choledochus into the intestine. If all the bile were absorbed there would be no need for the liver to go on forming it, but this is not the case, for only a part of it is reabsorbed, and the remainder is decomposed and excreted with the fæces.

So long as the liver does its duty properly, and excretes again all the bile which is absorbed by the portal blood from the intestine, very little bile can pass through the organ into the vena cava and thence into the general circulation. But whenever so much bile is taken up from the intestines that the liver cannot excrete it all, it will find its way out of the portal into the systemic circulation, and will exert an injurious action on the nervous system. The same effect will follow anything which diminishes the excreting power of the liver and renders it unable to excrete the normal amount. It is evident that if anything should cause the liver to form more bile than usual at any time, it will have extra work to do in the way of excreting it after its absorption, and there will be more bile circulating in the portal blood for some time afterwards, or at any rate until the extra quantity has been got rid of or compensation has been established by the liver forming less. Many experiments have shown that an abundant supply of food causes the liver to form more bile, and we all know that heavy dinners are apt to cause biliousness. Fasting, on the other hand, diminishes the quantity of bile secreted, and every one knows that if he fasts for a day after taking an especially heavy dinner he may be none the worse for it, but if he dines out every night he is almost sure to become bilious, unless he takes measures to prevent it by using purgatives.

It has not yet been shown by direct experiment that the symptoms usually grouped under the head of "biliousness" are due to the presence of an excess of bile in the blood; but the rapidity with which they disappear after the removal of bile from the system, either by vomiting or purgation, renders it extremely probable. Frequently we find that the fit of vomiting which has expelled a quantity of bile is hardly over when the appetite returns, the brownish-white fur disappears from the tongue, the face loses its dingy hue, the languor disappears, the irritability of temper is replaced by equanimity, and stupidity and laziness give place to sprightliness and activity. But vomiting is a disagreeable process, and few submit willingly to it, although it would be well worth while if the same end could be gained by

no other means. As most old practitioners have found, however, a mercurial pill and a saline purgative produce all the good effects of vomiting without its trouble and discomfort, and they have long been in the habit of ascribing the beneficial action of the mercury to its "cholagogue" properties. They felt convinced that biliousness was due to bile in the blood, and believed that its removal was due to the liver being stimulated by the mercury to excrete the bile more rapidly. But the careful experiments made by the Edinburgh Committee of the British Association* on dogs with biliary fistula showed that neither mercurials nor other purgatives increased the flow of bile from the liver, and these results seemed at first sight to contradict the views entertained by most practitioners regarding their cholagogue action. The contradiction is apparent, but not real, for in the experiments the bile was regularly removed from the body as soon as it was formed, and none of it ever reached the intestine Consequently, any diminution in the quantity collected simply showed that the liver was forming less. Other experiments have given somewhat different results from those of the Edinburgh Committee, and Röhrig† has found that the administration of purgatives, as well as other measures which increase the circulation in the portal system, augment the formation of bile. The important question in regard to the treatment of biliousness, however, is not whether the liver forms more or less new bile, but whether the bile already circulating in the blood is removed from it. The liver may be doing its best to effect this purpose, but it will not succeed if the bile it removes from the portal blood is again absorbed as quickly as it is poured into the intestine. But if the peristaltic action of the whole intestinal canal is quickened by a purgative, the bile will be hurried rapidly onwards and evacuated before there has been time for its reabsorption, and the liver being thus relieved will be able to excrete any bile still remaining in the blood. This result will not be affected by any purgative acting on the large intestine alone, for a considerable part of the bile will in all probability have been absorbed before it gets so far; but any simple purgative or mixture of purgatives which stimulates the duodenum and small intestine as well as the large one will prove most effectual. Now, the green color which the fæces present after the administration of mercurials, and which is so distinctive that the name of "calomel stools" has been applied to them, has long been regarded as an evidence of bile and appealed to as a proof of the cholagogue action of these remedies. The opponents of this doctrine have declared that the color was simply caused by the presence of black sulphide of mercury, just as a somewhat similar color may be occasioned by the presence of a small quantity of sulphide of iron after the administration of mild ferruginous preparations. Their statement has been disproved by Bucheim, who has shown that the color

* Report of the British Association, 1863, p. 214.
† Stricker's Medicinische Jahrbucher, 1873, p. 250.

is really due to bile, and thus established the fact that calomel induces its expulsion from the intestine. It may therefore well be called a cholagogue, and it is evident from what has already been said that it must diminish the quantity circulating in the blood, whatever its effect may be on the amount formed by the liver.

Other substances besides bile are found in calomel stools, and among the most important of these are leucine and tyrosine. These bodies are produced by the action of pancreatic juice on albuminous substances, and their presence, which was discovered by Radziejewski, indicates that the contents of the duodenum and small intestine have been expelled before much absorption has taken place. Now, the duodenum not only contains half-digested food and bile, but also the gastric and pancreatic juices and the ferments to which they owe their activity. It is generally taken for granted that after these ferments have once aided in digesting a meal they are destroyed or evacuated, and no importance, so far as I know, has ever been attached to their reabsorption. It appears from the experiments of Brücke, who found pepsin in the muscles,* that it is reabsorbed, at least in part, and is indeed excreted in the urine, as is also a diastatic ferment derived from the saliva or pancreas.† Pancreatic ferments are also probably absorbed, for Hüfner has found some possessing like them the properties of digesting fibrin as well as converting starch into sugar in the salivary glands and lungs.‡ If these ferments, then, are poured into the intestine and absorbed from it again in the same way as bile, it seems highly probable that they also are excreted by the same glands which formed them. The function of the gastric follicles and pancreas would thus be a double one like that of the liver, and they would constantly excrete the ferments absorbed from the intestine and brought to them by the blood, as well as form new quantities of them to replace those which were carried off in the fæces or destroyed in the process of digestion. This view derives some probability from the observation of Schiff, that after the stomach has already digested a copious meal and become empty, its power to digest albumen is almost entirely lost,§ and the fact noticed by Bernard that when the pancreatic juice is drawn away by means of a fistula, what flows from the gland some time after the operation does not possess the power of digesting albumen like the juice which has been collected immediately after the insertion of the cannula.‖ These facts have been explained in a different way by Schiff and Bernard, but it seems to me that the explanation just given supplements without excluding theirs, and clears up some points which they have not touched.

* Brucke, "Sitzungsbericht der Wiener Academie," 1861, xliii. pp. 622, 619.
‡ Conheim, "Virchow's Archiv," xxviii. p. 250.
† Hufner, "Journal fur practischen Chemie," vol. v., p. 372.
§ Schiff, "Physiologie de la Digestion" tom. ii. p. 195.
‖ Bernard, "Physiologie Experimentale," tom. ii. p. 229; compare also p. 223, where he states that the juice becomes watery towards the end of digestion.

There is this important difference between the glands just mentioned and the liver, viz., that the bile can circulate in the portal system between the liver and intestine without reaching the systemic circulation, but the gastric and pancreatic ferments absorbed from the intestine cannot reach the stomach and pancreas again without mixing with the general current and the blood, and being conveyed to other organs as well. Pepsin cannot act in an alkaline fluid like the blood, but pancreatic ferment can; and although I do not know that any experiments have been made with it, yet Binz and Siegen found that a ferment derived from the liver, and possessing like the pancreatic one a diastatic power, raised the temperature of an animal when injected into it.* This rise was due to its action as a ferment, and not to its mere presence in the blood as a foreign body, for it had no action whatever when it was injected after its fermentative power had been destroyed by boiling. It is therefore quite possible that the temperature of the body is normally maintained to some extent by means of the pancreatic ferments circulating in the blood, and if purgatives diminish its quantity in the way I have supposed they will tend to lower the temperature.

It must be remembered that these are only suppositions as yet, and require much further substantiation, but they help us at any rate to form some idea of the way in which purgatives prove useful when given at the commencement of a fever. They also give us some notion of the reason why persons so often take cold after the use of purgatives, and one of the dangers of their administration to old people, who produce little heat at any rate, and can only slowly form new supplies of any ferment once carried away.

It is possible that purgatives have an additional action in remittent and intermittent fevers due to malaria, and even in continued fevers due to other poisons. Lussana supposes that the malarious poison which certainly produces some of its most marked effects on the spleen and liver, circulates like other poisons in the portal circulation.† If this hypothesis be correct, purgatives may be productive of benefit by removing part of the poison as well as by lessening the temperature.

The pancreatic and gastric ferments have a very positive and certain use in digesting food in the intestine, even should they not possess the hypothetical action in the blood to which I have just referred; and if they are usually absorbed and excreted again, a constant course of purgatives will seriously diminish their quantity. In consequence of this, the digestion of food will be carried on slowly and imperfectly, and the general health will suffer! But this will only be the case if purgatives are used

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* Siegen, "Ueber die pharmacologischen Eigenschaften von Eucalyptus Globulus." Inaugural Dissertation. Bonn, 1873, pp, 32, 34.

† Lussana, op. cit. p. 358.

which act on the whole of the bowels, for those which affect the large intestine only will interfere but slightly with the ferments, a considerable portion of which will probably have been absorbed before they get so far. We can thus perfectly understand how a constant course of blue pill* and black draught† may have most disastrous consequences, while an aloetic pill may be swallowed nightly for months together, without doing any appreciable harm.

The experiments of Moreau and Vulpian, as well as my own, show that a large quantity of fluid is drained away from the blood into the intestine by the action of purgatives, and we can thus readily understand their use in removing fluid in dropsies. The abstraction of so much fluid will tend to empty the blood-vessels, and at the same time the irritation caused by the purgative will attract a large proportion of blood to the intestinal vessels, and thus still further lessen the blood-pressure in other parts of the body. The blood being no longer urged onward with the same force, the congestion in any inflamed part diminishes, and the painful throbbing which is felt at every pulsation when certain parts of the body are inflamed will be diminished, or may disappear, at least for a time.

When the kidney is the organ affected, the benefit afforded by purgatives will be twofold, for they both diminish the work it has to do by eliminating water in the bowels, and at the same time lessen congestion, and thus remove an impediment to the proper performance of its function. Accordingly the administration of a purgative, such as elaterium, is found to lessen and sometimes to remove albumen from the urine, to render the secretion copious even when no diuretic has been given, and greatly to increase the activity of diuretics, which may have been unable to produce any action so long as the bowels were left alone.‡

In the Hastings Prize Essay upon the pathology and treatment of ovarian diseases, Dr. Lawson Tait makes the following remarks :

The most common diseases of the ovary are those due to incomplete or perverted functions. Some rare cases there have been of inflammatory attacks, of cystic degeneration, and even of cancer, of the ovary in infancy and childhood; but, as a rule, the ovary is free from disease till after the age of puberty. During infancy and childhood, the processes of cell-growth and shedding go on, but without any indication of their action until those mysterious changes take place which indicate that a new

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* Prout. "Stomach and Renal Diseases," 5th ed., p. 52.

† Pancreaticiferment appears in the fæces after the use of senna. (Radziejewski, Reichert and Du Bois Re mond's Archives, 1870, p. 72.)

‡ Geo. Johnson, *Brit. Med. Journal*, 1868, March 7, p. 215.

function of the organism is about to come into action, the maturation of the ovum, and the possibility of its impregnation. The chief external sign of this is the menstrual flow; but that this is no necessary part of the process is abundantly evidenced by the facts, that some women have large families without ever once having seen a catamenial discharge; that girls have become pregnant before the external evidences of puberty have appeared, a case of this kind having recently come under my own notice; and, further, that women sometimes become pregnant after the entire cessation of the monthly flow for many years. Since we know, then, that the ovarian cell-growth is quite independent of the menstrual flux, and that it may even complete its functions without it, we can only regard the flow as an accompanying phenomenon, and as neither a cause nor a result. That this is really the case is further proved by disease; for, in cases where both ovaries have been removed for follicular dropsy, and where it has been absolutely impossible that any evolution of ova could have taken place for many months, if not years, before the operation, the menstrual flow has occurred in normal regularity and quantity up to the time of the ovariotomy. On the other hand, we know that the flow of blood is intimately associated with the ovaries, for it usually ceases after the removal of both, and continues with uninterrupted regularity after the removal of only one, if the other be healthy; further, that any interference with the ovarian nerves, as their division by the écraseur or their inclusion in a clamp, will bring on the flow in a few hours.

The accession of puberty alters the nutrition of the ovary to the extent that, at the monthly periods, it shares in the general state of hyperæmia and excitement then common to all the sexual organs, and the whole economy seems to share more or less in the disturbance. Normally, this change takes place in the fourteenth or fifteenth year of life in this country; at an earlier date in hot climates. In strong, healthy girls, especially those engaged in active out-door work, still more those living a life approaching to the primitive state, the moliminal change is effected without suffering; but in girls brought up in refinement, of delicate habit and strumous parentage, there is much trouble. As a rule, this seems to be due to the onset of menstruation and the other signs of the change while the ovary is still in its infantile or incompletely developed condition; that is, it is forming incomplete cells whose nuclei are incapable of fulfilling their great functions, and the whole mechanism of ovulation is out of gear. In such cases, we find that the menstrual flux comes on either at irregular times or in insufficient quantity; or that, if it come regularly, it is overabundant, and it is always accompanied by severe ovarian pain. This arrest of development may be so complete that the ovaries may be said to be absent, though, clinically, this condition cannot be said to exist. The entire absence of the ovaries has been proved only in deformed fœtuses.

The arrest of development may, however, be so complete that menstruation may never occur at all, or only once or twice. In such extreme cases, the development of the whole sexual apparatus is generally arrested, the sexual appetite is in abeyance, and there is comparatively little suffering after the first few months, during which an effort seems to be made by the system to establish the change. This is, provided epilepsy does not supervene; but it is only too common an accompaniment of arrested sexual development in women. Women who are thus affected have frequently an absence of those external peculiarities of their sex evident in roundness of form, a *prononcé* bust, smooth and hairless skin, and highly pitched voice; and they often partake in some slight degree of the characters of the opposite sex, especially in the growth of straggling tufts of hair on the upper lip and on the chin in a line with the canine and premolar teeth.

A great number of cases have the arrest at a later stage, and in them menstruation is established, after much difficulty and suffering, between 16 and 19 years of age, and, though it may last with fair regularity, but deficient quantity, for four or five years, it then ceases completely. In many of these cases, however, if marriage should occur during the time that menstruation is in action, and if the patient should be fortunate enough to become pregnant, a cure may result; that is, her periods will become more abundant and her suffering less; her health will be improved, and she may go on menstruating for many years, and may even have a number of children. Even without the occurrence of pregnancy, marriage often establishes the health of a woman afflicted with arrest of ovarian development.

The great bulk of cases of this kind are those which are afflicted to a less degree, but whose sufferings are nearly always sufficient to require medical assistance; and it is a very singular fact that a very large percentage of the cases are found in women of splendid physical development, who, to any but one well acquainted with such cases, look the most likely to possess capacity for procreation. In these women, menstruation is established later than the normal time by a few months or a year or two. They have at first irregular times and much pain, but, after a while, the flow is established with normal quantity and regularity, and with but little suffering. In this way, they go on for eight or ten years, and, if they marry in the interval, their menstrual career may run an ordinary course. If they remain single, however, they begin to suffer from ovarian dysmenorrhœa between 25 and 30, and, after about ten years' suffering, they undergo a premature climacteric change. It is also noticeable in these women, that their menstrual function is suspended on slight provocation. Any chronic disease, even of an unimportant nature, any occupation which necessitates an overstrain on their system, mental anxiety or sudden fright, will check their menstruation for months or years, or, perhaps, for ever. In fact, this slight excess of functional power which the ovary became possessed of

at their puberty is readily and soon exhausted, and its extruded cells, on slight provocation, assume an immature form, and the systematic conditions become correlated. In fact, ovarian amenorrhœa, and similarly to a less extent ovarian dysmenorrhœa, is a temporary resumption of the infantile condition of the ovarian functions; or, it may be, a complete and premature assumption of its senility. The amenorrhœa of pregnancy and lactation are partial resumptions of the infantile condition. This view has been admirably expressed by Dr. Charles Ritchie: "In early infancy, extreme old age, and long-continued organic disease, the ova are minute, transparent, and structureless; and, in advanced childhood, soon after the critical age and during pregnancy and lactation, they are more or less organized, larger, and, in the latter stage, are often so well maturated, that about one-third of the renewed pregnancies of married women take place while they nurse."

In these slighter cases of ovarian dysmenorrhœa, the uterus is generally normally developed, and it is frequently so in some of the most severe cases. There is a converse condition, where the uterus is infantile and the ovaries normal, much more rare and far more severe in its symptoms.

In ovarian dysmenorrhœa, the general symptoms are pretty constant and distinctive. Besides the menstrual irregularities and deficiency, there is almost always a persistent, sickening, and well marked ovarian pain, occurring in the less severe cases only at the menstrual periods, but in others being seldom absent and always greatly increased at the periods. It originates in the ovarian region, and shoots down the thigh, often also down the leg and round to the back. There is also often present, especially on the accession of atrophy, the peculiar submammary pain of ovarian disease, generally felt in the left side only. Headache, nausea, or even sickness and great general discomfort, are always present more or less.

In the milder cases, treatment is generally successful in mitigating the sufferings, and often the ovary may be made, even in some very well marked cases of arrested development, to fulfil its functions completely. First of all therapeutic remedies there stands iron, which will be found in such cases to be of great use, even though there should be no general indications for its employment. There can be no doubt that many forms of this remedy have a specific power over the sexual organs, male and female; for, in a case of chronic metritis or subinvolution, smart hæmorrhage may be induced by large doses of iron. In ovarian dysmenorrhœa, it is best given during the intermenstrual periods in small doses, one to five drops of the liquor ferri perchloridi, well diluted, and increased suddenly to fifteen or twenty for a day or two previous to and during the menstrual flow; or better still is the substitution for this large dose of an iron and aloes pill, there being few better combinations in the Pharmacopœia than that old-fashioned remedy. Hot hip-baths and leeches to

the perinæum at the period are often useful additions, with an occasional blister on the sacrum. To such as this, the treatment of delayed or difficult menstruation at puberty, due to inefficient ovarian development, must be confined; for the other means are only allowable in very obstinate cases, after the patient has been married, or where there are indications of premature ovarian atrophy. Marriage is, perhaps, the most efficient remedy, and one we ought seldom to hesitate to recommend; for even if the patients may not have children, they will have better health, and they may even become pregnant if they marry early enough and are not mismanaged.

The last and most powerful aid is mechanical irritation of the uterus; but as it is not free from risk, and therefore requires careful use, it is not always to be recommended. It is besides, in the class of cases where the uterus is most at fault that it is least risky and most serviceable. The method of irritation I generally employ, as the most convenient and least troublesome, is the insertion of Simpson's galvanic pessary. This instrument has by some writers been very much decried, but I think by those only who seem to have used it indiscriminately and without reference to a proper selection of cases.

The irritation set up by the presence of a galvanic stem in the uterus is communicated indirectly to the ovaries in a manner that is not as yet explicable, but that it has an influence is beyond doubt, and, if it remain within bounds, it is in a large number of cases beneficial. A large experience has shown me that it is only in occasional instances that the stem cannot be borne, and that, if carefully watched during the first few weeks of its use, these cases are easily eliminated. In a case where I have been led to regard the use of the stem as advisable, I always begin with a small size, and, after this has been worn for two or three months, I change it for a larger one. For the first week after its introduction it is not unusual for the galvanic stem to give rise to considerable discomfort and even positive pain, but this usually passes off if the patient keeps her bed for a few days, and there is no further trouble save from the leucorrhœal discharge, which is a part of the process. The action of the stem is not purely mechanical, as has been stated; for, very soon after its insertion the zinc becomes coated with an albuminous deposit, from which the copper is free, and the zinc becomes corroded. It is certain therefore, that there is a galvanic action set up, and the stimulating effects are due partly to this, and partly to the interior of the uterus being constantly bathed in a weak solution of chloride of zinc. However produced, it is certain that the uterus rapidly enlarges under the action, and there is every reason to believe that the ovaries take part in the increased activity. If once the uterus become accustomed to the presence of the galvanic stem, it may be worn for many months, and the longer it is retained the more permanent will be the benefit; but, if after a trial of a few months, say four or five, there is no

apparent alteration for the better, the attempt may be given up and the case considered as hopeless.

In a very large number of cases of incompletely developed ovaries, another remnant of infantile life is met with in an exaggeration of the normal curve of the uterus, amounting sometimes to complete anteflexion, and in this class of cases the galvanic stem is especially serviceable.

The results of my attempts to arrest premature atrophy of the ovary from any cause, when once begun, have been far from satfactory; but this has been more especially the case when that atrophy has been due to a constitutional disease, such as tubercle. Sir James Simpson had a belief that the pretubercular amenorrhœa, so often seen in young women, was a cause of the subsequent disease; and he, therefore, directed his attention to the restoration of the utero-ovarian function, as a means of treatment or prevention of the consumption. From the views previously expressed, it will easily be seen that I consider his theory to be based on error, though in some cases his treatment would seem to have been successful; but how much of his success was due to local, and how much to general treatment, cannot now be determined. It is not, however, a practice likely to meet with many followers.

A singular condition has been noticed recently by Dr. Priestly, of intermenstrual pain occurring about midway between the periods, which is almost certainly due to an ovarian condition, though it is not clear of what kind. Since reading his paper I have seen two cases, but have been unable to refer them to any category.—*British Medical Journal*, May 30, 1874.

The Treatment of Typhoid Fever by the External Use of Water. (*Allgemeine Militär. Zeitung.*)

Regimental Surgeon Lederer during the past five years has treated many cases of typhoid fever with cold water, and at a meeting of the Society of the medical officers of the garrison of Vienna, in April of this year, he gave the results of his experience. After calling attention to the prominent rôle played by typhoid fever among the diseases incident to military life, and noting the high rate of mortality which attended it in the late civil war, he proposed the following three queries. 1. Are the results obtained from the cold-water treatment better than those from other modes? 2. Can this treatment be advantageously carried out in military hospitals? 3. What symptoms should be regarded as indications for the use of the cold-water treatment? The extreme hydropathists would give an unqualified affirmative to the first query, for they assert that by their mode of treatment the mortality is greatly reduced. Some of them even assert that water is a specific in the treatment of this fever, and that if the

patient is properly treated at an early stage the disease can be cut short. From his observations, Lederer does not look upon water as possessing this specific power, and he thinks that the trials of this treatment have not as yet been sufficiently numerous to warrant the formation of conclusions as to its value. He is convinced, however, that under this treatment the disease runs a milder course, and that some of the graver symptoms may not manifest themselves. The intense headache which is sometimes one of the initial symptoms may vanish after the first baths. So, too, the delirium which commonly appears at the end of the second week may by the use of baths be so greatly modified as to be of a very mild type, or even entirely done away with. In support of this he adduces several cases in which the rapid transition from a state of wild delirium to quiet after the use of a few baths was most marked. The influence upon the return of the appetite is also marked; the tongue becomes cleaner, and the diarrhœa is not so excessive. The influence of the bath, too, in tending to produce sleep, is most marked. Bed-sores are not so apt to occur, and convalescence is more speedy. The advantages of the cleanliness promoted by the frequent use of the bath upon the general hygiene of the ward can be readily understood. As to the methods to be employed, Lederer thinks that packing the patients in damp sheets is not suitable to military hospitals, since the frequent changes of the cloths which are required call for the services of too many nurses. He therefore does not use this method, nor the one in which the surface is sponged off, but advocates the use of the cold bath, either with or without the douche. The patient, when the temperature in the axilla is above 101.5° Fahr., is placed in the bath, and a greater or less amount of water is poured upon him as the cerebral symptoms are more or less pronounced. These applications are made from 7 a. m. until 12 at night, at intervals of three or four hours, the temperature of the bath varying from 68° to 77° Fahr., that of the douche from 54° to 60° Fahr.

In answer to the third query, as to the indications for the use of the bath, he thinks that every patient with typhoid whose axillary temperature rises to 101° or above is a fit subject for this treatment, and that the earlier the cold bath is used the better it will be borne, and the more promising are the results to be looked for. If symptoms of cerebral involvement are present, the indications are still more pressing. He thinks that more care is to be exercised in the use of the bath upon such patients as are very restless, or have marked hyperæsthesia of the skin, and if the first bath is not well borne in such a case it should not be repeated. If complications arise during the course of the disease, such as infiltration of the lung or peritonitis, the baths must of course be discontinued. In conclusion, then, he thinks that this mode of treatment is of advantage in many cases, that it tends to diminish the risk of injury to delirious patients, that the risk of the occurrence of bed-sores is lessened by it, and that

its use tends to limit the spread of the disease.—*Philadelphia Medical Times*, July 4, 1874.

Guenel on Mercurial Frictions in serious Syphilitic Complications.

In the *Thèses de Paris*, 1874, No. 145, Dr. Augustin Guenel demonstrates from numerous instances the heroic action of mercurial frictions in the treatment of syphilitic complications; more especially when they affect the cerebro-spinal axis. He shows that this method, which has the advantage of not distressing the digestive tube, acts as well against secondary accidents as against tertiary, and that it often succeeds where internal medication has failed.—*London Medical Record.*

Simbat on the Use of Chloride of Zinc in the Treatment of Fistula.

In a paper published in the *Thèses de Paris*, 1874, No. 73, and based on cases for the most part observed in M. Gaujot's service at the Val-de-Grace, Dr. Simbat shows the good effects which may be obtained by the use of Canquoin's paste in the treatment of fistula, and particularly in the use of anal and even urinary fistula. The chloride of zinc is employed with advantage in the treatment of fistula, in consequence of the granulating power it imparts to their walls; by reason of the facility with which it is applied; also in consequence of the absence of the accidents which might accompany wounds from cutting instruments; and, lastly, because it is more likely to prevent recurrence of the evil than other methods of operation.—*London Medical Record.*

[As long since as 1859 we employed chloride of zinc in the treatment of fistula in ano. The mode of preparing and using it was suggested by an article published in the *Gazette Médicale de Paris*, by MM. Salmon and Manoury. Our testimony is favorable to its use, especially in cases where the knife cannot be employed, on account of the patient's non-consent, or because he cannot give the time from his ordinary pursuits. For the preparation of this caustic, it is necessary to dissolve some gutta percha in a porcelain cup. When in a state of fusion throw in the determined amount of chloride of zinc, which should be thoroughly incorporated with the gutta percha by means of a spatula. We thus obtain a plastic paste which may be moulded like nitrate of silver, into cylinders, or in sheets like Canquoin's paste. There is nothing more simple, but it is necessary to seize the proper moment to withdraw the mixture from the fire.

This is extracted from the translation we made of the article at the time of its publication. Its mode of application consisted in introducing one of these cylinders into the fistula, and allowing it to remain for a length of time varying from two to six hours.]

Injections of a Solution of Ergotin in Prolapsus Ani.

Von Langenbeck, of Berlin, is said to have been very successful in his treatment of prolapsus ani with hypodermic injections of a solution of ergotin, in the proportion of ten parts to one hundred parts of distilled water.

After the bowel has been replaced and the point of the syringe inserted into the cellular tissue, two grains of ergotin are injected. This should be repeated for four weeks on every third day.—*Physician and Pharmacist.*

External Employment of Perchloride of Iron in Varicose Veins.

Dr. Linon uses compresses wet with a solution of perchloride of iron, formed by adding one part of the liq. ferri perchlorid. to twenty-five parts of water. The compresses are kept in position over the seats of the varices by means of a bandage smoothly and somewhat firmly applied. After these compresses have been in place for twenty-four hours the venous dilatations seem almost entirely removed. The compresses are kept applied, however, until they are entirely gone, which is usually in from eight to ten days, according to volume.

By this dressing Dr. Linon says he has succeeded little by little in dissipating enormous varices, accompanied by violent pain and the appearance of black points on the skin. That the good effect thus produced is not due to the bandaging alone, is shown by the fact that the varices do not return after the bandages are taken off. Flannel bandages, Dr. L. thinks, are preferable to those made of linen, since they are more elastic, will contain more of the liquid, and do not become stiff and incapable of absorbing.—*Philadelphia Medical Times.*

The following ointment has been used successfully by Dr. Kennard in the treatment of incontinence of urine in paralytic cases:

℞ Morphiæ sulph.,
Veratriæ, aa gr. x.,
Axungiæ, ʒii.—M.

Rub well into the Perineum three times daily.—*Philadelphia Medical Times.*

New Antiseptic Ointment.

Professor Lister, according to the *Students' Journal and Hospital Gazette*, is at present using, with great success, an ointment composed of paraffin, 2 parts; white wax, 1 part; sweet oil of almonds, 2 parts; and boracic acid (powdered), 1 part.—*Medical and Surgical Reporter.*

Unstopping a Deaf Ear.

Editor Medical and Surgical Reporter:—June 3d., 1874. C. G. B., aged sixty, complained of sudden deafness with the right ear, accompanied with a feeling of pressure on that side of the head, roaring and buzzing noise, etc. Examination of the meatus auditorius with speculum revealed a dark mass at or near the tympanum. Ordered injected into the meatus, with a fountain syringe, small nozzle, nitrate potassæ, gr. x. in aqua, 98 Fahr., one quart; in the evening to have dropped in the ear gtts. iv. of the following:

R. Glycerine, ℥j.,
Acid carbolic, gtts. ij. M.

And a pellet of wool to be kept in the ear. This treatment was repeated once each day to the 8th inst., at which time the injection brought away a large quantity of inspissated cerumen, with immediate relief of pressure, noises, and restoration at hearing.

Fulton, N. Y. Chas. G. Bacon.

Treatment of Cholera by the Hypodermic Injection of Chloral Hydrate.

The supplement to the official *Gazette of India* contains a report from the civil surgeon, Kheri, in Oudh, with copies of communications from Dr. Hall and the Deputy Surgeon-General Indian Medical Service, Lucknow Circle, on the treatment of cholera by the hypodermic injection of chloral hydrate. It appears that Dr. Hall, in a paper published in the *Indian Annals of Medical Science* for March, 1870, stated that in the cold stage of cholera, instead of exhaustion of the nervous system, as was generally supposed, there is intense irritation of certain sets of nerves. He suggested that the principle which should guide us in the treatment of this condition was the endeavor to quiet the nervous system by the action of pure sedatives, and recommended the practice of hypodermic injection of sedatives in the stage of collapse. Subsequent experiments as to the physiological effects of chloral hydrate induced Dr. Hall to make a trial of this agent in cholera, and his practice has been followed by others with satisfactory results, so far as the reports at present extend.—*Lancet*, May 2, 1874.

[While our readers must estimate this excerptum according

to their own ideas of its value, we take this occasion to state that in the treatment of an unusually aggravated case of acute dysentery just dismissed as convalescent, we derived most unquestionable benefit from chloral hydrate by giving it every night in sufficiently large doses to ensure sleep.]

Painless Method of Cauterizing with Nitric Acid.

It is found at the Charity Hospital, New York (*New York Med. Journ.*, May, 1874), that chancroids can be cauterized with nitric acid without causing severe pain, by first applying to the sore pure carbolic acid. The carbolic acid serves as a local anæsthetic, and prevents the nitric acid from causing pain which is not easily borne by the patient.

[The *American Journal of Pharmacy* for June, 1874, contains the following recipe for a cement for affixing labels to tin and other metallic substances:

Tragacanth mucilage, 10 parts,
Honey, - - 1 part,
Flour, - - - 1 part.]

Treatment of Tapeworm.

Dr. Schafhirt recommends, in the *Druggists' Circular*, the following preparation in tapeworm:

℞. Bark of pomegranate root, ℥ss.,
Pumpkin seeds, - - ℥j.,
Ethereal extract of male fern, ʒj.,
Powdered ergot, - - ʒss.,
Powdered gum arabic, - ʒij.,
Croton oil, - - - gtt. ij.

For Flatus.

The following pills are used in the Roosevelt Hospital, New York, for flatus in the bowels:

℞. Pulv. camphor,
" capsicum,
" ginger, aa gr. j. M.

Divide into six pills.
S. One p. r. n.
They are said to afford immediate relief.—*Med. and Surg. Rep.*

Pepsin in the Treatment of Diphtheria.

Dr. Doughly advises the following formula for softening and dissolving the false membranes of croup and diphtheria:

℞. Pepsin, - - 6 grammes,
Dilute muriatic acid, gtt. v.,
Distilled water, - 100 grammes.

To be used by inhalation.—*Med. and Surg. Reporter.*

Chloral in Premature Labor.

M. Martineau makes use of enemata containing sixteen grains chloral hydrate in four ounces of water, in cases where premature labor has set in. If the first injection does not quiet the pains, it may be repeated at intervals of several hours. Chloroform is said to be formed in the bowel by the action of the fæces, which are usually alkaline.—*Philadelphia Medical Times.*

For Constipation.—

℞. Aloës Socotrine, gr. xv;
Ext. anthemidis, gr. xv;
Ext. rhei, ʒss;
Zingiberis pulv., gr. viii.—℔.

Divide into twenty pills; one or more at night as required.—*Philadelphia Medical Times.*

Lotion for Fetid Perspiration of the Feet.

℞. Potassii permanganat, gr. xxx.
Aquæ, fʒiv.—℔

The lotion to be applied morning and evening, and the feet to be afterwards powdered with lycopodium or starch.—*Philadelphia Medical Times.*

Pomade for Venereal Alopecia.

℞. Hydrarg. protiodid., gr. xv;
Axungiæ, ʒv;
Tinct. cantharidis, ℥iss to ℥iv.—℔.

Apply morning and evening, using at the same time appropriate internal treatment.—*Philadelphia Medical Times.*

Latest Modification of Cod-Liver Oil Emulsion.

We have several times called attention to an emulsion of cod-liver oil and phosphoric acid. The last report of the Utica Insane Asylum contains a formula for an emulsion that has long been in use in that institution, and to which our attention was first called by Dr. Andrews. We have experimented considerably with various modifications of the original prescription. The latest formula, and one that suits us better than any other is the following:

(Jamaica rum seems to cover the taste better than sherry wine, which has usually been employed.)

℞. Cod-Liver Oil, ℥ iv.
Glyconin, ʒ ix.

Glyconin is made by thoroughly triturating glycerine and yolk of egg, equal parts. Add to the glyconin thirty drops of the essential oil of bitter almonds; then add the oil to the glyconin *very slowly*, drop by drop, stirring vigorously all the time. The success of the emulsion depends on the thoroughness with which this task is performed.

Then add—

Jamaica Rum, ℥ ii.
Dilute Phosphoric acid, ℥ ss to ℥ i

The average dose is one tablespoonful after meals, being regulated mainly by the phosphoric acid.

The above combination is an excellent brain and nerve food. If properly prepared, it does not separate, keeps for a long time, and is rather agreeable to the taste. If need be, pyrophosphate of iron can be added, or strychnine, or Fowler's Solution. We have used it especially in hysteria and allied affections, and in organic diseases of the nervous system it is also valuable. Consumptives take it in preference to cod-liver oil. As cod-liver oil has a somewhat unpalatable name, it is sometimes better, in prescribing for nervous patients, to call this the phosphoric emulsion. The fishy odor cannot be entirely neutralized; but for those who are not familiar with cod-liver oil, neither the odor nor taste of this emulsion, when well made, suggest the presence of the oil.—*Archives of Electrology and Neurology.*

New Mode of Administering Raw Meat.

Dr. Yvon gives the following method of preparing a palatable mixture having all the virtues of fresh meat while possessing an agreeable taste:

Raw meat (fillet of beef),	250 parts,
Sweet almonds (charred),	75 "
Bitter almonds, - -	50 "
White sugar, - -	80 "

The mixture is to be rubbed slowly up in a mortar until a homogeneous paste is obtained, adding from time to time a sufficient quantity of water to give a proper consistency. The amount of water may be so varied as to give a solid or a liquid mixture. The liquid, which is in the form of an emulsion, will slowly settle, but may easily be mixed again by shaking lightly. It may be preserved unchanged for a long time if kept in a cool and dry place. In order to render it more nourishing, the yolks of one or more eggs may be added.—*Philadelphia Times*, June 20, 1874.

Formulæ for Pills.

The following formulæ, furnished by Mr. Proctor in his recent work on "Practical Pharmacy," are specially selected for approval by his reviewer in the London *Chemist and Druggist:*

CREOSOTE PILLS.

Creosote - - - gtt. xxiv.,
Ft. pil. xij., with
P. amygdal., - - - gr. xxx.,
Glycerine, - - - gtt. xij.

CAMPHOR PILLS.

Camphor, - - - 3j.,
Ft. pil. xij., with
Ol. ricini, - - - gtt. iv.,
P. sapon., - - - - gr. ij.

Rub the camphor to a powder with the oil, and work into a mass with the addition of the soap.

CARBOLIC ACID PILLS.

Acid carbolic, - - - gr. iij.
Ft. pil. j. Mitte xlviij.

"144 grains of purest carbolic acid and 24 grains of yellow wax were melted together in a test-tube, poured into a cold mortar, and rubbed into a rather adhesive powder; soft soap was then added by small degrees till 8 grains had been added, when it was found that the whole could be worked into a mass and divided into pills."

CHLORAL HYDRATE PILLS.

Chloral hydrat. - - gr. lx.,
Pulv. tragacanth, - - gr. iij.,
Soluble cream of tartar, - gr. xij.

Mix, and divide into twenty pills.

Calcined magnesia seems to be the best excipient for balsam of copaiva and Chio turpentine; treacle, for iron compounds, and syrup. aurantii for Pil. Rhei Comp.—*Boston Jour. Chemistry.*

Edes on Pepsin.

Dr. R. T. Edes has examined many of the preparations of pepsin now offered for sale, and gives (*Boston Medical and Surgical Journal*, January 1, 1874) the following as his conclusions.

"Much of the dissatisfaction with pepsin expressed by physicians is due to the use of preparations which contain little or none of it.

"The pepsin made by Sheffer's process is by far superior to any other in ordinary use. The wine is feeble, but necessarily inert. Elixirs of pepsin and bismuth are humbugs. Pepsin should be administered with an acid, and with as few drugs as possible. A small amount of alcohol is not inadmissible, but a large amount retards digestion.

"Its beneficial action is not limited by the amount of albumen which it dissolves in a test-tube without change or renewal of any of the contents."—*London Medical Record.*

[The following very important statements respecting the value of the elastic ligature, in surgery, are from the *London Medical Record.*]

"In nævus, after transfixing it with needles as for ordinary ligature, I have the tumor fixed by an assistant during the tying. A single, circular ligature is sufficient. In the two cases in which I used this treatment, the nævus fell off in eight days.

"In fistula ani or sinuses about the rectum, the elastic ligature has especial advantages where the inner opening of the fistula lies high up or the sinus extends far. In the case of sinus, an inner opening (into the rectum) is first made by means of a trocar. The trocar being withdrawn, the elastic thread is introduced through the canula and drawn out through the rectum. This proceeding is rendered more easy by first introducing a metalic thread into the rectum through the canula, seizing it with the finger or forceps, drawing it through the anus, and removing the canula. The outer end of the wire is now fastened to the small elastic tubing by means of a waxed thread. In this way the elastic cord is very easily drawn through, if the index finger of the left hand can be passed up the rectum as far as the opening. Both ends of the ligature are drawn upon, and tied rather tight. The bridge of intestine with the sphincter is generally cut through in three or four days, and the patient finds the ligature, contracted into a ring, lying in his bed. In complete fistula ani, the metal wire is carried into the intestine along the groove of a director, and the use of the trocar is unnecessary.

"In prolapsus ani, the protruding fold is seized with hooks or polypus-forceps, as for the application of the ordinary ligature, and drawn down a little. In order that the fold may not escape from the forceps while the ligature is being applied, an assistant must press against the fold after it is seized, a second must stretch the cord, and a third must fix between his fingers a small portion of the cord corresponding to the fold, so that it remains only for the operator to tie the ligature. The ligature falls off in three or four days.

"In tumors, the elastic ligature has its application when they are pedunculated, or when they can be isolated, or when it is not possible or necessary to save the skin. It is thus inapplicable in diffuse infiltration. I have had only one opportunity of applying the elastic ligature to a tumor. The patient was Frau H., aged 74, and at her age neither I nor another surgeon would have undertaken the risk of a bloody operation. But, having already ascertained that the elastic ligature did not produce any fever, and as the old lady earnestly desired that the tumor—a fibrous cancer of the right breast—should be removed, I concluded to make this first attempt. The cancer had at its base a vertical diameter of about four inches, and a transverse diameter of five inches, and was movable. I passed a Flenrand's trocar through, beneath the tumor, in the direction of the vertical diameter; and, having withdrawn the trocar, I introduced through the canula two waxed threads, and by means of them drew through two pieces of small India-rubber tubing; the canula was then withdrawn, leaving the tubes. I now, while an assistant drew forward the tumor, embraced each half in the corresponding portion of tube, and tied the ends of the tubing firmly in a double knot. This was done in the out-patient department, on January 9, 1873. On the second day, I had the patient taken into ward No. 81, as she was rather anxious on account of some pain. The furrow produced by the ligature became deeper daily before our eyes, leaving a granulating surface, while the tumor correspondingly collapsed, sloughed, and fell on the tenth day. During the whole time, the patient had no fever, only an increase of thirst on the eighth day. The old lady was right well pleased with the result.

"I have applied the elastic ligature to arteries in the following cases:

"*a.* To the popliteal artery, on the occasion of amputating the left thigh after Gritti's method. The ligature was thrown off on the sixteenth day.

"*b.* To the anterior tibial artery in two cases of amputation of the left leg, the ligature falling off on the seventh day; and once in a Pirogoff's amputation.

"*c.* To small branches of the anterior and posterior tibial and peroneal arteries."

NOTICES OF NEW BOOKS.

A Treatise on Food and Dietetics, Physiologically and Therapeutically considered. By F. W. Pavy, M.D., F.R.S., Fellow of the Royal College of Physicians; Physician to, and Lecturer on Physiology at, Guy's Hospital. Philadelphia: Henry C. Lea; 1874.

This may be considered a sequel to a late work by the same author, entitled "Digestion, its Disorders and their Treatment," which was republished about five years ago by the house of Henry C. Lea. Dr. Pavy has for several years been well known as an investigator and writer on Physiological topics connected with the great subject of nutrition, and his favorable reputation is sufficient to attract attention to the present work, which may be pronounced in advance the most thorough treatise in our language on the subjects treated. A general idea of these subjects, we believe, will best be gained from the detailed table of contents, which is herewith presented in full.

A glance at this table shows a large number of interesting topics, which are now occupying the minds of the most advanced scientific men of the age, and it hardly need be remarked that the author is fully acquainted with their views. Many of these are no longer novelties, having been adopted as elements of our scientific creed—a creed, however which must always be regarded as provisional; but obsolete and exploded theories need not be apprehended here, like spectres lingering on the shore of a ruder and more credulous age.

We are now ready to notice some of the distinctive features of the work. The modern doctrine of the correlation and conservation of the physical forces recognizes the sun as the grand source of supply for nearly every manifestation of power and action on our planet. Vegetation is dependent on this source for its growth, and it stores up potential force, which is acquired by animals through assimilation of their matter. The latent animal force thus acquired is rendered actual by the process of oxydation; in other words, chemical action, a form of molecular motion, is transformed and manifested as mechanical motion, very much as the combustion of fuel is transformed into motion through the agency of expanding water on the steam engine.

The doctrine of Liebig, that muscular action is dependent on, and proportioned to, the oxydation of muscular fibre, manifested by the elimination of urea; that nitrogenous aliments are appropriated solely for the repair of waste; that the fatty, starchy and saccharine elements of food only serve, by their oxydation, to support animal heat, is no longer adhered to by the most advanced scientific men. While it is admitted that muscular tissue is nourished solely by azotized food, it is maintained that this kind of food is capable of supporting combustion by the separation of urea, leaving a hydro-carbonaceous compound for oxidation. That this must be the case is proved by the Guachos of the South American pampas, who subsist only on animal food, consisting chiefly of muscular flesh. Dr. Pavy holds that this splitting up of the protein elements of food takes place in the liver, and therefore urea is formed in that organ, rather than in the kidneys or the system at large, as held successively by earlier physiologists.

The author's classification of alimentary substances is, first, into Organic and Inorganic. The Organic is subdivided into Nitrogenous and Non-nitrogenous, and the Non-nitrogenous into

Fats and Carbohydrates. Fats contain carbon and hydrogen, with but a small proportion of oxygen; while the carbohydrates contain hydrogen and oxygen in the proportion to form water. In the former all the carbon and most of the hydrogen are susceptible of oxidation, and in the latter the carbon only.

The experiment of Drs. Fick and Wislicenus on themselves, and that of Dr. Parkes on two soldiers, are adduced to show that the amount of urea excreted bears relation not to degree of muscular effort, but to amount of nitrogen in the ingesta. On the other hand, it is conclusively proved that the carbonic acid exhaled is precisely in the ratio of physical labor performed, and every one is familiar with the fact that the energy of respiration has the same ratio. Thus it is evident that force is generated in animal bodies as it is in the steam-engine—by the oxydation of combustible materials, chiefly carbonaceous. The following table is given by the author, showing the respective values of the different classes of food as force-producing agents, based upon the oxydable material presented for combustion.

	Amount of oxygen appropriated in oxidizing 100 parts as consumed within the body.
Grape-sugar (anhydrous),	106
Starch,	120
Albumen,	150
Fat.	293

The author's views in regard to the special uses of the respective alimentary principles are comprehensively stated in the following paragraph:

While nitrogenous matter may be regarded as forming the essential basis of structures possessing active or living properties, the non-nitrogenous principles may be looked upon as supplying the source of power. The one may be spoken of as holding the position of the instrument of action, while the other supplies the motive power. Nitrogenous alimentary matter may, it is true, by oxidation contribute to the generation of the moving force, but, as has been explained, in fulfilling this office there is evidence before us to show that it is split up into two distinct portions, one containing the nitrogen which is eliminated as useless, and a residuary non-nitrogenous portion which is retained and utilized in force-production. It is true also, as will be shown hereafter, that non-nitrogenous matter may be applied to tissue formation [adipose], but it is probable that, in doing so, it is simply for the purpose of being stored up for subsequent appropriation to force-production, according as circumstance may require.

His ideas in regard to the changes and uses of the carbohydrate group of aliments are certainly very interesting. He holds that sugar, unchanged, is absorbed into the portal circulation, while starchy aliments undergo this absorption after transformation into sugar. In the liver this undergoes a further change into what is styled by the author "amyloid substance," where it is retained for subsequent use. It has been found, he states, that the livers of animals fed first chiefly on azotized aliments, and afterwards on those belonging to the carbohydrogen group, have notably increased in size, with great augmentation of this "amyloid substance." He considers the proof satisfactory that these aliments reach the condition of "amyloid substance," or glycogen, as styled by Bernard, but admits that there are now some steps which cannot with certainty be traced. For reasons adduced, he adopts the belief that this substance undergoes transformation to adipose deposit, subject to future demands for the production of heat and force. The evidence is gathered from observations in the natural and artificial feeding of animals. Bees produce their wax, which belongs to the group of fats, from a saccharine diet. The fattening of geese for the production of *foie gras* is effected on a diet chiefly amylaceous, in which the fatty constituent falls far short of the gain of this substance. The production of butter by milch cows affords another example of the same transformation, as also the fattening of swine on food chiefly amylaceous. This destination of carbohydrate ingesta is maintained, in opposition to the view of former physiologists, that the absorbed sugar as such is oxydized in the circulation. It is now known that sugar, introduced into the circulation elsewhere than in the portal circle, is speedily eliminated through the kidneys, and this is held by the author as proof that it is not oxydized to any considerable extent.

With regard to the part played by alcoholic beverages in the animal economy, the following paragraph gives the summary of Dr. Pavy's view:

From a review of the evidence as it at present stands, it may reasonably be inferred that there is sufficient before us to justify the conclusion that the main portion of the alcohol ingested becomes destroyed within the system, and, if this be the case, it may be fairly assumed that the destruction is attended with oxidation and a corresponding liberation of force, unless, indeed, it should undergo metamorphosis into a principle to be temporarily retained, but nevertheless ultimately applied to force-production. The

subject appears to me to be open to physiological as well as chemical investigation, and probably some additional light may be hereafter thrown upon it by an approach through the former channel.

It has been asserted by some that alcohol is mostly eliminated unchanged through the kidneys, skin and lungs, and the chromic acid test has been adduced as proof. No quantitative estimate seems to have been made, and the truth is that this test is one of such extreme delicacy as to afford its characteristic reaction even in case of teetotallers. This fact has led to the theory already made public by Prof. W. H. Ford, that the human economy is capable, to some extent, of forming an alcoholic product.

It will be observed that a large space is occupied with the consideration of Alimentary Substances. On account of the great number noticed, most of them receive but very brief mention, and none are treated exhaustively. Proximate analyses are given of the most important articles—for instance, the percentage of nitrogenous matter, of fat, of carbohydrate (amyloid or saccharine matter), of saline matter, of water. The nutritive values of various articles are thus readily shown.

An occasional want of precision might be detected by the critical reader. For instance, speaking of tripe, he remarks—"This is the only instance of any part of the alimentary canal being applied to our own use, excepting in case of the pig, where the chitterlings are cleansed and eaten." He intends the statement to be taken only in the culinary sense, probably, but does not say so; and therefore the rennet prepared from the stomach of the calf, and pepsin from that of the calf and pig, are really ignored.

Speaking of eggs, we read: "Various eggs are eaten, including those of reptiles—as, for instance, the turtle—as well as birds; but it is especially the egg of *the fowl* [italics ours] which is employed as a general article of food," &c. By "the fowl" it may be inferred that the gallinaceous fowl is meant, though the eggs of such fowls as ducks, turkeys, guinea hens and geese, are more in request than those of reptiles and birds not domesticated.

The potato disease is stated to have made its appearance in 1845. It certainly existed the previous year in the United States.

The effects of alcohol on the system are considered rather in a physiological than therapeutical aspect. The observations of

Drs. Parkes and Anstie only are quoted, and altogether too narrow a view is taken to render it of appreciable value. He inclines to the belief that alcohol does not increase the ability or facility to perform work, but that it may be temporarily useful as a stimulant to the appetite, to digestion, and to the flagging muscular system. This is all in accordance with general experience. It may be added that it is to be gathered from numerous observations, quoted by Dr. H. C. Wood in his recent work on Therapeutics, that alcohol slightly diminishes the excretion of urea, and more considerably that of carbonic acid. This property, by diminishing waste of tissues and lowering animal heat, gives it valuable therapeutic qualities in continued fevers, while it would lessen the amount of force represented by muscular effort. Experience will show, we believe, that the continued use of alcoholic potations diminishes the actual force exerted, though their occasional sparing use as stimulants may increase both heat and force.

In the article on the Principles of Dietetics, experiments are related, which show that neither nitrogenous nor non-nitrogenous aliments alone suffice for the nutrition of the body. This is undoubtedly correct: the former are needed to supply tissue-waste, while the latter are not entirely absent even from the jerked beef diet of the South American Guachos. It remains to be considered whether it is a matter of indifference what variety of non-nitrogenous food is taken. Let the author speak for himself.

The question as to whether non-nitrogenous matter should enter into the composition of food has been sufficiently discussed already, but another question presents itself: Are both fats and carbohydrates necessary? If we look to the diets of different nations we almost invariably find that both these principles are represented. Still it is evident that fat alone will suffice for yielding the non-nitrogenous matter required for the support of life, for we find in certain parts of the globe that there are large numbers of persons who subsist, and maintain themselves in good health, exclusively on animal food, in which fat forms the only representative of non-nitrogenous matter. As to whether, however, the carbohydrates can similarly supply what is wanted forms a question that is not so summarily to be disposed of. It is true, there are some articles of vegetable food which are capable of sustaining life, and which, whilst freely containing a carbohydrate, contain a comparatively insignificant quantity of fat, but the presence of fat, as has been already mentioned, appears to have been of service in promoting the metamorphosis of the carbohydrates in the system. It also exerts a favorable influence over the assimilation of nitrogenous

matter, and the processes of tissue formation and nutrition; and it may be said that there is strong reason to believe that the association of a certain amount of fatty matter with the carbohydrates is probably necessary for the maintenance of the organism in perfect health. The belief is further entertained that its deficiency is sometimes the source of the development of the tuberculous diathesis.

As is reasonable and in accordance with the habits of mankind, the author holds that the quantities and proportions of the different elements of food should be adapted to the varying requirements of climate and avocation; and that, with sufficient nitrogenous material to keep the animal structure in repair, the most efficient source of heat and force is the fatty element of food. The proportion of animal food he would limit to about one-fourth of the whole. More than this he believes subjects the excretory organs to undue effort, with danger of contamination to the system by retention of effete nitrogenous matters. On this point we think much is to be determined by custom, and habits of life as regards especially muscular activity. The flesh eating peoples on this continent do not appear to be unusually subject to gouty and calculous disorders.

An interesting and important doctrine in regard to the use of nitrogenous and non-nitrogenous food is enunciated—that a large proportion of the former element renders the blood rich in red corpuscles, promotes the growth of bone and muscle, and stimulates vital activity; while decided preponderance of the latter promotes adipose deposit. The system of Mr. Banting, for reducing corpulence, is in accordance with this doctrine, and consists briefly in avoiding the non-nitrogenous, or fat-producing, class of aliments.

Throughout the work a general tone of conservatism is to be observed in the harmony which exists between the views expressed and approved customs of life. In the article on Diet of Infants the author is at variance with the prevailing custom in this part of the country—the time of weaning. Instead of terminating lactation at the age of ten months, the infant is very generally suckled from eight to twelve months longer. Protracted lactation seems to have no disadvantage for the child, and most mothers are willing to undergo the prolonged drain on their powers for the sake of a lengthened immunity from pregnancy.

When it is necessary to provide a substitute for maternal lac-

tation, the author recommends milk taken from a single animal, according to the popular notion. If we were certain of obtaining this from one animal constantly, and that it would always agree with the child, the plan would undoubtedly be correct; but with the varying condition of the individual cow and the uncertain dependence to be placed on the average dairyman in our cities, it seems to us that the risk would be less to take the mixed milk from the whole dairy, and have a mean of all its qualities. "*In medio tutissimus ibis*" is a very good rule to go by.

On the subject of Therapeutic Dietetics, abstention from fluids is recommended for the relief of nasal catarrh and pleurisy with effusion, on the principle of drying up the source of the catarrh in the one case and promoting absorption of the effusion in the other. For the first this is the "dry plan," recommended a number of years ago by Dr. C. J. B. Williams; but most people, we presume, would consider the remedy worse than the disease. Allusion is made to the same plan, as used by Mr. Tuffnell for the treatment of internal aneurism, in conjunction with regulating the posture. By these means it is aimed to diminish the volume of blood and promote the coagulation of fibrin within the aneurismal sac.

Similar in principle is the plan suggested of treating Bright's disease, by reducing the nitrogenous ingesta to the lowest limits compatible with the wants of the system, thereby lightening the labor of the kidneys. This he considers of more importance than the restoration of the albumen lost in the urine.

It is observed that animal food has a tendency to render the urine acid, while vegetable food produces alkalinity. This principle is turned to therapeutic account by reducing the proportion of animal food and increasing that of the vegetable element in the uric acid diathesis, and pursuing the opposite course in the phosphatic diathesis. The above observation is not new, and, according to Dr. Carpenter, the fact may be attributed to the ingestion, in vegetable food, of alkaline bases in combination with the organic acids (citric, tartaric and oxalic), which are decomposed in the primæ viæ, leaving their bases free; also to diminished production of sulphuric and phosphoric acids from disintegration of tissues, when one subsists on vegetable food. The plan of treatment is therefore quite obvious, but is brought out more clearly by the author than we have seen it elsewhere.

Directions are given for a number of dietetic preparations, and

among them an invention of the author, which represents in fluid form all the properties of lean meat. It is prepared by a process of artificial digestion, one pound being contained in about four fluid ounces of the extract. It may be used alone, or in combination with non-nitrogenous substances. It is to be regretted that he has not informed us whether it is palatable. This, of course, would not be important, when the liquid is to be used by enema, for which it is eminently adapted from its already digested condition.

So much space has now been occupied in the analysis of this work, that the consideration of its general merits must be dispatched with brevity; and it is likely that the reader by this time knows enough of its contents to judge for himself. We frankly regard the work as deserving of great praise, both in its plan and in its execution, and sure to add to the already high reputation of its author. The mechanical execution of the book is in the usual neat and accurate style of the experienced publisher. S. S. H.

A Conspectus of the Medical Sciences: comprising Manuals of Anatomy, Physiology, Chemistry, Materia Medica, Practice of Medicine, Surgery and Obstetrics. For the use of Students. By Henry Hartshorne, A.M., M.D., Professor of Hygiene, University of Pennsylvania, etc. Second Edition. Enlarged and thoroughly revised. 12mo, pp. 1024; with 477 illustrations. Philadelphia; Henry C. Lea: 1874.

This work, as its title indicates, is intended for the use of medical students while attending lectures and preparing for examination. Experience proves the impracticability of using large works for this purpose in our country, where the yearly lecture season is compressed within the period of four or five months, and the time so fully occupied with lectures, hospital attendance and dissecting as to leave scarcely a leisure hour out of those needed for sleep. In the preparation of the first edition the author acknowledged important aid from several collaborators, but the revision has been made without such assistance.

Although the scope of the work would not admit of discussion of debatable points, we think that views now adopted by the highest medical authorities should at least have some notice. For instance, on the assimilative functions of the liver and spleen nothing satisfactory is expressed, though the most emi-

nent physiologists have advanced the theory that the former has an important part in the elaboration of albuminous and carbohydrate aliments, as evidenced by the appearance of albumen and sugar in the urine, when albuminous and saccharine fluids reach the general circulation otherwise than through the intermediate tract of the portal circle. The further elaboration of albuminous matter in the spleen is not alluded to; and its office in regard to the formation and destruction of blood corpuscles is mentioned with distrust. The antiquated idea of its serving chiefly as a safety reservoir for the blood is advocated.

The old notion of the purely sedative action of digitalis on the circulation—diminishing both the frequency and force of the pulse—is adhered to, notwithstanding recent observations indicating its tonic effect on the cardiac muscular structure.

Yellow fever receives its full share of attention, and it may be presumed that the article on this subject conforms closely to what is taught in Philadelphia. It is all well enough for students who prepare for an examination and expect never to meet a case in practice; but, for our part, we would rather risk the chance of dying a natural death, than to fall into the hands of one who has studied this disease by precept only. We have seen a treatise on the Art of Swimming, which might be very interesting to a lunarian who couldn't find water enough to qualify his "whiskey straight;" but a man would surely be a lunatic to trust himself in the water with this book as a life-preserver, literally or figuratively. So much might be said were all the author's views in strict accordance with experience, which is not the case. For instance, *black vomit* is declared to be pathognomonic of yellow fever, when we know here that there is no pathognomonic sign whatever. Again, "All the result of the use of *quinine* is, that it is not likely to do good at any early stage, but only when prostration begins to appear; and then in tonic or supporting, not *cinchonizing* doses." The old plan here was to give it in sedative doses at the very outset only, and this plan has still adherents among us. In the stage of prostration it would be inadmissible by the stomach, and our reliance is on alcoholic stimulants.

After alluding to the antiseptic treatment of small pox with disapproval, on this subject he remarks: "The experiments with antiseptic and antizymotic substances, as chlorine and the sulphites, made with other affections analagous to yellow fever,

might be properly tried with it also" The *analogous affections* must be tropical malarial fevers, but we have heard of no flattering results from antiseptic remedies, and should expect none, after their signal failure in small pox and diseases strictly septic.

On the subject of syphilis the author seems to be decidedly neither "unicist" nor "dualist." All venereal sores are styled *chancres*, and the mercurial treatment is recommended indiscriminately during the early stage. Most of our readers, we fancy, would consider the remedy worse than the disease, in the majority of cases; while there is high authority for withholding it in all cases, until the appearance of secondary symptoms. Iodide of potassium is directed in 4 grain doses. Can the author be ignorant that practitioners now-a-days prescribe it in 30 grains and upwards, and often fail with smaller doses?

Other points open to criticism might be noticed, did time and space permit. Undoubtedly it is easier to find fault with a piece of work than to do the same thing better; and if the author were to answer our criticisms with a challenge to produce a better book, we should find no better retort than *Chacun à son métier*. Candidly, we consider the book well adapted to its purpose, and hope another revised edition, up to the existing standard of medical knowledge, will be brought out at an early date.

The whole mechanical execution of the book is praiseworthy.

S. S. H.

Materia Medica, for the Use of Students. By John B. Biddle, M.D., Professor of Materia Medica and Therapeutics in the Jefferson Medical College, etc. Sixth Edition, revised and enlarged, with illustrations. Philadelphia: Lindsay & Blakiston; 1874.

This book is dedicated to the medical students of the United States, and is excellent in its construction and adaptation to their wants.

Electro-Therapeutics; a Condensed Manual of Medical Electricity. By D. F. Lincoln, M.D., Physician to the Department of Diseases of the Nervous System, Boston Dispensary. Philadelphia: Henry C. Lea; 1874.

In these days when "respectable charlatans!!" are wont to resort to medical electricity as a means of displacing more deserv-

ing practitioners, it has become the duty of every physician to give at least sufficient study to the subject to protect his patients against humbuggery. If, possessing this amount of knowledge, he should find a patient suffering with symptoms for which electricity would be beneficial, he may be able to select the operator, and choose one who is not a juggling, dishonest extortioner from helpless sufferers. I can say but little of the merits of this book, except that its author is a man of acknowledged reputation in the medical profession. His preface is short and modest, and in my opinion a recommendation to the book.

"The chief aim of the present volume has been an analysis of the principles which ought to govern the use of electricity. The portion describing the practical applications which have been made, in various disorders, is not intended to be exhaustive, but it is hoped that enough has been said to satisfy the needs of the general practitioner. Reports of cases are purposely omitted, as their introduction would have extended the volume beyond the size designed for it."

CORRESPONDENCE.

New Orleans, July 22d, 1874.

Mr. Editor:—What an amount of talk is now being held upon the subject of milk! Murchison has discovered that the germs of typhoid fever may be conveyed into families with their milk supply. Indeed, his remarkable facts seem to indicate that this usually bland and innocuous fluid may become a vehicle of transportation for the poison of typhoid fever, which perfectly conserves, and perhaps intensifies, its virulence and certainty of action. Do not naturalists assert that reptile and insect poisons always consist of an active principle in combination with albuminoid matters? Whether this holds true external to the human system or not, we now believe that the albumen of the blood is the hot-bed of those poisons which appear to be capable of self multiplication in the system, and to which we apply the term zymotic.

In the Southern section of this country the belief is exceedingly common and wide spread, that attacks of the endemic maladies, particularly those due to malaria, are in some manner induced by drinking milk. When for the first time I came in contact with these opinions, I supposed them to be mere local prejudices; but

finding how deep rooted they are in the minds of many intelligent persons, both professional and non-professional, I now consider them entitled to more respect. In truth, a maxim which travellers are prone to adopt for its wisdom, holds it to be proper to accept the philosophy and mode of living of the people amongst whom they sojourn.* But conformity to the popular sentiment does not satisfy the demands of science, which call for a settlement of the question either by experimentation or a satisfactory amount of observation, with its subsequent sound rationalism. I shall beg, Mr. Editor, that you call the attention of your readers, particularly the attention of those who reside in the country and in malarious regions, for an expression of their opinions in regard to all those questions connected with the use of milk as a diet, which this letter presents.

But if it should come to be the accepted doctrine that the use of milk does in some manner increase the liability to malarial attacks, how shall we explain its action? Shall we say that its reception into the stomach and system increases the activity of disease germs, whose presence is not due to the ingestion of milk, but occurs as a mere coincidence? Or shall we hold that the milk is simply a vehicle for the introduction of (Salisbury or Bartlett's) malarial germs into the system, and that it is mischievous only when it is impregnated with those germs?

There is an opinion in whose support a good deal may be said, which teaches that zymotic poisons run their most ungovernable riot in highly albuminous conditions of the blood. The English or German immigrants who arrive here in "high health" during a yellow fever epidemic furnish us attacks the most typical as it respects suddenness and virulence. We are unable to say that some degree of analogy does not hold true in respect to the exposure of similarly conditioned subjects to the malarial poison.

Again, if the second proposition be found true, there is another dilemma to solve, in showing whether the milk is impregnated after it is drawn from the udders of the cow, or whether the malarial poison be received into the animal's system, and be eliminated through the lacteal ducts in sufficient quantities to give rise to its specific effects upon those who drink the secretion?

* Upon arriving in a malarial country, adopt without delay the mode of living peculiar to the inhabitants. Drink grain whiskey upon the banks of the Vistula; prune brandy in the Bannat; and in Hungary eat no melons or cucumbers without seasoning them strongly with pepper; in Italy drink a great deal of black coffee and lemonade, and omit supper. *Niemeyer.*

In regard to the former of these suggestions, one may safely assert that impregnation of milk with swamp poison is fully as likely to occur after it has been drawn, as its impregnation with typhoid poison under similar circumstances. The malarial poison is known to be air-borne, possesses ponderability, is likely to become entangled with æriform moisture, is held in solution or suspension by water, and as a mobile agent is most rife during the night. The possibility that vessels containing milk, which are exposed to the night air may become more or less saturated with malarial, or yellow fever poison, must in our present state of knowledge be accepted as a most plausible hypothesis.

The proposition that milk may be infected as a secretion directly from the blood, is at variance with those remarkable laws of conservatism, upon the integrity of which the perpetuation of species may be said in great part to depend. As physicians we all know that the attempt to formulate medication of the child through the mother's milk are most positive failures. It is also true that we everywhere witness infants drawing nourishment from the breasts of mothers who are dying, or dead, of the various violent epidemic affections, without the least evidence of baneful effects.

Still it must be confessed that some remarkable exceptions to this law of conservatism are patent to our profession. The disease called "Trembles," or "Milk Diséase," is at the head of this list. In this affection the flesh of the affected cows, as well as the milk, butter, or cheese obtained from them, is capable of transmitting the disorder. While hitherto the actual cause of this malady has eluded all research, facts indicate that the poison is derived from an organism belonging to the vegetable world. It is well known that many members of the vegetable kingdom are capable of imparting peculiar flavors to cow's milk. Wild onions, green rye, clover, and certain members of the misnamed ambrosia family, are notable examples of such plants.

Within the last few days a woman has informed me that she observed a child refuse its bottle, and upon examining its contents, was able at once to determine that the milk was rendered bitter and unwholesome by feeding the cow upon cotton seed. It is to be hoped that your readers will supply your Journal with reliable information, as it respects their observations of instances of deleterious qualities being imparted to milk by the food of cows.

Donkin, in his very interesting, although too cursory discussion of the therapeutic properties of milk, states that "Baccius, physician to Pope Sextus V., and Professor of Botany in Rome, in the latter part of the fifteenth century, informs us in his writings that in Italy, especially at Naples, meadows were specially cultivated and applied to medical purposes; certain plants were cultivated in them according to the direction of physicians, and these served as pasturage for animals kept to yield milk for invalids. Each sick person was allowed a cow, a goat, or an ass, according to the nature of the case. This he led to graze on the particular spot on which grew the plants best suited for his malady." If any degree of precision attended the therapeutical application of medical substances at that day, after this mode, the profession must now class it among the lost arts.

But your correspondent should admit that the subject of this letter was suggested by reading a paper in the July issue of the Nashville *Journal of Medicine and Surgery*, in which a "new departure" is attempted to be enforced in regard to use of milk in febrile affections. In this article milk is treated of as a diet, not as a therapeutic agent. While it is certain that the great office which milk performs is that of nutrition, and while it "may justly be regarded as a natural device for the transference of the blood of the adult mother into the vascular system of the young mammalian—*a kind of natural transfusion*"*—it is none the less expedient that the physician should investigate its effects as a dietetic agent upon various diseases, or morbid states of the system. From the earliest times of which any account can be obtained, it seems that various opinions have held sway in regard to the effects of milk upon disease. The classic Burton, who reflected the learning which had accumulated to his day, says: "Milk, and all that comes of milk, as butter and cheese, curds, &c., increase melancholy (whey only excepted, which is most wholesome): some except asses milk. The rest, to such as are sound, is nutritive and good, especially for young children, but because soon turned to corruption, not good for those that have unclean stomachs, are subject to headache, or have green wounds, stone," &c.†

Donkin quotes Galen as recommending the use of milk in consumption, and on the same page makes the following extract from an English book published 300 years ago: "Yet common

* Donkin, American reprint of *London Lancet*, January, 1870.

† Anatomy of Melancholy.

experience prooveth that woman's mylke sucked from the breast is without comparison the best of all in consumption, whereof a notable example was showed of late yeares in the olde Earle of Cumberland, who being brought to utter weaknesse by a consuming fever, by means of a woman's sucke, together with the good counsayle of learned physicians, so recovered his strength that, before being destitute of heires male of his own body, he gatte that most worthy gentleman that now is inheritor both of his father's virtues and honor."

The "new departure" of the writer previously alluded to, consists in the fact that he first adopts Dr. H. F. Campbell's classification of fevers, and then instructs us that milk is baneful to subjects of the former class, and beneficial to subjects of the latter class. Let us, however, first adduce Dr. Campbell's classification, as quoted by Dr. Washington, and then permit Dr. W. to state his experience in his own language.

We would, therefore, announce, as our classification of febrile diseases, two grand divisions of fevers, corresponding with the two grand divisions of the nervous system, thus:

1. Cerebro-spinal fevers—all *paroxysmal*. The secretions and nutrition only *secondarily* affected.

2. Ganglionic fevers—all continued. The secretions and nutrition *primarily* affected

1. Under the head of *Cerebro-Spinal Fevers* we would place the whole family of paroxysmal fevers, whatever type they may assume, and also the various forms of neuralgia which are nearly always intermittent, as well as the *sthenic* forms of traumatic fever, together with the fever accompanying simple pharyngitis, pneumonitis, dysentery, and many other diseases of malarial districts.

2. Under the head of *Ganglionic Fevers*, or *Fevers of the Secretory System of Nerves*, we think we find ample ground for bringing together many diseases heretofore widely estranged from each other. Thus as the archetypal forms of ganglionic fevers, we place, at the head of the list, typhus and typhoid fevers; then, allied to these in various degrees of affinity, but all, *equally*, in the one essential element, that they present themselves as manifestations of disease through the ganglionic system, are variola, scarlatina, rubeola, varicella, and many other forms of eruptive fevers heretofore not classified by nosologists.

All of these last diseases are marked by fever of a *continued*, or non-paroxysmal, character; all present marked aberrations of nutrition and secretion, and each has its own peculiar eruptive character; and furthermore, each one is definitely self-limited in its duration, no remedial interference having, as yet, been found competent to arrest or shorten their progress.

Dr. Washington then defines his experiences as follows:

Now my experience has been that, in the ganglionic fevers, the use of milk is highly beneficial, and in the cerebro-spinal fevers the use of milk is decidedly injurious. As a sample of the latter, I will mention a case:

Some twenty-five or twenty-six years ago, a young man requested me to try my skill in stopping his chills, stating that Dr. Boas Roberts had been trying for six months without success—come back they would. "Well," replied I, "you can be cured if you will promise not to touch milk; and if you do not so promise, I will not prescribe for you; you will have to employ some one else." The promise was given, and the chills were stopped forthwith, and remained stopped for sixty days, a period of freedom from them so much longer than any before passed, that he considered himself well. At the end of that time he took a glass of very cold milk at his supper, and had a chill by nine o'clock next morning.

The patient sent for me, and said, "Doctor, I broke my promise last night; but if you will agree to cure me once more, I will pledge you my honor to let milk alone hereafter." The chills were stopped again, and remained cured four years that he remained under my observation; for it mattered not where he was visiting, no persuasion could induce him to touch a drop of milk, so well satisfied was he of the correctness of my notion.

On the other hand, in the ganglionic fevers, milk and whisky are all important, and many cases would undoubtedly die unless they were freely used.

It is certainly well calculated to lead a young practitioner into trouble, to recommend milk indiscriminately for all cases, and I would therefore give it, as the result of my experience, that the use of milk in the cerebro-spinal fevers is injurious, and should not be allowed until convalescence is perfectly established; and, on the other hand, in the ganglionic fevers it is highly beneficial, and should be recommended without hesitation.

Now all this discussion about the classification of fevers into "Cerebro-Spinal" and "Ganglionic" is very "neat," looking at it as a purely scientific arrangement; but my experiences are so different from those of Dr. Washington, that I devoutly believe young practitioners will be led into trouble much more frequently by attempting to apply it at the bedside, in the arbitrary manner inculcated, than by ignoring it *in toto*.

Let us rather look to the condition of the individual patient, as demanding or countermanding the use of animal food.

If the patient has burning fever, foul tongue, headache, throbbing carotids, dry skin and scanty urine, confine him to water, weak tea, or other diluted drinks, fruit and farinaceous foods. When these sthenic symptoms begin to be replaced by others,

which denote that the time has arrived when the wise physician should turn his attention to the repair of waste, and the restoration of diminished forces, the most natural and proper suggestion which occurs to his mind is the administration of nutritious food and stimulants. No animal food containing as much nutriment is so universal in its application, in filling the bill of expectations on the part of both physician and patient, as pure milk. It has sometimes occurred to my mind that Sydenham's favorite sick man's dish, of roasted apple and sweet cream, was a sort of compromise between a nutritious and a cooling regimen. At all events, I have often prescribed it for patients when on the boundary line between a sthenic and an asthenic state.

I have prescribed milk over and over again, in every form of fever common to the western or southern country. I can safely affirm that no bad effects have ever been patent to my observation. Of course, as we learn more in regard to those circumstances which impart to milk, qualities which are poisonous, we will know better how to avoid such results. When we shall have arrived at such perfection of knowledge, we shall cease to bring disrepute upon an important and useful article of the dietary of the sick, because a poison entirely foreign to its natural composition has been imparted through the blood of the lactifer, or accidentally become entangled with it, after exposure to the air, and thus finds admission to the human system.

As your readers will perceive, I have not ventured in this letter to touch upon the chemical composition of milk, either in its healthy or unhealthy conditions. The observation has often been made that milk may prove deleterious when nothing in its physical appearances, and nothing elicited by chemical analysis, can be apprehended to distinguish it from a perfectly healthy secretion. We should remember that if the blood and flesh of animals used for food, are capable of undergoing such a degradation of normal constituency as to injure the health of the consumer; an equal liability to deterioration attends secretions from such animals. A period of malnutrition, whether induced by feeding cattle on "swill," or other incompetent nutriment, or by confining them in badly ventilated stables, or interrupting their assimilation of food by fright, as on railroad cars, by driving, or cruel treatment, may prove quite sufficient to render their flesh and milk dangerous to human life. It should not be assumed in these cases that any special or tangible poison is added to the flesh and

milk, but they may have been rendered unwholesome by interruption of processes of change in the system of the ill-conditioned animals which renders them unfit for digestion. I have always had strong doubts in regard to the reliability of those stories found on many pages of medical annals, which tell of sudden conversion of milk into a poisonous fluid by moral influences affecting the mother. Once I saw an enraged woman nursing her babe, and within a few hours I was called to prescribe for it in a violent fit of indigestion, but this may have been a mere coincidence. Still, upon whatever facts their ruling philosophy may be based, dairymen tell us that a better yield of milk, and better quality of milk, are obtained from cows which are treated with gentleness and humanity than from those which are cruelly used. It is likewise true that growers of blooded horses are especially careful that nursing dams shall not become overheated or fretted.

Let us dismiss this lacteous subject. Some members of our profession at this time, stand under the liability which a late brilliant, but drunken wit charged upon his sweetheart, when he discovered her gazing at the stars in company with a handsome young man. He told her that such practices, too often indulged, might eventuate in her own translation to the "milky way." In soberness, however, it is well that none of our profession will suffer their minds to fall into the hazy, nebulous state, attributed to that great astronomical phenomenon.

New Orleans, August, 3d, 1874.

For over two months past, a type of fever has prevailed in this city which has elicited some discussion as to its true character. While its prevalence has not been attended by any considerable increase of fatality, its clinical course has been tedious, and its therapeutics not always clearly indicated. Another fact, which has perplexed attempts at a solution of its real nature, is, that the subjects of its attacks were for the most part young persons, and in my experience, young males largely preponderated.

The first cases seen in my practice were attacked during the first week in June. The mortality reports of the city indicate an earlier date to its origin. The number of deaths attributed to malarial and typhoid fevers during the week ending May 31st

was ten, being 5.29 per cent. of the total mortality for that week. This ratio was quite regularly increased, until, for the week ending June 29th, it amounted to 13.63 of total mortality. During the month of May the total rain-fall was twelve hundredths of an inch. In the month of June 8.97 inches of water fell, but by referring to the meteorological table published in the *Journal*, the reader will perceive that rain-fall occurred upon only ten days of the month. After a study of these meteorological statistics, especially as it regards alternations of dry and wet weather, and then, after recalling the fact that the range of temperature during the past winter exhibited an unusually high record, your readers are quite well instructed in respect to conditions favorable, or unfavorable, for the development of malaria.

Let us now look to the symptomatic phenomena of this fever for the solution of questions respecting its character, for we must still confess, that however much telluric conditions appear to favor the evolution of malaria, they alone afford us no warrant of the actual presence of malaria as a morbific factor in those diseases which prevail, even amid such conditions. We can find positive proof of its presence no otherwise, than in symptoms so characteristic as not to admit of dispute.

E. C. A., aged 23 or 24, a gentleman of excellent constitution, and strictly regular habits, had never lived in a malarious country until he arrived in this city, some time during the past winter. While here his residence and business places were in the central parts of ths city, where malarial manifestations do not ordinarily assume a violent type. He, however, spent some weeks of April and May in making a business tour of Texas. I saw Mr. A. for the first time on the 10th day of June. He at this time complained of alternate flushings of heat and chilly sensations, of headache, pain in his back, epigastric fulness and uneasiness, and of an exceeding bitter taste and complete anorexia. His tongue was large, white, slimy; pulse accelerated, temperature not thermometrically noted, but above normal. The bowels were inactive, and urine high colored and scanty. The patient had been suffering under these symptoms for several days, and had been persuaded by his friends to take homœopathic globules. In addition to these he had taken some saline purgatives. I ordered blue mass; comp. extr. colocy, each five grains, to be taken immediately. Three hours afterwards a

seidlitz powder was taken, and as soon as catharsis resulted, the following prescription was given.

℞. Sulph. quinine, - ℈j,
Pul. opium, - - gr. ½,
Extr. hyoscyam, grs. vj,
M S.—Pil. no. vi. s., two every second hour until all are taken.

June 11th.—Patient has but little pain, but complains of insipid or bitterish taste, and acidity and scantiness of urine; slightly feverish; thirst not considerable; appetite lost. Ordered—

℞. Neutral mixture - - ℥iiiss,
Sprts. nitr. dulce,
Syrup Morphia, - - aa ʒij,
Tinct. aconite (Flemings), gtt xij.
M. S.—Tablespoonful every second hour.

June 12th.—Found my patient seated at the breakfast table, but he admitted that he was entirely without appetite. Ordered as a tonic—

Nitro-hydrochlor. acid dilute, ʒiss,
Infusion wild cherry bark, ℥vj.
Tablespoonful thrice daily.

Was summoned at midnight, and found that the patient had imprudently gone out to make a business call upon some person living on the same square with himself. At the time of my visit he complained of the most violent cephalalgia, especially in the occipital region; pulse quick and tense; pupils natural; no nausea; tongue coated, white; no stool since morning of 11th. Ordered calomel, comp. extr. colocynth, aa grs. v; make two pills. Take immediately. Frictions with Raspail's sedative were made over seat of pain. Catharsis was followed by a scruple of quinine in three equal doses, at intervals of two hours. With each dose of the quinine he took a tablespoonful of the following mixture: Neutral mixture, ℥iiiss, spts. nitr. dulce, syrup morphia aa ʒij, tinct. veratrum viride gtt xxiv. This latter prescription was continued after the quinine had been exhibited.

On the 14th the patient was found in an adynamic condition, which characterized the case through its subsequent course. Even the little muscular effort required for the use of the night-vessel, or in taking food, produced exhaustion. The nervous symptoms were muttering during sleep, slight delirium, vigilance, and such excessive trembling of the hands that his efforts to affix

his name to a business paper were altogether unsuccessful. Febrile exacerbations occurred daily, generally in the afternoon or early part of the night. For the most part they were slight, and followed by free perspirations. To combat this periodical complexion of the case, from six to nine grains of quinine were administered during the morning of every day, a three-grain pill being given every third hour during apyrexia. For a portion of the time five drops of nux vomica were given with each pill, but not being able to note any benefit from this addition to the quinine, its use was abandoned. Full doses of Dovers' powder, or of Battley's sedative, were given at bed time, and repeated in three hours time, if the first dose failed to procure sleep.

The most careful attention was given to the patient's nutrition by concentrated meat essences, and well cooked farinaceous foods. Brandy juleps, sherry, and fine claret, were freely urged upon the patient.

The symptoms relating to the digestive apparatus are important to note. The tongue was large, white, moist, and indeed slimy, with the increased secretions covering its dorsal surface. The altered taste was greatly complained of, and the terms "bitterish, insipid, and execrable," were employed to denote the discomfort it produced. I think these symptoms were present during the early progress of every case I have seen. The patient complained of epigastric weight and uneasiness, but did not vomit. His surface was slightly jaundiced. The bowels were inactive, but catharsis followed ordinary doses of laxative medicines. One symptom which caused this patient a great deal of pain was present in every case I observed. This was distention of the bowels from gaseous accumulations. These accumulations did not seem to be limited wholly, or chiefly, to the large bowels, as in true typhoid fever, but the whole intestinal tube appeared at times to be distended. The pain, as of a persistent colic, and so much pressure upon the diaphragm as to impede the respiratory process, resulted from this cause. For its relief, hot stupes were applied over the abdomen, and Battley's sedative given in infusion of ginger, or burned brandy. The abdomen was somewhat tender to pressure, but not more so in the ileo-cæcal region than elsewhere; neither was gurgling a marked symptom in this or any other case under my observation. Frequent sponging of the skin, with cool or tepid water, brought so much comfort and quietude to the patients, that I regarded it as an important

measure of treatment. I consider this fever to be a pathologic compound of malaria and catarrh. It is altogether inutile to discuss questions regarding a name for it, which should clearly set forth the one of those factors whose ruling influence occasioned the epidemic. It is certainly true, however, that catarrhal affections were unusually prevalent during the period when this fever was most rife. The surfaces affected were those which are usually most liable to such inflammations, as those of the larynx, the bronchi, and the rectum and colon. Inflammations of some one or more of these surfaces attended some of the cases of fever, but a still greater number of such inflammations occurred which ran an ordinary uncomplicated career. The statement, previously made, that the subjects were nearly all persons in that period of life most obnoxious to catarrh, must be allowed its weight in determining a nomenclature based upon the ruling pathological element. But whether the fever be decided to be "catarrho-malarial," or "malario-catarrhal," the practitioner is equally well prepared to treat it, provided he keeps in view its compound nature.

I am well aware that some excellent confrères will take issue with me, in regard to these opinions, and ask, first of all, how do you account for the adynamic symptoms so constantly present in cases of this fever? I shall certainly not undertake to answer a question whose proper solution is so often an impossible problem in connection with various epidemics. One thing, however, I can positively affirm, and that is, that in no solitary case which I saw did the range of temperature, the presence of an eruption, or ileo-cæcal tenderness, denote the existence of true typhoid fever. The patient whose case is sketched in this letter, was dismissed from treatment on the 26th of June, and I think the duration of his case would fall somewhat short of the average.

TATTLER.

BREMOND, TEXAS, June 1st, 1874.

Mr. Editor:—When practising in California, in 1857, it was my fortune to meet with a good many surgical cases; and being remote from cities where surgical appliances could be obtained, I was obliged to extemporize such apparatus as the materials and mechanical skill at hand would afford. Amongst other appliances I was obliged, in one case of severe and extensive com-

pound comminuted fracture of the leg, where it was extremely painful to the patient to subject him to any change from a supine position, and where manual assistance could not be had when it was necessary, to devise an arrangement of the bedstead which enabled one assistant to attend him when it was required to evacuate his bowels, and without the necessity of changing the patient's position. Upon relating the circumstance, and describing the arrangement to the Bremond Medical Association, at a late meeting, they expressed the opinion that it should be published for the benefit of the fraternity at large. If you, Mr. Editor, are of like opinion, it is at your service.

Have a narrow bedstead sufficiently high for the nurse to get under it easily; the slat immediately under the buttocks of the patient should be 16 or 18 inches wide; through the middle and centre of this a hole should be cut 10 or 12 inches in diameter; the circular piece cut out is to be attached on the under side and next the head, by a stout hinge, and to be secured by a bolt on the opposite side, so that it will open downwards and hang towards the head; the mattress to have a hole cut through it somewhat smaller than that in the bedstead; the circular piece from this to be re-covered and formed into a plug that can be removed and replaced when desired; through the hole in the bedstead a loop to be sewed on the under side of plug for convenience of removal. When this plug is in place, and the trap-door in the bedstead closed and bolted, there is no perceivable defect in the mattress. A stool, of such hight as will raise a vessel placed upon it to this hole, completes the apparatus.

Jas. S. Snelling, M.D.

Richmond, Va., August 5th, 1874.

The Virginia State Board of Health was organized in 1872, in accordance with a Legislative act of the previous session; but no pecuniary appropriation was made for the support of the Board, and the result, of course, has been that the Board has attempted nothing involving expense. It was only after some persuasion that the members of the Board could be induced to continue their relations thereto under the circumstances; but it was thought that the new Legislature of 1873-4 would remedy the fault. When the bill was introduced at this last session it

was passed by a very handsome majority; but when it came to a count of the votes cast, it was found that not two-thirds of the members were present at the time, which caused the defeat of the bill. A ruling of that session of the Legislature required that when an appropriation bill was to be made, the vote should stand two-thirds of the entire number of delegates in favor of it. But the plurality vote was regarded so favorably as to encourage the Board at a recent meeting to continue the organization until after the next Legislature.

The death of Dr. Francis T. Stribling, Superintendent of Western Lunatic Asylum, at Staunton, Va., July 23d, 1874, is a great calamity. When he took charge of the asylum, in 1840, he at once instituted reforms in the management of the insane, which are now generally adopted in Europe and this country, and which at once gave him a reputation which is as extensive as is the special field in which he devoted so much of his time and labor. No man in the State was more highly respected—no one in the profession has a more enviable name. It will be hard to fill his place. Who his successor will be it is impossible to say, though I hear there are already a great number of candidates.

By the by, this thing of medical office-seeking is becoming quite common. As an instance, it is said that, when the Physician to the State Penitentiary was to be appointed (with an annual salary of $900), there were not less than 100 applicants. The proportion to the entire number of active practitioners was as 1 to 18, for it is estimated that there are about 1800 practising physicians of all schools in Virginia. Several other illustrations might be mentioned.

The cause of this general office-seeking among doctors is, that they are wofully forgotten when pay-day comes around. A country physician who makes an average of 20 visits a day is considered *very*, VERY fortunate, if he collects as much as $900 during the year, even if a large part of the $900 be "in kind," such as hay, oats, hogs, sheep, wood, day-labor, &c., for which the highest market rates are allowed. The city physician does a good business who collects 25 or 30 *per cent.* of his bills. I am not prepared to say that all this is due to poverty of the people; for certainly much of it seems attributable alone to their education regarding paying doctors, as is shown by the fact that all

other honest bills are paid by many of the parties who neglect their doctor's fees.

This has been a remarkably healthy summer thoughout Virginia, and the temperature, too, has been so delightful, even in our cities, as to make it very comfortable to stay at home instead of going to the Springs. For the past night or two, while the windows are kept up, sleeping has been much more pleasant if a light blanket is thrown over the body. This fact is remarkable for this season in Richmond.

Professionally, nothing of interest is transpiring. Cremation was the last subject discussed in the Richmond Academy of Medicine, and the next for discussion is the "Effect of Street-sprinkling on the Health of Communities," which may be a new proposition to some. There are several physicians who maintain that this almost universally adopted habit in our cities is exceedingly injurious, and I understand that they propose to furnish confirmatory statistics to-morrow night. What is the experience of New Orleans physicians on this subject?

The fifth annual session of the Medical Society of Virginia will convene October 13th, 1874, in Abingdon. This organization is gradually becoming *the* influential body in this State in a medical point of view. The attendance of any of the Southern regularly recognized physicians upon the meetings will be most pleasant, and true Virginia welcome would be given. Several papers of interest are said to be in process of preparation. Dr. Alfred G. Tebault, Princess Anne county, is the president, and he is a most active and efficient officer.

LANDON B. EDWARDS, M.D.

BOSTON, JULY 12th, 1874.

My dear Sir:—You need not fear a very long letter with the thermometer playing such tricks with us that we think geographers have made a mistake about our latitude. You are pleased to pay me the compliment counting me as somewhat exceptional in uniting the functions of verse-wright and medical practitioner.

I suspect there are more rhymesters in the medical profession than you suppose. I know a good many who have let me into their secret propensity in a quiet way. I will not say that every one of them has a right to call himself a poet, but some that can write very well do not care to make themselves prominent in

literature. I think they are quite right. I do not believe much in rhyming doctors, and since I have fallen into that way myself, I have had no professional homicides laid to my charge, and that for the best of reasons—I do not profess to practice medicine. In my opinion a man who has life and death, and the bodily and mental welfare of his fellow creatures in his hands, should have no ambition outside of his proper calling. To do his duty by his patients will tax all his powers. There have been a good many physicians who have also been poets and *littèrateurs*, but very few instances that I can at once recall where they have really excelled in both ways. Haller was the most accomplished man I can think of in medicine, and he made himself a name as a poet before he became celebrated in medical science; but after all it is as a scholar, a bibliographer, and a writer on Physiology, that he is known, and not by any reputation as a working practitioner.

Arbuthnot is the most successful instance, perhaps, of the combination of *littèrateur* and physician—a man whom Swift and Johnson agreed in ranking above all his famous contemporaries, and whom Queen Anne thought worthy of attending her in her last illness. Perhaps we might mention Guy Patin as his French parallel. Erasmus Darwin and John Mason Good were very scholarly physicians, but I have no doubt that there were plain men in their neighborhood who thought of their patients and nothing else, while these great people were selecting their fine phrases and learned epithets, and whom one would have rather had to care for him than the author of the Botanic Garden or the translator of Lucretius. Akenside wrote good verse, but was altogether too dainty a personage for a medical practitioner. Armstrong was not famous for his success as a medical man, and his poem—"The Art of Preserving Health"—is a less agreeable sudorific than ten grains of Dover's powder. Goldsmith and Smollett were great writers but medical failures.

In this country Dr. Samuel S. Mitchill, who meddled with almost everything, wrote verse among other things, but he, I think, was not named as a practitioner with the Bards and Hosack. James G. Percival had a good deal of reputation as a poet, not much as a physician, though he had taken a medical degree. I do not think either of the Warrens was ever proved guilty of writing a line of verse. I cannot accept the epitaph on

Governor William Bradford as a proof that there was a rhyming medical man in the colony at that early date. If there had been, I think the Pilgrim Fathers would have sent him back again. For a patient to have his epitaph written in doggerel verse by the doctor who has just worked him off would be too much. Let me think; there *was* one rhyming doctor, but his rhymes got him into trouble. March 12th, 1651, Dr. William Snelling was fined ten shillings and the fees of court for uttering the following metrical toast:

I'll pledge my friends;
And for my foes,
A plague for their heels
And a p** for their toes.

but he only repeated these lines, and did not make them.

You must have seen some pleasant verses written by the late Dr. John K. Mitchell. Some of them are to be found in Duyckinck's Cyclopœdia of American Literature; and have you never read the ingenious microcosm of Dr. Abraham Coles, of New Jersey, or his numerous versions of the *Dies Iræ?* Of course you do not forget Dr. Holland.

I am afraid I should do violence to the modesty of some of my friends here and elsewhere, if I should mention the poems I have had the privilege of reading, composed in the intervals of their busy professional labors. These have very generally been sentimental rather than professional. The earlier medical poets confined themselves more closely to their specialty. Fracastorius chose one of the least inviting of maladies, and I have before me a formidable Latin poem entitled *Caroli Sponii Myologia Heroico carmine expressa.*

I write hastily, picking up the few facts nearest at hand. But I have my doubts whether much can be found by the most diligent search to show that the old connection between verse, music and the art of healing, has anything stronger than a mythological foundation to rest upon.

As for "Christopher Caustic, M.D.," the author of "Terrible Tractoration," to whom you refer, he was Thomas G. Fessenden, and no M.D. at all. The poem was a satire on the medical profession, and a defence of the tractors.

My letter, brief as it is, is longer than I meant it to be, and if it reaches you about bed time will be as good as a night-cap.

Yours very truly, O. W. Holmes.

NEW ORLEANS, August 13th, 1874.

Mr. Editor:—I thank you for the opportunity to read Prof. Holmes' agreeable letter in manuscript. I find that my authority for placing the elder Warren among medical poets is Rufus W. Griswold, in his addendum to D'Israeli's "Curiosities of Literature,"—D. Appleton & Co., 1844. On the 29th page of his "Curiosities of American Literature," he says, "Gen Warren was a song writer as well as an orator, but his verses, though very popular at the commencement of the Revolution, have less merit than his reputation as a man of cultivated taste would lead us to anticipate." I cheerfully accept the corrections made by my respected critic.

"TATTLER."

EDITORIAL.

In a previous issue we have called the attention of our readers to the efforts which are being made to collect all information, possible to be obtained, in regard to the cholera epidemic of 1873. These efforts are under the auspices of the national Government, and two of the most competent and efficient medical officers in the United States service have been appointed to perform the labor.

There is but one contingency which can hinder a most thorough and perfectly creditable completion of their work. This is a lack of coöperation on the part of those of our profession who had opportunities to observe the disease during the year 1873. The report of the commission will no doubt be printed at Government expense, and the information it may contain will be placed within reach of the medical profession, so that we, together with mankind at large, are the actual beneficiaries of the undertaking.

We trust that no charge of insensibility to such purposes will ever be sustained against our profession. At the request of Surgeon Woodworth, we publish the following circular setting forth the items of information particularly sought for.

OFFICE OF THE SUPERVISING SURGEON,
U. S. MARINE HOSPITAL-SERVICE,

Treasury Department, August, 1874.

DOCTOR:

The Supervising Surgeon of the United States Marine-Hospital

Service having been designated by Joint Resolution of the XLIIId Congress, approved March 25, 1874, in connection with a medical officer of the Army, "to confer with the health authorities and resident physicians of such towns [as were visited by the Cholera Epidemic of 1873,] and to collect, so far as possible, all facts of importance with regard to such epidemic"—for the purpose of making a report of the same to the President of the United States to be submitted to Congress,—I have the honor respectfully to solicit a detail of the facts which came under your observation concerning the propagation and spread of the disease during that year.

The following memorandum embraces, substantially, the points upon which information is desired:

1. Name, sex, and age of patient.
2. Residence of patient—town, street, and number.
3. Day and hour of attack.
4. Premonitory symptoms, their nature and duration.
5. Progress of the disease:
   a. Day and hour of beginning of rice-water discharges.
   b. Day and hour of beginning of cramps.
   c. Day and hour of beginning of collapse.
   d. Period and extent of suspension of renal function.
   e. Nature of Treatment and result.
   f. Day and hour when convalescence began.
   g. Day and hour when death occurred.
   h. Post mortem appearances in detail.
6. Story of house occupied and height of floor from ground.
7. Sanitary condition of house and enclosure:
   i. As to cleanliness of room—clean, neglected, filthy.
   k. As to ventilation and light—good, defective, bad.
   l. As to drainage of house—good, obstructed, absent.
   m. As to drainage of ground—good, obstructed, absent.
   n. As to location and condition of privies or water-closets, connection with street-sewer, mode of flushing, of ventilation of soil-pipe, disinfection, etc.
   o. As to surface-water, garbage, or filth about the premises.
8. Source and quality of water-supply—if from a well or cistern; proximity of privy, sewer, or drain thereto, and chance of pollution.
9. General topography of localities in a given town where cholera prevailed.
10. Character of soil.
11. Character of drainage.
12. Occupation and habits of patient, and whether a resident of house where attacked for two weeks or over.
13. The facts in any case where the patient was attacked within two weeks after removing from an infected district into one previously free from the disease, specifying the respective districts and character of exposure.

14. The sequence of cases where more than one was attended, with their relations to each other, and to the cases of other physicians, with names of such physicians.
15. The means and agents used by the physician, by the family, and by the municipal authorities, to prevent the spread of the disease, and the result of such preventive measures.
16. Public measures taken to prevent the introduction of the disease into your community, with the result.
17. Temperature, rain-fall, and prevailing winds for as long a period as practicable prior to appearance of cholera, and also during its continuance.
18. Dates of first and last cases of cholera in the locality in 1873—total number of cases and mortality.
19. Connection, if any, between first cases in 1873 and the localities of the disease in the immediately preceding epidemic.
20. Names of cities, towns, and villages known to you where cholera occurred during 1873, with any facts relating to the introduction of the disease to such, and the address of some respectable practitioner residing in each of the places named.

Contributions to this investigation, by answers to the foregoing—or to so much thereof as is practicable—will be fully acknowledged in the official report, the value of which, it is hardly necessary to say, will largely depend upon the coöperation of the profession thus sought.

Copies of any reports or papers which you may have already prepared on the subject, or of those prepared by others and annotated or emended by yourself, will also be of service, and may be forwarded, to be returned if desired.

I am, Doctor, very respectfully,

JNO. W. WOODWORTH,
Supervising Surgeon.

The first volume of Prof. Joseph Jones's "Medical and Surgical Memoirs" will be issued about the 1st of January, 1875. The work will be distributed in great part to subscribers, for whom the price will be fixed at five dollars per volume. As the volumes will be carefully printed, and as they are illustrated by a large number of microscopical observations, this price is "just sufficient to cover the actual cost." Subscribers should address the author, box 1500 P. O., New Orleans.

A New Quarterly.

We have before us a prospectus, announcing a new quarterly journal, to be called the "Archives of Dermatology." The editor

is L. Duncan Bulkely, A.M., M.D, assisted by a corps of able collaborators. The first number will be issued on the first day of October, by G P. Putnam, Publisher, New York. The subscription is three dollars a year, single numbers one dollar.

Correction.

In the July issue of this JOURNAL, under the caption of "Proceedings of the New Orleans Medical and Surgical Association," is published a case of punctured wound of the kidney. Through some inadvertence the authorship of this paper was ascribed to Dr. A. W. Perry, instead of Dr. J. J. Castellanos, to whom it was properly due. Upon discovery of the error, letters were addressed to our leading exchanges asking to correct it. We observe that one, at least, of these exchanges, has copied the paper and given proper credit to the author.

The Heated Term.

Upon reference to the meteorological reports upon subsequent pages, the reader will observe that this city has passed through a heated term almost or quite unparalelled in its previous history. The mean range of the thermometer for the week ending August 16th was 89°. Its highest record, as reported by the Board of Health, 99°. This long period of excessive heat produced a large increase in mortality, which amounted during the week to 296 of all diseases.

It is a fact which may seem somewhat strange to those who live in the large cities of the West, and also those living in some of our seaboard towns, as, for example, New York, that cases of sun stroke so seldom occur in New Orleans, that its medical practitioners are found to differ widely in regard to the classification of the causes of the sudden deaths, which were unquestionably due to the effects of inordinate elevation of temperature. For the purpose of arriving at proper results in our computation of the number of deaths actually due to this cause, we have requested the careful and efficient clerk at the office of the Board of Health to arrange the deaths imputed to those various causes whose morbid phenomena would bear a close resemblance to those produced by heat, in columns so placed as to admit of

ready comparison, and in this manner we will be enabled to arrive at a very close approximation to the mortality occasioned by the excessive temperature. Taking, for instance, apoplexy, congestion of the brain, and congestive fever. The excess of deaths from these causes may be fairly put down as due to exhausted innervation, or some other pernicious mode of action of heat. Our estimate is, that the deaths properly ascribable to heat amounted to more than one hundred during the week ending August 16th. It was our purpose to have written something upon the subject of the pathology of these sudden deaths, but the questions involved require a more lengthy discussion than our space will permit, and they are therefore deferred until the November issue.

The Reason Why.

Editors of scientific journals are often placed in positions not readily understood by the non-professional person who employs their papers as advertising mediums. It is so entirely customary for the editors of our daily and weekly periodicals to offer "complimentary notices" to all new advertisements, that a large number of advertisers expect similar favors from technical journals.

The editor of a scientific journal which, like that for which we now write, is owned by another party, has several important duties to keep in mind. We recognize as the chief one of these duties, and one which can have no "let up," a faithful service to the cause of science, and to all ends and purposes which tend to general professional good. Next after this, come the obligations of the editor to advance the proprietor's interest in every manner compatible with the above mentioned paramount duty.

Again, the advertiser who pays for his advertisement should be entitled to such commendatory notices as the objects of his advertisement merit—no more, no less. Our opinion is, that the medical editor who gives a complimentary notice of a new remedy, or enterprise, assumes responsibility for its published virtues and its genuineness. If the advertisement is inserted without such notice, he has no further responsibility in the matter than he is supposed to have for the opinions of his contributors. We are always willing to give favorable editorial notices of every article or instrument advertised in this JOURNAL, of the value of which we are personally and unmistakably informed.

METEOROLOGICAL REPORT FOR NEW ORLEANS.

Table I---July.

Day of Month.	Temperature.			Mean Barometer Daily.	Relative Humidity—Mean.	Rain fall—inches.
	Maximum.	Minimum.	Range.			
1	92	77	15	30.00	71	.03
2	91	76.5	14.5	29.95	68	.23
3	83	75.5	7.5	29.91	76	3.70
4	75	72	3	29.95	89	4.31
5	83	——	——	30.09	84	1.04
6	89	74	15	30.11	74	.00
7	91	77.5	13.5	30.04	67	.00
8	92	77	15	30.00	68	.02
9	81.5	75.5	6	29.99	75	.02
10	87	74	13	30.03	78	.76
11	80	71	9	30.00	90	.23
12	89.5	72	17.5	30.05	78	.00
13	87.5	75.5	12	30.16	76	.00
14	87	74	13	30.23	85	1.04
15	87	74	13	30.21	84	.01
16	87	74	13	30.13	75	.00
17	90	74.5	15.5	30.06	71	.00
18	90	76.5	13.5	30.08	72	.00
19	88.5	76	12.5	30.13	78	.02
20	91	77.5	13.5	30.12	73	.00
21	91	77	14	30.10	67	.00
22	90	76.5	13.5	30.12	73	.00
23	88	77	11	30.11	75	.02
24	89.5	76.5	13	30.05	75	.31
25	92.5	77	15.5	30.02	70	.00
26	——	——	——	30.02	72	.00
27	94	76	18	29.95	69	.00
28	95	79	16	29.90	74	.00
29	93.5	77.5	16	29.95	66	.00
30	93	77	16	30.02	59	.00
31	94	78	16	30.08	64	.00
Mean..	88.948	75 707	13.241	30.049	74.08	Total. 11.74

Table II---August.

Day of Month.	Temperature.			Mean Barometer Daily.	Relative Humidity—Mean.	Rain fall—inches.
	Maximum.	Minimum.	Range.			
1	93.5	76.5	17	29.99	63	.00
2	94	77.5	16.5	29.98	69	.00
3	96	78.5	17.5	30.01	67	.00
4	96	79.5	16.5	30.04	63	.00
5	96	81	15	30.04	57	.00
6	96	80	16	30.02	65	.00
7	95.5	80	15.5	30.04	64	.00
8	94	79	15	30.06	65	.00
9	97	79	18	30.06	65	.00
10	98	80	18	30.02	63	.00
11	98	82.5	15.5	30.06	66	.00
12	96	82	13	30.03	65	.00
13	99	80.5	18.5	29.92	67	.24
14	97	83	14	29.83	67	.00
15	97.5	80	17.5	29.83	67	.00
16	96	76 5	19.5	29.84	66	.69
17	88	74	14	29.90	75	.30
18	83	74.5	8.5	29.93	81	.08
19	83	75.5	7.5	30.02	89	.03
20	85.5	75	10.5	30.04	92	1.88
21	88	76	12	30.01	82	.00
22	83.5	77.5	6	29.97	90	.37
23	87	76.5	10.5	29.95	84	.01
24	88	77	11	30.00	73	.03
25	92	76.5	15.5	30.04	71	.00
Mean..						Total.

Mortality in New Orleans from June 30th, 1874, to August 23d, 1874, inclusive.

Week Ending	Apoplexy.	Congestion of Brain.	Congestive Fever.	Sun Stroke.	Malarial Fevers.	Typhoid Fevers.	Trismus Nascentium.	Total Mortality.
July 5..	0	6	3	0	14	4	6	130
July 12	1	4	6	0	14	4	5	140
July 19	3	1	6	0	17	4	6	153
July 26	1	3	3	0	11	2	6	131
Aug. 2	3	8	6	2	15	3	2	146
Aug. 9	2	11	13	0	19	5	3	153
Aug. 16	8	52	47	22	72	4	4	296
Aug. 23	0	8	12	1	20	5	5	130
Total..	18	93	96	25	182	31	37	1279

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ORIGINAL COMMUNICATIONS.

ARTICLE I. *Erysipelas; Its Treatment with Sulphate of Quinine.* By Y. R. LEMONNIER, M.D., Visiting Surgeon Charity Hospital, New Orleans; Member N. O. Medical and Surgical Association.

The term erysipelas is a Greek one, signifying a tendency to spread. It is an exanthematous disease, characterized by an eruption *sui generis*. "Erysipelas," says Dr. Gross, "is not only a frequent but a most formidable disease. Produced by various causes, both local and constitutional, it may exist as a primary affection, or occur as a complication of other lesions, modifying their character, interfering with their evolution, and even at times entirely supplanting them."

The disease is said to be idiopathic if it depends on some constitutional vice, or traumatic if it be associated with an external injury. It occurs at all ages, and in both sexes. Whether or not temperament exerts any influence upon its production is as yet unknown. "Nervous, irritable, and intemperate persons," says Dr. Gross, "are particularly subject to it." Judging from the number of "*alcoholics*" among the observations which follow, I conclude that alcoholism might be looked upon as predisposing to the disease. I did not notice any tendency on the part of the nervous and irritable temperaments to influence the march of the disease. Case No. 10, in which the exanthema seemed to be

most persistent, and where it will be seen to have returned at three different times, was a man of remarkable phlegmatic temperament.

Erysipelas prevailed in my ward epidemically for five months, during which time I was not a day without one or more cases. I have been often obliged to postpone all operations in which delay was admissible, for fear of giving rise to it. The smallest scratch was often the starting point, bringing on an attack which was more or less severe, and in fact, fatal in one instance (Case 12).

Apart from the cases in which alcoholism seemed to be a predisposing cause, I cannot account for the other cases in any other way, except by a vitiated state of the atmosphere of the hospital, which, during the whole winter, had not been without erysipelas in some of its wards. This had caused me to anticipate the distemper, for several months before it made its appearance in my ward. It will not be amiss here to state that, whereas my colleagues had erysipelas among their patients, I had pyæmia among mine. Of the latter disease I had three cases, closely following each other, in beds adjoining each other. Two of these died and one recovered. Those who had pyæmia did not have erysipelas, *et vice versa*. Once the latter had made its appearance, the former disappeared.

My object here is not to write a treatise on erysipelas, but an essay "*on its treatment with sulphate of quinine*," stating what, in my opinion, is the modus operandi of this drug in this disease. The theories here put forth are based on recent experiments and discoveries made by different authors. The result of this treatment is, twelve cases with eleven cures and one death.

Among the many theories concerning the pathology of erysipelas, one in particular has attracted my attention as probably the most correct. It is the one which considers the infiltration of the skin of erysipelatous parts, by the white corpuscles of the blood, as being the probable cause of the disease. Upon this is based the quinine treatment. In 1868, M. Vulpian, and after him, Koster, Volkmann, and Studener, reported, as the result of their investigations, the presence of a large amount of white blood globules, infiltrated in the thickened tissues of the skin of erysipelatous parts. M. Vulpian (Archives de Physiologie, 1868, p. 316.), says: "In thin slices of the skin which have macerated in alcohol, it is easy to recognize a thickening of the tissue, and the presence of

a large number of leucocytes. They are so numerous, that in many places they are contiguous to each other. In slices that are not very thin, the greater portion of the dermic fibre is hidden by them. They are disseminated throughout the thickness of the skin, being more numerous in the papillary layer and its neighborhood than in the deeper portions. These leucocytes are not to be found, except in very rare instances, in the subcutaneous cellular tissue. I am satisfied that they are colorless globules, from their similarity to those of the blood and pus, and from their form and their dimensions. Also from the result of the action of acetic acid, which caused them to fade, and show the presence of 1, 2 or 3 small nucleoli with distinct borders."

On the 25th of last April, I examined the liquid contained in the phlyctenæ of erysipelas (Case 3), and found it to be composed of a large amount of white corpuscles. Not a single red one was to be seen.

It would seem that these globules are rapidly absorbed, and disappear, since 2 or 3 days after the disappearance of the exanthema they are not to be found.*

Such, therefore, are the facts as seen with the microscope. But to what is due this great accumulation of leucocytes is the next and more difficult problem to be solved. Many theories are brought to bear on this point. But theories must do when we cannot do better, yet they only serve to hide our ignorance. The facts reported by Waller, of London, in *Philosophical Magazine*, in 1846; those of Conheim, of Berlin, relating to the transudation of the white corpuscles of the blood through the parietes of their blood-vessels, were disbelieved. M. Vulpian himself, while writing the result of his microscopical examinations, did not then (1868) know how to account for it, and rejected the theory of Waller and Conheim. Further on he says, "*Cependant il y avait quelques leucocytes dans la paroi de certains vaisseaux; mais assurément c'était là une exception*" (And yet there were a few leucocytes in the parietes of certain vessels; but assuredly this was an exception). He finishes his article thus: "On a cherché à voir si l'on ne pourrait pas, dans ce cas, reconaître le mode d'origine des leucocytes, disséminés, en si grand nombre, dans le derme; mais on n'a pu arriver à se former les préparations sous les yeux, aucune idée nette sur ce sujet" (In vain we tried to account, in this case, for the origin of the leucocytes, dissemi-

*Perroud, *Annals de Dermatologie and de Syphiligraphie*, 1869-70, p. 242.

nated, in such great number, in the dermic tissue. With the preparations under our eyes we could obtain no satisfactory result). Such was his language in March, 1868. But since then, new facts were made known, those of Stricker, in 1867,* Prussack, of Vienna, etc., confirming the conclusions of Waller and Conheim. Finally, in 1870, we find M.M. Vulpian and Hayem† reading before the Académie de Médecine of Paris a paper on this subject, supporting the views of these authors. Among the French micrographers, Lortet,‡ and Ranvier,‖ had already entertained the same views.

The transudation of the white corpuscles of the blood, through the parietes of their blood-vessels, is now a recognized fact, and it is admitted by many, that it is solely to this transudation that is due the formation of pus.

Physiology has long ago taught us that, the white corpuscles of the blood were endowed with a movement, *sui generis*, called *amœboid movement.* Lortet has proven that it was due to it, that the white corpuscles transuded through the parietes of their vessels.

The transudation being known, as also its cause, the question now is, how to stop it. If we know not what the intimate cause of erysipelas is, let us at least deduct from its pathology, if we can, a treatment. Such is the object of this paper.

From the learned researches of authors, and in particular those of Dr. Binz,§ we have been taught that quinine had the power of stopping the amœboid movement of the white corpuscles of the blood. It was with the *neutral muriate of quinine* that the doctor made his experiments, as it is the most soluble of the quinine salts, dissolving in 60 times its volume of water. The object of those researches was to prove that quinine possessed *antiseptic* properties. Admitting that the works of MM. Schultze, Schwann, Pasteur, and many others, are correct, he admits, *a priori*, that the animalcules developed in putrefied liquids are the cause of the chemical modifications which take place in these liquids. In proving the powerful toxic effect of quinine on the inferior organisms, he has established the antiseptic properties of this drug.

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* Journal de l'Anatomie et de la Physiologie de Robin, 1867.

† Academie de Medecine, seance du 15 fevrier, 1870

‡ Lortet—Annales de la Societie des Sciences Medicales de Lyon, 1868.

‖ Ranvier—Comptes rendus de la Societie de Biologie, 1868.

§ Experimentelle Untersuchungen uber das Wesen der Chininwirekung, von Dr. C. Binz (Experimental Researches on the modus operandi of Quinine, by Dr. Binz, in *Archives de Physiologie*, 1868, p, 747.

In order to obtain better results, he first experimented on (microscopically speaking) infusoriæ of a colossal size, namely, the paramecia and the colpoda, which are to be found in vegetable infusions. Later, it was discovered that the vibrios and bacteria were also destroyed by quinine.

A solution of muriate of quinine, one part to 800 of water, instantly kills the voluminous infusoriæ. One part to 2000 kills them in a few minutes, and one part to 20,000 in a few hours. Such is the special action of quinine, which the other vegetable alkaloids do not possess to the same degree. A solution of salicine, of one part to forty of water, has no visible action on these animalcules. Muriate of morphine, one part to 120 of water, does not kill them completely in the space of one hour. Nitrate of strychnia, one part to 200 of water, only acts after a contact of a few minutes.

Among the reagents which do not possess corrosive properties, hypermanganate of potash alone seems, in this respect, to be more powerful than quinine. Of the active chemical substances, the salts of copper and zinc, and also creosote, have a less energetic action. Bichloride of mercury alone seems to be equal to quinine.

Among the smaller infusoriæ, the vibrios, spirilla, bacteria, are liable to the same results as the paramecia and colpoda. The monads seem to offer a greater resistance to the effects of the poison. In concluding, Dr. Binz says: "A solution of quinine sufficiently concentrated has a marked antiputrid action, and surpasses in this respect a number of substances whose conservative powers have long since been recognized." Here I fully agree with this distinguished physiologist, as I have often had the occasion of verifying this antiputrescent power of quinine.

As regards the antiphlogistic properties of this drug, we shall see to what conclusions Dr. Binz comes, from the results of the researches above mentioned.

"The spontaneous movements of the white blood corpuscles were first mentioned by Lieberkühn, and later by Schultze. If to perfectly pure serum be added a drop of blood, taken from an animal in full digestion, we shall see the amœboid movements. Let this experiment be repeated with serum containing $\frac{1}{2000}$ part of quinine, and we shall see the movements of the white corpuscles stopped, whereas the red ones have not in the least been affected.

"Two young cats having taken each half an ounce of milk, one of them received several subcutaneous injections of quinine. Their blood was afterwards examined with the microscope. The white globules were seen to be greatly diminished in the blood of the one which had received the injection of the quinine. Before the experiment, this animal showed a larger amount of white globules than the other (294 against 214); after the experiment he had *twenty times less* (17 against 344)."

From this effect of quinine on the white corpuscles, the doctor concludes that quinine is an antiphlogistic. He admits with Conheim, that in the majority of cases, inflammation is not characterized by a proliferation of the elements of the conjunctival tissue, but by an increase in the number of the leucocytes, and their issue through the vascular parietes, to scatter themselves in the surrounding tissues. Viewing things in this light, it is easy to understand the action of quinine on inflamed parts. From the numerous experiments, made especially on the batracians, and reported in the paper of Dr. Sebarrenbroich, Dr. Binz affirms that, subcutaneous injections of quinine delay the development of inflammations, and when their effects have had time to show themselves, cause them (the inflammations) to disappear.

If quinine acts so powerfully against inflammatory affections, such as pyæmia, etc., how is it that we do not always obtain a success? To this Dr. Binz answers as follows: "This is due to two principal causes. First, the doses given are not large enough. To prescribe efficaciously against the progress of a peritonitis, in a subject weighing 150 lbs (the dose of the drug should evidently be in proportion to the mass of the individual) we must give from 12 *to* 15 *grammes of quinine* (3 to $3\frac{3}{4}$ drams) in 24 hours.

"Second, it frequently happens that a large portion of the drug is not absorbed. This is what often happens when we make use of the sub-sulphate (or neutral sulphate) of quinine. This powder is often inert, and goes through the bowels without being absorbed." For this last reason the author hopes that the muriate of quinine—a soluble salt, *par excellence*—will substitute the sulphate in daily practice.

I believe that practitioners have exaggerated the fears of large doses of quinine, but yet I do not, for the present, advocate such large doses as advised by Dr. Binz, i. e., in proportion to the weight of the individual. As to the second clause, the non-

absorption of the drug, all practitioners have had occasions to regret this.

The great solubility of the muriate of quinine, and its easy introduction into the system by subcutaneous injection, should be a sufficient reason to give it the preference. Not only the chances for non-absorption by this method are less than by any other, but its action is more rapid.

Concerning the doses of quinine, it will be seen that I gave 2 grains every hour, then every 2 hours, 3 hours, and 3 times daily. The patients, except in a few instances, never taking their medicine regularly, which decreases the supposed dose of the kinick salt taken in the 24 hours. Others have prescribed with satisfaction the sulphate quinine in this disease, in doses of 6 or 8 grains in the 24 hours. Dr. Binz, no doubt, according to his theories, would have prescribed larger ones than I have.

Sum toto, this proves that the quinine is paramount, and the dose secondary. It is only by comparative statistics that we can know what is the proper quantity to be given.

Another point of great interest is to continue the administration of the remedy for several days, say one week, after all traces of the erysipelas have disappeared. If this rule is not strictly adhered to, relapses are most frequent (Cases 8 and 10).

Few of the cases reported, as will be seen, have suffered from secondary abscesses, and in those who have, the abscesses have been insignificant.

But one case proved fatal, and it was one of traumatic erysipelas of the foot. I have had to contend against no serious accident in those whose head was affected by the disease. No brain symptoms of any kind, though in one case, where the head and face were affected, there were three relapses.

Quinine was given as soon as the first symptom, initiatory chill or *rapid rise of temperature*, showed itself. In some cases the drug was prescribed before the appearance of the eruption.

Except the case which terminated fatally, none offered those alarming symptoms which we so often have to battle against in the cases entering hospital in an advanced stage of the disease, in which no treatment, or an improper one, has been instituted prior to their admittance. No delirium, or if any, a very light one; no ataxo-adynamic symptoms; not even any tendency to deep seated abscesses. I was told by my colleague of the hos-

pital, Dr. S. Logan, that several of his cases had either had some deep seated abscesses or presented a tendency to their formation.

Are these mild symptoms in the majority of my cases to be attributed to the treatment employed, quinine? I think so. I am not prepared to say whether quinine possesses a marked superiority over all other methods of treatment in this disease, as I have not inquired into the *modus curandi* of my colleagues at the hospital, but I may say, that in the hands of Dr. Perroud,* of the the Hotel Dieu de Lyon, as also in mine, it has proved most valuable.

Except a purgative now and then, when a marked saburral state of the tongue existed, or in cases accompanied with constipation, the kinick salt was the only treatment. Where the local heat was very great (as in Case 12), an evaporating lotion was prescribed, which was cooling and agreeable to the patient. Anything else that might have been ordered were placebos, calculated to quiet the patient's mind, and not with any hope of a curative effect. I have little or no faith in the topical treatment, and depend on the constitutional one for a cure or alleviation.

In several instances, it will be seen that I prescribed collodion *loco dolenti*. It was with a view of testing the abortive power of this topical agent in erysipelas. Dr. Paul Broca, Chîrurgîen des Hopitaux de Paris, strongly recommends it, and recognizes its power in arresting the march of the exanthema when properly applied. Dr. B. is a high authority, and whatever he recommends deserves a trial. His method is to surround the affected parts, say half an inch beyond the line of demarcation, with a band of collodion, which, if properly kept up, will prevent the spread of the eruption. I have applied this method according to directions, and as I have seen it employed, in Paris, by M. Broca himself, and am sorry to say that, in every instance, the result has been a negative one. Why is this? The Parisian surgeon reports cases of success. It does not behoove me, a young surgeon, to criticise the results of my former and time-honored teacher of the hospitals of Paris. Perhaps on a future occasion, with a more extensive experience, I will be better able to put forth an opinion.

Twelve cases of erysipelas are reported, which are divided as follows: Idiopathic 2, Traumatic 10. They are subdivided as

* Loc. cit.

follows: Erysipelas of the face and head 5, trunk 4, superior extremities 1, inferior extremities 2. The diatheses attached to these 12 cases are, alcoholism 3, syphilis 1, scrofula 1, tuberculosis 1, none 6—total 12.

Two cases, Nos. 8 and 10, had each 3 relapses, i. e., new attacks of erysipelas returning after a complete disappearance of the eruption, from a too early discontinuance of the drug. Counting the relapses as so many cases, we have 16 cases, with 118 days as the sum of their duration, which gives 7⅜ days as the mean duration of each case. Five cases, Nos. 5, 6, 7, 11 and 12, had a shorter duration.

Observations.

Case 1.—Extensive syphilitic caries and necrosis of right shoulder; intercurrent traumatic, erratic erysipelas; abscess of left side; syphilitic cephalæa. Cured.

April 15th—*Antecedents.*—This man, aged 44 years, entered my ward on March 5th, 1874, for an extensive caries and necrosis of right shoulder dating back four years. He has had syphilis in former days; has suffered for the last two months from a cephalæa, which nothing has relieved. Since his admittance, he has been taking 20 grains iodide potassium 3 times daily. His headache is gone and shoulder improving.

Day before yesterday (13th) erysipelas having broken out in the ward, I advised him to leave and come back again in a few days, when the disease would have disappeared. I feared he might catch it, which would be serious on account of the extent of the injury and his bad state of health. He left that day but came back the next morning (14th), with the intention of remaining in the hospital. I again warned him against exposing himself to the exanthema, but he insisted upon remaining.

Present Condition.—This morning he has an extensive erysipelas of right shoulder, arm, and corresponding half of back at its scapular region. The arm is very much swollen and painful from the shoulder down to the hand inclusively. The erysipelatous parts are covered with phlyctænæ from the size of a small pea to that of a twenty-five cent piece. Pulse 72; temperature 101° 1′. Tongue good.

℞—2 grains sulphate quinine every hour. Encircle parts with

collodion, to stop if possible the march of erysipelas. Continue iodide potassium. Diet ad libitum.

April 16th. Feels better. Exanthema fades where it first broke out and progresses in the opposite direction, in spite of the circle of collodion. A concentrated solution of tannin which had been applied to affected parts (placebo) is this day discontinued. Pulse 102; temperature 101° 1′. Tongue good; bowels regular; arm very much swollen. Continue quinine.

April 17th. Erysipelas advances on the sound parts, and is now reaching the left side of chest and base of posterior part of scalp. Swelling of arm decreasing. Pulse 72 (slow); temperature 100° 7′. Tongue good. Stop application of collodion; continue quinine and pot. iod.

April 18th. Swelling of arm going down. Erysipelas has reached left side of body and is descending. Here, over angles of 8th, 9th and 10th ribs, is a swelling, size of one's hand, longitudinal to the ribs, which seems to fluctuate; this is covered over with the eruption. Patient reports having had this lump several times. It comes and goes without anything being done. Pulse 72, slow; temperature 102° 5′. Tongue good; no appetite; bowels regular. Treatment as yesterday.

April 19th. March of erysipelas continuing. Swelling of left side dissappears. Pulse 84; temperature 103°. Tongue good. Same treatment.

April 20th. Erysipelas principally over scalp and upper portion of forehead; almost gone from the trunk. Forearm, and hand, right side, very œdematous (impediment to venous circulation). Pulse very weak; temperature 99° 6′. Bowels costive. Same treatment.

April 21st. Erysipelas over both temporal regions. Pulse 72; temperature 99° 3′. Tongue good; bowels operated upon. Same treatment.

April 22d. Erysipelas disappearing fast. Pulse 66, slow (kinick); temperature 97° 5′. Tongue good; bowels regular. ℞—2 grains sulphate quinine every 2 hours; continue syphilitic treatment.

April 23d. Small patch of erysipelas over right malar bone. Swelling of hand and forearm almost gone. Pulse 60; temperature 96° 8′. Take quinine every 3 hours; keep hand elevated.

April 24th. Patch of erysipelas over right malar bone, size of

a twenty-five cent piece. Pulse 68; temperature 97° 5′. Tongue very good; patient much better. Continue.

April 25th. Patch of erysipelas over right malar bone slowly disappearing. Pulse 68, weak; temperature 97° 2′. ℞—Tinct. cinch. comp., ℥vj; ferri citratis, ʒj. M. S. Tablespoonful 3 times daily. Continue potassi iodidi.

April 26th. Improving. Pulse 72; temperature 98. Same treatment.

April 27th. Right arm still swollen and cold. This swelling came after the disappearance of the erysipelas on this limb. Pulse 78; temperature 101° 1′. Same treatment.

April 28th. Pulse 74; temperature 99° 3′.

April 29th. All traces of erysipelas gone. Pulse 86; temperature 99°. Tongue good. Treatment same. Duration of exanthema 14 days; syphilitic lesions improving.

Remarks.—My present intentions being to write on erysipelas, I here stop my observations in this case. The reader will at once see that the two affections traveled side by side without seemingly influencing each other. I have said nothing of the nature of the lesions of the right shoulder, from the fact that I did not notice any change in these parts which could have been attributed to the effects of the exanthema. Both treatments—internal for the erysipelas, and internal and external for the syphilitic diathesis—were kept up during the 14 days that the eruption lasted. No serious symptom of any kind showed itself during this lapse of time, although the patient was in a very bad state of health from a long continued (4 years) disease.

I will here state that the threatened abscess of the left side did come and go, without any special treatment, as it is reported by the patient to have done, on several former occasions. Twenty-four hours after all traces of erysipelas had disappeared, i. e., when the patient was cured, and several days after the disappearance of the eruption from the left side of body, the tumor again reappeared. This time it fluctuated and had finally to be opened. What was its origin I know not. I suspect a caries of one of the ribs, but my probe could discover none. At all events, while the erysipelas lasted, the lesions of the right shoulder and the general health, in a syphilitic point of view, nevertheless improved. The abscess, as proven above, depended on this diathesis, and was not caused by the erysipelas.

This patient is to-day (June 24th) cured, and will leave the hospital at the end of the month.

Now, to what is due the mildness of this erysipelas in a patient with such a dilapidated constitution? Is it not rational to think that it is due to the treatment instituted?

Case 2.—Traumatic erysipelas of right hand. Cured.

March 31st, 1874. James Rooney, act. 28 years, native of England, in America 3 years, stonecutter. Entered my ward on the 27th of March (erysipelas was then in the ward), for necrosis of some of the bones of the right hand, the result of an accident produced by a circular saw some two months previous. Yesterday, at about 2 p. m., he was suddenly taken with a "strong chill, lasting about 3 hours" (sic.), "followed by fever."

Present Condition.—Pulse 72; temperature 99° 5′. Commencing erysipelas at seat of injury; wounds look bad. Previous to this the injured parts looked well. Bowels regular; patient emaciated: this he reports as the consequence of his injuries, as previously he enjoyed good health. Removed another sequestrum.

℞—Quiniæ sulphatis, 2 grains every hour Patient not to be disturbed when asleep. Charpie l. d.

April 1st. Pulse 116; temperature 105° 8′. Erysipelas has extended to the whole surface of the hand and is progressing up the forearm. Along the dorsal surface of the latter, reaching up almost to the elbow, is an irregular reddish line of angioleucitis. Local heat great; bowels regular; tongue pointed and dry. No pains of any kind at either hepatic or other regions. Continue quinine. Tinct. arnica and water *aa* locally. Above elbow, arm encircled with collodion to check, if possible, the march of the exanthema.

April 2d. Pulse 80; temperature 101° 1′. Greatly improved. Erysipelas seems stationary. Extracted one piece of bone from first metacarpal. Continue treatment.

April 3d. Pulse 90; temperature 103° 6′. Erysipelas stationary; has not overstepped the band of collodion. Tongue pointed and reddish; bowels regular. Continue.

April 4th. Pulse 94; temperature 102° 5′. Tongue pointed and good. Threatened abscess of dorsal surfaces of hand and wrist. A few small pieces of bone come out with the dressing. Erysipelas has ascended above the band of collodion. Same treatment continued.

April 5th. Pulse 84, slow; temperature 101° 3′. Tongue good. Abscess has opened and discharged pus through one of the wounds. Erysipelas fading. Continue.

April 6th. Pulse 96; temperature 103°. Tongue pointed, with tip and edges red. Erysipelas ascending the arm; phlegmonous inflammation of hand. Extracted a few spiculæ of bones; wounds look bad. Continue quinine; stop collodion.

April 7th. Pulse 84; temperature 99° 7′. Crepitation of necrosed bones being very distinct, suppuration abundant and unhealthy, the removal of the former is decided upon. Patient is chloroformed; Esmarch's method employed. The necrosis involves most of the bones of the wrist. The index finger is amputated at its metacarpo-phalangeal articulation; a portion of its metacarpal bone, at carpal extremity, is resected; metacarpo-phalangeal articulation of middle finger crepitates; ulna at radio-carpal articulation partially dislocated. After the operation 3 drainage tubes are placed; two perforate the wrist, and one lies longitudinally over metacarpal space of index finger. The operation lasted 40 minutes and was bloodless. Erysipelas is fading.

℞—Sulphate morphine (q. s.) to relieve pain. Continue quinine; continued irrigation with carbolic acid (℥i to Oj) water. Best diet.

April 8th. Pulse 80; temperature 100° 4′. Tongue good; erysipelas disappearing; patient doing very well. Same treatment.

April 9th. Pulse 72; temperature 99°. Doing well; bowels costive. Continue quinine and continued irrigation. Castor oil ℥j to be taken in the morning.

April 11th. Pulse 70; temperature 99°. All traces of erysipelas have disappeared.

℞—Tinct. cinch. comp. ℥vj, ferri cit. ʒi. ℥. Tablespoonful 3 times daily.

Duration 11 days.

This patient is still in my ward, but his hand is saved, his final cure being but a question of time. From the critical condition of this hand at the time the erysipelas broke out, I had reasons to apprehend a serious result. Its (the erysipelas) effect was to rouse up a chronic inflammation to an active one, and force on my part an interference which I would certainly have delayed until the eruption had disappeared. This was a gain of time. I operated while the exanthema existed, and no recrudes-

cence occurred. But the administration of quinine was kept up regularly, and to it is this no doubt due, as also the weakness of the traumatic fever subsequent to the operation.

As will be seen, I thought at one time I was about having a success in the arrest of the erysipelas by the local application of collodion, but it was a delusive hope of only 48 hours.

Case 3. Traumatic erysipelas. Sulphate quinine. Cured.

April 21st. Albino M. Peralta, aet. 40 years, baker, entered my ward April 19th, 1874, suffering from a fistule of the anus. He was operated upon on day of admittance, by incision. Last night he had fever, and to-day an erysipelas exists at seat of injury.

Present Condition.—Pulse 102; temperature 104°. Diffused and spreading inflammation of the skin around the seat of wound. Patient has no appetite. Headache.

R—Sulph. quinine 2 grains every hour.

Evening. Pulse 100; temperature 104° 3′.

April 22d. Pulse 90; temperature 102° 5′. Inflammation of skin continues to spread in all directions. Tongue furred; headache; no appetite. Continue quinine.

Evening. Pulse 100; temperature 104° 7.

April 23d. Pulse 92; temperature 103° 2′. Erysipelas still progressing. Patient had no sleep last night. Tongue swollen and thick, furred in the centre, red at edges. Same treatment.

Evening. Pulse 92; temperature 104°.

April 24th—Evening. Pulse 90; temperature 103° 2′. Patient better. Inflammation of skin disappearing; erysipelatous parts covered with phlyctenæ. Continue quinine.

April 25th. Pulse 80; temperature 101° 4′. Erysipelas disappearing. Patient better. Liquid from phlyctenæ examined under the microscope and seen to be an exudation of white blood corpuscles. Continue quinine.

Evening. Pulse 90; temperature 100° 7.

April 26th. Pulse 76; temperature 100° 1. Exanthema still disappearing. Tongue red. Same treatment.

Evening. Pulse 100; temperature 104°. Patient not well this evening. Pulse strong and frequent.

April 27th. Pulse 74; temperature 100° 7′. Feels much better. Erysipelas fades where it first showed itself, and descends in an opposite direction.

Evening. Pulse 86; temperature 102° 2′. Continue quinine.

May 1st, Pulse 68; temperature 98° 2 . Bowels costive; inflammation of skin descends along left leg; patient otherwise well. ℞—Pil. cath. comp. No. 4. S. 2 at once; others 4 hours after if not purged.

Evening. Pulse 80; temperature 102° 2′.

May 2d. Pulse 68; temperature 98° 2′. Erysipelas has reached the knee and is fading. Continue quinine.

May 4th. Pulse 68; temperature 97° 7′. Exanthema gone. ℞—2 grains quinine every 2 hours.

May 5th. Both constitutional and local symptoms have disappeared. Patient weak. ℞—Sulph. quinine 2 grains 3 times daily.

May 9th. Patient cured of his erysipelas and almost cured of his fistule is, though weak and anæmic, at his request discharged. ℞—Ferrated cinchona wine.

Duration 13 days.

Remarks.—In favor of the theory which considers erysipelas as the result of a transudation of the white corpuscles through the parietes of the blood-vessels, we have the microscopic examination of the 25th April, when the liquid contained in the phlyctenæ was seen to be composed of white corpuscles. No amœboid movements were seen. We account for their absence, when we remember that these corpuscles were not, when removed, in their proper channels, and consequently must have undergone changes affecting their ordinary movement.

The exanthema was of a light inflammatory type. Its contagion is here plainly seen. A case in the adjoining bed, with the same surgeon, students and nurse, instruments, &c., attending on both. For what we know the same basin and sponge might have been used for each patient. Its incubation was of 48 hours duration; its starting point the wound. Fever most probably preceded the eruption, setting in with a high temperature, which reached its acme on the evening of the 2d day, with a morning remission, after which defervescence commenced. The evening rise and morning fall were well marked, until the total disappearance of the eruption, when the ascents ceased. No premonitory chill ushered in this erysipelas. No gastric symptoms accompanied it. Effects of quinine, dizziness, etc., but moderately marked, though the patient, the first 2 or 3 days, took regularly 2 grains every hour.

Case 4.—Idiopathic erysipelas of head and face. Alcoholism-Cured.

Thomas Flemming, aet. 33 years, native of Ireland, laborer, entered ward 3, Charity Hospital, May 3d, 1872.

May 3d—*Antecedents.*—This man came here on foot. He is an alcoholic. Is unable to tell when he was first taken sick, but thinks his sickness dates back 5 or 6 days.

Present State.—The swollen condition of his head and face attracted my attention These parts were œdematous, swollen, red, sensitive to the touch, with a high temperature. There existed no injury of the head. The patient thought the swelling was caused by poisonous vines, of which there were many where he had been at work. I diagnosed an "*idiopathic erysipelas.*"

His breath was offensive; mind clear; temperature very high, 107° 2′; pulse 98, regular. Circumference of head 25 inches.

℞—Quinine 2 grains every hour; sulph. morphine or hydt. chloral q. s. at bed-time to procure sleep. Nourishment ad libitum. Rest in bed.

May 9th. Erysipelas increased for several days, then seemed to decrease, but to-day there exists a recrudescence of the exanthema on the right cheek, with high temperature (104°), and rapid pulse, (104). Bowels constipated. His eyes were closed by the tumefaction of the lids, and the conjunctivæ blood-shot.

℞—Continue treatment; purgatives.

May 19th. Up to this day the symptoms of alcoholism have been more or less marked, increasing by night. Conjunctivæ, up to about the 15th day of disease, being often blood-shot. The tongue at the height of the sickness dry, red and cracked. Delirium was so intense, that for several days and nights he had to be strapped to his bed. Costiveness very great, for which active purgatives were freely ordered. In the meanwhile, erysipelas followed suit to these symptoms—one day better, the next worse. To-day it has disappeared entirely, the fever is abated, and the circumference of the head has fallen to 22 inches.

Stop quinine. Comp. tinct. cinchona by tablespoonful 3 times daily. Best nourishment.

Subsequent to this; secondary (metastatic?) abscesses formed under the scalp; but these not being the subject of this paper, suffice it to say that, from this day, all symptoms of erysipelas

disappeared, and that the man left the hospital cured on the 15th day of June, 1872.

Duration of erysipelas 16 days.

Résumé.—An idiopathic erysipelas of head and face, with bilious complication, as evinced by the saburral state of the tongue. Subsequent to this, well marked symptoms of alcoholism set in, as epiphenomena, which, for a time being, overruled those of the erysipelas, and were treated by sulph. morphine, as much as one grain in the night, and hydrate of chloral which, on one occasion, was given in a ʒij dose. As the patient improved, these doses were diminished, and finally the medicines stopped.

Sulphate quinine in 2 grains doses every hour, as an antizymotic, was prescribed against the erysipelas. The bilious state of the primæ viæ was combatted by appropriate purgatives—calomel and bicarb. soda, castor oil, and the neutral salts. These acting besides as a derivative of the cerebral circulation. No local applications.

Case 5.—Thos. Conway, aet. 42 years, Irishman, laborer, entered my ward April 16th, 1872. Discharged cured, May 7th, 1872. Diagnosis—traumatic erysipelas of head and face. Entered hospital on fifth day of disease. On first day of treatment, sixth of sickness, temperature 104° 9′; 2d day, 104° 5′; 3d day, 104° 4′. On the 4th day after admittance, he was without fever. For the first three days he was delirious at night. Treatment consisted in 1 grain doses of sulphate of quinine every hour (patient not to be disturbed when asleep) and narcotics at night. On the 20th he took a dose of castor oil. From this day all treatment ceases. On April 24th abscess of superior right eyelid lanced.

On day of admittance, and the following 3 or 4 days, his breath was of a peculiarly offensive smell. He had a diarrhœa, for which no treatment was instituted. He could not tell how the disease commenced. He is an alcoholic.

Erysipelas cured after 4 days of treatment.

Case 6.—Ricardo Barbaso, Mexican, aet. 30 years, entered May 5th, 1873, for scrofulous necrosis—intercurrent traumatic erysipelas. For the diathesis, cod-liver oil and comp. tinct. cinch. aa, were prescrib d, and for the erysipelas 2 grains sulph. quinine every hour. On the 4th day the latter was cured.

Duration 4 days.

Case 7.—Traumatic erysipelas. Peter Fahey, aet. 36 years, entered ward 8, Charity Hospital, April 20th, 1874, for a stricture of rectum. On May 1st, subsequent to manipulations for gradual dilatation of stricture, an erysipelas of anus broke out which, on the 3d had reached the scrotum, where an abscess of its cellular tissue had to be lanced on the 7th. He was put on the quinine treatment, 2 grains every hour, then 3 times daily. On the 9th, all traces of erysipelas had disappeared, and on the 11th quinine was stopped. Patient recovered.

Duration 6 days.

Case 8.—Traumatic erysipelas, complicated with alcoholism.

John Donovan, aet. 25 years, Irish, sailor, entered ward 8, April 15th, 1874. Discharged, on his demand, May 14th, 1874. This man was an alcoholic. When he entered my ward, his face and head were very much swollen from a traumatic(?) erysipelas. We supposed it to be of a traumatic origin from the fact that two days after his admittance a small wound of the scalp was discovered. Delirium at night during the first 3–4 days, which necessitated the administration of morphine. He could not say when he was first taken sick. He was put on the usual quinine treatment, two grains every hour, then every two hours, and finally 3 times daily. On April 20th, 10 grains each calomel and bicarb. soda were successfully prescribed for a constipation accompanied with a bilious tongue. On the 22d, an abscess of the right upper eyelid was lanced. There being then no more traces of erysipelas quinine was stopped, but had to be administered two days later (24th), for a relapse of the exanthema. May 1st—appetite being wanting, pulse small and weak, comp. tinct. cinch. (℥ss. 3 times daily) was ordered. May 5th—lanced abscess on left side of lower jaw, where no traces of erysipelas had existed. This shows the similarity between this disease and pyæmia, as regards the formation of metastatic abscesses. May 11th—all traces of erysipelas having again disappeared since 3–4 days, after having attacked different portions of the face and head, general health being ameliorated, quinine was again stopped. Two days later, a third attack of the exanthema, milder than the preceding one, quinine again prescribed. When he left on the 14th (to go to Marine Hospital), a trace of erysipelas still existed at the root of the nose and adjacent parts.

This case shows the necessity of continuing the quinine treat-

ment several days after all traces of the erysipelas have disappeared. The first attack was cured in less than 7 days. The second, is of an erratic and mild type, and lasted 14 days. When he left, he bore a trace of the 3d attack, which had commenced 24 hours previously. Despite the symptoms of alcoholism, especially marked at night, this case proved a mild one.

Case 9.—Traumatic erysipelas. Cured.

Mary Newes, æt. 17 yrs, Ward 36, Charity Hospital. Through the courtesy of my colleague, Dr. Alexander Hart, I was asked to operate on this young girl on the 12th of May, 1874. The following day she had fever, preceded by a chill, and followed by an erysipelas at the seat of injury. The operation was long, and necessitated the use of a large amount of chloroform, which caused an irritability of the stomach.

May 13th. Erysipelatous inflammation around seat of wound. Irritable stomach.

℞—2 grains sulph. quinine every hour, when not asleep.

May 14th. Erysipelas increased, accompanied with irritable stomach and phlyctenæ. No medicine could be retained. Continue quinine. Carbolic acid wash ʒj to Oj., l. d.

May 15th. Erysipelas still progresses. Irritability of stomach continues; rejects all that she takes. Continue quinine and carbolic acid wash. ℞—Sulph. morphine q. s., to quiet stomach.

May 18th. Erysipelas descends along the leg. Irritable stomach continues. Costiveness. Pulse small and weak. Tongue red and dry at centre, white at the edges. Irritability of stomach prevents the injestion of food or medicines.

℞—Pil. cath. comp. No. 4, to be followed by salt and water enema if necessary. Morphiæ sulph. grij, into 12 papers. S. One p. r. n., to quiet stomach. Continue quinine. Tinct. cinch. comp. ʒss 3 times daily. Carbolic wash, l. d.

May 19th. Erysipelas descends along the leg. Bowels costive. Stomach is so irritable that nothing is retained. ℞—Hypodermic injection of sulph morphiæ (½ grain) at epigastric region. Comp. tinct. cinch. and carbolic acid wash l.d., as before. Salt and water enema. Discontinue quinine.

May 21st. Tongue clean. Stomach no longer irritable. Erysipelas has reached the knee. Patient craves for acids. Continue comp. tinct. cinch. and local treatment to wound.

May 25th. Exanthema has reached the foot. Found patient eating ham and mustard.

Continue the same treatment.

May 27th. Foot œdematous. No more erysipelas. Threatening abscess on dorsal surface of foot. Same treatment. Poultice to foot.

May 28th. The most pointing part of the swelling lanced. A serous liquid escaped. Same treatment.

May 29th. Foot doing well. Furfuraceous desquamation of those parts of the skin attacked by the erysipelas.

Remarks.—This girl was considered by many as a "gone case." Except the sick stomach, I do not see that any troublesome symptoms accompanied the erysipelas. But for the gastric irritability, caused by the chloroform, which prevented the injestion of drugs, the exanthema would have ended sooner. At no time did my student, Mr. Deslattes, or myself ever doubt a successful result.

Duration 14 days.

Case 10. Erratic traumatic erysipelas of head and face recurring three times. Cured.

Laurent Duchenne, æt. 41, Frenchman, milkman. Entered Charity Hospital, February 21st, 1874. Discharged cured, May 17th, 1874.

March 12th—*Antecedents*—This man entered my ward on the 21st of February for an extensive injury of the head. While driving his cart, at 1 a.m., he fell, the wheel passing over his head, denuding the skull of its scalp on the anterior right side and posterior left side. Here was a lacerated wound of the scalp, semicircular in shape, 8 inches in length, below and to the left of the occipital protuberance. In front, at an inch (?) to the right of the median line, on the top of the head, was another lacerated wound of the scalp, eleven inches in length, extending from the occipital bone to the inner angle of the right eye, partially tearing off the superior lid. The whole of that side of the skull was denuded of its scalp, which hung over the right ear. At the parietal protuberance a portion of the periosteum, of a circular shape, about 2 inches in diameter, was torn off, adhering to the scalp. The supra-orbital nerve was partially lacerated.

The wounds were dressed, *secundum artem*, the posterior healing by first intention, the anterior by first and second intentions.

The wound was healing kindly when on this day erysipelas is discovered.

Present Condition.—Pulse 96; temperature 102°2′. Commencing erysipelas on right anterior side of head and right superior eyelid. No precursory chill has ushered in this exanthema. Tongue good. Appetite poor.

℞—2 grains sulph. quinine every hour. Not to be disturbed when asleep. Encircle erysipelatous patch with collodion. Continue local treatment.

March 13th. Pulse 92; temperature 101°4. An abscess of right superior eyelid has opened at inner angle of eye and runs freely. Erysipelatous inflammation about gone. Bowels regular. Continue treatment.

March 14th. Pulse 72; temperature 101°4. Doing quite well; all traces of erysipelas gone. Local treatment.

March 16th. Pulse 120; temperature 106°3′. Tongue good. Wounds look bad; will probably have erysipelas. No operation from bowels. ℞—Pil. cath. comp. No. 8. S. 4 at once. If not purged by morning, give the other four. Continue local treatment.

March 17th. Pulse 86; temperature 100°. Very light erysipelas at seat of injury, at occipital region. Three operations from bowels. Took 8 pills.

March 18th. Pulse 84; temperature 100° 1′. Doing better.

March 19th. Pulse 102; temperature 101° 4. Not very well; no appetite. Wound doing well. ℞—Tinct. cinch. comp., ℥vj, tablespoonful 3 times daily.

March 20th. Pulse 72, weak; temperature 103° 6′. Tongue saburral. No appetite. Feels weak. Erysipelas of right superior eyelid and corresponding side of head; horripilations. Continue tinct. cinch.

March 21st. Pulse 108; temperature 106° 3′. Tongue furred and trembling. Horripilations. Bowels regular. Two large phlyctenæ at erysipelatous spot on right side of forehead.

℞—2 grains quinine every hour. Continue comp. tinct. cinch.

March 22d. Pulse 108; temperature 104° 7′. Tongue cleaner. Bowels loose, 7 operations. Wound doing better. Erysipelas on left side of forehead. Horripilations. Continue medicine.

March 23d. Pulse 82; temperature 102° 2′. Much better. Erysipelas over nose and right ear. 2 grains quinine every 3 hours.

March 24th Pulse 102; temperature 104° 3′. Erysipelas on left side of face and ear. Wound better. Quinine, 2 grains every hour.

March 25th. Pulse 90; temperature 96° 8′. Much better. Continue quinine.

March 28th. Pulse 78; temperature 99° 3. No more erysipelas. Stop quinine.

March 29th. Pulse 120; temperature 105° 4′. Erysipelas has again broken out on right side of forehead. Tongue furred, with a tendency to become dry. Pus of wounds unhealthy.

℞—2 grains sulph. quinine every hour. Continue local treatment.

March 30th Pulse 100; temperature 104° 3′. Erysipelas over right side of face and forehead. Continue treatment.

March 31st. Pulse 90; temperature 100° 1′. Improving. Continue treatment.

April 3d. Pulse 78; temperature 96° 8′. Much better. Sulph. quinine, 2 grains every 3 hours.

April 5th. Pulse 84; temperature 99° 5′. No more erysipelas.

April 7th. Doing very well. Lanced abscess over eyelid.

April 8th: Improving. Appetite very good. Scalp adhering to bone.

April 20th. Improving rapidly.

May 8th. Leaves hospital to-day, though his wound is not entirely healed up, on account of the erysipelas which prevails here. He has now been without the distemper for over a month.

Duration 25 days.

Remarks.—By carefully reading this case one cannot fail to see the marked effects of quinine on the march of erysipelas, on the pulse, on the temperature, and the necessity of continuing the drug for several days after all traces of the exanthema have disappeared. If this precaution be not strictly adhered to, the eruption soon returns, as it did here on three different occasions. I look upon this case as a typical one, to prove the good results to be obtained from the administration of the kinick salts in this disease. The erysipelas is seen to disappear rapidly under the use of this drug, and reappear if it be too soon discontinued. The pulse and temperature are seen to fall under its use. This fall will be accounted for further on.

Case 11.—Idiopathic erysipelas of face.

William Bass, aet. 40 years, carpenter, Missourian, entered my ward on the 10th December, 1873. *Antecedents.*—Patient entered hospital for a stricture of urethra, for which external urethrotomy was performed on February 28th, 1874. On the 11th day after operation, whereas the wound of perineum was doing very well, with rosy granulations and healthy pus, as testified by my friend Dr. Castellanos, an idiopathic erysipelas of face broke out. This patient is of a tuberculous constitution.

March 10—*Present Condition.*—Pulse 120; temperature 102° 5′. Tongue dry. Did not sleep on account of pain in the face. Erysipelas over the left malar bone and the nose.

℞—2 grains quinine every hour.

March 11th. Pulse 108; temperature 102°. Tongue good. Bowels loose. Passed a restless night. Erysipelas disappearing on one side of the face, commencing on the other.

℞—Quin. sulph. ℈ijss. acidi sulph. q. s.; tinct. opii ʒij; aq. ℥xii. ℳ. S.—Tablespoonful every hour.

March 12th. Pulse 108; temperature 101° 3′. Tongue very good. Erysipelas disappearing. Continue medicine.

March 13th. Pulse 114, very small and weak; temperature 98° 2′; respiration 20, normal. Profuse hemorrhage from lungs yesterday at 4 p. m., for which 20 drops muriated tinct. of iron, every hour, was prescribed by the assistant house surgeon. The erysipelas has disappeared. Tongue moist and anæmic. Perineal wound doing well.

℞—20 drops Tinct. iron to-night ℞—Olei morrhuæ, tinct. cinch. comp. aa: tablespoonful 3 times daily.

March 14th. Pulse 116, weak, rapid, small; temperature 101° 8. Tongue good. Pains in both sides of face (probably a return of the erysipelas). Wound of perineum and scrotum does not look well. Stop medicines. Take 2 grains quinine every hour.

March 15th. In statu quo. Pulse 120; temperature 103° 2′. Continue quinine.

March 16th. Better. Pulse 104; temperature 100° 7′. ℞—Olei Morrhuæ, tinct. cinch. comp. aa. By tablespoonful 3 times daily. From this day no more fears of erysipelas.

Duration 4 days.

Remarks.—The appearance of an idiopathic erysipelas of the face, where not a scratch existed, in one with an extensive wound of perineum and scrotum (a portion of scrotum had gangrened away), shows that the cause of erysipelas is not dependent on

wounds. When it is associated with a wound it comes in as an epiphenomenon, and should be treated as such. It may be so serious as to become, for a time being, the principal affection, and in fact cause death, as was the case in the following observation.

Case 12.—Traumatic erysipelas of foot. Ataxic symptoms. Death. Necroscopy.

April 1st—*Antecedents.*—Charles Hoffmann, aet. 27 years, sailor, entered my ward, bed 14, March 24th, 1874. Eight days previous to admittance, cut his foot with an axe. Hemorrhage copious at time of accident. The dressing was removed for the first time the day after his admittance to the hospital. No union of wound, which was very dirty with blood and soot (the latter had been applied by the patient at time of accident). The incision was on the track of the dorsal artery of the foot, crossing it very obliquely at the first interosseous space where, at the metatarso-phalangeal articulation, the artery was cut. The pulsations which were distinctly felt at the cardiac end, were absent at the distal end. A strip of plaster was loosely placed across the injury, which was about 1½ inches in length, and its lips approximated. Forty-eight hours after this dressing, a secondary hemorrhage took place. I removed the dressings, which had been placed pro tempore by the student, and no hemorrhage followed. The pulsations at the cardiac end were visible, A bit of lint folded up, placed on the track of the artery, one inch above the cut and kept in place by a strip of sticking plaster, which encircled the foot, completely stopped the pulsations below. Forty-eight hours after, though the pressure above was still kept up, pulsations had returned below. Besides a small puffiness existed at the seat of injury. A stronger pressure on the dorsal artery diminished these pulsations, but did not stop them. It was evident that a communication existed at the distal end. This was with the plantar arch, through the communicating artery. A diffused traumatic aneurism between the latter and the dorsal arteries was forming. Pressure on both the posterior tibial and dorsal arteries, stopped the pulsations in the aneurismal sac. Compression was kept on the dorsal, and a strip of sticking plaster over the wound and aneurism. The latter being very small, might disappear by the cicatrisation of the former. Another hemorrhage took place on the following morning, March 30th. I

then ligated the dorsalis pedis, an inch above the seat of injury, scooped out the blood from the aneurismal sac, and, unable to ligate the communicating artery, which was imbedded in the first interosseous space, I made the acupressure with one of Dr. J. Y. Simpson's needles and according to his method. The bleeding was free and copious from the distal end, which necessitated the compression of the posterior tibial, by my assistant, Mr. Deslattes. Acupressure needle removed next morning. No hemorrhage. Patient anæmic.

Yesterday morning, March 31st, when I removed the acupressure needle, fever was quite high. I neglected to take the temperature and count the pulse, which previous to this little operation were normal. I did not then (yesterday) think that such a trifling operation could cause such a high traumatic fever. This morning the cause is known—erysipelas. He was delirious the latter part of the night. Took a dose of oil yesterday.

Present Condition.—Pulse 109; temperature 105° 4. Tongue pointed, moist and anæmic. Thirst great. Oil operated twice. At seat of injury commencing erysipelas, spreading in all directions. Wound looks bad. No hemorrhage. Local heat very high. No pains at either hepatic or other regions.

℞—Parts surrounded by collodion, to prevent, if possible, the exanthema from spreading. 2 grains sulph. quinine every hour. Tinct. arnica and water aa, loco-dolenti.

April 2d. Pulse 110; temperature 106°5′. Tongue moist and anæmic. Horripilations. Bowels loose. Thirst very great. Erysipelas disappearing. ℞—Continue medicine. ⅛ grain sulph. morphine every 3 hours.

April 3d. Pulse 108, small and weak; temperature 104°3. Tongue pointed and very dirty. Bowels checked. Wound looks healthier. Eryripelas almost gone. Delirium during night.

℞—Continue quinine. Ferrated comp. tinct. cinch. by tablespoonful every hour.

April 4th. Pulse 144, weak, rapid; temperature 105°4′; respiration 60. Tongue furred, white, and dry. Hiccough since yesterday. Delirium. Wound dry. Erysipelas gone. No operation from bowels since yesterday morning. Micturates freely. ℞—Ammoniæ carb. ʒj, potassii brom. ℥ss, aquæ ℥vj. ℳ. S. Tablespoonful every hour. ℞—Salt and water enema at once. Water and tinct. arnica to wound.

Evening. In statu quo. Carphologia. ℞—½ grain sulph.

morphine every three hours, to procure sleep. 20 grains carb. ammonia every three hours. Salt and water enema at once.

April 5th. Died at 4 a.m., quietly.

Duration of erysipelas 5 days.

Necroscopy.—6 hours after death. Body anæmic; muscles well developed, exsanguineous. *Cranial Cavity.*—Except a slight congestion of the blood-vessels, and especially those of the pia mater, the contents of the skull were normal. *Thorax.—Lungs*, two or three small pleuritic adhesions; otherwise perfectly healthy. *Heart*, normal; contained fibrinous (yellow) heart clots in all its cavities. Two typical "*plaques laiteuses*," about size of a 25 cent piece, on its outer surface, traces of former pericarditis. *Pericardium*, healthy. *Abdomen.—Liver*, seemed normal. *Gall Bladder*, contracted down; contained but a small quantity of dark green bile. *Spleen*, slightly hypertrophied. *Stomach and Bowels*, were not examined, as they looked healthy, the latter filled with gases. *Pancreas*, normal. *Kidneys*, seemed hypertrophied; 3 or 4 sub-serous ecchymoess; otherwise healthy.

Résume.—Incised wound of the foot. Sudden elevation of temperature. Erysipelas. Looseness of bowels. Ataxia. Delirium. Hiccough. Death. Treatment for erysipelas: 2 grains sulph. quinine every hour; collodion and water and tinct. arnica l.d.; carb. ammonia; brom. potassium and sulph. morphine. Enemas p. r. n. For injury: Purgatives p. r. n.; compression, ligature and acupressure of vessels; co-aptation of lips of wound. Post-mortem: negative.

Remarks.—The sudden rise of the temperature in this case, reaching 105°4′ in 24 hours, was by itself sufficient to make the diagnosis, inasmuch that erysipelas was in the ward. At the same time the wound assumed an unhealthy appearance, with a very high local heat. Twenty-four hours after the first appearance of the eruption it faded, while a looseness of the bowels augured bad. This last symptom having been checked, was followed by adynamic symptoms, hiccough, and a suppression of the exanthema, which rendered the case a desperate one. These symptoms soon increased, and death, preceded by complete delirium and carphologia, followed before the completion of the sixth day.

This sudden disappearance of erysipelas, with such alarming brain symptoms, is what is sometimes called a metastasis or translation of the disease to the brain. Besides, that these

words do not give us a clue to the manner in which this sudden change takes place, we have seen that the results of the post mortem have been most unsatisfactory. It is certainly not a very slight congestion of the vessels of the brain, and especially those of the pia-mater, nor the heart clots, that have caused death. To be candid, let us admit that, once more, death has occurred from erysipelas, with our inability to account for it, and without any satisfactory result from the necroscopy.

The treatment has been the same in this case as in the preceding ones, which were all cured. Quinine formed the base of it, being given in small and repeated doses. This drug has been given in this disease for years back, with different explanations. It certainly seems to be of all drugs the one that has been the most patronized.

Deductions from the Preceding Observations.

If quinine has not the power of checking the march of erysipelas, or of abridging its duration, it certainly seems to be capable of mitigating its symptoms. I am not prepared to say positively that it does abridge, and in some cases cut short the disease, but observations do lead to this conclusion. Dr. Perroud* asserts that it does. He reports five cases in which the exanthema lasted from four to eight days after the administration of the drug. But at the time he made his experiments the disease did not reign epidemically in his wards, as it did in mine. For five months I have had the distemper reigning epidemically in my wards, so much so, that on many occasions, when practicable, I dared not use the knife. For three months previously pyæmia was of frequent occurrence.

I am satisfied that quinine is powerful for good in this disease, and will continue to believe it until proved otherwise. I am always ready to change or alter my opinions according to *facts*, the result of strict and impartial observation at the bedside. I hope that those of my confrères who may feel interested in this subject will pursue its study further, and permit us to hear from them.

When we think how serious and deadly this affection is when it becomes epidemic, and how powerless then is our therapeutics, we should try by all means to find a powerful treatment against

* *Loc., cit.*

its ravages. See how lethiferous was the epidemic which raged in Louisville and its hospitals in 1844–5–6.

All my cases but one have shown mild symptoms, in spite of the bad state of health of some of the patients, and the diatheses under which they labored—alcoholism, tuberculosis, scrofula, syphilis, etc.

As stated previously, erysipelas prevailed throughout the winter, in one or more wards of the hospital. Some of my colleagues have had to contend with deep seated abscesses and other alarming symptoms. But for Case 12, which terminated fatally, I would have been able to advance the probability that, with the quinine treatment erysipelas will not terminate fatally. But that treatment is the best which gives the best results. The future will decide. For the present, we have good grounds to try this modus curandi.

If we consult the works of the different authors, old and recent, on the treatment of this disease, we see almost every one, if not all, for one reason or another, recommend the use of quinine, without (except Dr. Perroud) stating what its modus operandi is.

It will be seen that I gave to my patients 2 grains, sometimes less, of quinine every hour or two, fed them ad libitum on the best diet, and yet cured them with little or no serious symptoms. What a difference with the views of the old school, and of those who practised some 30 years ago, when the mildest treatment was a blood-letting of some sixteen ounces or more, a powerful purge, and a strict diet! "And yet," says an author, "*the patients would die in spite of our doings, although we might have bled* 2, 3, and 4 *times.*" Were our predecessors to rise from their graves, they, like Rip Van Winkle, would be astonished at the change that has taken place, and wonder at the sight of their lancets rusted from disuse.

Action of Peruvian Bark on the Nervous System—Mr. Briquet, one of the many who have made a special study of the action of Peruvian bark, asserts and proves, from the many experiments he has made on animals, and the numerous observations gathered from both healthy and sick persons, that "the alkaloids of bark have a direct and almost instantaneous action on the cerebro-spinal axis."

He divides his experiments into two periods, according to the doses given, and thus enables us to account for the apparently contradictory results obtained daily by practitioners.

In the first period, if the salt be given in small doses, the cerebral functions are excited; in the second, if these doses be kept up and increased, they have a sedative effect. From his experiments on animals, he proves that the period of excitement is in direct ratio to the rapidity with which the quinine salt has reached the brain. The excitement is almost always of short duration.

The second or sedative period, on the contrary, acts in direct ratio to the slowness with which the drug reaches the brain, as, for instance, when it is given by the stomach progressively and gradually. In this case, the sedative period is as long as the absorption is slow.

Observation has proven, that the effects are the same on man as those obtained by experimenting on animals. Quinine given to man, in small doses, produces an excitement of short duration; but when a large quantity is given, in small and repeated doses, it has a sedative effect of long duration.

Eulenburg has made some very interesting experiments with sulphate quinine. His object was to find out what portion of the cerebro-spinal axis was first acted upon by the drug (Archiv. fur Anatomie, etc., 1865, iv., p. 423). He considers himself authorized to conclude that, sulphate of quinine first acts on the spinal marrow, where it paralyzes the centres of reflex action. Secondly on the brain, where the centres of sensibility and voluntary movements are paralyzed. He comes to this conclusion from the following fact: A frog, poisoned by sulphate of quinine, is, in the course of 10 or 20 minutes, insensible to physical and mechanical excitements. It still, however, possesses sensibility and motility, for if the animal be placed on its back, it will attempt to turn over on its belly. Quinine seems to have no action on the nerves, but if applied to a freshly cut muscular surface, it causes a contraction, whereas if the muscle be plunged in a solution of sulphate of quinine, it soon loses its contractility.

Action of Peruvian Bark on the Circulatory System.—Here again the action of bark is twofold and in apparent contradiction. Quinine in small doses and long intervals (3 to 6 grains in several doses) gives energy to the beatings of the heart, increases its force and the frequency of the pulse.

But given in large doses and progressively (i. e., from 20 to 60 grains in the 24 hours), quinine has a marked sedative effect on

the cardio-vascular system, as shown by a marked slowness and weakness in the beatings of the heart and pulse.

Besides this hyposthenic action on the circular system, quinine in large doses also possesses a depressive influence on calorification. While it diminishes the force and frequency of the pulse, it lowers to a marked degree the temperature of the skin. According to Dr. Briquet, this refrigeration is the direct result of the abatement of the circulation, and is always in proportion to it.

Such are the effects which I have obtained from the administration of quinine. I gave it as soon as the first febrile symptoms showed themselves, and kept up its use during the febrile stage. By its continued administration, the pulse and temperature were often seen to fall considerably, and rise again if the drug was too soon discontinued.

Quinine as a Prophylactic of Erysipelas.—The good results which I obtained from this drug in the treatment of erysipelas, the explanations set forth, as its modus operandi, led me to try it as a prophylactic. From the following facts I think we can consider it, until otherwise proved, as a preventative of that erysipelas which so often complicates wounds.

"When the disease is epidemic," says Dr. Gross, "it often shows itself within a very short time after the receipt of an injury, however slight or insignificant. Under such circumstances, indeed, I have, as already stated, known it to follow upon the most trivial wound,....."

I mentioned above, that the distemper raged in my wards for five months, during which time I dared not operate.

Case 3 shows that the patient was operated upon on the 19th, and on the morning of the 21st he had erysipelas at seat of injury. On that day, the 21st, I had to operate upon a patient for a fistule of the left nates. I had then three cases of the exanthema in the adjoining beds. It was then that I first prescribed quinine as a prophylactic, and with the following result:

Joseph Sanders, aet. 47 years, Canadian, laborer, entered my ward April 20th, 1874. Discharged, cured, May 8th, 1874. Diagnosis—Fistule of left nates. Treatment—Incision of fistule on April 21st; quinine as a prophylactic (2 grains every 2 hours, then 3 times daily), there being erysipelas in the ward.

Duration 17 days.

He was therefore 17 days in an infected locality; his wound

progressed satisfactorily, and he was discharged cured without having had the distemper.

From this day I gave quinine, in 2 grains doses 3 times daily, to all the patients upon whom I operated, and have not had any new cases of erysipelas among them. One patient (vide Case 7), to whom I did not give it, not thinking it necessary, was taken sick with the disease on May 1st, 1874.

It is to be hoped that future observations will prove quinine to be a prophylactic against erysipelas.

Before dropping my pen, I wish here to return my thanks to my friend, Mr. J. L. Deslattes, Resident Student Charity Hospital, for the valuable services he has rendered me, and for the promptness and accuracy he has always shown in observing my patients during my absence and in gathering information.

28 Conti Street, New Orleans, July, 1874.

ARTICLE II. *On Trismus Neonatorum.* By Dr. E. GOLDMANN, of Galveston, Texas.

The treatment of this disease by Professor Widerhofer, of Vienna (whom doubtless many of my readers remember, as I do, for his kindness, urbanity, and principally for his excellent clinical instructions), has also proved successful in the case I am about to give in its details. In the main, the hydrate of chloral was the remedy relied upon; but, many eventualities intercurring, other auxiliaries were gradually brought into the contest which was going on for the life of our little patient, who, I am glad to say, is at present out of danger from the terrible disease by which he was assailed.

Before I relate the particulars, I deem it worth mentioning, that I have seen many cases of this fatal disease before, in which no lesion of any kind could be traced to the navel, as some authors on "Diseases of Children" maintain to be invariably the case. It is natural with the physician, first before all, to examine the navel of the little patient, in order to find some visible cause for such a serious disorder. Although in a small minority of the cases that were under my treatment, ulceration of the navel did exist, on the other hand, I have seen a number of children with navels ulcerating for weeks, with considerable tumefaction and

redness around, and yet without tetanus intervening. Nor can I join in such vague but convenient phrases as "*a poison in the blood*," or its being "*epidemical at certain seasons in different parts of the world*." It would appear more progressive, to me, to seek the cause in certain atmospheric changes, such as increased moisture, sudden lowering of temperature, violent fluctuations of electric currents, which may bring forth, as a consequence, that children born almost within the same day and exposed to the same influences, should be attacked within the same time and by the same disease. It is certainly remarkable that the disease is comparatively rare in districts situated high and far from the sea shore, although I do not mean to say that they enjoy any immunity from it. But the fact will scarcely be contested, that the proximity of the sea is one of the predisposing causes, since the largest number of children succumb to this disease in countries, and principally on islands, which are exposed to the strong and moist currents of air prevailing on the ocean and its inlets. In the case before us the cord fell off on the 3d day; there was not even the slightest excoriation; the child was as healthy and strong a little boy as could well be seen. The parents are in easy circumstances; the residence was comfortable, well ventilated, and high above the ground; the care bestowed upon the child was almost overweening, and the nurses thoroughly competent. The birth of the child took place on the 12th of August, during which month an excessive heat prevailed, the more intolerable for the absence of the southern breeze which, in other summers that I have passed here, made the city the most pleasant residence one could desire during the hot season. The 5th and 6th days after the birth of the child were particularly warm, sultry and close, so that, in anticipation of a thunderstorm, I went to the house and gave the direction, if such should take place, to shut the windows of the hall which communicated with the room of the mother and child. During the evening the expected storm, with a heavy rain, came on suddenly. No one was there to shut the hall windows immediately, and thus the atmospheric changes, which took place quickly and violently, could make their effect felt in the room of the child. During the night following the child showed the first signs of distress; it moaned, cried, and took the breast with evident difficulty. The parents being of the Israelitish faith, the little boy was circumcised the next day. During that day and

the following night the described symptoms increased rapidly, and early the next morning, when I saw the child, I was dismayed by finding a fully established case of trismus before me. A few days after, I heard of one more case of trismus which had manifested itself about the same day and terminated fatally within 24 hours. Here then, we have two children exposed to the same causes, namely, increased moisture, sudden lowering of temperature, and violent fluctuations of electric currents, and affected in the same manner. The question naturally arises, why not as well that a simple coryza should ensue? This I can not answer. But I can understand that outward influences upon the surface of a new-born babe with only a very thin epithelial covering of the skin, so that the papillæ are almost unprotected, may act as an irritant to the endings of the nerves of the cutis, in the same manner as a traumatic injury may do to one or more filaments. I dwell upon this point, not with the intention of advancing a theory (because the view is neither new nor original), but merely to express my conviction that neither physician nor midwife, can be held responsible for such occurrences, as is often and unjustly done by an ignorant public who have been taught to consider the navel the source of all mishaps of this kind. I have already mentioned how, and when, the first signs of trismus made their appearance. The circumcision certainly did not improve matters, although there is no doubt in my mind that it was NOT the original cause. Nor is the disease particularly frequent among oriental Semitic nations, who hold circumcision as their highest consecration. That it may lead to it can not be doubted however, since the operation is a traumatic injury of a serious character. But in the case before us, the first symptoms appeared BEFORE it was performed. Nevertheless, I should consider it at this place, a good precaution to have it deferred to a later day than the 8th, when the danger of trismus is still impending, even without that operation, and I shall in the future insist upon its postponement wherever I can make my advice prevail. In the above I have given already the history of the case as it developed itself, and I shall now describe the treatment we followed. In the management of the case I was associated with my excellent and accomplished confrere, Dr. Randall, of this city. We began with doses of half a grain of hydrate of chloral, given at first every hour until the tetanic spasms diminished in frequency. After several doses had been

administered, the child became quiet and fell asleep. We then directed the same dose to be given every time the child awoke from its slumber. Our directions for the remainder were—not to touch the child, unless it was absolutely necessary; to avoid all loud talking, slamming of doors, or even fanning, although the little sufferer perspired profusely sometimes; to give three times daily an enema to empty the bowels; to draw the milk from the breast of the mother by means of the breast-pump, and to give it by a teaspoon, or a nursing bottle with an India rubber nipple. This was at first not easily performed, but after a day of quiet had passed, it offered less difficulty, and the child swallowed eagerly, though slowly, the small quantity of nourishment thus allotted to him. Our direction to give directly on awaking a dose of the medicine was strictly followed, and, in this manner the spasms were kept away, although not completely. The greatest difficulty attended the efforts for evacuation of the bowels, which could hardly be moved. We used rhubarb, but it failed to act, and it appeared that, while the tonic spasms of the voluntary muscles could be nearly controlled by the chloral, the muscular fibres of the intestines did not yield to its influence. We consequently gave one grain of calomel in a little syrup (on the 3d day), and, as the spasms returned repeatedly at night, increased the dose of the chloral to one grain. We were prompted to do this because we observed considerable tenesmus, which made matters still more distressing for the mother, to whom we represented the sickness as not so serious, in order to keep her mind easy, and thus to avoid having the nourishment the child had to rely on, impaired. The 6th night vomiting ensued, together with spasms, and recurred several times. Having been called, we ordered the solution of the chloral, 1½ grains pro dosi, per anum, (which we continued up to the termination of the case). Also ordered the breast of the mother, who had been very much chagrined the day preceding, to be emptied by the breast pump and the milk to be thrown away; the child to have as nourishment a thin decoction of arrow-root and, from time to time, a teaspoonful of fresh rain water. The vomiting ceased, and the fresh milk from the mother's breast was again borne well the next day. The spasms did not return on the 7th and 8th days, but again we observed tenesmus, and therefore administered on that and the next day one grain of calomel in the morning. The 9th day a slight spasm took place, and the same thing on the 10th. The

11th and 12th days brought nothing remarkable, the spasms not having returned. Yet the child could not open the mouth; there was no opisthotonos, yet the flexor muscles had gained such a preponderance over the extensors, that the little hands still remained firmly clenched, although sometimes they could be opened without any trouble. The injections of the chloral were given all this time, at least four times in twenty-four hours. On the 13th day the child gaped several times, opening the mouth wide and without giving any signs of pain, to the delight of the parents. We thereon directed the injections of chloral to be reduced to two within twenty-four hours, and gave by the mouth some rhubarb and magnesia, which now acted perfectly. On the 15th day considerable mucus appeared in the passages from the bowels, and once even a drop of blood with it. We ordered to stop the chloral injections, and substituted those of boiled starch with ¼ grain of acetate of lead and ½ drop of laudanum Sydenhami. The mucous discharges not ceasing the next day (the 16th), we had the same injections continued, and by this means soon overcame that trouble. On the 18th day the child took the breast for the first time again. It was however, by means of unyielding perseverance only that this was effected. It seems that the child had, so to speak, forgotten the mechanism of sucking, and only after many attempts he managed to do it again. The bowels moving still sluggishly, 3 grains of Hufland's powder were given every day. Even this could be left off on the 20th day, when the passages came regularly several times daily, as with every healthy child it ought to be.

What I look upon as particularly important in this case, is the benefit we derived from the use of the calomel, which produced liquid passages, and thus relieved, and afterwards prevented, the tenesmus that always gave rise to tetanic spasms. The chloral hydrate alone was not adequate to effect both. As for the use of the chloral by injections, it is in my opinion even preferable to the use by the mouth, inasmuch as the difficulty of swallowing is often not to be overcome, while the enemata can be made small and very gradually introduced into the rectum. Their long continued use brought on some irritation of the lower intestine, which, however, was not of a formidable character. But it may not in all cases be necessary to continue the enemata as long as we had to do it this time. In all probability the use of chloral by the mouth would have had to be abandoned,

because the child had repeatedly vomited the remedy, and the little nourishment it could take, and it was all-important that the latter was given as far as could be done. As stated before, the little patient is now well and under the care of his nurses. It will be seen from the above, that on the 10th day the last (and slight) spasm occurred; that after that time none returned, and that on the 13th day the trismus was passed. Professor Widerhofer has certainly the merit to have diminished the terrors of this fearful disease.

ARTICLE III. *On Placenta Prævia.* By Dr. E. GOLDMANN, of Galveston, Texas.

It is not my intention to write a treatise on the above subject, but simply to report a case which is interesting in so far as the same complication occurred with the same woman TWICE WITHIN TWELVE MONTHS. The lady who had to undergo these severe trials, I may as well state here, is at this moment enjoying good health, and is fully able to attend to her household duties. The hemorrhage was considerable each time, notwithstanding my strenuous efforts to limit it; but the first time it had lasted long enough to create serious alarm before my assistance was called for, while at the second time I managed the case from its beginning, and therefore, could better control it. The first time the placenta prævia was lateral, the second time it was central. This explains to me the earlier appearance of hæmorrhage in the second pregnancy; since, although it is of comparatively rare occurrence that such takes place before the termination of the sixth month, it was in this instance at the end of the fourth, when it first appeared in quantity so insignificant however, as to exclude any immediate interference. Professor Naegele, of Heidelberg, published at one time, a number of cases of this abnormity, and expressed the conviction, that the most frequent, and the principal cause of placenta prævia, was an enlargement of the cavity of the uterus, in consequence of frequent child-bearing. The views of this distinguished obstetrician find a further proof of their correctness in the case before us, the lady who was under my treatment being the mother of nine living children. I shall give the particulars in the following lines:

Mrs. —— is now 38 years of age, healthy, although not robust;

has never miscarried; her confinements were generally easy, and passed without dangerous complications. An accomplished and refined lady, she has lately been obliged to task herself more by work than in former times, but, according to her own statement, without any bad effect on her constitution. She was pregnant for six months when she was alarmed, while working in the kitchen, by a slight hemorrhage. The midwife, who had repeatedly assisted her in her confinements, was consulted and gave the assurance that this happened sometimes in pregnancy, and thus quieted all apprehensions. The hemorrhage occurring from time to time, the husband, after some time spent in arguments succeeded in convincing his wife that the intervention of a medical man was necessary. Meanwhile, the beginning of the eighth month was arrived at, and the losses of blood had increased to an alarming extent. Being consulted, I found, on examining, the os uteri very soft, its anterior lips swelled; the finger could easily penetrate into the canal far enough to feel the cotyledons of the placenta. The head of the fœtus could be felt through the posterior laquear vaginæ. Blood was oozing all the time, but sometimes a clot was expelled, followed by a gush. The pulse of the patient was weak, quick and fluttering; the patient felt depressed and faint; the sounds of the fœtal heart weak and scarcely audible. Of course I followed the first indication—to stop the loss of blood. At the same time, considering the state of the woman, the propriety suggested itself to me to produce, if necessary, a premature confinement. To meet the first requirement I followed Scanzoni's method, and tampooned in the following manner. I had a little bag sewed, which fitted exactly over a common glass speculum, slipped it over it, and, after anointing it, introduced it into the vagina. This done, I filled the speculum with charpie, part of which I had dipped in a diluted solution of perchloride of iron, then keeping the bag and contents "in situ" by means of the handle of a probang, I gradually withdrew the speculum, and thus effectually stopped the loss of blood for the time being. After the lapse of eight hours I took out, by small portions, and with the utmost care, the charpie and the bag which contained it. Some blood again came slowly oozing. I introduced my finger to ascertain how matters stood. The os was not dilated, but I found that dilatation could be easily produced. About three hours later flooding set in again, and I then resolved not to delay any longer, but to deliver the woman

at once. I consequently put her under the influence of chloroform; introduced my hand; gradually dilated the os; passed the placenta where it was not any longer adherent; entered the cavity of the uterus; broke the membranes; turned and brought down both feet through the introitus vaginæ. The hemorrhage now ceased, the pelvis of the child acting as a tampon. Labor pains soon commenced, partly produced by frequently rubbing the fundus uteri, partly by often repeated cold applications to the lower abdomen. After one hour more, the birth of a dead child was accomplished, the heart had some time previously ceased to pulsate. My efforts, therefore, were chiefly to be directed towards stopping the bleeding from the open surface, from which a part of the placenta was detached. Fortunately, this was easily managed by injections of icewater and the inward administration of ergotine, with ratanhia and diluted sulphuric acid. The placenta, after some time, was expelled. The uterus contracted well, and without any further complications the woman gradually recovered, so that she could leave the bed for an hour on the 15th day after the confinement.

Evidently the considerable loss of blood in the case left no alternative but the "accouchment forcé." There was no hope of preserving the lives of either the mother or child by procrastination, while every probability pointed in the direction of the loss of both by temporizing. In my opinion, the life of the child could only be saved, if such had been possible, by this means; because the insufficient nutrition "in utero," from a weak current of blood through a placenta which was to a great extent not any longer in communication with the uterine vessels, must have produced anæmia and death. Such has been undoubtedly the cause of it in this case, the sounds of the fœtal heart having been very weak as early as 12 hours before the confinement. This view is confirmed by the sequel, which I shall describe also, in order to give a complete review of the case.

About 10½ months after the above occurrence I was again called to the same lady, who was pregnant about four months. She stated to me that almost every two or three days she had a discharge of a watery liquid, I enjoined quiet and rest upon her, and called after a week again. The watery discharges had repeated themselves at almost regular intervals of two days, and, upon my calling her attention to it, she stated that she felt every time a slight pain, such as she experienced sometimes during

her catemenia, after which with one gush they terminated. Evidently the uterus contracted, although slightly. But little blood was mixed with the liquid thus eliminated. Matters went on in this way for one month and a half without any interference on my part, except the administration of tonics and a generous diet, in order to compensate for the loss thus occasioned to the economy of the system, when one night I was called on account of a sudden and considerable hæmorrhage, which was still going on when I arrived at the bedside. At the same time the woman suffered severe pains—according to her expression—worse than any labor she had as yet passed through. She was moreover desponding, and gave vent to her gloomy forebodings, by telling me she could not survive this, a state of mind we have the most ample reasons to dread with women in this condition. On examining, I found the os soft and swelled in its entire circumference, admitting with ease the examining index finger, which, this time again, felt the uneven cotyledons of the placenta. An attempt to tampoon I had soon to abandon, on account of the pains, which increased all the time. In fact, I did not think it feasible at this juncture, yet I wished to do something to control the loss of blood, which increased with the violence of the pains and as the os gradually dilated. By this time the reader undoubtedly has arrived at the conclusion that abortion was inevitable, and thus I viewed it, especially since I could not hear any sounds of the fœtal heart, although I sought for them most assiduously. I therefore administered chloroform to spare the sufferings to my patient, placed her pelvis as high as could be done, and injected from time to time some matico docoction, which I have found repeatedly a most excellent hæmostatic in similar cases. After the lapse of one hour I found the os so far dilated that I could attempt to pass my hand. I then found the placenta adhering all around, but easily separated from its place of attachment, and as my fingers gradually groped their way into the cavity of the uterus they met, to my great relief, the feet of the fœtus. The labor pains forced them downwards, almost without any exertion on my part being required, and in a short time the fœtus was delivered. The hæmorrhage which followed I had to combat in every possible way by the application of cold, this being almost all I had at my command at that time. I injected icewater in one of the viens of the umbilical cord; had some ice crushed by a hatchet in rough towel, which I introduced into

the vagina, and made cold applications on the region of the uterus. The pains, which had intermitted for a few but LONG minutes, now returned, and finally accomplished the expulsion of the placenta. During the intervals of the pains, however, the uterus would relax to such a degree, and once commenced to enlarge in such a manner, that the danger of hæmorrhage into its cavity became imminent. I counteracted this by holding the organ with my two hands together, thus preventing its distention. The fœtus was very small, thin, and badly nourished. It was evidently dead for some time, its life having in all probability been terminated by, and in consequence of, the insufficient current of blood, which was supplied through a placenta partly not in communication with the uterine vessels.

What I find difficult to explain is, how the constant hydrorhœa could occur without leading sooner to the catastrophe that it really did produce. The distention of the uterus by liquid; its contraction in order to eliminate it, and the reflex action thus setting in, should lead us to think that at an earlier date abortus might have taken place. I expressed my apprehensions of such a result (to the husband of the lady) after the first several times the hydrorhœa had occurred. Kiwisch, however, in his work mentions several cases of this kind in some of which the fœtus even reached maturity. I have myself seen cases resembling the above, but in all these it was a flow of *thin mucus from the vagina*, while in the one before us it was a liquid resembling the liquor amnii, and evidently *a secretion of the mucous membrane of the uterus.* As I stated previously, the lady is at this hour enjoying her usual good health.

ARTICLE IV. *Direct Local Medication in the Treatment of Chronic Catarrhal Inflammation of the Nasal and Pharyngo-Nasal Cavities.* BY THOMAS F. RUMBOLD, M.D., St. Louis.

[Reprinted from the Transactions of the Eighth Annual Meeting of the Medical Association of Missouri, April 21st, 1871.]

There are no cavities in the human body, whose inspection requires reflectors, that are more easily examined than the nasal and pharyngo-nasal; at the same time, none are so difficult to cleanse and make applications to, without doing injury equal to any benefits intended. The importance of thoroughly freeing the surfaces of the extraneous products of inflammation, is quite

equal to the therapeutic effects of the remedial agents employed. Repeated observations during the last few years, have shown that if the surfaces are maintained *clear* of irritating secretions, a large number of patients will grow well without other treatment. This is true, especially of the young. Again, experience has taught another equally important lesson, i. e., whatever method for attaining this result is chosen, it should cause but very slight irritation, not lasting beyond a few seconds.

The instruments adapted for accomplishing this result in the mildest and most complete manner, are Spray Producers. They blow the muco-purulent secretions away from their lodging places, and force the medicated solution into every irregularity—into places that cannot possibly be reached by the brush or sponge; or even viewed during life.

The spray producers that I use for this purpose are made of glass. The tubes are obtained from the glass-blower with the reservoirs already blown on them. I form the points and bend the tubes to their proper shape over a small alcohol lamp or gas-light. The two tubes are fastened together by first heating them, and then placing a small strip of gutta-percha between them. A short experience will enable any one having a little "knack" to form and bend the points for either a fine or coarse spray.

As the spray-producer can throw a stream in one direction only, acting upon a surface of about three-fourths of an inch in diameter, and as the avenues for applications to these extensive surfaces are the narrow space behind the sensitive soft palate, and the anterior nares, therefore, a number of nebulizers, whose points are so constructed as to throw streams in the various directions that are requisite to cleanse and medicate every portion of the diseased surfaces, are indispensable. My experience has taught me that five directions (Fig. 1, Nos. 1 to 5 inclusive)

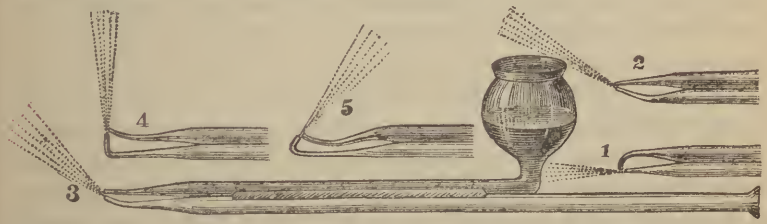

Fig. 1.—Spray Producers for the Pharynx, the Pharyngo-Nasal and Nasal Cavities (reduced one-half).

No. 1—Throws a spray directly forward into the mouth. No. 2—into the anterior nares. No. 3—upon the posterior wall of the pharyngo-nasal cavity. No. 4—the superior wall of same. No 5—into posterior nares.

are necessary to effect this, viz.: One throwing a horizontal stream (No. 1), washing the anterior portion of the soft palate, the tonsils, and by elevation of the outer extremity, the posterior wall of the pharynx, from the third cervical vertebra, upward to the middle of the second; one throwing a stream at an elevation of forty-five degrees (No. 3), directing the spray against that portion of the posterior wall covering the second and first cervical vertebræ; one throwing its stream vertically (No. 4), reaching the superior wall of the pharyngo-nasal cavity; one reaching through the posterior nasal openings, throwing a stream upward and forward, at an angle of forty-five degrees (No. 5), washing the under surfaces, borders and inner sides of the superior, middle and inferior turbinated processes, the point of the instrument should be placed a little behind the lower border of the soft palate, alternately upon the right and left side of the uvula; and still one other, intermediate in direction between the first and second named (No. 2), is introduced into the anterior nares, and by elevation and depression of the outer extremity, made to wash and medicate the remaining portions of the nasal passages; and the whole superior surface of each side back to the first cervical vertebra. Slight rotation of the instruments will be required to treat the lateral walls. Air forced by India rubber bulbs is the mechanical means that produces the spray.

I have employed this system daily since 1866, and consider it greatly superior to using either the brush or sponges, except for treating phagedæna, and then only when stronger medications are required than to the general surface.

When the muco-purulent secretions have formed incrustations which are closely adherent, the spray producers have not sufficient force to remove them; this may be accomplished by the posterior nares syringe, but the objection to this instrument is, that however carefully handled, it causes by its introduction and force of the current, sensations and occasional injuries which the patient is unwilling to submit to. The instrument that is almost universally employed in such cases is the Weber Nasal Douche (improperly called Thudichums*), but from the lack of the adaptation of this method to accomplish the result intended, it must very frequently fail. That this mode of cleansing is beneficial, either when the incrustation is wholly situate low enough for the medicated stream to wash it, which is very seldom, or

* See Roosa on the Ear, p. 292.

when the secretions are so abundant as to extend or flow to the inferior portion of the cavity, is not doubted; but it is needless to say that it is of no benefit when the diseased surfaces are beyond its reach.

A careful examination of the location incrusted, and the surfaces irrigated by this douche, will show that it *must fail* in a very large majority of cases, and that it is the most unphilo-

Fig. 2—Antero-posterior section of the face and head. *a*, inferior turbinated process. *b*, middle turbinated process. *c*, superior turbinated process. *d d*, location of incrustations to be removed. *e*, line showing the height of the water in the nostril irrigated by the Weber douche. "Dotted line" indicates the position of the septum nasi, the turning point of the liquid in leaving the other nostril.

sophical application that has ever become popular with the profession. When the head is inclined, as necessary in the use of the Weber douche (fig. 2), and the rubber tube inserted into the nasal opening of the side affected, *the fluid can only rise to the lower border of the posterior nasal opening of that side* (line *e*), *and this is equal in height to only one-half of the nasal fossa*—THE HALF SELDOM AFFECTED, OR REQUIRING TREATMENT.*

In the other nasal passage the floor only is washed by the escaping liquid, the elevation of the soft palate against the posterior wall of the pharynx will neither alter the turning point of the liquid, nor raise it much higher in the cavity, as closure only prevents it from going down into the throat. The advocates of this douche may say that sufficient force can be employed to throw the liquid up to the proper place, but such advice will only be given by the inexperienced, as this procedure would occasion irritation, muscular contraction, and lowering of the soft palate, with escape of the liquid into the larynx. According to

* In this connection I would add, that the upper half of each nasal cavity is oftener the location of diseased action, both because of greater delicacy of structure, and, that its posterior upper surface receives *directly* the currents of air during respiration.

St. John Roosa and L. Turnbull, the fluid may enter the middle ear by the Eustachian tube, and induce severe inflammation. When masses of fetid matter are loosened by this procedure, it is an evidence that the secretions were so abundant as to fall within reach of the irrigating fluid, or it may be that the irritation of the medicated liquid excited a greater flow of mucus in the whole cavity, thus detaching a portion of the scab, having an effect similar to an irritating powder, when snuffed into the nostrils. If the practitioner will carefully examine a case of this description, after this apparent thorough cleansing, he will find in the superior portion of the passages a large quantity of the purulent secretion remaining, which can not be reached by this means.

During four and a half years I have used an apparatus called by me the Catheter Nasal Douche, which is so constructed that it will throw a shower or coarse spray from the floor of the nostril upward, reaching every portion of the irregular surface of these passages. The only sensation occasioned by its use with warm salt water, is that of tickling, never objected to by the patient. It is a flask-shaped bottle (*a*), holding about one and a half pints.

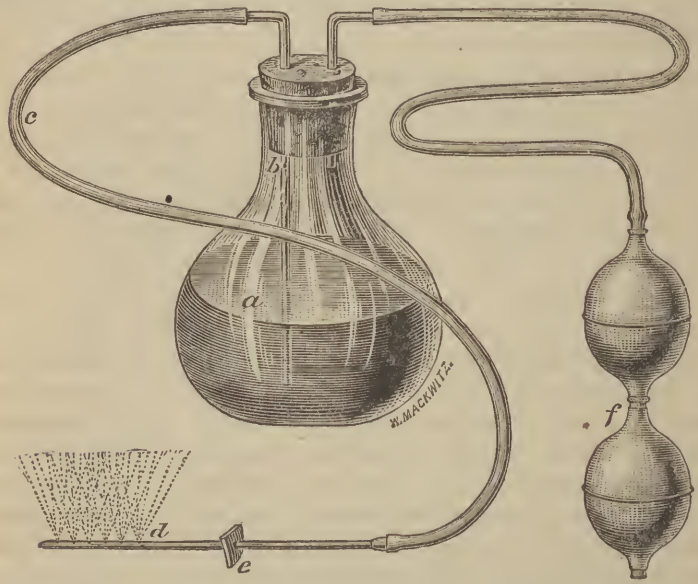

Fig. 3.—Catheter Nasal Douche (reduced to one-third). *a*, reservoir; *b*, metal tube for the passage of the fluid, having a small apertnre in its side for the entrance of air; *c*, rubber hose: *e*, catheter; *d*, foramina for escape of coarse spray.

Into the rubber stopper is inserted two metal tubes whose outer extremities are bent at right angles, and turned in opposite directions. One of these only pierces the stopper, and has attached to it the India rubber bulbs (*f*); the other almost reaches the bottom of the vessel (*b*), and has attached to its outer extremity a rubber hose (*c*); and to the hose is fastened a No. 4 catheter (*e*), at the further end of which are made five small openings (*d*), on a line with its axis. The free extremity of the catheter is closed. The tube whose lower extremity dips into the medicated fluid, has a small aperture in its side, just under the rubber cork (*b*);* this is to allow air to enter during the passage of the liquid, the effect of which is, causing the tube to contain beads of air and fluid in close succession, so that when escaping from the small openings in the catheter, it will resemble a coarse spray. The catheter is introduced into the affected nostril, and the stream directed upward. A slight rotation on its exis will cause this spattering current of fluid and air to *wash* and *blow* the secretions away from their lodging places, in a milder and yet more efficient manner than a steady flow of liquid.

The cleansing process may be greatly assisted by the patient closing the nostril that is not treated, and then giving a quick and forcible blow out of the one being washed, sending the liquid and everything loose, out with great force.

1205 Washington Avenue.

ARTICLE V. *On Nélaton's Method of Resuscitation from Chloroform Narcosis.* By J. MARION SIMS, M.D., Surgeon to the Woman's Hospital of the State of New York, etc.

[Read at the 42d Annual Meeting of the British Medical Association, held in Norwich, August, 1874.]

[The advanced sheets of this paper were received directly from Dr. Sims, probably that it might be published in this country simultaneously with its appearance in the British journals; probably, also, with the intention of procuring its publication where American writers upon the subject he so ably presents, should have full opportunity to read the paper, and reply if thought proper to continue the discussion. Our columns are freely offered to the writers personally alluded to, and indeed to all

* This opening should neither be too large nor too small, as either will prevent the formation of a coarse spray, and thus interfere with the efficacy of the instrument.

members of the profession who wish to debate either the question of priority of resort to this method of resuscitation from chloroform narcosis, or the yet more important question of practical advantages to be obtained by its adoption.]

Dr. Charles James Campbell, the distinguished accoucheur of Paris, has recently written two papers on anæsthesia in obstetrics,* in which he ably sustains the views long taught by Nélaton, that death from chloroform is due to syncope or cerebral anæmia. And amongst other strong arguments to prove his position, he gave a graphic description of a case of chloroform narcosis which occurred in my practice in Paris, where M. Nélaton, by his method, unquestionably saved the life of the patient. She was young, beautiful, and accomplished, and belonged to one of the oldest and best families in France. Married at twenty, she gave birth to her first child a year afterwards. The head was enormous (hydrocephalic), impacted in the pelvis nearly twenty-four hours, and the delivery of a dead child was ultimately accomplished with instruments. Dr. Bouchacour, of Lyons, was called in consultation, and applied the forceps. In a week afterwards the urine began to dribble away, and in a fortnight an immense slough was thrown off. The case, surgically considered, was one of the most interesting I ever saw, and the operation was one of the most difficult I ever performed on any one in her station in life. The base of the bladder was destroyed, and the fundus fell through the fistulous opening; it was therefore inverted, and protruded between the labia majora as a herniary mass of the size of an apricot, its external covering being the internal or lining membrane of the bladder, which was of a deep vermillion red color. The vaginal portion of the cervix uteri and the posterior cul-de-sac were destroyed; and by the reparative process, the cervix and the posterior wall of the vagina were blended into one common cicatricial mass, which was firm, inelastic, and immovable. The case appeared desperate, and M. Nélaton had pronounced it incurable. A preparatory operation was necessary, viz.: to open the cervix uteri, by dissecting it from the posterior wall of the vagina, and thus to reconstitute the canal of the vagina up to the canal of the cervix; and by a subsequent operation, to draw forward the flap thus formed, secure it to the neck of the bladder anteriorly, and hereby close the fistula. The first, or preparatory operation, was performed at the country house of the family near Dijon, on November 3d, 1861, Dr. Dugast, of Dijon assisting, and giving chloroform. The second, or operation for the radical cure, was performed on the 19th of the month, at St. Germain, about an hour's distance from Paris by rail. M. Nélaton, Dr. Campbell, Dr. Beylard, Dr. Johnston, and Mr., now Dr., Alan Herbert, were present. I sel-

* 1. Memoire sur l'Anesthesie Obstetricale; 2 Etude sur la Tolerance Anesthesique Obstetricale, par le Dr. Charles James Campbell, Ancien Interne de la Maternite de Paris, Ancien Chef de Clinique Obstetricale de la Faculte de Paris. G. Masson. 1874.

dom give an anæsthetic in private practice for operation on the walls of the vagina, as the pain is generally not sufficient to call for it. But in this case, as the slightest touch was unbearable, an anæsthetic was indispensable. Dr. Campbell was selected by the family, as well as by M. Nélaton and myself, to administer the chloroform, especially as he was in the daily habit of giving it in his large obstetrical practice, and we all had entire confidence in his caution, skill, and judgment. The patient was soon anæsthetized. The operation was begun at 10 a.m., and I thought it would require about an hour to finish it.

Many years ago I imbibed the convictions of my countrymen against chloroform in general surgery, and have always used ether in preference, never feeling the least dread of danger from it under any circumstances. It is otherwise with chloroform, and in this particular case I felt the greatest anxiety, frequently stopping during the operation to ask Dr. Campbell if all was going on well with the patient. At the end of forty minutes the sutures (twelve or thirteen) were all placed, and ready to be secured, and I was secretly congratulating myself that the operation would be finished in a few minutes more, when all at once I discovered an unusual bluish livid appearance of the vagina, as if the blood were stagnant, and I called Dr. Johnston's attention to it. As this lividity seemed to increase, I felt rather uneasy about it, and I asked Dr. Campbell if all was right with the pulse. He replied, "All right, go on." Scarcely were these words uttered, when he suddenly cried out, "Stop! stop! No pulse, no breathing." And, looking at M. Nélaton, he said, "Tête en bas, n'est-ce pas?" Nélaton replied, "Certainly; there is nothing else to do." Immediately the body was inverted, the head hanging down, while the heels were raised high in the air by Dr. Johnston, the legs resting, one on each of his shoulders. Dr. Campbell supported the thorax. Mr. Herbert was sent to an adjoining room for a spoon, with the handle of which the jaws were held open, and I handed M. Nélaton a tenaculum, which he hooked into the tongue, and gave in charge of Mr. Herbert; while to Dr. Beylard was assigned the duty of making efforts at artificial respiration, by pressure alternately on the thorax and abdomen. M. Nélaton ordered and overlooked every movement, while I stood aloof and watched the proceedings with, of course, the most intense anxiety. They held the patient in this inverted position for a long time, before there was any manifestation of returning life. Dr. Campbell, in his report, says it was fifteen minutes, and that it seemed an age. My notes of the case, written a few hours afterwards, make it twenty minutes. Be this as it may, the time was so long that I thought it useless to make any further efforts, and I said, "Gentlemen, she is certainly dead, and you might as well let her alone." But the great and good Nélaton never lost hope, and by his quiet, cool, brave manner, he seemed to infuse his spirit into his aids. At last there was a feeble inspiration, and after a long time another;

and by and by another; and then the breathing became pretty regular, and Dr. Campbell said, "The pulse returns, thank God; she will soon be all right again." Dr. Beylard, who always sees the cheerful side of everything in life, was disposed to laugh at the fear I manifested for the safety of our patient. I must confess that never before or since have I felt such a grave responsibility. When the pulse and respiration were well rëestablished, M. Nélaton ordered the patient to be laid on the table. This was done gently. But what was our horror, when, at the moment the body was placed horizontally, the pulse and breathing instantly ceased. Quick as thought the body was again inverted, the head downwards and the feet over Dr. Johnston's shoulders, and the same manœuvres as before were put in execution. Dr. Campbell thinks it did not take such a long time to rëestablish the action of the lungs and heart as in the first instance. It may have lacked a few seconds of the time; but it seemed to me to be quite as long. For the same tedious, painful, protracted, and anxious efforts were made as before; and she seemed, if possible, more dead than before; but, thanks to the brave men who had her in charge, feeble signs of returning life eventually made their appearance. Respiration was at first irregular, and at long intervals; soon it became more regular, and the pulse could then be counted, but it was very feeble, and would intermit. I began again to be hopeful, and even dared to think that at last there was an end of this dreadful suspense, when they laid her horizontally on the table again, saying, "She is all right this time." To witness two such painful scenes of danger to a young and valuable life, and to experience such agony of anxiety, produced a tension of heart and mind and soul that cannot be imagined. What, then, must have been our dismay, our feeling of despair, when, incredible, as it may seem, the moment the body was laid in the horizontal position again, the respiration ceased a third time, the pulse was gone, and she looked the perfect picture of death. Then I gave up all as lost; for I thought that the blood was so poisoned, so charged with chloroform, that it was no longer able to sustain life. But Nélaton, and Campbell, and Johnston, and Beylard, and Herbert, by a consentaneous effort, quickly inverted the body a third time, thus throwing all the blood possible to the brain, and again they began their efforts at artificial respiration. It seemed to me that she would never breathe again; but at last there was a spasmodic gasp, and, after a long while, there was another effort at inspiration; and, after another long interval, there was a third; they were "far between;" then we watched, and waited, and wondered if there would ever be a fourth; at length it came, and more profoundly, and there was a long yawn, and the respiration became tolerably regular. Soon Dr. Beylard says, "I feel the pulse again, but it is very weak." Nélaton, after some moments, ejaculates, "The color of the tongue and lips is more natural." Campbell says, "The vomiting is favorable: see, she moves her hands; she is

pushing against me." But I was by no means sure that these movements were not merely signs of the last death-struggle, and so I expressed myself. Presently Dr. Johnston said, "See here, doctor, see how she kicks; she is coming round again;" and very soon they all said, "She is safe at last." I replied, "For heaven's sake, I beg you not to put her on the table again till she is conscious." This was the first and only suggestion I made during all these anxious moments, and it was acted upon; for she was held in the vertical position till she, in a manner, recovered semi-consciousness, opened her eyes, looked wildly around, and asked what was the matter. She was then, and not till then, laid on the table, and all present felt quite as solemn and as thankful as I did; and we all in turn grasped Nélaton's hand, and thanked him for having saved the life of this lovely woman.

In a few minutes more the operation was finished, but, of course, without chloroform. The sutures were quickly assorted and separately twisted, and the patient put to bed, and, on the eighth day thereafter, I had the happiness to remove the sutures in the presence of M. Nélaton, and to show him the success of the operation.

I have detailed the circumstances of this interesting case at great length, because I believe it goes as far to establish a principle of treatment as any one case ever did, or possibly can.

If the recovery had been complete and perfect with the first effort at reversing the body, there might have been a doubt whether the vertical position was really the cause of resuscitation; but, when the horizontal position was again and again followed by a cessation of all evidence of life, and when life was again and again rëestablished by a position that favored only the gravitation of the blood (poisoned as it was) to the brain, the inference is very clear that death in such cases is due to syncope or cerebral anæmia. Exhaust the brain of blood in any way, and death follows. Fill it speedily with blood again, and life returns.

I have another case to relate, which goes far to establish the principle of treatment in chloroform narcosis, so forcibly illustrated by the case at St. Germain.

In January, 1873, I amputated the cervix uteri at the Woman's Hospital, drew the vaginal tissue over the stump, and secured it by silver sutures. The junior house-surgeon gave the anæsthetic. When the operation was nearly finished, he cried out, "The patient has stopped breathing," and immediately added, "she has no pulse." As before stated, I always use ether as an anæsthetic, and could not realise the fact that my patient was in any danger whatever till I was told that they were giving her a mixture of chloroform and ether (one part to four), which some of the surgeons had been using a few days previously. On examining the patient, I found her as it were, dead; there was not the slightest muscular rigidity; the arms and head fell by their own gravity in any way they were directed; the neck was as limber as if it

were a mere band of soft linen stretching from the head to the trunk; there was not the least sign of breathing or of the pulse; she was, to all intents and purposes, dead; and I believe she would certainly have remained so if she had been left alone; and I doubt very much whether she could possibly have been resuscitated by any other method than that of Nélaton's.

I quickly inverted the body, and had it held thus; and then I shook the thorax, agitating the head laterally, so as to add an impetus to the movement of the blood, which, with the body in this vertical position, would naturally gravitate toward the brain; the jaws were held asunder, and the tongue hooked with a tenaculum, and pulled forward; in a few minutes the breathing was rëestablished, and then the pulse returned; and soon the patient was placed again on the table in the lateral semi-prone position in which all my operations on the uterus are performed; and the operation was finished, but without any more of the anæsthetic.

These two cases comprise my personal experience with Nélaton's method in chloroform narcosis.

The *New Orleans Medical and Surgical Journal* for November, 1873, says: "In the course of an extended experience in the administration of chloroform it has happened three times to Dr. M. Schuppert, that, to all appearances, the narcotised subject died—that is, respiration ceased, the heart stopped beating, and muscular contractility became extinct. The method he adopted for resuscitating these patients consisted in reversing the body, either by hanging them up by the feet, or laying them over a bed or table, so that the greater part of the body with the head hung down. In that position, artificial respiration was also tried. In one case, five minutes elapsed before there was a natural inhalation. All of them recovered. Dr. Schuppert believes that in cases of death from chloroform, the primary cause of the cessation of the respiration and circulation rests in anæmia of the brain, and not in impregnation of the blood with carbonic acid."

Another American authority, Dr. E. L. Holmes (*Chicago Medical Journal*, September, 1868), says that whenever there is any failure of the heart's action, as is nearly always the case, the body should be laid at an angle of 40° with the head downwards, so as to favor the passage of arterialized blood to the brain.

I take it for granted that Dr. Schuppert and Dr. Holmes must have obtained their knowledge of this method of resuscitation either directly or indirectly from the teachings of Nélaton; for he had for years been in the habit of explaining his method in his lectures and at his *cliniques*, and Dr. Johnston published an account of it in the American papers in 1861. Ten years ago, there was a story prevalent in Paris that M. Nélaton had derived the hint of reversing the body in chloroform-poisoning from a discovery accidentally made by his little son, then some seven or eight years old; that the little boy had killed some mice with chloroform; that, without thought or reason, he had taken up a

dead mouse by the tail, and was twirling it round, when, to his surprise, it began to manifest signs of life and recovered entirely, while the mice left lying were dead; and that the great surgeon was thus taught a great lesson, if not by babes and sucklings, at least by a little boy. This is a very pretty story as it is, and it seems a pity to spoil it. A few days ago, when in Paris, I called to see young Nélaton (who is now a student of medicine, and will graduate next year), and I asked him for the facts of the mouse story. He said that when they lived on the Quay Voltaire, the house was infested with mice; that great numbers were caught in traps almost daily; that he was in the habit of killing them with chloroform by covering the trap with a napkin and pouring the chloroform on it; and that his only idea was that of an easy death for the mice. One day, when he had given a happy dispatch to some, his father accidentally came into the room, and, seeing the dead mice, he told his son if he would take up one by the tail, and hold it with the head downwards, that it would revive, while the others would not. He did this, and found it was true. And he told me that he had, when a boy, performed the same experiment on mice some forty or fifty times or more; and always with the same unvarying result. He says that he has often heard his father speak, not only of the case that occurred at St. Germain, but of other cases that he had saved in the same way before the time of the mouse story, which dates back to 1857 or 1858.

As the facts now laid before you fully explain themselves, it is unnecessary for me to indulge in any lengthened remarks on the subject. In my own country, the accoucheurs often use chloroform, and the surgeons mostly use ether. I believe there has not as yet been a single death from chloroform given during labor; while deaths from it in general surgery occur constantly, and for unimportant operations. There must be a reason for this. I believe that it can be explained only on the theory that death from chloroform is, as a rule, due to syncope or cerebral anæmia. Now, we know that in active labor there can be no cerebral anæmia, for every pain throws the blood violently to the head, producing fulness and congestion of the blood-vessels, thereby counteracting the tendency of the chloroform to produce a contrary condition. It may be said that the recumbent position has some influence in determining the safety of chloroform in labor; and so it has, but it gives no immunity under other circumstances. Chloroform, given intermittingly, as in labor, is thought to be less dangerous; but patients in labor are often kept for hours under its influence with safety, and occasionally it is necessary to produce complete and profound narcosis in some obstetrical operations; and yet, I believe, I can safely reiterate, what I have already said, that no woman has as yet died in labor from the effects of this anæsthetic. In puerperal convulsions, where the brain is believed to be overcharged with blood—and that, too, when the blood is known to be poisoned with urea—

we formerly bled the patient, and we do so now sometimes, but our chief remedy is chloroform, which acts by arresting spasmodic movements, and by producing that very state of cerebral anæmia so necessary to a successful result. Whether puerperal convulsions are less frequent in labors under chloroform than in those without it, I do not know.

I believe that obstetrics may take a lesson from Nélaton's method of resuscitation, by adopting it in cases of threatened death from *post partum* hemorrhage. Let us not be satisfied with simply placing the head low; but let us, in addition to the means usually adopted, invert the body, and throw what little blood there is left in it wholly to the brain. I have never seen a death from uterine hemorrhage; but from recollections of the few alarming cases I have witnessed, I now feel sure that recovery might have been hastened if I had known of and adopted Nélaton's method of inversion.

Whether death from chloroform is due to cerebral anæmia or not, it is at least safe to adopt Nélaton's method in all cases of supposed or threatened danger; but I think the safest plan is to relinquish the use of chloroform altogether, except in obstetrics. The frequent cases of death from the use of chloroform in surgical operations that have occurred amongst us, even of late, should warn us to give up this dangerous agent, if we can find another that is as efficient, and, at the same time, free from danger. Ether fulfils the indications to a remarkable degree; but, while it is safe, it is unfortunately unpleasant to the physician and bystanders, as well as to the patient. He who will give us an anæsthetic as pleasant to take as chloroform and as safe as ether, will confer the greatest boon upon science and humanity.

PROCEEDINGS OF THE NEW ORLEANS MEDICAL AND SURGICAL ASSOCIATION.

A Case of Myositis Successfully Treated by Hydropathy. By J. J. Castellanos, M.D.

In the latter part of May, 1868, I was consulted by a stout and apparently healthy lad, 19 years of age, for a very distressing pain which he referred to the left hypochondriac region, circumscribed in its extent, and superficially seated. The following was the history he gave me of his case; Three days previous he had very actively participated in our local game of "raquette," as a champion player, for which prominent part he seemed to be well fitted, if I were to judge from his athletic build. This ball game, at one time highly relished and indulged in by our downtown creole population, calls into play an unusual amount of muscular strength and agility, consisting in ball-tossing at a

great distance, struggling, wrestling, dodging, all of which involve violent muscular straining. With the exception of some general weariness, that could be easily accounted for by the foregoing description of the nature of the "raquette" game, our young patient felt no inconvenience the following day (Monday.) While engaged at work, near a window on the following Tuesday, and after having frequently stooped to look out from the window, his left side pressing upon the window sill, he felt some pain in that part. This pain increasing, he resolved to consult me. Pressure with the tips of the fingers, bearing edgewise, and at a right angle with the muscular plane of the affected part, created intolerable pain, which was, however, limited to an extent of from four to five inches. It was evident that there was a muscular lesion or affection seated in that part of the external oblique situated immediately under the cartilage of the last false rib. From the repeated assurances of the patient, syphilis and rheumatism were to be excluded from the diagnosis. No sign of malarial cachexy was present to justify any splenic lesion, and palpation failed to discover any enlargement of that organ. Nor was the seat of the pain that of intercostal neuralgia. The violent nature of the exercise to which he had been submitted but a few days before led me to the only justifiable conclusion under the existing circumstances, viz: that their was a muscular lesion. Was there rupture of muscular fibre, subacute inflammation of the muscular fibre sheath, or were these both combined in my case? With these queries before my mind, I did not venture to answer until I should have seen more of the case. The treatment was the appropriate one—rest, antiphlogistics, local bleedings in the neighborhood of the affected parts, local sedatives, narcotics to assuage pain were successively but I am sorry to say unsuccessfully resorted to. The pain increased, attended with fever, anorexia, sleeplessness and emaciation. The local condition of the parts had also assumed some peculiar physical features which seemed to characterize the nature and shape out for me, as it were, a certain diagnosis of the affection. These consisted in a slight elevation of the affected above surrounding parts, more distinctly perceptible to touch than to sight—a very distinct sense of hardness, analogous to that of pasteboard, in the muscular structure of the affected region, and a most painful contraction of the trunk upon the pelvis, partly forwards, and partly laterally. This most harrowing, flexed condition, was rendered forcibly permanent by the excruciating torture which any attempt from the patient at assuming an erect position awakened, thus compelling him to spend his sleepless nights in an arm-chair. After nearly two months of active and rational treatment, with no abatement, but rather increase in the severity of symptoms, and leading to the condition here above described, with a disheartened patient and a disheartening affection before me, I hastened to avail myself of what little there was left of the patient's former unbounded

confidence in me, and suggested the hydropathic plan of treatment, which was cheerfully and readily acceded to.

I will not overburden your attention with minute details, but I deem it pertinent to state that the treatment consisted in inducing sudation by wrapping up the patient, previously stripped of his garments, in heavy woollen blankets, with the wet sheet interposed between the skin and the blankets. The sweating process is accelerated by repeated and copious ingestion of cold water. When this process was thoroughly gone through, the patient, stripped of his coverings, and reeking with perspiration, was suddenly and thoroughly plunged in cold water. The temperature of the bath, which was during the first days that of water drawn from a cool cistern or well, was gradually lowered by addition of ice. The duration of the cold bath was of course limited (from a quarter to one-half a minute), reaction being alone required. The patient bore his baths remarkably well, and readily experienced their sedative effect, a quiet, refreshing sleep having been induced by the fourth bath. These baths were repeated once every day for at least ten or twelve days, when a decided improvement in the muscular contractions, degree of pain, &c., induced me to allow them at intervals of three or more days. Hydropathy thus applied brought on a cure in this protracted case, after a month's treatment. In reporting so lengthily and apparently "con amore," this successful result, let me not be understood as claiming any exclusive or strictly special advantage in behalf of hydropathy. Without being necessarily wedded to "specialties," I deem it the duty of every conscientious and enlightened physician to exercise a judicious discrimination and a wise moderation, both in the choice and in the use of the weapons which the rich and varied panoply of rational medical science offer to his grasp.

The Antiseptic Treatment of Small-Pox. By J. J. Meylor, M.D.

Having, during the late prevalence of variola in our city, had some experience in the antiseptic treatment of this disease, I consider it incumbent on me to communicate to this Society (which I notice to be a practical and working one), what has been the result of that experience, and what conclusions I have drawn from it. As it would be entirely superfluous to describe this disease, its prodromès, its various stages and their symptoms, the characteristic eruption and its progress and decline, I will detail briefly the treatment I have pursued, without making mention of the modifications which the peculiarities of individual cases demanded. While the disease continued in the initial stage, whether the symptoms pointed to the case as likely to be one of small-pox or not, it was treated on general principles. As soon however, as the decline of the fever, the appear-

ance of the eruption, &c., showed that the disease was variola, especially if there was any reason to suppose that the case might be a severe one, I immediately, as a general rule, began the antiseptic treatment. I say as a general rule, for if there was any doubt about the eruption being variolous, I permitted it to proceed unchecked till it assumed the characteristic appearances of variola. The antiseptic treatment consisted in giving the sulphocarbolate of sodium and sulphurous acid internally, and applying carbolic acid externally. Half drachm doses of sulphocarbolate of sodium in solution were given every four hours, and one teaspoonful of sulphurous acid in the same intervals of time, the drachm of acid being put into a glass of water, which was taken by mouthfuls now and then till finished. This, the constitutional part of the treatment, was continued throughout the disease, till the patient was out of danger. When however, the eruption began to become vesicular, showing fluid on the apices of the hitherto papules, then the second step, the local, in the treatment of the disease was entered on, and that consisted in touching the tops of as many as convenient of the vesicles with liquid carbolic acid once, rarely twice, a day, and smearing the eruption, especially if attended with itching, with carbolic oil (1 to 3) two, three, or more times a day. Such is the treatment that was followed, modified or assisted by other remedies, as the circumstances of the case called for.

As to the result of this treatment, I will state that only one patient, submitted to it as above delineated, died. This case, an adult male, was a very malignant one, the eruption coming out tardily and presenting different appearances in different parts of the body, showing itself as one vast, dark, irregular, rough-surfaced mound on the forehead and parts of the face, as purplish patches, and as circumscribed, circular, dark discolorations of the surface, with darker and apparently depressed central spots on many parts, as prominent white papules, with scattered normal vesicles and pustules on the forearms and legs, and as blebs of various sizes and shapes, and filled with blood and bloody fluid, on the feet. In this case, the specific treatment was delayed, watch being kept for the regular variolous eruption to begin the specific treatment. Finding that the characteristic eruption did not make its appearance on the face, and as the case was plainly a most threatening one, the constitutional part of the antiseptic treatment was begun without any further delay; but as there was not much fluid eruption, that namely on the forearms and legs, there was only small opportunity of employing the local specific treatment. In this, the only fatal case, the antiseptic and constitutional treatment was perhaps postponed too long, and the local treatment, owing to the nature of the eruption, was impossible.

The conclusions I have drawn from what I have seen of the effects of the antiseptic treatment of small-pox are, that it diminishes the percentage of mortality from the disease, that it fre-

quently shortens the case, that it prevents or shortens in a marked manner, and very rapidly, all throat symptoms, that, if fully employed, it prevents the pitting or diminishes the amount of it.

That the antiseptic treatment of small-pox diminishes the percentage of mortality is, I think, proved by the fact that, of twenty or twenty-five patients treated antiseptically, only one died. That it shortens the duration of the disease I have often noticed, for in many cases the carbolized vesicles, especially those on the face, dried up sooner than usual; very many papules in the neighborhood of those carbolized, aborted, did not advance beyond the papular stage, and in one, probably two, severe confluent cases, when there was secondary fever, it was mild and of short duration. Perhaps one of the most noticeable events, and one that attracted my attention most forcibly, was the very decided and uniform control the treatment, the sulphurous acid especially, had over the throat symptoms, for they invariably disappeared soon after the acid was begun with.

With regard to the pitting, I think the antiseptic mode of treatment shows good results, for in several bad cases, some of them unvaccinated, there were no marks left, though I had reason to dread them. At the same time however, it must be acknowledged that there were two, perhaps more, cases in which, though judging from previous ones it was expected there would be no pitting, there was considerable. This result may, I think, be attributed to the local treatment not having been thoroughly carried out, for I know that, in my great reliance on the effects of the remedies, I did not follow up the local treatment as energetically in my latter as I did in my earlier cases.

In the severe confluent cases there was no secondary fever or very little, the swelling and disfigurement of the face not great, and the patients, after they had been got freely under the influence of the remedies, seemed quite comfortable.

Among my patients were several adults and children who had not been vaccinated. In three there were hemorrhages, besides the hemorrhagic eruptions of the skin. In one, the fatal case, the bleeding was from the bowels; in the second, from the nose; in the third, female, colored, from the gums and nose.

In the above, gentlemen, I have communicated to you what I have seen by experience of the antiseptic treatment of small-pox. In my hands it has done good service; in fact I feel that, in cases where the constitutional and local treatment can be fully carried out, this treatment of variola is as certain a cure of the disease as quinine is of intermittent fever. This, no doubt, seems an over-strong assertion, and perhaps it is; still I must say that I always feel very confident of success when called to see a case of this disease.

Whether or not the above deductions are correct, I leave to others to determine. It would be worth while perhaps, were others who are called on to attend cases of this disease, to put

this manner of treatment to the test and let the profession know of their success.

In conclusion, I will state that I lay no claim to originality in this matter of the antiseptic treatment of variola, for anything I know about it I have learned from the very excellent papers of Dr. Sansom, of London, and Dr. Foot, of Meath.

Case of Irreducible Inguinal Hernia with Abscess of Liver. By L. F. Salomon, M.D.

Gentlemen: I desire to report to you the following singular and interesting case, which came under my notice, more as a clinical curiosity than for any practical advantage which may arise from it, as I doubt if there is another such case on record. I have never read or heard of one similar.

A negro man, aged about 25 years, presented himself to me, stating that he had a tumor in his right groin, and that his testicle was enlarged. On making an examination I found the tumor and supposed swelled testicle to consist of a right inguinal hernia, which the man stated he had had for six weeks or two months; in fact, was uncertain as to the exact time at which the rupture occurred. Taxis persisted in for a reasonable length of time failed to reduce it. There was no strangulation. The bowels acted regularly, and the patient complained of nothing else except a slight pain in the right shoulder, which he attributed to rheumatism.

There being no distressing symptoms, and as an irreducible hernia may often through well known causes become reducible, I instructed my patient to assume the recumbent posture, and to remain perfectly quiet, promising to see him the next day, when I intended to again make an effort at reduction. Imagine my surprise, however, the next morning at finding that the man had died during the night. Such an unlooked-for event perplexed me not a little, as I could in no manner account for it. However, a post-mortem examination at once solved the mystery. On opening the abdominal cavity, the first thing which attracted attention was an enormously enlarged liver, occupying the whole of the epigastric and right lumbar regions and a goodly portion of the umbilical. On plunging a knife into this greatly enlarged organ there was poured forth about two or more gallons of pus; nothing but a mere shell remaining of the liver structure. Of course death was easily accounted for now.

The hernial sac next being opened, it was found that the hernia consisted of the *appendix vermiformis* bound down to the scrotum by strong adhesions. The walls of this portion of the intestine had increased to about one quarter of an inch in thickness, and imbedded in its extreme end was found a small nail, such as is used in nailing the iron around the head of a flour

barrel. There was also a small quantity of pus found in this situation. On dissecting up the superior mesenteric vein pus was found in it also. The whole progress of the case was now mapped out very plainly, the cause of the abscess of the liver which occasioned death being evident. Doubtless the patient in eating bread without proper mastication (which would have discovered a foreign body therein) swallowed the nail, which found its way into the *appendix vermiformis*, and there remained; the hernia, which was merely accidental, occurring about the same time, or probably existing for a short time previous. The presence of the nail gave rise to inflammation, which on the external and serous coat of the *appendix* terminated in adhesions, thus converting it into an irreducible hernia, and on the internal coat terminated in suppuration. The presence of pus in the superior mesenteric vein shows how the liver became affected. Some may object to this reasoning, and attribute the presence of pus in this vein to the abscess of the liver. It is my belief, however, that *the affection of the liver was secondary*, as that portion of the intestine immediately surrounding the nail was gangrenous, and doubtless the disease was extended from there to the liver through the portal system.

What is worthy of notice in this case is the singular fact that the patient complained of no symptoms pointing to hepatic disease, if we may except the slight pain in the right shoulder, to which I gave no attention at the time, as the man stated that he had been subject to rheumatism, and ascribed it to that malady.

I have reported this case as one of uncommon interest to the Association; for, in all probability, neither you, gentlemen, nor I will ever see another like it. Fortunately the autopsy was obtained, which cleared the mystery of the unexpected death, or I should ever have been in ignorance of its cause.

Chronic Pyæmia (Chronic Purulent Infection); Nature of the Tertiary Accidents of Variola, Cholera, Scurvy, &c. By Dr. Armand Duprê (Surgeon to the "Hopital Cochin," Adjunct Professor to the Faculty of Paris).

[Translated from Archives Generales de Medecine, Sept. 1874, By Jno. J. Castellanos, M.D., of New Orleans.]

There are two orders of indisputably acquired and well proven facts, admitting of daily verification, which lead to the establishment of the following expressed doctrine:

1st. Infectious diseases, such as glanders, acute pyæmia, and plague, offer characteristic features, common with one another, consisting of disseminated, rapidly developed metastatic lesions,

bearing a suppurating character, with clearly defined and homogeneous pus.

2d. Long established chronic suppuration of bones leads to pulmonary tuberculosis, even when any predisposing causes are all absent. This assertion was first advanced by Velpeau, and subsequently confirmed by Bazin.

No one can be ignorant of the mutual connection of these two series of facts; for the parallelism between the original lesion and the final pulmonary lesions, is very appreciably established.

Congenital or hereditary scrofula, of which no exact formula has ever been given, seems to constitute the type of what may be called chronic hereditary pyæmia, while acquired scrofula can be designated under the name of chronic acquired pyæmia. Several pathologists, also, evince a disposition to ascribe to scrofula chronic suppuration of bone, followed by tuberculosis. But these are threadbare and *a priori* hypotheses, which should be controlled by observation. We therefore deem ourselves enabled, with the assistance of recently gathered observations, to throw some light upon the subject.

1st. Origin and process of metastatic abscesses in both acute and chronic pyæmia.

Inflammatory diseases terminating in suppuration, are characterized by complete cessation of vitality at the seat of suppuration, and by temporary cessation of the phenomena of capillary circulation in the neighborhood of the abscess. The capillary vessels are obliterated by the aggregated blood corpuscles. This obliteration which reveals itself externally by redness and œdema is far from being a permanent condition; in fact, it is not more so than in capillaries observed in inflamed parts that have no tendency towards suppuration. These phenomena are clearly exhibited in the interdigital membrane of the frog when simple inflammation has been therein induced.

These obliterations are transient, and exhibit the following phenomena: either there is rupture of the blood-vessels, in which case the blood corpuscles scatter throughout the tissues, segregate and undergo a metamorphosis into embryo-plastic elements, fat and pigment; or the vessel becomes, little by little, permeable, and the corpuscles adhering to one another in groups of from 3 to 15 in number, enter into the circulation, and are finally arrested in some of the viscera, more readily in the lung, the liver, and the spleen, and sometimes in the capillaries of the

digestive mucous membrane or those of the skin. These migratory and adhering corpuscles constitute embola, and have the same claim to that title as the voluminous venous clots which, as it were, founder in the pulmonary vessels where their onward progress is stemmed. These are now acquired and proven facts, from the labors of Virchow, Coze and Feltz, and account for the recognition of purulent infection, or acute pyæmia with metastatic abscesses, as an "embolic" affection—and for the final return of pathologists to the old theory of Gaspard Cruveilhier and Ducrest. In other words, the theory of capillary phlebitis has been restored to its former supremacy, by such among pathological anatomists as were the most fervent believers in the theory of transference of the products of inflammation through the circulatory current. It has been proved, from observations published quite recently, that metallic dust, putrid matter, even tubercles, have determined metastatic abscesses in the lungs. Nay more, Mr. Lionville's experiments have established the fact that the inoculated blood of a tuberculous subject induced accidents similar to those produced by tuberculous pus.

By the side of acute pyæmia or purulent infection, which offers such striking phenomena, we may class a series of diseases, partaking of a suppurative character, or chronic forms of inflammation known under the vague appellation of scrofula, lymphatisma, cold abscess, tubercles of bone, tuberculosis, and visceral syphilis, the distinguishing character of these affections from the former (acute pyæmia), being the longer interval between the primary lesion and its secondary accidents. In a similar manner do we frequently observe the sudden formation of abscesses, arising from the presence of certain foreign bodies in the living tissues; whereas others are permanently lodged therein, without producing inflammation, until many years after their introduction.

But here, may the following objection be urged, viz: Wherefore the occurrence of accidents, akin to those we thought proper to call chronic metastatic abscesses, in individuals who up to the present disease had been free from any constitutional affection or form of chronic suppuration? The answer is readily the following: in such condition physicians must needs invoke the assistance of a hereditary diathesis, be it scrofulous, syphilis, or tuberculous, and that there are no grounds upon which to exclude the hereditary character of chronic pyæmia. Much is

left for study and observation, to elucidate the relations of causation between diseases which might have affected parents, and more particularly such as might have occurred shortly previous to the birth of children who have become scrofulous.

I have for several years been struck with the occurrence of cold abscess, caries (usually observed in scrofulous or tuberculous cases), in individuals living in tolerably fair hygienic conditions, whose family records are free from scrofulous or tuberculous taint, and who had never heretofore betrayed symptoms of any of these diatheses. After diligent researches, I have been enabled to point out some former diseases in those patients as the cause of these tardily developed lesions, which could be properly called tertiary accidents or metastatic abscesses, all of which would contribute to prove the existence of chronic pyæmia."

2d. Observations of cases of variola followed, long afterwards, by periostitis, ulcers, and congestive abscesses.

The author now enters into an enumeration of facts and observations which, for fear of fatiguing the reader's attention, and trespassing beyond the limits so kindly assigned to this article in your present number, I shall briefly analyze and group, following, at the same time, the author's own order, and, as much as possible, own language.

The first and most numerous group of facts comprises such as relate to the ulterior, and which may properly be called the tertiary lesions of variola.

1*st Observation.*—A healthy young man, free from any acquired or hereditary taint, in January, 1871, when a soldier, and prisoner in Prussia, is taken down with confluent variola, for which he remains one month in the hospital.

He, shortly afterwards, had multiple, consecutive abscesses, kerato-conjunctivitis; and in November, 1872, a painful tumor, of progressive growth, appeared in the left iliac region. It proved to be a congestive abscess, symptomatic of caries of the ileum. It opened externally, and created a fistula.

2*d Observation.*—Chronic arthritis, a severe case following recent and confluent variola, threatening with deep suppuration the articular extremities of the bones. Patient is free from any diathesis, either scrofulous, tuberculous, syphilitic, or rheumatic. April 4th, 1870—Patient fell upon both knees while ascending a staircase. With the exception of slight inflammatory symptoms

in the knee-joints, which soon subsided, leaving however, some persistent pain, with predominance in the left knee, patient could be considered cured, when, June 14th of the same year, he was taken with confluent variola, followed by keratitis and secondary abscesses, for which he entered the hospital, presenting at the same time symptoms of arthritis, then incipient and now so severe. The author cursorily alludes to malarial complications, upon which he lays no stress, as he deems them unimportant and having no share in the lesions described above.

Observation 3d is that of sub-periostial abscess of the right femur, with considerable increase of the volume of the bone. Patient, aet 29, was admitted April 23d, 1873; has had a similar abscess two months previous; has had confluent variola when 7 years old, after which he was taken with severe and protracted "mucous" fever.

Seven or eight years ago, suffered from cervical ganglionic abscesses, and besides the periodical abscesses referred to above, presents a tumor on the left side, on a line with the greater trochanter, which is undoubtedly a chronic abscess, not thoroughly developed.

The multiplicity of lesions, the absence of any diathetic antecedents in the patient's family (he being the brother of six healthy children), constitutes the peculiar feature of this observation.

Observation 4th.—Ulcerated acne—variola, anterior. Patient is 54 years old, and born of healthy parents. With the exception of suppurative inflammation of four cervical lymphatic ganglia, at the age of four or five years, has always been healthy and free from any taint until five years ago, when, after a benign form of variola, the following lesions made their appearance: ulcerated patches upon the shoulders and neck, of the size of a "centime" coin, irregular in shape, and seemingly formed by the coalescence of several ulcerated acne pimples, with a golden-yellow crust. No glandular engorgement was to be found in the axillæ. This eruption, the first which the patient had ever borne, did not partake of the character of any classified dartrous eruption, and bore a striking resemblance to ulcerated acne.

Observation 5th.—White swelling of the carpal articulation—tubercles of testicles—general tuberculosis—confluent variola, anterior.

Patient, 45 years old, free from any taint and born of healthy

parents, had confluent variola at the age of 31 years. Thirteen years after, he fell upon his wrist from a ladder, with no other result than that of a sprain or wrench of the joint. Swelling and inflammatory lesions supervened, proving refractory to treatment, and insensibly proving a starting point for multiple white swelling, involving both carpal and metacarpal articulations.

In October, patient submitted to amputation. The stump was affected subsequently with hospital gangrene.

In September cystitis had also supervened, and was modified by treatment. The testicles became hard and nodulated. The epydidymis became tuberculous. During November, symptoms of pulmonary tuberculosis made their appearance, and the patient died on December 8th.

Besides the pulmonary and pleural lesions to be met with in phthisis, which an autopsy of this case disclosed, the left kidney was completely softened, at its central portion, into a caseous substance, while its surface was studded with fine, whitish tubercular granulations.

"This patient, then, under the influence of a traumatic cause sufficiently slight not to have produced fracture, has had synovitis of the wrist-joint, with surrounding osteo-periostitis (which latter lesion might have been the primary one), during the process of formation of this white swelling, or perhaps anteriorly to it, if we are to judge from the extent of the affection, there must have been tuberculous disease of the kidney. Pulmonary tuberculosis was, in this case, a consecutive disease. All scrofulous and syphilitic cachexia pursue an analagous course."

I must necessarily omit, for want of time and space, some other, and perhaps less conclusive, observations, and hasten to give a brief account of the author's views as regards the action of certain diseases, such as cholera and measles, upon the production of gummy lesions and cold abscesses. In order the more firmly to consolidate his theory as to the embolic origin of these tardily-developed lesions, he has observed cases in which scurvy and severe contusions were the sole causes to which the gummy forms of ulcers or caries could be ascribed.

"Tuberculosis consequent upon measles, and even whooping cough, in children born of healthy parents, is a certain proof that other diseases besides variola can give rise to chronic pyæmia. Patients affected with scurvy who have died tuberculous yield a ready proof to the assertion that the bloody infarctus is

the focus of chronic pyæmia. By the same mechanical process chronic pyæmia is brought on by cholera.

Observation 10th is one of gummy tumors, scrofula—cholera having occurred eight years previous. The patient, aet. 16 years, exhibits at the middle and anterior portion of the leg, in front of the crest of the tibia, a wide ulcer, resulting from evolution of a scrofulous gumma and a consecutive abscess, and also on a level with the first dorsal vertebra a congestive abscess of the size of a hen's egg, very likely resulting from caries of the vertebral lamina. Patient had, one year ago, osteitis in the left foot. Previous to that date, no scrofulous symptoms had ever appeared. Patient, however, had a severe attack of cholera eight years ago, from which he slowly rallied. He underwent also the privations and sufferings that attended the Siege of Paris. He is the third child of a family of seven children. His parents have personally given us every assurance of their own healthy condition and of that of their remaining children. The exclusion of any presumption of hereditary taint in the subject of our observation, to which the above declaration is apt to lead us, together with the consideration that he is the only member of his family who has had a severe infectious affection (cholera) fully establishes the relation of causation between the latter and the lesions referred to above.

From what is known of cholera, we cannot reject the possibility of bloody infarctus. Choleraic cyanosis we admit to be the cause of multiple thrombosis. Now thrombosis of the small veins constitutes embola, that are susceptible of migration, and seek entrance into the smaller vessels under the reduced size of capillary embola, and over several parts of the body.

Traumatic injuries amounting to multiple contusions give rise to lesions analagous to those that are found in chronic pyæmia, which fact demonstrates the embolic origin of the metastatic and tardily developed lesions that are, generally, peculiar to infectious diseases.

MISCELLANEOUS COMMUNICATIONS.

Squirt contra Squirt. By M. Schuppert, M.D.

A pamphlet written by Stephen Rogers, M.D., reprinted from the New York Medical Journal, September, 1874, and kindly

sent to me by the author, contains his opposition, objection and condemnation of the method of a Dr. F. D. Lente in treating intermittent fever. Whilst Dr. Lente favors the method of treating that fever hypodermically with quinine, Dr. Rogers is in favor of the remedial administration per anum. Considering the difference of opinion in that special question as a matter of taste, I might in all probability have let that controversy alone, were it not for the extreme views of Dr. Rogers, evidently opposed in general to the method of administering different medicines subcutaneously, and that the name of the author in deciding a scientific matter of so high an import would bring into that controversy an undue and improper weight.

The title of the pamphlet under consideration reads "the hypodermic use of quinine, a dangerous experimental medication, and rarely justifiable." *Dangerous and experimental!* Whoever is cognizant of the historical progress the method of subcutaneous injections has made on both sides of the Atlantic, will hardly consent to the use of these adjectives in association with the nomen they have reference to. A method which has become so universally adopted as the hypodermic use of remedies should not be called experimental, and when I consider the thousands of injections I have made with different medicines, without ever meeting with more than a few abscesses, I cannot see wherein the danger lies. I have also treated intermittent fever subcutaneously by injecting a watery solution of bi-sulphate of quinia, and never met with any of the threatened "inflammation," "abcess," nor "has the patient been crippled for a month," nor has "excessive pain of long duration," nor even, *horribile dictu*, "tetanus," been caused by it! I have in all of my injections taken proper care never to inject in the "areolar tissue," but carried the needle always deep into the muscles, be it on the back, the arm, or thigh. In this way I never experienced that "the fluid was hardly absorbed at all, or exuded drop by drop for hours from the puncture." In upwards of 20,000 injections with sulphate and muriate of morphia, and other remedies, like quinia, strychnia, atropia, caffeine, bin-iodide of mercury, etc., I have met but five or six times with abscess, whilst no other accidents happened. One patient frequented my office four times a day for over one year, receiving each time $\frac{1}{4}$ of a grain of sulphate of morphia, in all, per year, 1460 subcutaneous injections without ever suffering from an abscess. If any one should be disposed

to call this good luck, may he do so, but I think that it is to be attributed with more justice to the method of carrying the needle deep into the muscles, since I have frequently observed others to cause abscesses whenever they did not go beyond the areolar tissue of the skin. Dr. Rogers in the beginning of his article says "the rusty and neglected hypodermic syringes, formerly so actively employed by the expert hands of many of our professional acquaintances had led us to fondly hope for no further provocation to return to it." Dr. Roger's hope that the syringes might have become rusty, it seems from an annotation, was based on the objections he had made to that method at different times. Those objections must have been very weighty that he should think to have for all time demolished the adepts in that "dangerous experimental medication." The "senior members of the profession" were not included in his proselytism, but given up as "incorrigibles." His words and warning were directed rather to "the younger members," on account of the "greater risks they run from that unnatural style of giving medicines." In the perseverance and vehemence of his condemnation of this method, Dr. Rogers further says: "Few of the fashions from the days of Sangrado's bleeding and hot water, have been carried to more *ridiculous* and *destructive extremes*, than this *wild fashion* of medicating mankind under the skin." "And while many of the misguided but prudent physicians in great numbers have abandoned this mode, and laid aside their syringes, unfortunately for humanity, there are still too many practitioners who are pursuing this method with all the ardor of a new idea." A high-wrought, impassioned diction, big with the fate of humanity! Sorry as I must feel to be numbered amongst the "incorrigibles," I have still the weakness to find a consolation in being associated with the greatest of our living surgeons. Being in the van of the "incorrigibles," who have the audacity to look upon the hypodermic method as one of the greatest improvements in modern surgery, I can with others leave to the "misguided but prudent physicians who in great numbers have abandoned this mode," the enjoyment of the squirt in the mephitic rear. I do not know if Dr. Rogers has even read any of the researches and writings of such men as Langenbeck, Hüter, and others. I will call his attention to Langenbeck's splendid results in the treatment of aneurism, where the operation of ligating is out of question, by hypodermic injections of ergotine in the neighborhood of

the tumor; and of the good effects of the same remedy tried by others in *post partum* hemorrhage, I call his attention to the newly published splendid results of Hüter's treatment of parenchymatous swellings, by hypodermic injections of carbolic acid acting as an antiphlogistic remedy in *tumor albus*, bubo, phlegmonous glandular tumors, resulting in a few hours in the shrinking of the tissues, disappearance of pain and of fever, and accomplishment of a cure without further inflammation, or formation of pus; even in traumatic erysipelas acting with a localizing of the disease, a decrease and subsiding of fever. Hüter made those injections even into joints (a 2 per cent. watery solution of carbolic acid).

What will Dr. Rogers say to such bold surgical operations? I, and other surgeons, have tried them, and am bound to say *probatæ sunt*. Whoever has been relieved from an intense neuralgia, headache, almost momentarily by a subcutaneous injection of morphia, will never do without it even if he had to take an abscess into the bargain. But I will close this article, already too extended, in mentioning the almost magical results in the treatment of secondary syphilis with subcutaneous injections of a salt of bin iodide of mercury with ioduret of soda. Try it, Dr. Rogers; get out your old rusty syringe of the armory, or better, buy a new one—they have since been improved, and become cheaper, too; but before all, beg forgiveness for the maltreatment and injustice you have dealt out to the right and left, and for a most unwarranted attack of a method which no surgeon of any experience would permit to be stricken from the list of our most potential therapeutics.

Imperforate Anus in a Colt—Operation and Cure. By Robert E. Richardson, M.D., Fayette, Miss.

On the 9th of May, Hon. G. W. S—— consulted me with reference to the condition of a colt, foaled three days prior to this date. The animal was perfect in every way except the anus, and I was requested to operate, if I thought a successful result was possible. The colt being highly prized by the owner, and every argument being in favor of an operation, and nothing against it, I determined to do the best I could, sure of gaining some practical knowledge for myself, as well as gratifying the

wishes of my friend, whose earnestness in the matter gave evidence of a most generous heart and expansive benevolence.

There was not the slightest sign of an opening. Where the anus should have been, there was only a rounded protuberance, hard, and unyielding to pressure. The operation consisted in first slicing off the end of this protuberance, of the diameter of half a dollar. I then made a cut, with a narrow blade knife, carefully inwards and towards the sacrum, (using the sacrum as a guide), to the depth of ¾ of an inch, through a dense fibrous tissue, to the rectum. The index finger was next introduced to explore the anus and gut, and learn if the sphincter muscles existed. The operation was completed by dividing the sphincters freely above, to the right and left, to facilitate the free and easy discharge of the accumulated fœces. With the free use of a Davidson's syringe and warm soap suds, the bowels were emptied of a large mass of impacted matter, to the great and immediate relief of the little animal. Prior to the operation, the colt was dull and inclined to lie down all the time, and showed but little disposition to suck, never taking more than a single mouthful at a time. Six hours after the operation, the colt was lively and sucked ravenously, and continued to do well; indeed, without a single bad symptom, growing in strength and healthy appearance daily, the anus doing perfect duty. The hostler had been instructed to introduce one and then two fingers, well oiled, night and morning, so long as any evidence of the cut remained, and to keep the mother and colt in a secure lot to themselves. The order was well observed for a week, when, from some carelessness or indifference, the colt was permitted to get its feet and legs fastened in the picket fence, at night, in which condition it fell an easy prey to the voracious appetite of a hog. A hole was eaten in its belly before it was discovered, from which my patient soon died. The operation was a perfect success, and all trace of the knife was rapidly disappearing.

Free division of the sphincter muscles, to permit the easy and complete discharge of fœces, and to prevent accumulation and distention in and of the rectum, suggests that a similar operation might be resorted to with profit, to both operator and patient, in cases of recto-vaginal fistula, after that operation.

It seems clear to my mind, that the force necessarily required to overcome resistance of the sphincter muscles in defecations, is a prolific cause of failure, by tearing loose the stitches before ad-

hesion is perfect. Will not division of the sphincter muscles overcome much of the difficulty attending this most disagreeable and often unsatisfactory operation?

Report of a Case of Gunshot Wound of Head. By J. F. Gladney, M.D.

A robust negro man, aged 24 years, received, July 29th, 1874, at 2½ o'clock, p. m., a gunshot wound, the ball penetrating the cranium through the frontal bone, about a ¼ of an inch above the frontal eminence, on the right side, passing through the gray and white matter of the right hemisphere the distance of three inches, then passing between the lobes, was found, at the autopsy, lodged near the centre of the occipital bone. The negro stated that his face was turned downwards when shot. I saw the case soon after the wound was inflicted, and found the patient in the following condition. Rational; circulation good, and suffering but little pain. I was informed that for about fifteen minutes after receiving the wound the patient was unconscious. I visited him frequently until his death, and he was in full possession of his mental faculties, asking for water, and taking the fluid nourishment prescribed. He related (on the fourth day) the circumstances which led to his receiving the wound—his statement corresponding with that made by others. He slept well at night, but during the day suffered occasionally with paroxysms of pain, which were invariably relieved by ⅛ grain of morphine. Bowels readily moved with sulph. magnesia.

No evidence of paralysis about the limbs, tongue, or facial muscles. Taste unimpaired. Pulse ranging from 70 to 80. On second day the temperature indicated by thermometer 99°; the mercury in instrument used stands at 98¼ in health. On the other days the temperature was normal. Death occurred at 6 o'clock on the morning of August 4th following. About three hours before death he suffered intense pain, and became comatose one hour before death.

I must express my thanks to Dr. L. A. Cormick, who visited him with me before his death, and kindly assisted at the autopsy; also to Dr. E. H. Langford, who saw him several times, and witnessed the post mortem.

Queries—Why were there no evidences of paralysis in this

case? How can we account for the fact that he retained consciousness to within a short time of death, and why was there (nearly the whole time) freedom from pain?

Adulterations in Medicines. By Wm. C. Harrison, Ph. D.

Mr. Editor—In this, our enlightened age and land, this matter of adulteration has become so firmly rooted—so does it permeate all avenues and channels of business—so gigantic are all its hideous proportions, that one and all decline the hazard of an encounter; still there is much that could be said. Need I mention the common adulterations of daily use? It is useless to tell you what we get mixed with cream of tartar, pulverized cayenne, jalap, ipecac, quinine, blue pill, &c. Shall I go over the list of fine chemicals, and show how often their inefficiency in the hands of medical men is attributable to their imperfect manufacture, for the purpose of cheapening the product? Shall I rehearse the history of essential oils, and tell you that nine-tenths of them are mixed or otherwise adulterated? No! it is too well known for repetition.

Now does any one wonder that druggists buy and sell adulterated drugs? While it appeals to the common sense of every man, that it is the physician's interest to secure a pure drug in any and every case where he may be called upon to use it in his practice, it must also be kept as clearly in view that he may be unconsciously abetting that which he is endeavoring to put down, by a mere inattention to the true state of the case. What are the causes, remote or immediate, that lead to the so prevalent practice of adulterating and sophisticating drugs? Perhaps this might be answered on the instant, to the effect that it is owing to nothing more or less than too much avarice or dishonesty on the part of dealers; or to put the matter in a little more soothing form, the cause or causes may be a mere desire to check the rising quotation in prices, and no harm be, in reality, intended. Still whatever may be thought of this, may it not be in some manner attributable to the fact that many physicians keep medicines which they dispense themselves, in addition to their consultations, so reaping a second profit, and so in turn create a sort of necessity for regular dealers, who pay a "license tax," to make some shift to keep up the balance of their just profits. Druggists are dependent upon the prescription business to a great

extent. The diagnosis of disease, and the adaptation of medicine to its cure, should then, as a matter of right, be practically distinct from the business of dispensing the medicines prescribed. Many of the fine chemicals and expensive medicinal preparations involve in their manufacture, not only a good amount of time, large outlay in apparatus, but experience and skill, which have only been obtained by long and patient study. Now if these preparations are to be vended for a mere nominal profit, it discourages scientific men in their efforts at exactness, and immediately diminishes the care taken in their manufacture; the consequence is, they are not to be depended on for purity or strength.

The prime essential to a physician's success is a certain knowledge of the powers and properties of the agents he uses as remedial of disease, and his skill all goes for naught if it is, at every turn, thwarted by the dishonesty of those whose profession it is to prepare and dispense medicines, and on whom he is, more or less, wholly dependent. In cases where a certain agent is unmistakably indicated, and where there is every prospect of good success following its administration, the physician has been time and again mortified by the impurity of his drugs, and the only remedy is to impose a certain surveillance over these articles of trade. If any change is to be brought about, the movement must be instituted and carried out by the medical profession. It is their interests that are especially affected, and it is their power alone that can put an end to this wholesale imposition. They must not allow medicines bought where they can buy cheapest, but at those places where they can be obtained pure. It is well known that some of the more powerful medicines require a great deal of manipulation in their preparation, and so involve considerable expense and loss of time to the pharmaceutist. For all such medicines he is justified in charging an adequate price, and this should be willingly paid. The physician should feel himself capable, in every case, of judging of the quality of the medicines he uses, and insist upon their absolute purity. The influence on pharmaceutist and dispenser would be in the highest degree healthy, tending to give dignity to the art, and making a scientific education necessary to those engaging in it, while we shall become more independent of importations from abroad, in the constantly supplied demand for a more liberal scholarship at home. It is evident that there is no end to the adulteration, and the honest efforts of every honest man should be exerted to

their utmost in this direction, to the satisfaction of morals and the preservation of human life.

Caries of Superior Maxillary Bone. By J. P. Pugh, M.D., Fairfield, Texas.

About the 20th of June, I was called by Drs. Miles and Sneed to see a lady who, for four months, had been suffering very greatly from an abscess, commencing at the root of the second bicuspid tooth and pointing into the antrum. The walls of the antrum were completely necrosed, and the soft covering had sloughed, leaving the dead bone completely exposed.

At the time of my visit the superior maxillary bone, from the symphisis back, was dead, and could be pinched off in fragments by the finger. There was also a constant flow of pus from the right nostril.

Dr. Miles had removed all the teeth on the affected side in his efforts to alleviate her tortures, but it is probable that this was postponed too long to prove serviceable, as sloughing of the soft tissues had already begun.

It was, upon consultation, unanimously determined that the removal of the carious bone should be attempted at once, as it was acting as a foreign substance. We first thought of performing Liston's operation, as described in Erichsen's Surgery, but concluded to adopt a method which would cause less deformity and wound the tissues to a less degree.

With a common gum lancet we dissected away the gum, and then with a pair of bone forceps (scissors-shaped), I divided the dead from the living bone in the mesial line, and found that the progress of the disease had so detached the bone from its normal connections, that I then had no trouble in lifting it out with a pair of straight forceps. A few days after the operation, Dr. Miles took one or two small pieces of bone from the abscess. We had but little hemorrhage, as the superior dental artery had become obliterated by the amount of inflammation that had existed so long. We injected the cavity with carbolic lotion. In one month's time she was perfectly well; the swelling which had closed her eye had all disappeared.

Dysentery in the Village of Union, Iowa.

[We are not sure that the subjoined sketch of the prevalence

of dysentery in the village of Union, Iowa, was written for publication. The subject of the letter is one of great interest and the indications of cure well epitomized. The writer is Dr. J. Lewis.]

"During the month of July our village was the scene of an outbreak of epidemic dysentery, of unusual severity, which continued to rage until late in August. At its first appearance among us, there seemed to be a general prevalence of good health throughout the whole community; no cases of dysentery heard of till it broke out here, but during the hot weeks of August it was prevalent to some extent in nearly all parts of the State. At no place however, did it appear in such violence as in our ill-fated village, where, out of 456 inhabitants 119 were attacked, 70 completely prostrated for a period of six days and upwards, with 11 deaths.

"The village location is bad, being on 'second bottom,' near the Iowa River, on bald prairie, shut in on the north and east by wooded bluffs, on the west and south by a range of prairie hills, the town and its surroundings without shade trees, water none the best, and being a new place, without incorporation, the sanitary condition is utterly neglected.

"The course of the disease was remarkably uniform in all the cases where it was fully developed, making allowance, of course, for individual peculiarities and variation of external circumstances. The *mortality* was chiefly among children under 4 years of age, though the disease was about equally prevalent among all ages. Cases were treated by two regular physicians and *seven 'irregulars;'* but whether regular, homœopathic, 'eclectic,' domestic, or what not, that treatment was most uniformly successful which most nearly filled the following indications:

"To prevent the accumulation of fœcal matter in the intestines and consequent formation of scybala.

"To allay, as far as possible, the tormina, and the urgent tenesmus, and thus give rest to the diseased part.

"To watch and head off complications, and to maintain the strength of the patient till the crisis of the disease was passed, and the appetite began to revive."

"The means resorted to, to meet these indications, were various, and where the indications were disregarded unfavorable results were more frequent. In cases resulting favorably, a crisis seemed to occur pretty uniformly on the seventh or eighth day. The course of convalescence was well marked in most cases.

EXCERPTA.

On the Alleged Dangers of Dentition, and the Practice of Lancing the Gums. By James Finlayson, M.D.

The question as to the relation which the process of teething bears to infantile disease is of practical as well as of scientific importance. If (as one says) "there is scarcely any affection we do not meet with in one case or other of difficult dentition," it must lie at the very root of the study of the diseases of children. If, as alleged by another, "there never is, and never has been since the world began, any such disease," the other view must stand in the way of all true diagnosis. Nor is it merely diagnosis that is involved. Treatment must also vary; and so we find that the operation of lancing the gums is regarded by one as "an all-important remedy," and that he recommends us to "lance the gums *freely and deeply* over the greater part of their extent *daily*, or *even twice a day;*" while another, "convinced from experience of its futility," has "avoided making it a source of revenue." Such statements represent the two extremes, and nearly all intermediate degrees of opinion may be found to exist among ourselves at the present time. It has appeared to me, from personal observation, that exaggerated and superstitious ideas regarding the potency of dentition as a cause of disease are injurious to the study of this branch of medicine, and no less pernicious in their practical operation in the treatment of children. As, however, such opinions prevail in the profession as well as amongst the public, it was thought that an investigation of the doctrine of teething, at different times and in different countries, might throw some light on current ideas. I must, however, pass over much of the inquiry on this occasion.

While some writers seem to see in the local process of dentition an event fraught with danger to the general health and the whole economy of the child, others have regarded the irregularities and peculiarities of dentition rather as manifestations of the general condition of the infant, and have looked upon most of the disasters overtaking infants during the teething period as due to faulty management, to bad nutrition, or to some defect in their own constitution or in that of their nurses, when they were not to be regarded in the light of simple coincidences.

The relationship existing between the quality of the milk and the earliness (and presumably the facility) of dentition, was referred to by Aristotle in two separate passages, and it is probable that it was never altogether lost sight of by subsequent writers. The phenomena of dentition, however, are so striking, and the importance of the teeth for the future life of the infant so obvious, that we need not wonder that great weight was attached to the local process. But, important as this process

without doubt was, some physicians began to see that it was but one of a multitude of changes occurring simultaneously in the history of the child, and many morbid phenomena popularly ascribed to teething could with greater propriety be referred to the other systems more directly concerned. Billard refers the attacks of diarrhœa, so common in children during the teething period, to the development of the intestinal follicles and of the other parts of the digestive system occurring at this epoch. He likewise points out that important changes take place in the brain of the child during the first year of its existence; and he maintains that the frequency of cerebral affections at that age ought to be attributed to these changes; this period, however, is the same as that of the appearance of the first few teeth, and, as the changes in the mouth are much more obvious than those in the brain, they have received the credit of producing the convulsive and other cerebral disorders of infancy. Armstrong also seems to have had some doubts about referring the convulsions of children during the period of dentition to this cause, alleging that convulsions occurred before and after the usual periods of cutting the teeth, and that these fits were of the same character as those which happened during the process. But from what age may symptoms fairly be referred to dentition? The earlier authors seem to have contented themselves with regarding the date at which the teeth usually appear, as the date also of the symptoms referable to this cause; in this view, the seventh month may be named. Soranus suggests that means of mollifying the gums may be commenced about the fifth month, so as to anticipate the dangers of the seventh. But physicians could not forget that symptoms almost precisely the same as those usually attributed to teething were frequently observed before the time for the appearance of the teeth had arrived, and that these symptoms, at whatever age, frequently appeared and subsided without any fresh tooth coming to the surface. To account for such cases, the theory was started that one set of symptoms might be due to the deeper processes of teething—"to the opening of the jaw by the tooth," as Sydenham called it—and another set to the piercing of the gums. This double set of dangers for every one of the twenty milk-teeth affords sufficient scope for the physician to set down any illness he may please during this period as coincident with and dependent on the process of dentition. Accordingly, we find that one writer is convinced of the error of waiting till the sixth month before attributing infantile diseases to teething; another authority begins to suspect teething as the cause of illness after the child has reached his fourth month; a third somewhat arbitrarily assigns the date of birth as the beginning of the troubles of dentition, while Hunter states somewhat vaguely that dentition "is a disease of early age, and, indeed, almost begins with life." It is rather surprising that no writer (so far as I have noticed) has sought to connect distinctly congenital disorders, such as club-foot, with the development of the fœtal

teeth. It is evident that to date teething from birth, as Marshall Hall does, is very arbitrary; and the congenital club-foot may surely with as much reason be referred to the irritation of teething as the similar deformity which results from the so-called "dental paralysis" affecting young children.

With an available range of time, extending almost from the beginning of life up to the completion of dentition, it is evident that it must depend very much on the preconceptions of the physician, as to which, if any, of the numerous ailments of infancy he is entitled to refer to teething. Some, indeed, have sought to substitute for this process other causes nearly equally universal, and they have not found it difficult to refer to them a multitude of diseases usually attributed to dentition. Dr. T. Ballard finds, in the various forms of fruitless sucking, a potent cause of infantile derangement, and it takes the place of dentition so efficiently, that he can make no room for this process in the nosology. Dr. Norton, at the Obstetrical Society, likewise contended that teething ought to be expunged from our list of disorders, and he regarded the evils usually ascribed to dentition as "due to the universal but unphysiological practice of feeding infants on starch food." These two ideas have at least the merit of basing their pathology of infantile disease on some departure from the normal process of nutrition, and if some departure be thus implied, some hope of averting the dangers may confidently be entertained. But there is reason to fear that it is precisely from its fatalistic tendency that the doctrine of teething as a cause of disease is so popular on all hands. If a child die "from its teeth," as is said, it is regarded as having succumbed in the course of a necessary process, for which neither parents nor nurse can be in any way blamed; and, if the doctor have scarified the gums, or ordered whatever is popular at the time, he can join in the self-satisfied lament as if over an inevitable catastrophe.

But, even among those who do not readily wish to acknowledge every illness during the teething period as due to this process, two sets of symptoms deserve special consideration—those which, from their local nature, seem directly related to the teeth, and those which either occur or subside about the time of the appearance of a given tooth. There is a general agreement regarding the possibility of obviously local inflammations being due to dentition; but it is seldom that these can be regarded as so serious as to give rise of themselves to any grave general constitutional disturbance.

With regard to the symptoms of an illness actually concurring with the visible protrusion of the teeth, the possibility of a mere coincidence must be carefully considered. Rilliet and Barthez speak of having seen a difficult dentition concurring so exactly with the development of meningitis, that they were at a loss to know whether the illness were due to a single or to a double cause. They likewise testify to having seen one or several teeth

piercing the gum during the course of meningitis, pneumonia, enteritis, and typhoid fever, without difficulty, and equally without any favorable modification of the course of the illness. This leads us to the consideration of the other phenomenon already referred to, which is, perhaps, even more striking to the popular mind, viz., the more or less sudden subsidence of symptoms about the time of the appearance of a tooth. It must be remembered that, although the distinguished authorities just quoted refer to cases in which teething went on in the midst of grave disease, this is not an usual occurrence; the common event is the cutting of a tooth *after* an illness which may, with great propriety, be regarded as having arrested its progress; and this, of course, may lead to the erroneous notion of the illness having been due to the struggles of the tooth to reach the surface. The cases in which certain symptoms recur with the cutting of every tooth, and without any other obvious cause, constitute by far the strongest argument in favor of their depending on dentition; and, when such an occurrence is well marked and carefully observed, it is not easy to overcome the force of its persuasion. Nor, indeed, is it the object of the present paper to deny that any illness is caused by teething. Its purpose will be served if (as Wichmann says) the reader be kept from attributing too much to dentition, remembering that this is not the only source of infantile disorders during the teething period, and searching carefully for other causes before the illness is referred to a physiological process.

The introduction, or at last the spread, of the practice of lancing the gums may be ascribed to Ambrose Paré. His celebrated case was that of the son of the Duke of Nevers. This child died when eight months old, and the dissection was made and recorded by Bonetus. No cause of death was discovered, except a hardness of the gums, and, on these being laid open, the teeth were found ready to appear. From this it was argued that, if this incision had been made during life, the child might have been saved. Such a remarkable inference received, in the course of time, a still more remarkable confirmation. Called to see a child suffering from teething, and hearing of another whose death had just occurred from the same cause, M. le Monnier was anxious to observe the state of the teeth and gums in such a case. He made a large incision into the gums, but his pathological inquiries were arrested by the restoration of the child to life! But, notwithstanding the experience and authority which accumulated in its favor, incision of the gum does not seem always to have been followed either by the appearance of the tooth, or by the cessation of the symptoms to be relieved. The wound in the gum healed up, and a great question was raised as to whether the scar did not hinder the subsequent progress of the tooth. This fear was alleged to be founded on ignorance; for, it was maintained, "all parts which have been the seat of wounds or sores are always more ready to give way to pressure." This reply,

however, was met by the assertion that, while new cicatrices were easily torn, old ones were not, and that real trouble had been experienced from this cause. The controversy as to the cicatrix is closely related to another question; viz., at what stage of dentition may the operation be performed with advantage? While some warn us not to operate too soon, saying that the operation has no room except when the tooth is far advanced and almost visible, others equally warn us against the possibly fatal delay engendered by timidity or inexperience, contending that, if we wait till the gums are considerably elevated and pointed, "the chief danger and pain are then at an end, and nature is sufficient for the purpose." With the practice of early incision, there is naturally associated the justification of repeated lancing. John Hunter was not ashamed to confess that he had "performed the operation above ten times upon the same teeth," and Dr. Churchill tells of a case in which he "was obliged to use the lancet thirty or forty times, each tooth requiring several operations." Underwood recommends, in obstinate convulsions, the lancing of the gums for five or six days in succession. Even this was not enough for Marshall Hall. "The idea of merely dividing the gum to allow the teeth to penetrate it, is inadequate (he said) to the real importance of this all-important remedy. In order to accomplish all that this measure is capable of effecting, we should lance the gums *freely* and *deeply* over a great part of their extent *daily*, or *even twice a day*, and apply a sponge with warm water, so as to encourage the flow of blood."

The method of cutting the gum was in like manner subject to much variation, alterations probably being made when it was found that the results were not so brilliant as expected. Various instruments, including the finger-nail and the edge of a sixpence, were used; but, when the operation was regarded in a serious light, some proper cutting instrument was usually preferred. But, even when a lancet was used, a few fibres might be left undivided; this was held to account for the want of success too often observed, and was alleged as a reason for the increase, instead of the relief of the pain which sometimes followed the operation. Crucial incisions were preferred by some, not only as dividing the gum more completely, but as allowing more easily of the flaps being dissected up and snipped off with scissors. But even this might not suffice; it was alleged by M. Baumes that a piece of the alveolar process might have to be taken away, so as to remove the obstruction; and the offending tooth itself might even have to be extracted.

The very proposal of such violent measures indicates clearly enough that, in the hands of its most devoted admirers, incision of the gums was far from being so satisfactory as was usually represented; the amelioration of the symptoms which resulted was often more apparent than real, and was frequently found to be but temporary.

The tendency of opinion at present seems to assent to Dr.

West's dictum, that the circumstances in which the use of the gum-lancet is really indicated are comparatively few. Rilliet and Barthez could only recall one case in which any real benefit resulted from the operation, and the best Trousseau could say of it was, that the practice was useless. Even the most sceptical, however, seem to have encountered occasional cases where convulsions ceased on the lancing of the gums; but such a result is also obtained at times from other most unlikely remedies. It may here be stated that, in his careful study of 102 cases of convulsions in children, Dr. Gee could find no reason to believe that the teeth bore any part in the causation of the fits, and in none of the cases did it seem necessary to lance the gums.

But, it may be said, although the benefit may be very doubtful, why hesitate to give any child the chance of profiting, in its peril or suffering, by such a simple operation? and it is very probable that this idea regulates the conduct of many in dealing with infantile disorders. Such a proceeding has very properly been stigmatized as "nothing better than a piece of barbarous empiricism, which causes the infant much pain, and is useless or mischievous in a dozen instances for one in which it affords relief." It may, however, be well to consider shortly whether the absence of danger from lancing is so complete as is usually represented. And here we may call in evidence the great modern upholder of the practice, Marshall Hall himself. He was much too consistent an advocate of his own views to ignore the danger of such frequent tampering with the mouth and gums of an excitable infant as he had himself recommended, and he admitted this disturbance as a real and true objection to the use of the gum-lancet. But this objection—no trivial one when fully considered—is not all. Local disasters have also happened. Passing by as doubtful any injurious influence on the ultimate growth of the teeth, suppuration and ulceration of the gums, and even gangrene, are admitted by its advocates to have been seen after this operation. Dangerous or fatal hemorrhage from lancing the gums, although not likely to be readily recorded, has been published in several cases. Even M. Baumes admits the danger from hemorrhage in incising the gums when much engorged, and he points out that the swallowing of the blood may conceal the extreme peril of the infant. Hamilton, although he had never seen a death from this cause, heard of one on evidence which he could not controvert. Dr. Churchill admits that bleeding from the wound has sometimes been excessive, requiring pressure, astringents, and caustics. Rilliet and Barthez have known it to require plugging. Dr. B. W. Richardson speaks of having "had two or three very painful lessons of this description," and he mentions one death occurring to a country practitioner, and another accident with nearly fatal syncope in his own dispensary practice. Dr. Young (Edinburgh) narrated, a few years ago, two deaths from this cause, which occurred in his father's practice. Fatal hemorrhages have also been reported by Taynton, Ander-

son, Whitworth, Des Forges, and Nicol, and in only one of these cases was there supposed to be any special hemorrhagic tendency.

Further scrutiny of these cases shows, as we might expect, that nearly all these deaths were reported under somewhat exceptional circumstances; so that many more disasters have, doubtless, occurred and have been allowed to slip into oblivion. Without laying undue stress on these perils and calamities, occurring as they do amongst an enormous number of operations, they may well be seriously considered *when the generalization of the treatment is contended for on the ground of its absolutely innocuous character.*

The chief danger, however, of the wholesale use of the gum-lancet lies in its embodying in practice a theoretical view of the ailment, and so tending to close the mind against further inquiry into the diagnosis, etiology, and treatment of infantile disorders.—*British Medical Journal*, Sept. 19th, 1874.

Emulsio Carnis. By James Kemble.

* * * * * * * *

It will be seen by the experiments here made, that raw beef is applicable to every-day practice in hospitals, cities and places where there is access to markets for the beef. Physicians can prescribe the dose to suit their patients, and it will have to be made fresh every three or four days during warm weather. My experiments were made in July, with the thermometer ranging among the nineties. I judge that in cold weather, this preparation could be made to keep good and sweet for a week or more.

I would suggest a formula for general use, as follows, viz:

Fresh Raw Beef (lean)	℥vi
Sweet Almonds, deprived of their shells and roasted	℥i
Bitter Almonds	ʒvi
Sugar	ʒvi
Glycerine	℥ii
Water sufficient for emulsion	Oi

Rub or beat the beef, almonds and sugar, to a fine pulp in a wedge-wood or wooden mortar, then add water gradually until a smooth emulsion is formed, and strain through a sieve or coarse cloth; return the residuary mass to the mortar, manipulate with the balance of the water until f℥xiv are obtained, strain all through a finer strainer, add the glycerine and bottle; the bottle is to be kept well corked. Dose—f℥i, containing ʒiii of the beef.

The physician in prescribing, can order the addition of brandy, pepsin, or any other medicine he wishes to administer at the same time. I tried combining ferric pyrophosphate with the mixture; it combines well, but makes a dark, unsightly preparation, on account of the combination of the iron with the blood contained in the beef.—*Am. Jour. Pharmacy.*

We copy from the *Practitioner*, for August, extracts from an article on monobromide of camphor. We cannot give space to those portions of the paper which give details of experiments showing its physiological action upon inferior animals, and consequently reprint only the observations of its therapeutic properties.

Physiological and Therapeutical Researches on the Monobromide of Camphor. By Dr. Bourneville.

The monobromide of camphor, $C^{10}HO^{16}Br$, derived from bromide of camphor, $C^{10}H^{16}OBr^2$ is a white, solid substance, presenting the form of rather long transparent prisms, with a slight smell of bromine and a marked one of camphor.*

* * * * *

From all my experiments, amounting to about forty, and which entitle me to attribute powerful sedative properties to monobromide of camphor, I believe I am justified in drawing the following deductions—

1. *Monobromide of camphor* diminishes the number of *beatings* of the *heart*, and *determines contraction of the blood-vessels of the ears and eyelids.* 2. *It diminishes the number of inspirations.* 3. *It lowers temperature in a regular and constant manner.* In fatal cases the lowering increases till the end. In those which recover, the lowering is followed by an elevation of temperature, which returns to its initial figure, but in a longer time than that during which the lowering was effected. 4. *Monobromide of camphor possesses undeniable hypnotic properties. It seems to act principally on the cerebral nervous system.* 5. *It does not seem that the medicament is got accustomed to; and its protracted use determines rather rapid loss of flesh in cats and guinea-pigs.*

II. Therapeutics.

If I am to trust to the result of my numerous researches on the subject, medical publications on the therapeutical employment of the compounds of camphor and of bromide have been until now exceedingly rare. The first by date seems to be that of M. Deneffe (of Ghent). In a short but very interesting paper,* he announces that monobromide of camphor is "an excellent sedative of the nervous system," and in support of this he relates a case which I will briefly summarize.

Mr. N., age 38 years, addicted to drink since several years. When summoned to him, M. Deneffe described the following symptoms: Trembling of the fingers and hands, rendering writing difficult; extraordinary agitation; irrepressible requirement to move about from one place to another; very irritable charac-

* On Monobromide of Camphor and its application to Therapeutics. (*Presse Médicale Belge*, 1871, p. 405.)

ter; malaise; prostration. Every evening there was an exacerbation of the symptoms. Sleep would not come, and Mr. N. would pass the long hours of the night tossing about in bed; it was only very late in the night that he could get a little sleep. Towards four o'clock in the morning the agitated anxious sleep would be suddenly broken, and the patient would jump up in bed, bathed in sweat and not knowing where he was. Delirious, fantastic conceptions then took possession of him; he would fancy himself on the top of a high mountain, and feel himself suddenly rolling down an abyss; men and wild beasts pursued him. The room was full of unknown personages and animals, all more extraordinary the one than the other. And all this phantasmagoria, like a clouded sky, would change every minute, but constantly present the same hideous and alarming aspects. It was only on flying from bed that the patient could manage to get a little rest. The visions would then leave him, and, excepting the constant general excitement, he was free from nightmares till the next day. Pulse frequent, excited; eye, shining; loquaciousness. Delirium tremens appears evident. Prescription: Take every hour a pill containing 3 grains of monobromide of camphor.

The very next day, after having taken twenty of the pills, the patient was very much better. Excitement less; eye less shining; speech less rapid. The night had been better, and had been accompanied by less of nightmares and visions. During three days Mr. N. took 3 or 4 grammes (45 to 60 grains) in the shape of pills in the course of each twenty-four hours. The amendment became more and more marked every day; sleep came back, the visions and dreams disappeared entirely, and the tremulousness of the hand completely removed. During the eight days which followed recovery from these nervous phenomena, M. Deneffe continued the administration of the remedy in doses of from 20 to 45 grains daily. The recovery remained perfect.

Having had knowledge of the above case, Mr. W. A. Hammond (of New York) got some monobromide of camphor prepared by Professor Maisch, of Philadelphia, and administered it to several patients. He thus sums up his opinion on the therapeutical advantages of this medicament—

"My experience with the monobromide of camphor, though thus far limited, is eminently satisfactory. I have employed it in two cases of *infantile convulsions* due to the irritation of teething, with the effect in each instance of preventing the further occurrence of paroxysms which, previously to its administration, had been very frequent. In each case a grain was given every hour, rubbed up with a little mucilage of acacia. Three doses were sufficient in one case, and two in the other. The children were aged respectively fifteen and eighteen months.

"In a very obstinate case of *hysteria* occurring in a young married lady, in the form of paroxysms of weeping and laughing, alternating with epileptiform and choreiform convulsions. I

gave the monobromide of camphor in doses of 4 grains every hour. The influence was distinctly perceived after two doses were taken, but ten were necessary to entirely break up the attack. This was a very favorable result, as all previous seizures had lasted for from five to eleven days uninfluenced by medication or moral suasion. I have also employed it with excellent effect in several cases of *headache* occurring in women and young girls, and due to mental excitement and excessive study. One dose of 4 grains was generally sufficient to cut short the attack. In two cases, three doses at intervals of half an hour were necessary."*

I will now succinctly relate the results, as yet incomplete, which I have obtained at the hospice of La Salpétrière, in the wards of my beloved master, Professor Charcot.

Insomnia. 1. Mrs. ——, age 62, affected with disease of the heart, and suffering from persistent sleeplessness. I gave her, at bed-time, one, and afterwards two pills, each containing 2 grains of monobromide of camphor. This small dose quite sufficed to produce sleep.

2. Mart. (Elizabeth), age 46.—*Progressive Locomotor Ataxy.* Insomnia alternating with sleep agitated by nightmares. She speaks aloud, cries out, &c., in her sleep. Under the influence of 6, then of 8 grains of monobromide of camphor, she slept much better, was more composed, and no longer waked up her companions in the ward.

I incline to believe that monobromide of camphor will be of great service in combating insomnia, especially that due to cerebral hyperæmia (W. H. Hammond); as, at the post-mortem of the vast majority of animals which died slowly after strong doses, I have always found absence of congestion of the cerebro-spinal integuments, of the encephalon, and of the cord.

Paralysis Agitans. I have begun giving the monobromide of camphor to three women affected since a long time with paralysis agitans. The only modification which I have observed until now consists in diminution of agitation and a slight increase of sleep.† Before knowing exactly what we must think of the action of the drug on *tremulousness* it is necessary to prolong the treatment during several weeks and increase the strength of the doses.

Chorea Mat. ——, age 64. The disease first manifested itself at the age of 40. Has been addicted to excess of drink and been incapable of any work from the age of 55. Walking has become impossible for a year past; agitation and horrid faces, continual. Every day she falls out of bed. Bad sleep. Though the patient has been taking pills of monobromide of camphor for only a fortnight, the restlessness has already considerably diminished. She sometimes will remain comparatively easy for fifteen or twenty minutes together. She no longer falls out of bed. In this case, as in the

* Note relative to the Monobromide of Camphor. *New York Medical Journal*, 1872, p. 522.

† M. Charcot has insisted with care on the insomnia of these patients. Generally, they sleep only a few hours, and wait for daylight with impatience.

patients affected with paralysis agitans, therapeutical experiments must be carried on for some time; and, on account of the oldness of the affection, only more or less slight amendment can be expected. However, even at present, comparative calm has been the result.

Hystero-Epilepsy. In these cases, as well as in the following ones, the treatment was commenced on April 22. As I was not yet well acquainted with the medicament, I began with doses which were evidently too weak: 1 gr. during eight days, 2 grs. during six days, and so on; and I reached 16 grs. only on July 10. In such cases, I think it advisable to begin with four or five pills of 2 grs. each, making up 8 or 10 grs. daily, and to add 2 grs. to the dose every five days. The strongest dose which I have as yet prescribed is *fifteen grains*, but I believe it can be carried further. These preliminary statements were, I think, necessary to enable us to appreciate the results which I am going to analyse. As an element of comparison and appreciation I will describe the fits which took place during the two months which preceded treatment and during the two months in the course of which my patients took monobromide of camphor.

1. Lang, 18 years: March and April, five attacks; May and June, two. 2. Bug, 23 years: March and April, eight fits of hystero-epilepsy, and sixty-three attacks of hysteria; May and June, seven attacks of hystero-epilepsy, and twenty attacks of hysteria. Notable amendment. The patient is much easier, works better and more constantly.

Epilepsy. I must first of all state that I had to deal with very old standing cases of epilepsy. 1. Ducre, 28 years: March, April, fourteen fits, six vertigos; May, June, twelve fits, four vertigos. Slight amendment. 2. Aub, 17 years: March, April, five fits, eight vertigos; May, six fits, three vertigos; June, *no fit*, six vertigos. 3. Fonill, 12 years: March, April, five fits, 171 vertigos; May, June, six fits and 121 vertigos. In this case the giddiness only was amended. 4. Ray, 28 years: March, April, seventeen fits, three vertigos; May, June, ten fits, six vertigos. Fits amended. 5. Lob, 25 years: January to March, sixteen fits, forty-seven vertigos; April, May and June, six fits and six vertigos. In this case the amendment seems very manifest. The above statistics, such as they are, point to a good result. They are only data, but sufficiently satisfactory to encourage the practitioner in using monobromide of camphor in epilepsy.

PHARMACOLOGY. W. A. Hammond thus expresses himself—"The monobromide of camphor may be given in the form of pill, with confection of roses as the excipient, or as a mixture with mucilage of gum arabic and syrup. The dose for adults ranges from two to five grains."

I first administered the following pills—

Monobromide of camphor, gr. ij.
Gum and sugar, q. s. for one pill.
From one to ten daily.

At present I make use of the following preparation, due to Dr. Clin, former house-surgeon to the Paris hospitals—

Monobromide of camphor, gr. ij.,
Sugar, q. s. for converting into *dragées.*

These *dragées* are nothing else but tiny pills, in which the medicament occupies the centre, whilst a coating of sugar preserves the substance from the contact of air, and on the other hand facilitates its administration to patients. They melt easily in the stomach.

These various preparations are readily taken by ordinary patients; but in some special affections, such as tetanus, epilepsy, hydrophobia, &c., they would not be convenient; so I have endeavored to find out a solution which could be injected under the skin. The first solution that I made use of for my experiments had the inconvenience of producing abscess; but after some searching I was able to find out one which answers very well. It consists of monobromide of camphor 45 grs., alcohol 9 drachms, and glycerine 4½ drachms.

After having made sure that it brought no local accidents in animals, I made use of it in two aged epileptic patients. Each of them had already had six injections of from 40 to 55 drops, made either into the thighs or buttocks. Not the slightest accident has occurred. I believe I can therefore safely recommend its use. It is needless to add that the punctures must be watched over, and that no more than thirty or forty drops should ever be injected into the same situation, especially as concerns children (hooping-cough, chorea, &c).

Such are the present limits of my researches. I am fully aware of the shortcomings which they yet present; but I would be glad if the indications that I have been able to lay down were to lead other medical men to study the action of a drug which seems destined to do us some service.

Morphine-Poisoning successfully treated by Atropia and Electricity.

The following narrative of the successful treatment of a serious case of morphine-poisoning, of which I was personally the subject, seems to me to have a professional interest that renders it worthy of record.

On the morning of March 25th, on sitting down to breakfast at eight o'clock, I took, from a bottle of the size and general appearance of a one ounce quinine-bottle, a powder which I believed to be sulphate of quinine. It was removed from the bottle upon the end of a breakfast-knife, the quantity estimated to be some six or seven grains, my only anxiety being to take a sufficiently large dose. It was stirred up in a little cold water, and at once swallowed upon an empty stomach. To a young lady visiting in the family, I also gave what I estimated to be three grains;

and to my daughter, full grown, I gave somewhat less than two grains: both these had half finished breakfast.

Almost immediately after swallowing the dose, I became conscious of a certain amount of stimulating influence on the brain, a sensation not unlike the early physiological effects of quinine, and remarked that I had never known quinine to act so promptly upon my system before; recalling to mind instances in which small doses were reported as having acted almost immediately upon patients. At the same time I became conscious of a peculiar sensation about the fauces, an idiosyncrasy which I have all my life observed as following the slightest application of morphine to the lips. Strange as it may seem, both these circumstances were disregarded, and, more than that, the young lady to whom I had given the larger dose read "Sulphate of Morphine" upon the label of the bottle from which the powder was being taken without realizing its import. I ate very little breakfast. Occupied by morning duties, little notice was taken of the head-symptoms, of which I was all the time conscious, until my daughter remarked that she was suffering from nausea, as she always did after taking morphine. On accidentally overhearing this, and hastening to the breakfast-room, I read, to my dismay, upon the bottle, "Sulphate of Morphine."

By reference to the watch, I found just three-quarters of an hours had elapsed since the dose was swallowed. Realizing the gravity of the accident, I set about making such preparation as I knew to be necessary for the struggle for life that was to follow. From a scruple to a half-drachm of sulphate of zinc was immediately swallowed in solution, which very promptly produced a degree of nausea but no vomiting. In a few minutes the dose was repeated with absolutely no effect. By this time the influence of the morphine had stolen over the whole system, and was felt especially in the lower extremities. I had sent for my friend Dr. Taylor, but before his arrival had injected into the arm thirty drops of a solution of atropine, of one grain to the ounce of water. The antagonistic influence of the atropia was almost instantaneously perceptible. The effects upon the brain and nervous system were so marked, that I could not refrain from expressing to my family my admiration of the action of the remedy. But, alas! this was of short duration. Probably within two or three minutes the morphine got the mastery, and I immediately injected thirty drops more of the atropia. The absorption was now going on rapidly. Dr. Taylor had by this time arrived, and, having injected another dose of atropia, urged the trial of mustard and water. A large quantity of this and clear warm water was swallowed, and there was a very copious return of the emetic without a particle of food. The impression conveyed to me was that only the upper portion of the stomach contracted, and that the nerves of the lower portion, into which the morphine had been directly received, were paralyzed by its local action. Such partial action of the stomach, the emetic alone re-

turning, is not uncommon, as we all know, when vomiting is sought to be excited for the removal of ingesta. No perceptible effect followed the later injections of atropia.

At my request, Dr. Taylor wrote for a fresh solution of atropine of two grains to the ounce, and we weighed out a portion from the morphine-bottle, which I judged to be about equal to the quantity taken, and found it to be fully six grains.

That the electro-magnetic machine might be in readiness when it should be required, I had it brought out. Fortunately it had recently been put in order, but the action of one cell was feeble. A second cell that was accidentally in my possession was brought to me, but much embarrassment was experienced in coupling them. It was almost impossible to keep the attention fixed, though stimulated by the apprehension that others might not understand the mode of connection of this particular instrument, and that a failure might be fatal to myself. It appears, however, that the necessary connections were correctly made. With the help of others, I now hunted up the case of morphine-poisoning described in the March number of the *New York Medical Journal*, with the idea that the mode of application of the electric current there adopted might furnish to those having the case in hand some useful suggestions. The last recollections are of returning to the instrument, under the impression that it was not acting, though it was in perfect operation, and, with a feeling of despondency and indifference, soon abandoning it. After a few walks about the first floor of the house, the inferior extremities doubling under at every step, and within a few minutes of leaving the instrument, absolute unconsciousness supervened. This occurred, as nearly as can be fixed, at ten o'clock. It is interesting to note here, how, under a vigorous exercise of the will in one direction, notwithstanding a most urgent desire to succumb to sleep, a quite efficient control of the intellectual faculties was retained to the very verge of complete unconsciousness.

On the supervention of unconsciousness, Dr. Taylor sent to the city for assistance, having made a subcutaneous injection of thirty minims of the two-grain solution of atropia. He also caused the free administration of brandy and of strong coffee, and I was kept walking, supported by relays of assistants, until all power of motion was gone, and this occurred about eleven o'clock. A short time before this, however, spontaneous vomiting occurred while standing erect, to a most profuse degree, directly after which Dr. Taylor injected thirty minims more of the two-grain solution.

At 11.45 Dr. A. A. Smith, of No. 38 East Twenty-ninth street, New York, arrived, in the temporary absence of Dr. Taylor, and on seeing the gravity of the case, sent at once for additional counsel. To Dr. Smith's experience and skill in the use of the electric current, I feel that the successful result is in no small

degree due. Dr. Smith has kindly furnished the following notes of the case as observed by him during the time of his stay.

"I saw Dr. Trask at 11.45 a. m. He was profoundly comatose; all efforts to arouse him did not avail. The skin was moist, the pulse 124, feeble, and somewhat irregular; muscular power was completely gone; pupils moderately dilated (which I knew was due to the sulphate of atropia administered); respirations 13 and irregular. I was at once struck with the character of the respirations. The inspirations were scarcely perceptible, the expirations very long and given with a groan, certainly suggestive. The countenance was of a leaden hue.

"I asked for and obtained a battery (Kidder's make, two-celled, faradaic current). The battery worked admirably. I applied the poles to the phrenics, one to each, just above the clavicles, at first with a weak current, gradually increasing it until it caused him to take a deep, spasmodic inspiration. I found that the strength required was such as gave me great pain when I held the sponges in my own hands. This was continued for about twenty minutes, when I began to stimulate him, giving teaspoonful doses, of equal parts, of brandy and water. The first spoonful he was unable to swallow. The battery was continued, also irritating the hands and feet, which soon caused some movements more than reflex.

"Within half an hour the pulse became less rapid and stronger, the countenance assumed less of the leaden hue. More brandy was offered, which produced a reflex action, and caused him to cough and inspire deeply afterwards. He swallowed some of the brandy, and after this, brandy and water was given about every fifteen minutes, and the reflex action produced by the attempts at swallowing aided in the great indication of oxygenation of the blood.

"Dr. Taylor returned at about 12.15 p. m., and we agreed upon our line of treatment, viz., to keep up the influence of atropine, taking the pupils as our only guide, keeping them moderately dilated, stimulation, and the battery, not allowing the body to remain in one position more that fifteen or twenty minutes at a time; in this way guarding against pulmonary congestion and subsequent pneumonia. At 12.30 p. m., respiration was 14, perfectly comatose.

"At 2 p. m., Dr. Austin Flint saw him. The pulse was then 108, respiration 18; muscular movement was returning, and by great effort he was aroused and seemed to recognize Dr. Flint; previous to this the eyes had been two or three times opened, and he made efforts to talk. One pole of the battery was applied to the phrenic in the neck, and the other to the diaphragm, but this did not cause the spasmodic inspirations so well as when one pole was applied to each phrenic in the neck. We irritated the muscles by applying the poles to other parts of the body, as one to the brachial plexus, and the other to the forearm or hand, to the chest, abdomen, etc. The feet and hands were constantly

irritated. I left the doctor at 4.10 p. m., and by great effort succeeded in getting him to shake hands on parting.

"In two cases in which I have had the opportunity of observing the antidotal effects of sulphate of atropia in opium-poisoning, I was led to conclude that from the one thirty-second to one twenty-fourth grain of atropia counteracted the effects of a grain of morphine."

About the time of Dr. Smith's departure, active delirium began to manifest itself whenever aroused from stupor, the brain being occupied with the idea of driving horses, etc., this excitement being soon followed by a relapse into stupor. This was repeated as often as the patient was aroused, and continued unabated until about 6 p. m., gradually subsiding to about 8 p. m., when this delirium disappeared. During this period, from 4 to 8 p. m., I have distinct recollections of being highly incensed at the means taken to prevent sleep; the act of swallowing was also very difficult and irksome. I was distressed, during these brief periods of consciousness, by the impression that I had become insane, and that the watchfulness of friends was due to this. As the delirium subsided there was constant picking at objects seen in the air or lying around. From 7 to 10 p. m., almost continuous efforts of friends were required to keep me awake, notwithstanding there was a decided desire to engage in conversation. By 12 p. m., I was able to relate to a medical friend quite a lengthy medical case without once losing the thread of the story. Directly after this I was allowed to go to bed. Sleep was much broken, the intellect wandering. During the following day the distinctive symptoms of atropia-poisoning were very marked. There was a degree of indistinctness of vision, a heaviness of the eyelids that rendered it difficult to keep them open, and visions of beautiful scenery and brilliant colors, with grave and fantastic figures of human beings, immediately upon the eyelids being closed. There was also a continued sense of weariness, as well as a very disagreeable viscidity of the fauces that rendered swallowing irksome. All these symptoms disappeared suddenly after a refreshing sleep in the evening. I feel certain that all the symptoms due to the morphine disappeared by 12 o'clock, or sixteen hours after the morphine was swallowed, and that the influence of the atropia upon the cerebrum lasted at least about eighteen hours longer. There was a copious alvine dejection on the following day, and also very free diuresis on the evening of the accident, and during at least twenty hours after the atropic symptoms above described had passed away, both due unquestionably to the atropia. Subsequently, there was extreme torpor of the bowels. A very marked prostration of the nervous system followed this accident. For two or three days the digestive system participated to a degree, rendering care necessary in the selection of food, and the frequency with which it was taken. The appetite soon became excellent, and abundant nourishment and stimulus were taken with very little increase in general strength

The nerves of animal life seemed to have suffered a violent shock, from which they were slow to rally. At the end of two and a half weeks, there having been but little gain, a sea-trip was advised, and a two weeks' absence in cheerful travel perfected convalescence.

This case is interesting as one of recovery from a dose of morphine abundantly sufficient to destroy life, and there can be no doubt that the agents employed determined the favorable result. The morphine had been taken suspended in water, of which also a sufficient amount was immediately afterward taken to secure solution in the stomach—the stomach, moreover, being empty; and but a very small quantity of food was afterward swallowed. The morphine lay undisturbed in the stomach certainly until after the mustard-emetic took effect, a period of at least an hour and a quarter, and it is questionable if any of the morphine was ejected even by the action of the emetic. It is certain that a sufficient amount of morphine was absorbed to induce a very dangerous degree of narcotism.

The antagonistic influence of atropia was here very plainly proved. The recognized influence of the first dose in clearing up the mental faculties has already been referred to; and, though this was almost immediately overcome by the advancing narcotism, there can be no question that, through it and subsequent doses, the system throughout the narcotism was under the influence of the atropia also. This was shown by the dilated condition of the pupil throughout the day, and the early supervention of delirium. The atropia must, therefore, have been somewhat in excess of the morphia. Moreover, the symptoms of atropia-poisoning survived those of morphine by about eighteen hours; but this might have been simply due to the longer duration of the action of atropia on the human system. The duration of the more profound stage of narcotism was also much less than if the morphine had been left to its undisputed influence on the system. I think it must be a very unusual circumstance for the coma, etc., to subside so soon. This case thus affords additional confirmation of the antagonistic influence of these two agents; and, that such confirmation is even yet needed by the profession, is shown by the skepticism that still prevails with many on this point; men eminent in the profession having, since this occurrence, assured me that they had hitherto had no confidence in such antidotal effects.

A very important question arises as to the antidotal equivalents of these two powerful agents. It is by no means a matter of indifference how much of the one may be employed in order to neutralize a given quantity of the other, for it is quite possible that the patient might be destroyed by the very agent used to save him.

There were introduced subcutaneously, in the present instance, in the aggregate, seven-sixteenths of a grain of atropia to counteract about six grains of morphine practically swallowed in

solution, and remaining in the stomach at least one hour before the administration of the atropia was commenced. This is about one fourteenth of a grain of atropia to one grain of morphine, and, from the effects of the repeated doses, the atropia was regarded, by those having charge of the case, as being in excess. Dr. A. A. Smith gives, as the result of two cases, his impression that one grain of morphine is antagonized by one twenty-fourth to the thirty-second of a grain of atropia. Dr. S. Weir Mitchell, in a note with which he has favored me, states that he has given three one-third-grain injections of atropia in poisoning by four grains of morphine, and his patient recovered.

In the *American Journal of Medical Science*, July, 1865, Dr. S. Weir Mitchell and colleagues have published a highly instructive paper upon the antagonism of morphine and atropia in *medicinal* doses. They show that experiments upon animals subjected to the action of these poisons cannot be relied upon as indicating the operation of the same poisons upon the human system. Their extensive experiments upon the human subject demonstrate—

1. That while morphia, in medicinal doses, lowers the pulse slightly or not at all, atropia lowers it a few beats in ten minutes, and then raises it twenty to fifty beats within an hour. Morphia has no power to prevent atropia from depressing the pulse, so that as regards the *circulation* they do not counteract one another.

2. As regards the *eye* the two drugs are antagonistic.

3. The *cerebral symptoms* caused by either drug are, to a great extent, capable of being overcome by the other.

4. The nausea of morphine is not prevented or antagonized by belladonna.

In short, "as regards toxic effects upon the cerebral organs, the two agents are mutually antidotal, but this antagonism does not prevail throughout the whole range of their influence, so that in some respects they do not counteract one another, while, as concerns the bladder, both seem to affect it in a similar way." These gentlemen estimate that, as a general rule, one-fourth grain of morphine will neutralize for a time one-thirtieth of a grain of atropia, but the latter continues to act far longer than the former.

In the *American Journal of Medical Science*, for July, 1866, p. 270, is reported an extremely interesting case of poisoning from the swallowing of a solution of one-sixth to one fourth of a grain of atropia. The symptoms were extremely urgent, and, notwithstanding free venesection, five men were unable to restrain the patient. In ten minutes after the injection of one-fifth grain of acetate of morphine into the temple, he was perfectly quiet, and remained so for about one hour, when the excitement began to return. Two hours after this, one-fourth grain of acetate of morphine was injected. In seven minutes perfect calm followed and the patient recovered. Taking one-fifth grain of atropine as the average between one-fourth and one-sixth, this quantity was in

this instance neutralized by nine-twentieths of a grain, or something less than a half grain, of morphia. In atropia-poisoning, a dose of morphine that will neutralize for a time the atropia may require to be repeated several times, from the longer duration of the action of the atropia.

If it be asked if the doses of the two drugs may be regarded, in cases of poisoning, as reciprocal, that is, if, as in one case just cited, one-fifth grain of morphine controlled for the time one-fifth grain of atropia, would one-fifth grain of atropia be required to control the effects of one-fifth grain of morphine? the answer must be, plainly, No; experience showing that a very much less amount would suffice. Again, if in this case the practitioner had looked for direction to the estimate of Dr. Mitchell and his colleagues, viz., one-fourth grain of morphine to neutralize one-thirtieth grain of atropia, he would have felt called upon to increase sixfold the dose actually employed and proved to be sufficient. The difference of the mode of administration is, of course, to be considered, as given hypodermically or by the stomach.

It is evident that this is a subject upon which we need more positive information, and is therefore a field that would reward patient exploration. In the mean time, as a practical rule, in morphine-poisoning, it is better to administer the atropia hypodermically, in moderate doses, and to repeat it until the pupils are sensibly dilated, and to keep them dilated until the opium-symptoms disappear. There can be no advantage in carrying the atropia beyond this point. As an approximate guide, we have Dr. Mitchell's employment of a total of one grain of atropia to antagonize four grains of morphine, my own experience of a total of little less than a half grain of atropia against six grains of morphine, and Dr. Smith's estimate of one thirty-second to one twenty-fourth of a grain of atropia to every grain of morphia. By patient examination of medical journals doubtless many similar experiences could be found recorded, and something like a rule of practice established. In the case reported in the March number of this journal, it would seem that the full advantage of the atropia was not attained, only one-twentieth of a grain of atropia being used against seven grains of morphine, the attendant apparently preferring to rely upon the use of the battery, which was certainly very skillfully applied; but the protracted duration of the case is noteworthy.

The extreme prostration of the nervous system accompanying the convalescence in my own case has already been mentioned. This was especially remarked in the lower extremities, which from first to last seemed to suffer most from debility. It is an interesting inquiry, to what was this prostration, especially the weakness of the lower extremities, due—to the morphine, to the flagellations and other means practised to excite sensibility, including the action of a powerful battery, or to the atropia? As regards morphine, there is no evidence, so far as I know, that large doses of it are followed by extreme and protracted prostra-

tion. The muscular fatigue resulting from the causes just enumerated ought to have soon passed away. In relation to belladonna, I can find but little bearing on the question. Stillé (vol. ii., p. 35) says, among other symptoms following the application of one-sixth of a grain of atropia to a freshly-blistered surface: "The power of locomotion is sometimes lost, and the limbs feel as if asleeep. General sensibility is impaired or destroyed....Meanwhile *consciousness may be perfect, although the inability to move is complete.*" On the whole, I am inclined to charge the debility, especially that of the muscles of locomotion, to the influences of the atropia; and, if I am correct in this, it furnishes an additional incentive to discover the least dose that will neutralize the cerebral symptoms of a given quantity of morphine.

An interesting circumstance, daily noted by me throughout convalescence, was greater comparative feebleness in the early part of the day, notwithstanding the regular and free use of nourishment and stimulants throughout the night. This daily cycle, so far as I could discover, was entirely independent of all external influences. The same thing has often been observed in women suffering from nervous prostration, more particularly in diseases peculiar to their sex, but I have always regarded it as due to the failure of a supply of nourishment during the night.

The importance of the faradaic current in keeping up the respiration in cases of opium-poisoning cannot be over-estimated. Both in Dr. Schweig's case, as reported in the March number, and in my own case under the direction of Dr. Smith, the application of the electrodes over the phrenic nerves, just above the clavicle, gave, of all modes tried, the best results. In Dr. Schweig's case the patient was evidently saved by the electric current alone. It would seem not too much to affirm that, by the hypodermic injection of atropia and the judicious use of a *sufficiently powerful* faradaic battery, very few cases of opium-poisoning need prove fatal. It should be further stated that the young lady who swallowed three grains of the poison by ten o'clock, was quite seriously affected, but was kept walking in the open air for from three to four hours before she could be allowed to rest. In my daughter's case active exertion and mental anxiety prevented the development of narcotism.

The International Sanitary Conference and Quarantine.

The International Sanitary Conference in Vienna, which has recently terminated its meeting of a month's duration, has, besides agreeing to a series of resolutions on the origin, nature, and contagion of cholera, discussed the agent of quarantine. The following are the rules on which they have agreed.

"1. In seaports in which there are no quarantine institutions,

as in those of the Red and Caspian Seas, a sanitary authority shall be constituted, consisting of medical men and Government officials, with a staff of assistants. The number of members of the Board belonging to each class is to be regulated according to the amount of shipping intercourse in each port; but, under all circumstances, it must be sufficient to ensure a speedy dealing with the ship and crew, as well as with the passengers. The chief of the sanitary authority must keep himself in official communication with cholera-infected ports, which may come into contact, through intercourse by ships, with that to which he belongs.

"2. Every ship arriving from an unsuspected port, which the captain shall have declared on oath neither to have touched at an infected port nor to have had direct communication with an infected vessel, and on board which no suspected or distinct cases of, or deaths from, cholera shall have occurred during the voyage, is to have free *pratique.*

"3. All ships coming from infected or suspected ports, as well as those from uninfected ports, which during the voyage have touched at an infected place, or held any communication with an infected ship, or on which during the voyage suspected cases of illness or death from cholera have occurred, shall, on their arrival, be subjected as soon as possible to a strict medical examination as to the state of health of their crew and passengers, the captain, ship's officers, and doctor (when there is one) being bound to report to the examining doctor any suspicious symptoms of illness on board which may have come to their knowledge. If the medical investigation show that no case of illness in any way suspicious exists among the crew and passengers, the ship with all that it carries to be immediately admitted to free *pratique.*

"4. The bodies of those who have died of cholera, as well as all cholera patients found on a ship newly arrived, are to be taken on shore; the former immediately buried, the latter placed in a hospital always kept in readiness for such cases, or, when there is not one, in some house or barrack or isolated place, or in as isolated place as possible in the country. Should any suspected cases of cholera or deaths have occurred during the voyage, or should suspected or decided cases of cholera, or dead bodies whose death is traceable with probability or certainty to cholera, be found on board a ship on its arrival in port, the crew and passengers, after the removal of the sick and dead, shall undergo a process of cleansing and disinfection under the supervision of the Board.

"5. At the same time, all the clothes worn by the healthy individuals, or used by them during the voyage, as well as all their effects, are to undergo a thorough disinfection in a room arranged for the purpose, under the superintendence of the Board. After the disinfection, their effects will be given back to their possessors, who, in the meantime, will have taken a cleansing bath, and they will then be perfectly free.

"6. After the withdrawal of all persons, except those absolutely necessary for the service of the ship, any vessel arriving under the conditions named in section 3 is to be subjected in all its compartments to a thorough disinfection.

"7. The goods landed from the disinfected ship, even the rags and objects used by the cholera patients, after being properly disinfected, are to be considered innocuous."

A commission was appointed to discuss international regulations for the quarantine establishments in those States which intended to erect such, consisting of Drs. Hirsch (Berlin), von Alber Glanstäten (Austria), Seaton (England), Fauvel (France), and Bartoletti (Turkey). The report, which, without any important alterations, was accepted by the Conference, states that quarantine against the spread of cholera is applicable on sea to ships coming from infected ports. The following rules are then laid down.

"1. Vessels from infected ports must undergo an observation quarantine, which, according to circumstances, may last one to seven full days. In the Oriental ports of Europe and elsewhere, though only in certain exceptional cases, the surveillance may be prolonged to ten days.

"2. When the Board of Health have sufficient proof that, during the passage, no case of cholera, or anything like it, has occurred on board, the surveillance is to last three to seven days, reckoned from the medical inspection. If, under these circumstances, the voyage has lasted at least seven days, the surveillance is to be limited to twenty-four hours, to give time for the examination and disinfection considered as necessary. In cases under this category, the observation quarantine may be held on board as long as no case of cholera or suspicious circumstance occurs, and when the hygienic conditions of the ship allow of it. In these cases, the unloading of the ship for disinfection is not necessary.

"3. When, during the passage or after the ship's arrival, cases of real or suspected cholera occur, the surveillance for those who are not ill is to last seven full days; beginning from their isolation in a hospital or whatever place is assigned to them. The sick will be disembarked and properly attended to in a place separated from the persons under surveillance. The ship and all objects to undergo a thorough disinfection, after which those persons obliged to remain on board will be subjected to surveillance for seven days.

"4. Vessels from suspected ports, that is, such as lie near a port where cholera prevails, and are in intercourse with it, may be subjected to observation quarantine, which must not last more than five days, if no suspicious circumstance happens on board.

"5. The emigrant and pilgrim ships, and in general all vessels whose character is deemed especially dangerous to the public health, may, under the above-named conditions, be made the

objects of particular regulations, which the Board of Health will decide.

"6. When the conditions of a place do not allow of the prescribed regulations being carried out, the infected ship is to be despatched to the nearest hospital, after it has received all the assistance that its condition may require.

"7. A ship coming from an infected port which has touched at a port *en route*, and there being allowed free *pratique* without any quarantine, will still be regarded and treated as coming from an infected harbor.

"8. In cases of mere suspicion, the disinfection regulations are not prescribed, but they may be applied when the Board of Health consider them necessary.

"9. In ports where cholera is epidemic, especial quarantine is no longer to be kept, but only means of disinfection to be applied."

With the resolution to abolish river quarantine the stipulation was made that river boats, when cholera patients are found on them, should undergo a thorough inspection. At the same time, the intercourse between both banks to be kept open. When the *debouchures* of rivers are navigable for sea vessels, these are to be subjected to the rules for sea quarantine.

With regard to the establishment of an International Sanitary Commission, the Conference advanced seventeen articles as *desiderata*, the most important of which are the following.

"An International Epidemic Commission to be organized. To be permanent, and have its seat at Vienna. Its members to consist of delegates from all the Governments taking part in it. A president to be chosen from among them as representative of the Commission, who will correspond directly with the different Governments, and eventually with the various Sanitary Boards. The International Commission to be independent in its scientific labors, but administrative questions to be decided by a majority, every State having a vote in all final decisions. The ordinary routine of business to be settled by the Board itself. The Commission to be bound to communicate to their respective Governments the results of their scientific and administrative labors. The necessary computations of cost to be made by the Board and submitted to their respective Governments. The expenses to be covered by the contributions of the different States, and eventually by receipts. The sum total required to be apportioned among the different States with reference to the numbers of their population and the tonnage of their commercial fleet. The objects of the International Epidemic Commission to be the furtherance of the study of epidemics among mankind, proposals for the erection and administration of quarantine establishments; professional opinions in answer to inquiries from any Governments represented in the Commission, the arranging of international conferences as often as necessity requires, and the drawing up of their programmes. Where epidemics are constant,

permanent stations to be established, and, where they exist only for the time being, temporary arrangements to be made for the study of the epidemic on the spot, and inquiry into means of protection from it. In the same manner, during any great epidemic, arrangements to be made to follow it from place to place, with the object of studying the laws of its dissemination. For those territories which have no organized sanitary service, such epidemic stations to be established with the consent of the Governments to which they belong, on the principle proposed by the International Commission of assisting such countries in times of severe epidemics. The International epidemic stations to be subject to the Commission. The *personnel* will be appointed by them with the consent of those States on whose territory their labors are carried on. The present arrangement to last for ten years. The revision of separate details on the motion of any of the States concerned, or by vote of an international conference to be reserved."

The Conference decided that, like cholera, yellow fever is spread by communication, and therefore, it was the duty of the International Epidemic Commission to institute inquiries into its origin and causes. Like cholera, yellow fever must be opposed, in case of a severe outbreak, by a system of surveillance or quarantine, according to certain fixed rules. Every State is free to choose either method. The best places for the study of yellow fever are declared to be those ships which trade among the chief seats of this disease. Just as in cholera, the spread of yellow fever, according to place and time, must be followed with exactitude.

At the closing ceremony, Baron Orczy, of the Austrian Foreign Office, made a speech, in French and German, and expressed to the Conference, in the name of the Emperor, the satisfaction that his Majesty felt at the result of their deliberations.—*British Medical Journal.*

Cystitis.

[Cases of cystitis are so common, that the actively employed practitioner is rarely without one or more upon his visiting list. It may also be declared, that very few examples of disease usually found on his lists occasion him more mental perplexity, or manual trouble in effecting cures. The country practitioner, especially, finds this to be the case. If he trust the treatment to constitutional means, it is always after a protracted trial that he finds an occasional cure. If he employs local treatment, the worry of the daily visits and catheterization, causes him to dread the encounter with such a malady. I therefore experience a sense of

gratification at being able to afford some relief to practitioners by laying before them the very best mode of procedure in such cases. Within the past few days, I have dismissed from treatment, as cured, one of the most troublesome cases with which it has been my lot to meet. A married lady was delivered, at the age of 20 years, of a large still-born fœtus. No instruments were used. Through some inadvertence on the part of her accoucheur, the bladder was not emptied for more than thirty-six hours. Paralysis and cystitis resulted. The patient was dosed with every form of medication usually resorted to, in efforts to effect cures of this affection by constitutional treatment. Finding that three years of this mode of treatment was ineffectual, she came to this city in the month of April and placed herself under my care. At this time she was tormented with frequent calls to micturition, each act of which was attended by great pain, and by blood in quantities sufficient to color the urine very deeply, and occasionally to form a coagulum covering the bottom of the night vessel. Pus, mucus, and plates of phosphatic deposits, constituted the remaining abnormities of the urine. The finger in the vagina could readily detect the great degree of thickening about the urethra and lower part of the bladder. The only abnormity connected with the menstrual returns, was some degree of irregularity as to time, and temporary aggravation of the bladder troubles, due, no doubt, to the pelvic congestion attendant upon this function. The first steps of treatment consisted in repeatedly washing the bladder with warm water slightly acidulated with nitric acid. These washings were alternated with injections, formed by adding a tablespoonful of the following prescription to half a pint of warm water: ℞—Wine of opium, ℥j, carbolic acid gtt xl, muc. gum arabic, ℥iij. M. After some ten days of persistence in this initiatory treatment, cure was attempted by the injection of a solution of nitrate silver every fifth day, using in the intervals weak solutions of acetate zinc, in conjunction with acetate morphia and carbolic acid. The solutions of nitrate silver were gradually increased in strength from grs. v to as much as ʒij to water ℥j. The patient's improvement under this course of treatment was very decided. The calls to void the bladder were less frequent; indeed she often passed the night with not more than two interruptions to her sleep from this cause. The pain was greatly lessened, and the strong ammoniacal odor which constantly pervaded her person and room, alto-

gether disappeared. But while these indications of convalescence were manifest, and while the pus and mucus in the urine were reduced in amount, blood was never absent. For the purpose of controlling this symptom, I injected the bladder with ℥j of the following mixture added to ℥iij of water: ℞—Borate soda, ℥j, glycerine, ℥ij, gallic acid, ʒj, carbolic acid ʒss, water ℥ij. M. This wash was soothing in its effect, but failed to arrest the bleeding. I then injected the bladder with a solution of tannin in infusion of ergot, but with no better results. It then occurred to me that the effects of ergot upon the capillaries when administered internally might prove serviceable. I therefore ordered a tablespoonful of the following prescription to be taken every fourth hour during the day. ℞—Infusion of ergot, ℥iv, gallic acid, ℈ij, sulph. acid dil., gtt xxx. M. Up to this date in the management of this case I had forborne the use of opium for fear of creating the "habit" on the part of the young patient. But I determined to delay an appeal to its comforting and curative effects no longer. The patient was instructed to take ten grains of Dover's powder every night. These latter measures of treatment were instituted about the first of August. After about ten days the urine ceased to be stained with blood. The patient has not yet returned to her home, but seems to be entirely cured of her troublesome malady. The catheter used in this case was a double canula, with openings upon the sides. Whenever it was thought desirable to retain the fluid injected, the returning orifice was stopped by introducing a cork. I do not question the advantages of Dr. Hicks' method. Some four years ago circumstances, altogether unnecessary to relate, compelled me to treat a case of chronic cystitis by constitutional measures. In that case Dover's powder in large doses, warm hip baths, and occasional doses of balsam copaiba, brought about a rapid amendment. The patient passed from under my observation in an improved, but not cured condition. The use of copaiba should be confined to chronic conditions of the disease, and its effects carefully watched. I have not only used this drug with benefit, but have time and again violated Dr. Hicks' injunction against the use of alcoholic beverages by giving two or three tablespoonfuls of "Old Tom Gin" in the course of the day. These balsamic drugs seem to exert precisely the same curative effect upon the surfaces of chronically inflamed bladders, that we may witness after applying some stimulants to passive inflammations of the skin. Therefore,

while their occasional exhibition is a desirable spur in the direction of cure, their continuous use is almost surely prejudicial.]

*The Local Treatment of Cystitis in Women.** By. J. Braxton Hicks, M.D.

In choosing the heading of my paper as I have done, namely, "The Local Treatment of Cystitis in Women," I have been guided by the results of my own experience; because, although I do not altogether ignore the benefit of drugs given by mouth, nor the advantage of lulling the system by anodynes, particularly by opium, nor the benefit of rest and improvement of general health, yet, comparatively, their influence is very feeble, tedious and disappointing.

When I speak of cystitis, I mean the more severe forms; not the trivial mucous irritation which subsides in a few days, but rather that severe form which, amongst other causes, arises from the effects of labor-retention, from malignant disease of the organ, or from paralysis, etc. I have seen internal remedies tried in these cases for a long time, till one has been tired of using them; and, beyond the use of anodynes and those remedies which correct any excess of an acid or alkaline condition, I have no faith in any; I mean as direct means of cure. But, with regard to the local treatment, I can speak far otherwise; and I may say, in view of the benefits thus resulting, that to temporize with the constitutional plan is only to allow a very distressing malady to harass our patient when the relief lies in our hands, if we will use it. With regard to the correctives of the state of the urine, according as it is more acid or alkaline than normal, we should always examine it, so as to make out this point. If, as far as we can make out, the urine enters the bladder overcharged with lithates, then a cautious use of alkalies coupled with opiates will assist; taking care that the alkali be suspended as soon as the urine becomes natural in this respect. Again, should we find it alkaline, it may be advisable to employ some mineral acid for a time, also coupled with an opiate. With regard to an opiate, there is no doubt that this remedy is the most valuable of all internal remedies, partly because it lessens reflex sensitiveness, and thus the bladder is not so frequently hurting itself by its forcible contractions, and partly because it gives relief from pain.

Now I will suppose we have a case of severe acute cystitis, with much pain and frequent or constant desire to micturate; severe scalding along the urethra during micturition; the bearing down constant and intolerable; irritative fever; urine loaded with mucus, purulent and bloody. The first point to make out is, whether the urine be still acid or has become alkaline. I may here add, that it is seldom acid in a case such as is here

* Read before the East Kent District of the South-Eastern Branch.

supposed; but at the early stage it may be, before pus has appeared. Generally, it is very alkaline, and commonly also ammoniacal. Supposing it is alkaline and ammoniacal, then proceed in this way. Take a catheter—gum elastic is best, and still better if open at its end instead of the side, as is usually made—let it be well greased or oiled and passed gently just into the bladder. Draw off the urine. (The open-ended form is of great advantage in many cases where the mucus is copious and very ropy.) Be careful to enter the bladder the least distance possible, and withdraw the catheter just without the neck when the bladder is on the point of being emptied. This saves the mucous membrane from flapping down on the end. Then, with a syringe, throw up through the catheter warm water slightly acidulated with either nitric, hydrochloric, or acetic acid (vinegar does very well); if nitric or hydrochloric acid, about two drops of the strong to the ounce of warm water. As soon as the patient complains of desire to micturate, allow it to flow away again. More of this acidulated water may be used, till the bladder seems clear of the phosphates and mucus. About half a pint of acidulated water will generally suffice.

Then inject also through the catheter about one grain of morphia dissolved in about one ounce of water; quickly withdraw the catheter from the urethra, and instruct the patient to retain it as long as possible. It is a very rare instance if this single application does not produce very marked benefit. In the very acute cases, the passing of the catheter requires tenderness and care, because of the great sensitiveness of the urethra and lower bladder. But, if the precaution be taken of not introducing the catheter just within the bladder, much pain is saved. The exact distance can, at the first passing, be marked on the instrument. But to avoid the contact of the catheter with the bladder during the washing out, as much as possible, I always pass the injections into the bladder without entering its cavity. This can be readily accomplished by employing an open-ended catheter; a little extra force on the syringe will drive the fluid past the sphincter. If the injections are to be withdrawn, then the instrument can be made to enter, but only, as above mentioned, so far as may be necessary to allow the fluid to flow.

This treatment should be repeated twice daily, if the case be very severe, and especially in the case of retention, because of the ammoniacal decomposition of the urine; and, as the urine must be drawn off at least twice a day, there will be no additional distress given by passing the catheter.

In regard to the frequency, there is one point to be considered, viz., the irritation to the urethra by catheterization. Of course, it is undesirable that we should pass the instrument more frequently than needful; yet it is to be also noted that frequent micturition itself causes soreness or abrasion of the urethra; so that, if we succeed by our treatment in mitigating the frequency of urination, we shall compensate for this; and this is in ac-

cordance with my experience. By the treatment above described, we do very quickly lessen the frequency, and more than entirely compensate for the irritation of catheterization.

Where the urine has not become markedly alkaline, we may content ourselves by simply injecting the solution of morphia; yet, if we first of all wash out the bladder with warm water, or warm solution of permanganate of potash, we shall much expedite the cure. The strength of the lotion should be three times that generally in use, as it is very rapidly decomposed by the urine. When this has been allowed to flow away, we may inject the morphia solution.

After some days of this treatment, I sometimes employ, instead of the permanganate of potass, a solution of chlorate of potassa, about three or four grains to the ounce; using plenty, drawing it off after a few minutes, according as it gives pain, and then injecting the morphia.

Regarding the dose of morphia, I may add that, if there be no blood in the urine, and the patient feel no constitutional effects from a grain, I increase it to two grains. If blood appear in the urine, it is a sign of some abrasion, in which case the morphia is more likely to be absorbed; yet I have never seen any unpleasant symptoms even from two grains. The more we use, without affecting the system much, the better; locally subduing the nerve irritation and the tenesmus of the bladder, and the crushing of the mucous membrane which the contraction produces.

As the acute symptoms subside, we may employ more astringent washes, such as two or three grains of tannin in each ounce of warm water, or three or four drops of solution of perchloride of iron, using morphia immediately afterwards, as before mentioned. Our choice depends upon the amount of pain caused, the object being not to cause more than necessary. If the urethra be very tender, and catheterization seems to produce irritation, I omit all treatment for a day or two, after which we can resume it with much benefit. If after a fortnight the main symptoms be subdued, but the urethra seems very tender, I apply an astringent directly to it. A bougie or catheter, covered with tannin, made very smooth, and dipped in gum-water before introduction, is not very painful, and answers well. A probe, on which a film of nitrate of silver has been fused, may be tried. Of course, pain succeeds; but, on its subsidence, patients have expressed much relief. However, in the more acute stages and forms of cystitis, one should avoid this, unless the urethral irritation be very marked. Later on, this may be freely adopted and other remedies tried, as passing a stick of fused anhydrous sulphate of zinc, or even mopping over the urethra with solution of perchloride of iron, carried up by a probe covered with cotton-wool or similar material. This irritation of the urethra is the cause generally, in cases with frequent micturition, of much additional suffering, and, by its reflex action, keeps up the irritation of the bladder; so that we may say they play into each

other's hands. The urethra bears active treatment better than the bladder, and I have even dilated it and mopped it over with solution of perchloride of iron, with great relief the next day.

But, when cystitis has become chronic before we see it, or we have arrived at the same condition, but much earlier, by our treatment, we sometimes find it troublesome to completely cure the complaint. I have found, at this time, the injection of a solution of nitrate of silver of much service—from five to ten grains to the ounce. I have even used it up to fifteen grains to the ounce. This causes some rather severe pain for a short time, but much relief afterwards. Of course, two grains of morphia are left in afterwards. This may be repeated a week later. Or, instead of the nitrate of silver, I have employed perchloride of iron stronger than before mentioned (ten minims to the ounce). Now and then, for a few days together, it is well to leave off treatment in the chronic stage, to see how far the local interference may be rather keeping up irritation, I may be thought rather to be condemning local treatment by thus remarking, but really it is not so; we can, by local treatment, soon make a most distressing malady a bearable one; yet, at the same time, we know that frequent passing of the catheter may cause irritation, but slight only compared with acute severe cystitis. It is only when we come to the end of the case that we have to note the possibility of keeping up a slight irritation by our too frequent action.

I may add that, in the acute stages, the warm hip-bath, and warm sponge to the genitals, are not to be omitted, together with perfect rest in bed; the bowels must be kept gently relaxed; the dietary should be very simple and light, such as will naturally commend itself to our judgment upon common principles. But on no account should any kind of alcoholic beverage be permitted; and this rule should be observed rigidly during the whole period of the complaint, excepting only in such cases as it is imperatively necessary from other conditions of the patient's health.

I have thus only rather shortly sketched out the plan which I have used and would strongly recommend to your notice. The benefit of such management is very marked in cases of paralysis, where, from retention or the rapid ammoniacal decomposition of the urine, the distress and constitutional irritation are very distressing; and thus we can often lessen the chance of the extension of the irritation to the kidneys. Again, in malignant disease, the simple injection of acidulated warm water gives amazing comfort, removing phosphates and ammonia, and, when to this is added the morphia, a wonderful comfort is felt. Indeed, so much relief is obtained, that, with a large calculus in the bladder, its presence is almost entirely unfelt if morphia be daily injected.

I would recommend the extension of this treatment to inflammation of the male bladder. The same precautions can be taken regarding the entry of the catheter. I have found that it is not

needful to pass the end through the sphincter; but, if an open-ended catheter be employed, the fluid will find its way through readily. If the catheter be only passed about three or four inches, even then the fluid will find its way into the bladder; and this is an advantage for the male, as the area of urethral irritation by catheterization is thereby lessened.

After cystitis has lasted some time, irritability generally exists for some considerable period afterwards. This is much lessened by occasional morphia injections; but it may very possibly be due to the contracted state of the bladder, which cannot be quite overcome by the will, although the exercise of the power of retaining urine increases the power of the sphincter, and consequently of the capacity of the bladder. In this case, we may often succeed with a plan which I have some time ago, and again more recently, advocated, namely, gently distending the bladder with warm water by means of a syringe, which frequently overcomes the difficulty in a few days.—*British Medical Journal.*

[*The British Medical Journal*, of August 15th, contains the addresses delivered before the Forty-second Annual Meeting of the British Medical Association. While each one of these addresses will richly reward close perusal and study, there are some among them that enunciate philosophical doctrines which are valuable to the medical thinker. This is particularly true of Dr. J. Russell Reynolds' address in Medicine, and we reprint as much of it as we can afford the space for.]

In the somewhat perilous task that I have set before myself, it is not my intention to attempt to furnish an account of the details of recent scientific work in medicine; but rather, by an examination of our past and present relations to four great propositions, or,—if I may use the term—articles of creed, to show how they have affected our modes of investigation in the past, how they are governing or guiding our labors now, and what are the results which we see coming from the now existing and prevailing tendencies of thought.

The four articles of creed to which I refer as influencing for good or for evil the progress of scientific pathology, are a belief—1. In life; 2. In man; 3. In individuality; and 4. In the specificity of disease. The relation of the worker to the notions entertained by him with regard to the facts connoted by each of those four expressions, must determine the character and bearing of his work, and it is my object to try to bring before you the nature and effects of that relation.

1. Those who have gone before us in the study and treatment of disease have held, and that very strongly, to a belief, *first*, in Life, *i.e.*, in a power, force, or condition of matter which was *sui*

generis; which could not be explained away; which could not be referred to any combination of physical powers; which had an origin, the nature of which was beyond the reach of our modes of investigation; the career of which was, although often "sorely let and hindered," outside of and above all temporary and material obstacles; and the end of which, as we see it here, was but the beginning of another development, for the features of which we must patiently wait until we ourselves had passed into another state of being.

It is obvious that this view of life often led investigators of disease away from objects which they should have examined; drove them into conditions of thought which were fatal to a correct appreciation of the truths they wished to know; cramped their notions of the possibility of preventing or curing sundry affections of both mind and body; and perverted the efforts which they did make for the alleviation of ills they could neither arrest nor remove.

Some of the names of familiar diseases point to the nature and key-note of the pathology of the times in which they were first applied, and illustrate the first point that I have mentioned. The terms "morbus sacer," "morbus comitialis," "morbus sancti Weitii," the "king's evil," "St. Anthony's fire," and the like, conveyed meanings that are now happily lost to science. The affections thus denoted were only a few of many which it was profane to attempt to account for by other than occult and mysterious influences upon the greater mystery of life. The investigator was driven back and away from thoughts or processes of examination which should intrude into these secret places of the unknown.

Again, the notion that life-processes were totally dissimilar from all physical and chemical changes, so that the life-force held all these changes under its own control, effecting, by its direct agency, movements both coarse and molecular, such as bore no relation whatever to the results of well known chemical and physical forces, led the minds of the most earnest thinkers away from the proper track of scientific and legitimate research, and placed them in a condition of hopelessness and confusion such as nothing but a strong and, perhaps, excessive revulsion could remove.

Diseases which were held to be the result of some direct interference with the higher processes of life, such as insanity of mind and many of the disturbances of motility and hallucinations of the senses, were maltreated or untended, except by incantations or the administration of some mineral, vegetable, or animal substances selected on no observation of real therapeutic effects, but simply upon some mystic ground, some tradition, fable, or dream. And, again, the efforts that were made, within what was held to be the legitimate region of therapeutics, were misdirected and limited by the notions of volitional opposition to be overcome, not only in the patient, but in this organ and in

that; and the processes directed to overcome this resistance were often absurd and sometimes worse.

A change passed over all this mode of thinking; processes and phenomena which were held to be among the sacred mysteries of life have been shown to be facts of chemistry and physics—facts which can be recorded by figures, words, and drawings. The chemistry of digestion, of assimilation, of secretion and excretion, has been pursued and unraveled with scrupulous care and marvelous exactness. We have learned something of the rate of movement of nerve-change; of the nature and mode of the operation of impressions on the organs of special sense; of the conditions of muscular contraction; of the relation between definite movements of the limbs and certain portions of the brain; and, indeed, of many curious links between some of the higher processes of thought and special portions of the nervous centres. Diseases have been produced by artificial means; one disease has been used to destroy another, and vaccination has done much to diminish, and may probably succeed in eradicating, small-pox; diseases have been removed by chemical processes, both directly and indirectly; the diabetic process has been controlled by a diet rule; sarcinæ have been killed by sulphurous acid; and electrical appliances have restored wasted muscles and inactive nerves when these were apparently beyond all reach of hope. Modern pathology, by the aid of the microscope, the test-tube, and other means, has read many of the secrets of life, and the physician has been able so to wield the forces of life as to set right much that was wrong, and bring back the functions of organs which were virtually dead.

But, granting that we have removed much of the traditional mystery which obscured the facts of life, that we have resolved many so-called living actions into chemical and physical processes, and have described them in other than "vital" terms, it may still be questioned whether or no we have advanced many steps in the solution of the ultimate and real mystery of life. The tendency of the present day is to believe, and act upon the belief, that we have done so; and, as it seems to me, to push aside awkward facts as irrelevant or unreal, and to smother questionings by representing them as either solved, insoluble, or worthless. It seems to be almost believed by some that, if the chemical elements of which organized and living beings consist are brought together under favorable conditions, they arrange themselves into vital forms, and execute vital functions; and just as certain bodies in solution may assume definite crystalline form, so other bodies may agglomerate into organic form and exhibit the properties of life. Again, the obvious "correlation" of the physical and vital forces has led some to believe not only in the correlation of forces and actions, physical and vital, but, further, in the identical nature of the two; and so that which constitutes the true differentia of life appears to me to have been lost sight of in the very brightness of the light which

has been thrown upon the conditions of its action. It is well known that heat, electricity, light, air, food, and other external physical forces, impressions and materials, are the agents by which the processes of life or vital actions may be educed, and without which they fail to show themselves; but, when the idea is entertained that life itself is nothing more than an aggregation of the resultant movements of these conditions, materials, and forces, it seems to me that we are wrong. Impressions from without may determine the occurrence of motion or sensation; but the secret of life is still hidden in the organism which transmutes the one into the other. Physical force may be correlated to vital acts, but life itself is the special property or condition of the special material which effects that peculiar relation, and it is as far from comprehension now as it was a thousand years ago. Heat applied to a fertilized and "living" egg may set in motion all those chemical and physical changes and movements which make up the living processes of a growing bird; but, if the egg be not living, to begin with, heat but hastens another series of chemical changes—those of disintegration, putrefaction, and decay. Life is still hidden in the wonderful cells from which all these changes of structure and function take their start. Again, if we apply electricity to a nerve-trunk, a cerebral convolution, or a more central structure of the brain, motions of definite character, association, and sequence may result; but the secret of life lies hidden in the properties of these nervous elements which transmute such electrical impression into the facts of muscular contractions, and association of muscular contractions, in the movements of the limbs. To illustrate my meaning here, let me remind you that every so-called chemically "elementary body" is known as such only by the power which it possesses of so modifying materials and forces with which it is brought into contact, as to be distinct in its sum of properties from every other elementary body. We can describe it only by terms which involve some facts simpler than those of chemistry; but the idea is irresistible that, *e.g.*, gold, silver, and lead differ from one another in the fact that each of them has in itself a "something," we know not what, which determines the various bearings of each of those elementary bodies under the varying conditions to which they may be exposed—a something which makes them what they are, and to which we unconsciously refer in thought when we give them names and places in scientific categories. This individuality, or "tendency to individuation," as it has been called, is what constitutes that which has been termed their "life." The same mode of thought is true with regard to the living organic body—no matter how simple or how complex;—that which makes it living, which alone justifies the attribution to it of the properties of life, is the possession of an individual power of behaving in such a way under given circumstances, and amidst the brunt of conflicting forces, and sur-

rounded by varying materials, that it evolves the phenomena we designate as vital.

I do not mean to say that anyone distinctly says to others, or even to himself, "I can make living beings by putting together the elementary bodies of which they are composed under favorable conditions;" nor do I think that there are many who would say: "Life differs in no respect, in all its details, all its relations to personal consciousness, to moral sense, to intellectual product, and to social history, from the physical forces of which we know so accurately their modes of action as to be able to foretell the result of experiments we may make, or of processes which we see advancing;" but I do think this, that many are disposed to think that, if we could make our experiments only a little more clever, and if we could carry our machinery of experimentation but a little further—and there is no doubt that we shall do so very shortly—we shall get rid of the term and the idea of life itself, and so make a great advance in science. It may be that this will be done; but, for one, I believe that it will not be done; but that there will ever remain the same kind of mystery with regard to life itself—however keenly and satisfactorily many of its processes may be referred to simpler agencies—that still shrouds the nature of those simpler forces, such, for instance, as gravitation or heat, with regard to the nature of which we have ceased to question. We have admitted that their "essence" is beyond our ken, and that we can but study their phenomena; we have not tried, or have failed if we have tried, to reduce them to a common denominator; but, with regard to life, we have drifted into a sort of belief that it is to be decomposed, explained away or got rid of; and that our true line of action is to be found and followed by such a belief in the future results, rather than in the present or past facts, of science.

The mode in which this state of mind affects us, as physicians and surgeons, is, I think, injurious. Among other evils which come from it, I will mention two. The first is, the growing disregard of what may be termed "subjective symptoms" of disease, and the second is the effect which such frame of thought exerts upon therapeutics.

With regard to the former—"subjective symptoms"—is it not coming to this, that but little attention is often paid to the accounts which the patients give us themselves, their ideas, emotions, feelings, and physical sensations? These are things which we cannot weigh in our most guarded balances; measure by our finest scales; split up in our crucibles; or describe in any terms save those which are peculiar to themselves, and which we cannot decompose. These symptoms are often disregarded and set aside; and the patient, whose story of disease is made up of them, is thought fanciful, hypochondriacal, hysterical, nervous, or unreal; because, forsooth, we have physically examined thorax, abdomen, limbs, and excretions, and have found in them nothing wrong; because we have looked at

the retinæ, examined the limbs electrically, traced on paper the beatings of the pulse, weighed the patient and have not found him wanting. Still he is miserable in spite of placebo and assurance that there is "nothing organically wrong." There may be in him the consciousness of a deep unrest; or of a failing power, which he feels, but which he cannot see; or of a something worse than pain, a sense of "impending evil," that he is conscious of, in brain or heart; a want of the feeling of intellectual grasp, which he may call "failure of memory," but which memory, when we test it, seems free from fault; a want of the sense of "capacity for physical exertion," which seems, when we see him walk or run, to be a mere delusive notion, for he can do either well and easily to our eyes and those of others; and so he is called "nervous," and is told to do this or that, and disregard these warnings that come to him from the very centre of his life. And let me ask whether or no it has not again and again happened, in the course of such a history as that which I have only faintly sketched, that some terrible catastrophe has occurred? Do we not see minds gradually breaking down, while we say there is no evidence of organic change in the brain; hearts suddenly ceasing to do their work, when, after careful auscultation, we have said there was nought to fear? Suicide or sudden death sometimes disturbs the calm surface of our scientific prognosis of no evil; we may be startled, and may then see all that we ought to have seen before. But, when the ripples that such unforeseen events have occasioned on that smooth surface have subsided, we go on as we have already done, and still pay but little attention to what the patient feels, and delight ourselves in the precision of our knowledge with regard to physical conditions of which he may know nothing and may care still less. No one can appreciate more highly than I do the value of precise observation; but I do not believe that minute, delicate, and precise observation is limited to a class of facts which can be counted, measured or weighed. No one can see more distinctly than I do the wrong conclusions at which a physician may arrive by accepting as true the interpretations which fanciful patients may offer of their symptoms; but I am sure that, if we pay no heed to these mistaken notions of a suffering man, we lose our clue to the comprehension of the real nature of his malady. Morbid sensations and wrong notions are integral parts of the disease we have to study as a whole, and we are bound to interpret their value for ourselves; but we can ill afford to set them aside, when we are as yet in the dawn of scientific pathology, and are endeavoring to clear away the obstacles that hide the truths we hope hereafter to see more clearly about the mystery of disordered life. The value of such symptoms may be slight in some kinds of disease, when compared with that of those phenomena which may be directly observed; but we are bound to remember that there are many affections of which they furnish the earliest indi-

cation, and there are not a few of which they are throughout the only signs.

Again: the view that is taken of the correlation of vital and physical forces, when it assumes the form that I have mentioned, and which amounts to a practical ignoring of the fact of life, is, I think, mischievous in therapeutics. That which is the differentia of life is, as it seems to me, lying in the organism, and is that which makes it capable of transforming physical forces into vital acts. But what we are often attempting to do in our treatment of disease is to elicit vital action, rather than to conserve vital force. We see that, by giving such and such drugs, we change—and, as it seems, for the better—the mere processes of life; we may limit or increase muscular movements; we may augment the quantity of secretion here, or of excretion there. But, let me ask, do we not often see that, when we have effected these changes, when we have given diuretics, purgatives, diaphoretics, and the like, and have witnessed their appropriate physical results, the disease is no better than before, and the patient is worse. We have brought vital processes into play, but have used up the vital force in doing so. On the other hand, we try to check what appears to be excessive and exhausting discharges, or tiring and distressing acts; we try and often succeed in diminishing the frequency, force, or extent of certain vital functions that appear to be, and indeed are, beyond the normal range. But again, let me ask, do we not often see, when we have succeeded in lulling a cough and diminishing the amount of expectoration, that other and far graver troubles supervene; that, when a diarrhœa or diuresis has been cured, the patient is worse than before; that, when a skin eruption has been removed, some nervous trouble takes its place? Again: by the administration of alcohol or other stimulant or tonic, we may often help a man to get through some work for which, without such aid, he was totally or partially incompetent; we have evoked an amount of vital action that would otherwise have been impossible. But do we not often see that we have really done more harm than good; that the weakness has increased, and that the necessity for stimulation has become aggravated, and that what was really needed was food and rest, which should have nourished the organism, built up the tissues, and replaced what was wanted in living force? We have helped our patient to do things he could not otherwise have done, but we have used up his life in doing so.

Once more: in the present day, electricity, in its many forms, has come again into fashion, and constant and faradaic currents, chain-batteries, and magnetic belts are topics of common conversation and articles of dress, in boudoirs and clubs, as well as in the consulting room of the physician. The mischief that is being done by such abuses of a very valuable therapeutic agent is grave and manifold; but is the profession altogether blameless in this matter? There are some conditions in which electricity is very useful, and useful by calling into play the function of nerve

and muscle; but it has again and again been used when it could by no possibility have been productive of the slightest advantage, and when the production of such enforced action of muscle and nerve has but diminished the strength and exhausted both the energies and the endurance of those who had not one grain of either of those qualities to spare. Muscles and nerves have been driven into action when they needed rest; but that which has guided the practitioner into such mistaken practice has been the notion that to evolve function was the great end of treatment, whereas what was really needed was a conservation of the central nutrition, and a consequent addition to the stock of vital force. Rest, food, cod-liver oil, and soothing drugs, were needed; and not faradisation, alcohol, or strychnia.

These points are enough to show the class of evil which may arise from what I hold to be a wrong way of putting the notion of the relation between physical and vital force, between life and vital function; and I cannot but think that, by a review of our ideas of life, and by a regard to the objects, direction, and limits of our therapeutic agents, we may hereafter find it less frequently recorded in the domestic annals of our patients, that he or she had "suffered many things of many physicians, and was no better, but rather the worse."

As ancillary to the line of argument which I have followed, there is much to be gathered from a regard to the recognized causation of etiology of disease. The conditions which derange or disturb health may be grouped in the following manner. 1. Those which change the due income to the body of ponderable, measurable, intelligible, and appropriate material, such as food in all its forms, solid, liquid, and gaseous. These may be deficient, excessive, or unwholesome; and we know fairly well the results of bad eating and drinking, of bad water and bad air. 2. Those which interfere with the due elimination from the body of its waste products—faulty secretion and hindered excretion. 3. Those which diminish, derange, or exaggerate the amount of influence of certain imponderable agencies—light, heat, electricity, and the like; and here, too, I would place physical injuries. 4. Those which in any way disturb the due exercise of the body in such way as to distort the balance between income and expenditure of these imponderables—for example, overwork, underwork, deficient motion, deficient rest, care, anxiety, and toil.

I do not know what recognizable and ascertained cause of disease might not be included within one or the other of these four categories; and by their aid we may explain a vast number of the morbid conditions which we have to study and to treat. But let me ask if, after the most careful scrutiny, we are not again and again compelled to say we can find no cause for the particular malady presented to us in this person and in that. We fall back upon constitution, predisposition, hereditary taint, and other easily employed but most inadequately examined and

utterly uncomprehended words, to account for what we do not know and can by no means explain; and by the use of such phrases we only shunt the etiology into a siding, or throw back for one or two generations that which is inexplicable in the present.

We know much of the conditions determining the occurrence and favoring the development of the "acute specific diseases." We know much of the results of accidental exposures, of overwork, of faulty education, and of miserably defective hygiene. We know something of the conditions which may aid the growth of the group of "constitutional diseases," and which may antedate the processes of natural decay. We know much that may be useful in its application to masses of people; and to this I shall refer hereafter. But what, let me ask, do we know, and know definitely, of the starting-point of disease, of so-called "constitutional" sort, in a large proportion of the cases that come under our care? It is worse than idle to assume that we know as yet the true etiology of cancer, of Bright's disease, of tubercle, of Addison's disease, or, again, of that which determines the difference between the many maladies of the nervous centres which crop out in writers' cramp, torticollis, chronic chorea, locomotor, ataxy, epilepsy, diffused sclerotis, or general paralysis. We often lightly refer any one of them to conditions common to the causation or supposed etiology of all, and then fall back upon peculiar predisposition. Again, do we not often find that, upon the most careful scrutiny of all antecedents, we are unable to explain why this man has Addison's disease, and that suffers from ataxy, why one suffers from cancer, and another from writers' cramp? There is much that is yet withheld from our most earnest gaze—much that, as it seems to me, we must as yet refer to a change, the nature of which we do not know, in the very innermost recesses of what we call "life." There is something radically and essentially wrong in that upon which all these so-called "causes of disease" are brought to bear; and I think we are driven by the facts of daily experience to believe that the organism—the living material which, by virtue of its own endowments, determines, in obedience to external impressions, all vital acts—is in itself at fault; and that its departures from the normal condition vary, and are different the one from the other, in such way as to produce those various modifications of structure and function which we term "constitutional diseases." As physicians and surgeons, it is our part to conserve life by preventing, when we can, the inroad of disturbing agencies; to preserve life in its integrity of useful work and fit association with all that makes life happy; to help it in its weakness, heal its suffering, lessen its sorrows, and soothe its closing hours; but I think we shall accomplish these purposes successfully only by admitting its existence, its separateness from all other forces, acknowledging its mystery, bowing ourselves down before the

enigma of its origin, and reverently humbling ourselves in face of its unsearchable but wonder-teeming end.

[The address in Surgery by William Cadge, F.R.C.S., is mostly devoted to questions connected with the etiology and pathology of lithuria. The following extract exhibits the speaker's views on this point.]

Passing now to the second branch of my subject, permit me, before considering the causes of the excess of lithuria in this district, briefly to allude to its etiology and pathology. Although it is safe to assume that, when lithic acid deposit frequently occurs in the urine, there is probably excessive secretion, we must not forget that a diminution of the alkaline bases or over-acidity of the urine will lead to deposit, without any relative excess. The conditions of health and disease in which these circumstances obtain are too well known to need notice; but I would ask, as bearing on this inquiry, whence is lithic acid derived? and where is it fabricated? Admitting that it is derived from the disintegration of nitrogenized tissues and from the transformation of the excess of albuminous food, it yet remains a question where or by what agency it is actually generated; whether by the operation of chemical or electro-chemical changes in the blood itself, or in the tissues undergoing change, or by some secreting gland-cells in the liver, spleen or kidneys. The office of the kidney, with reference to lithic acid, is still a moot question; is its duty simply that of passive elimination? or has it the higher function of constructing and forming the lithic acid, urea, and other principles, out of the materials presented by the blood? Ever since the discovery of urate of soda in gouty blood by Dr. Garrod, there has been a disposition to degrade the function of the kidney, and to allow that it consisted in little more than mere osmosis, and that it simply removed that which already existed. Admitting that urea and uric acid may be formed independently of the kidney, it would seem probable, looking at the fact that healthy blood contains merely a trace of urea and uric acid, and also that, when the kidney is extirpated, these substances do not accumulate in the blood, it would, I say, seem probable that, in the ordinary healthy operations of nature, it is the proper function of the kidney to form as well as to eliminate the 10 grains of lithic acid and the 500 grains of urea which a healthy man voids daily. We know that, in certain diseased conditions of the kidney, the power of eliminating these nitrogenous principles is lost, or nearly so; is it not possible that, in certain other morbid states the opposite conditions obtain; and that, out of the healthy blood, the morbid kidney may form and eliminate an excessive amount of urea or uric acid? Who has not seen, in persons prone to the deposit of lithic acid, an enormous quantity, almost a teaspoonful, voided in a day? and that without any excess of diet, or sign of extreme waste of tissue or obvious

dyspepsia, or ill-health of any kind. If this excess of lithic acid existed in the blood, *quoad* urate of soda, surely the phenomena of gout would be present; but I have often and so keenly watched this occurrence, and failing to detect any of the usual etiological causes, it has appeared to me probable that, just as other secreting organs vary in the quantity and quality of the fluid they secrete, so the kidneys, owing to some inherent morbid condition, may form and secrete, out of healthy blood, urine having one or other of its constituents in excess.

But, if, as I have said, there be a tendency to reduce the function of the kidney to that of mere osmotic elimination, I must notice the attempted elevation of another organ, the liver, into what I may call universal supremacy over all the healthy and morbid processes of nutrition and secretion. Not only has it the important glycogenic function and the secretion of bile, but it is also credited with the conversion of albuminoid matters from the blood, the food, and the tissues, and the formation of urea and uric acid. Arguing from the fact that urea has been found in the liver, and from some doubtful and controverted experiments showing that rather more urea was to be found in the hepatic than in the portal vein, and forgetful of the other facts, that urea has also been detected in the brain, the lungs, and the spl en, and that only a mere trace is found in healthy blood, we are asked to believe that, out of these three functions, there flows, in chief part, oxidation, sanguification, animal heat, and the depuration of the blood. It makes one almost stand in awe of one's liver, and believe that, having a liver, we could dispense with all other organs of digestion.

But, if the liver be thus comprehensive as to its physiology, what has not been said as to its pathology? Passing by the structural diseases of the liver, which are well recognized and understood, the list of diseases and symptoms arising from mere functional disorder, as set forth by Dr. Murchison, the latest and stoutest champion of the pre-eminence of the liver in disease, is too long even for mention here; suffice it to say, that it includes most of the disorders of all the systems of the body; of the nervous system, of the circulatory, respiratory, cutaneous, and urinary systems and organs; and even such acute diseases as pyæmia and the typhoid state are supposed to be frequently the result of functional disorder of the liver. In my student-days, it used to be said that he who understood the nature and treatment of inflammation had acquired half the knowledge necessary for a successful practitioner. Now, it would be more correct to say, that he who understands the nature and treatment of hepatic diseases has little more to learn.

On this view, gout, of course, is said to be a mere symptom of liver-disorder, and the same is said of lithuria and all calculi composed of lithic acid or its salts; and the practical deduction inculcated by Dr. Murchison and also by Sir H. Thompson is that, in the treatment of gout and lithuria, those remedies which are

supposed to address themselves to the liver are to be preferred to those which diminish acidity and dissolve uric acid; that alkalies and alkaline waters are to be laid aside as inefficient, and, at the best, but of temporary benefit, and that saline aperients and waters of that character are alone of real use. To this I would reply, judging from ample observation, that the saline aperient waters are not proved to act specially on the liver; that they probably act chiefly on the stomach and alimentary canal; that they are not so directly capable of removing gout or lithuria as the alkaline waters; and that, when they do so act, they, like the alkaline remedies, are equally evanescent in their effect. I have the conviction that this tendency to exaggerate the influence of the liver in health and in illness displays the drawbacks of specialism in the study of disease, narrows the view to one stand-point, and encourages the old but loose habit of attributing every obscure symptom to a "disordered or torpid liver." On the whole, it is safer to attribute lithuria to dyspepsia and mal-assimilation, which probably concerns all the digestive organs, than to fix the fault mainly on one.

[The address in Obstetric Medicine is by Dr. J. Matthews Duncan, and the speaker has chosen the very interesting subject of puerperal pyæmia. It is a paper replete with practical matter, and we afford space for a considerable part of it.]

In connection with this subject, there is a preliminary inquiry whose importance is self evident, and which, remarkable to relate, has only recently been discussed formally, and with sufficient means—the mortality in childbed, or total mortality of childbirth and in childbed. How many women die from all causes during childbirth and in the puerperal state? Of course, such a question, lying on the surface, has been considered and answered, but the responses have been most insufficient and erroneous. The ordinary belief seems be to that there is, in connection with childbirth and lying-in, no mortality in a well conducted practice. Miss Nightingale says that deaths from puerperal diseases ought never to arise after delivery in a properly conducted and managed institution for lying-in women. In a late number of one of our principal medical journals, appears a report of one of these properly conducted and managed institutions. The hospital is a military one, and not a death is reported; and the article is evidently with a view to show benighted civil obstetricians what is the result of proper conduct and management. I have often heard sanguine medical men say that in the course of a long and large practice they had not had a single fatal case. Now all such beliefs, reports, and statements, are mere encumbrances of the inquiry, and are to be thrown overboard, if not more ungracefully dealt with. We have no time to trifle with such nonsense, for we are everywhere surrounded by awful deaths in childbirth

and in childbed, where there has been, so far as can be discovered by ordinary mortals, nothing but proper conduct and management.

Another response to this great question is familiar to all; it is derived from Merriman, and has often been repeated, not only as evidence of the mortality of childbirth, but also to show the successful progress of obstetrical therapeutics by the rapid diminution of the maternal mortality. It requires such a stretch of credulity to place the slightest confidence in Merriman's tables, that I do not regard myself as justified in taking up your time any longer with them. More recently, and on much higher, even on official authority, somewhat similar statistics have been proclaimed as evidence of the progress of obstetrical therapeutics from decade to decade; but, alas for us, the evidence will not bear inspection, and we shall not inspect it. Even now, in 1874, we are only striving to reach a sound conclusion as to this mortality; and after all our labor, official and private, have no statistics to be relied on for the comparison of the results of successive decades or of longer periods. The importance of the question needs no demonstration, and it must be solved in a scientific manner. We are seeking not what we fancy or wish, but what is. There can be no doubt that the death-rate does represent marriage and childbearing as a most perilous ordeal for a young woman to encounter; and it is not good reasoning to use this, as a great author has done, to prejudice us against receiving what may be proved. During the whole life, including intrauterine existence, the female half of mankind has a great advantage over the male in point of mortality, except that for a considerable time childbearing brings the female nearly to the level of the male; and that, for a less time, the risks of primiparity sink her below him. Childbearing is in these kingdoms the special, and, so far as known, the only special great cause of enormous increase of female mortality above what it would otherwise be. Primiparity produces a great exaggeration of the childbearing risks. It is this mortality that we seek to estimate, and you observe it is of the highest human interest; but it is for medicine of special importance, being a cardinal element in the solution of the question of the value of hospitals. These noble institutions, the lighthouses of practice, have had their reputations tampered with on the most insufficient grounds. If we are ignorant of what may be called the normal mortality of childbirth and lying-in, how can we justly judge the hospital mortality? If we do not estimate the excess of special, and, in a sense, just causes of mortality in hospitals over those acting in the country generally, how can we fairly measure the salubrity of maternities? It is impossible to do so, yet there has been a lamentable and injurious amount of such mere cavilling with institutions whose reputations should be too sacred for any but the most solemn and logical consideration.

Among the Chinese, puerperal mortality is held, according to

Dr. Jamieson, to rise as high as from 1 in 12 to 1 in 20—an alarming and scarcely credible statement; yet Dr. Thin, lately of Shanghae, believes it to be true. Some statistics of the city of New York, recently published by Fordyce Barker, yield a puerperal mortality of 1 in 35, a terrible result; and on the surely extravagant admission that a quarter or even half of the births were not registered, we have here a very high figure. Faye states the puerperal mortality of Prussia as 1 in 84; and that of Finland, according to Pippingskjöld, as 1 in 106, and that of Norway as one in 131. I found that in Edinburgh and Glasgow, in 1855, the mortality of married women within six weeks after delivery was 1 in 107 at least. From a large collection of data, and trying, however rudely, to get an approach to exactness, I estimated the mortality within four weeks after delivery as about 1 in 120.

Hervieux says there are places where for a long series of years this rate has not risen above 1 in 1000; but he must derive his information from some other planet than this, for certainly there is no such abode of the blest known among the inhabitants of this earth. Le Fort estimates the rate as 1 in 212; but his figures and reasoning are such as to render this determination unworthy of any reliance, as has been sufficiently demonstrated. Farr has carefully estimated this rate, and arrived at the conclusion that it is 1 in 190. But, however much we may be disposed to bow to his authority, we are bound to scrutinize his method; and, on doing so, it turns out to be very unsatisfactory. He is dealing with a system of registration which is not compulsory; he seeks to verify the returns relied on by appeal to the returners, which is something like trying to correct an error by itself; he made no independent search for the deaths of the delivered women; he made no correction for twins, nor for still-births. On all these accounts I regard his result as being not only out of keeping with the best of the others, but as not especially reliable. The data of foreign countries which I have given may be very good for aught I know, but then I have no positive knowledge of the care or of the circumstances under which they were compiled; yet we have always been led to regard the Swedish and Prussian returns as very valuable.

In this state of matters I was not disposed to allow the point to remain unsettled for this country, and I recently undertook the somewhat onerous task of thoroughly searching official returns, with a view of getting a figure that could be relied on. The determination which I am about to give can be erroneous only in the way of making the rate too low.

I found that there were registered in Edinburgh and Glasgow, in 1869 and 1870, about 52,000 births, and I found that within twenty-eight days after delivery at least 1 in 139 of the mothers had died. Now, several mothers additional may have died, and their deaths have been elsewhere registered, they having left their original residences. These would slightly increase the

rate if they were found; but the rate is too low for another reason—namely, that all births of dead children are omitted. Now this very serious omission of a large proportion of the most dangerous labors leads to this rate of 1 in 139 being far too low. How very far too low, we may to some extent conceive when I call to mind that, among Collins' 16,414 women delivered, 164 died; and of these 164, nearly one half had dead children! It may be said, then, as the result of this investigation—the most careful and complete, so far as I know—that at least 1 in 139 died; and I add, for the reasons above given, and for others, that I have no doubt that at least 1 in 120 died. These terrible results, gentlemen, or something closely approximating, we must accept meantime, however forcibly they may demonstrate that marriage and childbearing are a fearful ordeal for a young woman to encounter.

My estimate, gentlemen, of this lamentable mortality of lying-in women is 1 in 120 within four weeks; and it is useful to have a fixed period of four weeks for various reasons. But we must not allow ourselves to be misled into thinking that puerperal mortality is over in four weeks. You are well aware that many bad cases linger on beyond the month of four weeks, to die beyond the reach of these restricted statistics; and that many others owe their deaths to puerperality, although the occurrence is later than four weeks after the labor. I have already mentioned that I have most carefully prepared statistics showing a mortality of at least 1 in 107 within six weeks after delivery. Further, I have statistics analized which do, I believe, show that the mortality of puerperal women does not again fall to its ordinary level till a period not of weeks, but of months, after delivery. It would be a grand work for our young statisticians to show the wave of special mortality, beginning with conception and ending some months after delivery. Statistics have already shown the great rise of mortality, or the great wave of it that passes over the sex during the child-bearing age. But we want much more than this, and especially the wave for the average individual pregnancy, labor and lying-in.

I must conclude this already too long discussion by saying that I believe that in this country nearly 1 in every 100 women delivered at or near the full time dies in parturition, or before the puerperal state and its effects have passed over. This is, no doubt, an awful statement for women and for men. Whether it will deter them from marriage or not when they come to know it I cannot say, for I have no analogy to guide me. The risk from railway accidents is comparatively a mere bagatelle, when taken in any point of view, and I have made no inquiry as to its influence in deterring from travel.

Even the fear that women may be deterred from marriage and child-bearing must not deter us from unmasking the real extent of the dangers they encounter; but I must only spend a few words on puerperal morbility. Besides dying at a rate of nearly

1 in 100, women have to encounter a vast amount of disease and suffering which does not end fatally. This has been called morbility, in contradistinction to mortality. Miss Nightingale and Dr. Farr, besides having very favorable views of mothers' chances of survival and recovery, go a great deal further than this, and enunciate a doctrine to which it is difficult to believe they have ever given a moment's reflection. They regard ordinary women as having no need of long nursing after lying-in; for it is all over, say they, in a few days after retirement and delivery in the rude compartment of a hut. It is unnecessary to take up the time of any one of the most moderate experience in confinements and diseases of women among the poor or the rich, among the civilized or uncivilized, with a deliberate demonstration of the tragical injustice of their statement. I only refer to it here as it forms a contrast with the truth as to puerperal morbility. This subject has been illustrated by many athors, among them Späth and Landau, who confine their researches to morbility shortly after delivery. The latter, taking temperature as a criterion, estimates puerperal morbility as affecting 1 in 6. His valuable experience was in an obstetric hospital, and it requires corroboration. Besides this, he takes no account of the many ulterior diseases coming on after so-called recovery.

Deaths during parturition or the puerperal state are often conveniently arranged in three sets:

1. Childbirth deaths.
2. Puerperal or metria deaths.
3. Accidental deaths.

A woman dying during *post partum* hæmorrhage undergoes a childbirth death; a woman dying of puerperal fever undergoes a puerperal or metria death; a woman accidentally poisoned by laudanum shortly after delivery undergoes an accidental death. In cases such as these the placing of the death in its class is easy, but there are a large number of cases regarding which there may be just difference of opinion as to which of these three groups should receive them: hence the classification as used in practice cannot be relied on as embodying a scientifically accurate statement of any point—a circumstance which, for the conduct of various important discussions, is much to be regretted.

But though this is so, there is unanimity in placing puerperal fever deaths in the second category, that of puerperal or metria deaths, and in giving it the horrid pre-eminence over all other causes of mortality in the three combined categories. For obstetricians and for the world, then, this is the subject of first importance in midwifery, and it has attracted a corresponding amount of attention, and never more than at present, and certainly never with so much advantage.

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Pyæmia occurs in several forms, which are characterized each by more or less peculiar symptoms, but most distinctly by the pathological appearances discovered *post mortem*. There is that

most widely known when you have septic embola, and scattered abscesses caused by them, and perhaps otherwise also. There is that where you have inflammation of the peritoneum and other serous cavities, including the synovial and endocardial. There is that where the mucous membranes are chiefly affected—the muco-enteritic. And, lastly, there is that where the only results found after death are—alteration of the blood, enlargement of the spleen, the liver, and degeneration of their most important tissues, with similar degenerations in other organs. It is this last which, often rapidly fatal, was described by Helm, and is now often called acute septicæmia. These are the cases which the superficial pathology of our young days described as having no *post mortem* appearances at all. An autopsy in those days was made by any practitioner, occupied only a few minutes, and the observations made were of corresponding value. Now an autopsy is a matter understood to demand the labor for a long time, often for hours or even days, of an expert. On these fruitless necropsies, where no appearances were discovered and none supposed to be discoverable, was founded, as you will remember, an argument supposed to be of clenching potency in favor of the essential fever character of the disease. But I confess I have never been able to discover either the logic or the power of the demonstration.

Easily getting rid of this old argument, we come sharply into contact with a new difficulty. When I say that Weber, Bergmann, Billroth, Hüter, and Vernenil support the doctrine, that Olshausen holds it an open question, and that Sanderson, in his essay on the *Infective Product of Inflammation*, demonstrates truths which seem at least to favor its pretensions, you will see that the matter has already occupied great minds. The doctrine is antithetical to the essential fever notion, for it states that pyæmia, or so-called puerperal fever, does not essentially differ from ordinary inflammatory fever, such as is called healthy, except in degree, and that the modes of induction of these feverish states are identical, or nearly so. From the slightest pyrogenous effect or merest evidence of morbility, as discovered by the thermometer, up to the most rapid of Helm's cases of acute septicæmia, we have one disease in different degrees or forms, all depending on a chemical poison of Schmiedeberg and Panum, or on the bacterium of Mayrhofer, of Lister, of Klebs, of Waldeyer, of Heiberg, and of Orth, whether this bacterium be the poison, or only its carrier. That there are weighty reasons for entertaining this view must be admitted, and among them not the least are the wonderful results of the antiseptic system of treatment, as not only preventing pyæmia, but as preventing ordinary inflammatory fever. But practitioners of my own age, or greater, will find it difficult to get rid of, or controvert, the *prima facie* evidence in favor of the old views afforded by the great array of facts and ideas which forms the basis of our daily reasoning in the guidance of practice in healthy and unhealthy inflammations, and

which furnish a set of arguments which have been well stated by a reviewer in a late number of our medical quarterly journal.

The disuse of the term puerperal fever, and the replacement of it by puerperal pyæmia, is a change which has already been carried out by many of our best obstetric authors. The old designation is so impregnated with erroneous and misleading theory, that it cannot, within a reasonable time, be purified, and will probably be most advantageously subjected to destructive cremation. Fordyce Barker, a recent American author of great intelligence, still upholds the old banner—"an essential fever peculiar to puerperal women, as much a distinct disease as typhus or typhoid." He well knows how pathologists believe they have torn this view into tatters, and he ought to have given us good evidence of its being reparable, if not actually rehabilitated, but he does not even attempt the difficult task. When we are asked for evidence as to the specific characters of typhus or typhoid, we can easily produce them, and defy the farther destructive analysis of these diseases. It would be a waste of time to go over the special history of the causes, progress, and results of these diseases. They are well known, clear, and convincing to all. For puerperal fever, we have no such characters—no such evidence. All the evidence brings the disease into the closest alliance or identity with surgical pyæmia. The grand modern history of pyæmia is, in fact, at every step of its progress, the history of the elucidation of puerperal pyæmia, or of so-called childbed or puerperal fever.

One error is sure to bring another in its train; and so we have the widely prevalent belief that this disease is like cholera, or small-pox, or typhus, in occurring sporadically, but chiefly epidemically. Here it must be observed that many authors use the word epidemic carelessly, or as synonymous with endemic; a common error, which should never be committed. But, knowing this, we find them almost universally believing in real epidemics of puerperal fever, describing them as sweeping over a country or devastating a continent. It is well known, and indeed needs no further proof, that the disease has often the appearance of being endemic in an hospital, that it attaches itself to and follows certain individuals in their practices; but I have not been able to find anything worthy of the name of evidence to prove its epidemic prevalence at any time or in any large district. You are all, no doubt, familiar with the long descriptions and marvellous statistical compilations adduced as evidences of this doctrine by our best writers, especially by medical historians, among whom Hirsch is pre-eminent. But when these statistics are subjected to scrutiny, they are all found wanting, as may be made plain by one example, and the examples are all from old times. We have few epidemics described in recent times, and these few do not produce respect for the doctrine implied in their description. Yet though there are few descriptions now, there is still everywhere the erroneous belief. Epidemics are described as having

occurred in Edinburg in 1772, 1814, 1825, 1833. But it is mere assertion. There is not a tittle of proof that the disease was not as prevalent in every year as in the years of the so-called epidemics. When a physician, struck with awe by a few cases, writes a description of them, down it goes in the statistical tables as an epidemic; and the year of it is not always that of the cases, but sometimes that of the publication of the pamphlet or book. When the horrid mortality rises in an hospital, from overcrowding or other causes, down it goes again as an epidemic; and with this supply of burlesque evidence the manufacture of epidemics never ceases. Registration arrangements are not required to show the epidemic character of cholera, or of small-pox, or of other fevers; and, when we do get the valid registration statistics, we get the proof in proper form. But if puerperal fever is to be shown to be ever epidemic, it must be by regular modern registration statistics. The prevalence of it is happily never very great, comparatively speaking, and, again; it is never extensively or long absent; and, when we appeal to such statistics of metria as are accessible, we do not get proof of epidemic character. We find it always present, in every county, in every community. It is easy to get proof of the epidemic characters of fevers, but not of puerperal fever. He who studies this point will find metria to vary in its ravages as pneumonia does; he will find cholera or scarlatina varying in their ravages according to a quite different law.

As the disease is erroneously believed to be a fever and to occur in epidemics, so we have a corresponding erroneous theory of its origin or causation. Many authors delight to speak of cosmic or of telluric influences or of miasma as producing the disease, and such subjects are favorites with a certain class of minds which find it most agreeable to enter at great length upon those topics of which they know very little or even nothing. For such there should be a puerperal Zadkiel. It is very difficult to find any evidence for the existence of a miasma even in the air of a pestilential hospital, for puerperal pyæmia prevails in such a manner as is scarcely reconcilable with the miasma hypothesis, and, on the whole, easily reconcilable with more or less direct communication with diseased neighbors, as Veit has ably shown. Among the circumstances of prevalence to which I allude are the comparative immunity of women brought into the hospital already delivered, the special liability of primiparæ, the special liability of those who have tedious and difficult labors.

Again, as the disease is believed to be a real or essential fever, so, of course, it is believed to be contagious and infectious, or both—whatever these terms may mean. In recent times, this mode of communication has come to be regarded as so certain and yet so subtle and mysterious that many teachers and a great body of practitioners have been terrified out of their senses by it. One cries out that the obstetrician must not wear gloves, and it would be just as rational to say he must not wear clothes.

Perhaps he might be permitted to paint, and go about his practice as the ancient Britons fought. Many say that the practitioner who has a case of puerperal fever must give up his practice and go through various processes, and not return to his avocations for a period varying from a fortnight to six weeks. Others, on this point speaking logically, go further, and say the accoucheur should give up his practice, not only if he has on hand a case of puerperal fever, but of many other contagious diseases, such as scarlet fever, typhus, small-pox, measles, erysipelas, fœtid abscess, etc. Of the many who propound or teach such doctrines, I have never known one who practiced them, and I cannot say their feelings on reflection are to be envied. If such be good doctrines, they are, of course, equally good for physicians surgeons as for obstetricians, but the poor obstetrician is laden with restrictive burdens which his medical and surgical brethren do not recognize or touch with one of their fingers. Yet, the old proverb tells here, that what is sauce for the goose is sauce for the gander, and accordingly physicians and surgeons must follow the rules they inculcate on the accoucheur. In truth, these extreme practical doctrines of contagion are absurd, for they render all practice, whether medical, surgical, or obstetrical, an impossibility, or at least reduce the number of patients cared for at a time to one, which amounts to the same thing. In this matter much error and evil are, I believe, introduced by confusing the duties of the practitioner with those of the nurse, two quite different and almost, if not altogether, incompatible occupations. A practitioner must always, in such circumstances as we are now considering, carefully eschew undertaking the functions of a nurse, for, if he do, he must submit himself to the code of rules that regulates the conduct of nurses. If, avoiding a nurse's duties, a medical man of any kind cannot make himself medically clean in hands and person and dress, all kinds of medical practice as at present carried on are impossible with due regard to the safety of patients. Every one who knows the safety of actual medical practice must see that such views of contagion end in absurdity. But it is not, despite all this, to be supposed that practitioners are not bound by the most solemn considerations to take most scrupulous care against being disseminators of disease; and there is no disease with which they deal, where such care is more imperative on them than puerperal pyæmia. The puerperal woman presents in her contused, lacerated, and inevitably wounded passages the most favorable nidus for the reception of morbific material; and the woman suffering from puerperal pyæmia in any of its forms, and patients suffering from some of the allied diseases, present this morbific material in its subtlest and most potent essence. A well-demonstrated communicability arises from this source. No other has been demonstrated, but it is possible that in an ill managed hospital there may be some other. For the existence of another source, several of the best recent authors offer slight evidence; but, on the other hand, its

existence is rendered very doubtful by the alleged absence of pyæmia in those surgical hospitals or parts of hospitals where the antiseptic treatment of Lister is properly carried out.

Another result of this extravagant and superstitious dread of contagion is what I deliberately call the slandering of our noblest and most useful institutions—hospitals—and in that word I include all hospitals for the sick, not those for lying-in women only. Although it is against the latter that most of the foolish talk is directed, it is vain to suppose that they alone suffer. If one kind of infirmary be indefensible, so are all kinds; they must stand or fall together. The laws of pathology are not varied in nature with a view to the misfortunes specially of lying-in women. No doubt, the slandering is done with an excellent intention, under a good motive, but it is none the less what it is called, censuring injuriously and falsely, or without sufficient evidence. There have been, and there may be now, maternities which are justly calumniated as injurious, but that fact is no excuse for calumniating all. After abundant evidence has been adduced to show the directly and indirectly erroneous character of Le Fort's statement, that while the mortality of hospitals is 1 in 29, that in private practice is 1 in 212, a recent lecturer on pyæmia repeats it as if it were uncontested, and weakly appeals to authority on a point capable of scientific demonstration. We have reliable and large statistics to show what a moderately good hospital is, and we have no thoroughly reliable evidence that better results are anywhere obtained, whether within an hospital or private practice. Among the good, I place the Rotunda of Dublin, the reports of whose recent condition you may have seen from the pen of Dr. George Johnston, its present master. So great is this superstitious dread of hospitals and reliance on imperfect statistics, that one eminent author believes he has made out that amputation of the forearm performed upon a poor man in his cottage is thirty times less fatal than if it were performed in an hospital. The paradox is not to be received because its basis is inadequate; and, considering whence come most of our hospital patients, I wish we had from this author some theory of the healing virtues of the concentrated and various filth of a highland bothy or cottage, or of a den in Edinburgh Cowgate, or the London Ratcliffe Highway, accompanied, as it often is, by every abomination physical and moral.

As hospitals are in this facile manner made out to be bad, so, of course, large hospitals are worst, and statistics are again appealed to in support of this view. Were it not that a recent lecturer on pyæmia repeats this statement, I would not here allude to it, for it has been shown to be groundless by demonstrations as good as can well be imagined, but which seem not to have reached the eyes or mind of this eminent surgeon. Siebold believed that lying-in hospitals were so useful that shutting them up would bring far more serious evils than an occasional outbreak of puerperal fever. Whatever soundness there may be in Sie-

bold's judgment, I prefer, with Steele, to adopt a different view of maternities, and to look forward to the time when there will be no endemics of puerperal pyæmia, but only sporadic, or what are called autogenetic, cases.

Other errors connected with the old and still prevalent opinions regarding puerperal fever, though worthy of comment, must be passed over; but one, from its importence, demands notice. It is, that this disease is a kind of cholera, or a kind of typhus, or a kind of scarlatina, or owns the same or similar causes. In this country it is almost exclusively the identity with scarlatina that has found supporters. But at home and abroad the doctrine has, in some form or other, been extensively entertained. It is to be carefully distinguished from that reasonable view of Pouteau, Alison, Sidey, Nunneley, and Tilbury Fox, that the disease has close alliance with erysipelas, and, I might add, with the diffuse inflammation of Duncan, a view which has been partially adopted and signally illustrated by Virchow in his now celebrated paper on Diffuse Puerperal Parametritis. Scarlatina is a source of terrible danger to lying-in women, and scarlatinoid rashes are seen in some of the worst cases of septicæmia; and herein probably lies the attractiveness of the theory. But, unluckily for its supporters, and especially for the most recent, the theory has been disposed of by the researches of Hirsch and of Veit, who, by statistics of the comparative prevalence of scarlatina and of puerperal fever at different times and at the same time, have shown that there is no relation between the two. This argument against the theory is far stronger than any in favor of it, and must meantime be held as conclusive. It had been urged by these authors and by Späth before the *Lancet* did the good service of bringing Farr into the field on the same side.

Every change in doctrine or in name does not necessarily indicate progress; often, indeed, it indicates retrogrese. Such unfortunate changes arise more frequently from error in philosophizing than from error in observing. Among such, there is one in the history of our present subject. It has by some pathologists been proposed, not only to continue the name puerperal fever with all its adherent errors, but to go far towards introducing like errors into surgery by describing the allied diseases there as surgical fever. Had there not been the erroneous use of the word fever in child-bed diseases, there would undoubtedly have been one hindrance less of the general acceptance of modern views. Progress in surgical pathology runs no risk of being now impeded by the false name, surgical fever; and its adoption will certainly not do any good. By adopting, instead of puerperal or child-bed fever, the term puerperal pyæmia or some similar one, we by the mere use of words, enforce the argument for a great medical generalisation, making the gains of surgical and obstetrical science mutually beneficial.

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Many remedies for puerperal pyæmia have been proposed, and

their successful application loudly proclaimed and widely believed. Doulcet was even rewarded by the French Government for his discovery of the curability of this disease by ipecacuanha. In our own day new cures do not fail to make their appearance, and the advanced knowledge of our times would lead us to expect that they should be more rational, as the phrase is. But who is there of weight in the profession, now, who believes in any cure or in any system of specific treatment? All have been found wanting. Yet the wise physician of this formidable disease does not despair of guiding his patient through it, although he well knows its very dangerous character. Experience has shown him the utility of several means of relieving sufferings; and the favorable progress of a case may be encouraged, though not secured, by those invaluable directions which he may give as to diet and stimulants, as well as to more direct medicinal treatment of the genital passage, of the skin, of the bowels, and of the system generally.

But in this disease the physician has long been saying, not that prevention is better than cure, but that prophylaxis or prevention is to be chiefly looked after, and not cure. Great credit is due to Semmelweiss for the good he has done, especially to hospital patients, by his enlightened zeal in this cause; but the records of hospital practice sufficiently show that much more has yet to be accomplished. Prophylaxis is still farther to be carried out by attention to stop injurious communication between the sick and healthy, by disinfection, and by architectural arrangement, subjects which are all at present receiving much attention from the profession.

In the course of my remarks I have repeatedly referred to scientific researches as to the poison producing pyæmia, and as to the effects of its concentration, and as to the connection of this poison with the presence and diffusion of bacteria. These researches have been carried on mostly by observations and experiments on the lower animals, and of their very great value there can be but one opinion. But there is a variety of circumstances which seem to indicate that the lower animals are not subject to exactly the same laws as man is, and certainly there must be great caution exercised in arguing in human pathology from the analogy of the lower animals. The most important of the researches referred to, however,—those of Lister and his followers—have been mainly carried out in man, and consist, in a great degree, in the attainment of results in practice equally wonderful and valuable—results that can, so far as we at present know, be attained in no other way. These results go far to justify the belief that pyæmia is a septic disease, and that puerperal pyæmia may be almost, if not altogether, prevented by the application to delivery of a practice based on antiseptic principles. We know how much has already been, and is, I am happy to say, daily done with success in this direction. But the rules of Semmelweiss, or any other washing of the hands,

however carefully conducted, do not constitute treatment according to the manner of Lister. Such imperfect antiseptic precautions, by use of antiseptic gause and otherwise, I have used with apparent advantage; but we have yet a long way to go, in order to secure complete antiseptic delivery and subsequent treatment. To reach this desirable object, the efforts of several good minds are, I know, directed, both at home and abroad; and some recent unpublished cases of successful antiseptic treatment of wounds of the penis, where periodical discharges of urine have to be permitted, supply a sketch in miniature of plans that might be applied to ordinary confinements. To say more about them I have no right; but I conclude by calling upon you to give your best aid to forward the grand cause of the increased safety of lying-in women.

In the addresses delivered at the opening of the Section of Medicine, Dr. Eade thus sums up the advances of twenty or thirty years in medical science:

To mention only a few of the more purely medical subjects. Since the period alluded to—

The causes of fevers have been greatly elucidated, and their varieties more exactly differentiated.

The influence of poisoned air and poisoned water in producing disease has been fully recognized, and as strenuously combated by anti-septic and hygienic measures.

Very many local and parasitic diseases have been discovered or distinguished from each other.

Life, in its earliest and simplest manifestations, has been minutely examined, and new d ctrines of inflammation and other diseased actions derived therefrom.

Degenerations, natural and induced, have been inquired into.

The great questions of diathesis and hereditary influence, as well as the effects of alcohol and other articles of diet, have been critically examined.

The thermometer has been (medically speaking) almost invented afresh, and generally applied to practice.

Anæsthetics have come into general use as the solace of suffering humanity.

Thrombosis and embolism have become familiar to us as causes of disease.

And, last, not least, nervous action and nervous influence have been carefully and unceasingly studied.

A New Treatment For Burns.

Without offering comments upon the many remedies now in use for burns, the writer ventures to present one which may perhaps in part be new, and not without advantages.

For a considerable time it has been our custom to use eggs, the white and yolk together, well beaten up, as a local application, and the remedy has given a great degree of satisfaction. Our manner of applying is, after the eggs are well beaten, to saturate old and well worn pieces of muslin therein, and spread over the injured surface, two or three layers thereof being superimposed; the relief is immediate and complete. The dressing should be renewed each twelve hours, meantime, should it become dry, it may be moistened by dripping water over it. After the first two or three dressings we add a little carbolic acid and glycerine, to correct any disagreeable odor, and also stimulate the healing process. The dressing is easily removed, and leaves a clean, fresh-looking surface, not attainable under the old processes.—*Chicago Medical Journal.*

Treatment of Small-pox by Antiseptic Solution.

[Dr. Hyde relates the following case of small-pox with treatment to the Society of Physicans and Surgeons of Chicago.]

Some two years ago the attention of the profession was attracted to a form of treatment by antiseptic solution, first reported in a medical periodical of Canada, and largely copied by home journals. It consisted of carbolic acid, dr. j; of Squibb's pure medicinal sulphite of soda, dr. x; and f. oz. vj of water. Dose, children, one-half to one drachm; to adults, a tablespoonful, every three hours. Externally, a solution of carbolic acid (dr. ij), to glycerine (ʒiij). A febrifuge was also advised, of potassa chlorate, spts. nitre, and liq. ammonia.

Dr. Hyde employed the above treatment, modified, and on the sixth day of this patient's disease ordered: ℞. Acidi carbolici cryst. dr. j; sodæ sulphitis (Squibb's pure medicinal), dr. x; aq. menth. piperitæ, aq. puræ, *aa* f. oz. iij. M. Sig. one teaspoonful every three hours, day and night.

The external lotion was made of less strength, and modified thus: ℞. Acidi carboli, dr. j; glycerine, f. oz. iv. M. and use on the exposed parts of face and neck.

The result was as gratifying as surprising. Seventy-two hours afterwards, the ninth day of the disease, the little patient seemed practically cured of his malady; the eruption had everywhere subsided; no intumescence of the skin between the vari; the itching of the skin was very slight, and the child was dressed and at play. The subsequent history of the case is that of perfect restoration to health.—*Chicago Medical Journal.*

Treatment of Chronic Diarrhœa.

In the Brooklyn City Hospital (*New York Medical Record*), in

cases where the epithelium is stripped from the tongue, and the patient presents the cachexia of the disease, good results have been obtained by the administration of powdered ipecacuanha, in twelve-grain doses, three times daily, given mid-time between meals to prevent emesis.

This is continued until the stools are of a perfectly serous nature, when the ipecacuanha is discontinued, and four grains of oxide of zinc and six grains of extract of quassia are given in capsule three times daily.

Guarana in powder has been in used in similar cases with apparently very good cures; but as it is impossible to keep trace of the patient, the permanency of the cure is not established.—*London Medical Record.*

NOTICES OF NEW BOOKS.

The Physiology of Man: Designed to represent the Existing State of Physiological Science, as applied to the Functions of the Human Body. By Austin Flint, Jr., M.D., Professor of Physiology and Physiological Anatomy, Bellevue Hospital Medical College, &c. Vol. V. (with a General Index to the five volumes). *Special Senses: Generation.* 8vo., pp. 517. New York: D. Appleton & Co.; 1874.

This is the concluding volume of a work commenced eleven years ago, the general merits of which are so well known as not to require extended notice at this date. The several volumes have been favorably received, as they were successively published, and have given to their author, who is still a young man, a leading position among the physiologists of the day.

In the preface to this volume the author remarks that he adheres to the views enunciated in those which preceded, among which are named the production of sugar in the liver, and the greatly increased secretion of urea during severe muscular exercise. In the last number of this JOURNAL we had occasion to notice a recent work of Dr. F. W. Pavy, of London, which seems to us successfully to controvert these doctrines. The existence of sugar in the hepatic vein must be followed either by its consumption in the lungs or its appearance in the urine. Its oxydation in the lungs is not supported by any proof, while there is good evidence that the sugar which reaches the liver through the portal circulation contributes to the production of fat. Such is certainly the case in the fattening of animals on food chiefly amylaceous, in

the production of butter from milch cows and of wax by honey bees.

The doctrine that the excretion of urea has a constant relation to muscular force, or, in other words, that force represents the conversion of muscular tissue into urea, is controverted by the fact that there is a much more exact ratio between the ingestion of azotized food and the excretion of urea, and that the escape of carbonic acid from the lungs has a direct and precise ratio to the muscular force expended. This indicates clearly that force is produced, not by the consumption of muscle, but, like animal heat, by the consumption of carbonaceous matter, probably in the shape of fat.

The subject of dextral preëminence receives some attention in this volume. It has lately attracted the notice of several other writers. After mentioning several other views, the author gives preference to that of Dr. Ogle, who "conceives that dextral preëminence depends upon a natural predominance of the left side of the brain, the reverse obtaining in the left-handed." In confirmation of this, it is stated, on the authority of Dr. Boyd, that the weight of the left side of the brain exceeds that of the right almost invariably by about one-eighth of an ounce. If he had informed us that this difference existed in infancy, and that it was reversed in case of left-handedness, we might accept the statement as possessing great value; but in the absence of specification, we may fairly conclude that the brains examined were those of adults, mostly right-handed. Now the effect of continued use on the growth of muscles and organs is largely and familiarly exemplified, and it seems to us reasonable that left cerebral preponderance is rather the consequence than the cause of dextral preëminence.

The determining cause of right-handedness is, in our view, custom. Children acquire it by precept and example. That some should be incorrigibly left-handed is just as strange as any other perversity, and no more. As just remarked, most children are right-handed because so taught. Without such teaching they might as likely be left-handed, perhaps ambi-dexter, or probably do some things better with one hand and others better with the other, as determined by habit. To exemplify: the writer in his student days saw the advantage of being ambi-dexter in surgery, and commenced a system of training the left hand. The practice has been continued till many actions

are performed with about equal facility by either hand, and in some the left has the advantage. His children have been allowed free choice in this respect, and of three now old enough to be "handy" two are slightly left-handed, and not one shows a decided choice of hands in any use for them.

As it is likely that this work will before long reach another edition, a suggestion or two to the publishers may not be amiss. The book would be fully as convenient and much less expensive in two volumes than in five. It would be much more convenient and but slightly more expensive to contract with the binder for cutting the leaves. We remember reading many years ago a writer's preference of candles over lamps, inasmuch as the use of the snuffers gave time for reflection in the interruption of reading and writing. That may have been true in the days of slow coaches, but we are decidedly of the opinion that this necessity to stop and cut leaves is conducive rather to profanity than reflection. S. S. H.

Nomenclature of Diseases, Prepared for the Use of the Medical Officers of the United States Marine-Hospital Service. By the Supervising Surgeon (John M. Woodworth, M.D.) Being the Classification and English-Latin Terminology of the Provisional Nomenclature of the Royal College of Physicians, London. Washington: Government Printing Office; 1874.

We are indebted to Dr. Woodworth for a copy of this, the best edition which we have seen of the Provisional Nomenclature of the College of Physicians. The superiorities of this edition consist in a table of contents and a copious index, and it is to be regretted that it is not available for general circulation. Why could not Henry C. Lea or some other publisher issue one for the profession at large?

The Yellow Fever Epidemic of 1873. By Jerome Cochran, M.D., Professor of Public Hygiene and Medical Jurisprudence in the Medical College of Alabama.

Epidemic of Yellow Fever in Montgomery, Ala., during the Summer of 1873. By R. F. Michel, M.D.

The latter of these two pamphlets is restricted in its scope to the city of Montgomery. The infection is traced to Pensacola; the history of the visitation is briefly related; and then follows

the author's views on the natural history, the pathology and the treatment of the disease. In ascribing the outbreak at Montgomery to the arrival of two individuals from Pensacola, it is to be regretted that the author did not state whether they brought with them any effects which could serve as fomites. There is abundant evidence that the disease may be propagated and transported through the medium of fomites, but its transmission by simple personal contagion has been rendered very doubtful by a large mass of negative testimony, showing that persons who have contracted the disease in an infected locality may sicken at, or be removed subsequent to the access to, an uninfected locality without communicating the fever to persons liable to it.

The following statement is quoted, as deserving particular notice. "Another fact in connection with the pathology of this disease, is the development, during convalescence, of an inordinate venereal appetite. My attention was directed to this peculiarity by an intelligent professional friend; and after inquiry in reference to this matter, I find it was signally observable in both male and female. Why such a condition obtained, I can not imagine; especially when we are cognizant of the very tedious and dangerous convalescence in yellow fever."

From such information as we have been able to gain, this extraordinary venereal appetite has been quite the exception in convalescence from yellow fever in New Orleans, and some practitioners of many years standing had never heard of it. The same testimony generally agrees that convalescence in yellow fever is governed mostly by the behavior of the subject. Patients are apt not to recognize the extreme muscular prostration which attends this brief fever, and to do themselves great harm by early indulgence of appetite and leaving bed too soon. Under suitable restrictions such untoward results may be obviated, and then the rule is a convalescence rapid and safe, ending in perfect recovery.

The scope of Dr. Cochran's pamphlet is much more extensive, as may be seen from its table of

CONTENTS:

PART FIRST.

THE HISTORY OF THE EPIDEMIC.

Introductory—The Epidemic in New Orleans—The Epidemic in Memphis—The Epidemic in Shreveport—The Epidemic in Pensacola—The Epidemic in Montgomery—The Epidemic in Cal-

vert—The Epidemic in Greenwood—The Epidemic in Mobile—The Mobile Advisory Board of Health—Supplemental to the Account of the Mobile Epidemic—Communication of Dr. E. P. Gaines—Communication of Dr. C. M. France—Communication of Dr. E. H. Fournier.

PART SECOND.

THE LESSONS OF THE EPIDEMIC.

The Propagation of Yellow Fever—The Prevention of Yellow Fever—Quarantine—Disinfection—Yellow Fever and Dengue.

The history of the late visitation at the cities designated is derived from the most available sources of information, and we shall not now question its general accuracy. In tracing the mode of introduction of the fever at Mobile, Memphis, Shreveport, Calvert and Greenwood, it is attributed to the arrival of individuals from other places where it was previously prevailing, but the question of fomites is altogether ignored, as in the case of Montgomery previously mentioned.

The leading object of the writer in this pamphlet seems to be to attack the practice of disinfection, to prove its uselessness, and adduce evidence of its deleterious effects. We purpose to give attention chiefly to this portion of his essay, and see whether he has treated the subject fairly and thoroughly.

It is to be observed, first, that he restricts disinfection almost, if not altogether, to the use of carbolic acid; and it is to be added that our Board of Health in New Orleans do not undertake to disinfect the atmosphere with this agent, but on vessels and in apartments make use of fumigation with chlorine and sulphurous acid. To a limited extent atomized carbolic acid has been applied to the walls and ceilings of rooms, and the furniture therein, and this seems to have been the plan pursued on the vessels in the Third District last year. Dr. Cochran makes an argument against disinfection from the fact that this plan failed to arrest the fever on the shipping. Subsequent disinfection under the wharves was promptly followed by subsidence of the fever, but this is attributed by Dr. C. to the occurrence of frost just at this date—October 30. Reference to the meteorological record shows that the lowest temperature reached at and about that time was 38° F.—certainly not low enough to kill out yellow fever.

We have not space, nor is it necessary here, to note particularly the history of yellow fever in this city in 1871-2-3, in con-

nection with the efforts of the Board of Health at disinfection. It is not claimed by the Board that an actual demonstration has yet been attained of its protective value. It is believed, however, that the results justify perseverance in the practice, and that a comparison of our affliction from the pestilence with that which has fallen on several of our sister cities within the last three years is, to say the least, very favorable. In this statement reference is made to the ratio between aggregate mortality from yellow fever and the entire population, and not to percentage of mortality in cases reported. It is to be considered that our physicians have been generally reluctant to decide cases to be yellow fever until the disease has attained an undisputed and extensive foothold, and that serious complaints have been made of the use of crude carbolic acid; consequently it may be supposed that many cases have not been reported. But making all allowance for presumptive cases not reported, the fact remains that the fever has never attained alarming proportions in New Orleans since the epidemic of 1867. Dr. Cochran's estimate of 2000 cases for 1873 is acknowledged by him to be loosely made, and is very wide of the mark. He must have arrived at this result by a process similar to that used here two years ago, in publishing election returns without having possession of the ballots cast.

Another well-known fact should be borne in mind—that the earliest cases of yellow fever in any locality are generally marked by extraordinary fatality, and the same rule obtains in years when the fever appears only sporadically. On these grounds can be explained the apparent enormous fatality of yellow fever in New Orleans during the last three years. In confirmation, it is to be noted that the only eight cases which have occurred up to date this year (October 8th) have all proved fatal, though no carbolic acid was used about them till after their termination, except in a single instance, and then not near enough to be remarked by the patient.

Dr. Cochran writes in the most disparaging terms of the results of disinfection at Mobile last year. We have taken the trouble to inquire, and learn that it was carried out less thoroughly there than in New Orleans; that infected localities were enveloped by a belt sprinkled with carbolic acid, but that yards and areas within the enclosure were not thus treated. Let us compare the affliction of Mobile with that of Shreveport, where

no disinfection was attempted, and with that of Memphis, where it was begun too late and practiced, as he acknowledges, without system.

City.	Estimated Population	Estimated number of Cases.	Deaths.	Date of First Case	Remarks.
Mobile ...	35,000	200	30	August 21.	Confined to a small portion of the city.
Memphis..	45,000	7000 to 10,000	1800	August 10.	Extended over whole city.
Shreveport	10,000	3000	759	August 12.	Ditto.

The above figures, with the exception of the population of Mobile (estimated from the last census), are taken from Dr. C.'s pamphlet. He has not failed to denounce disinfection, but he has not attempted to explain why Mobile suffered less than the other two cities—less also than Pensacola and Montgomery. We do not undertake to assert that the comparative immunity of Mobile and New Orleans in 1873, and of New Orleans for several previous years, is entirely owing to disinfection, but it may reasonably be said that it looks like something else than accident, and that further trials are quite justifiable.

It is not claimed that anything like perfection has been attained in disinfection, either in choice of agents or mode of use. Last year the crude carbolic acid was used on the streets and in gutters, to the great disgust of the inhabitants of infected localities. This year a refined quality, having little odor and a strength allowing the addition of 50 parts of water, is used instead. The cost is no greater, and complaints are thus obviated. It is still premature to speak definitely of disinfection in New Orleans for the present season. It can be stated, however, that the fever has not spread beyond two centres where it has originated this year, nor beyond another where it has been introduced from Pascagoula; while the first cases occurred more than six weeks ago. S. S. H.

Transactions of the Medical Association of the State of Alabama, 27th Session, 1874. Pp. 428.

The first fifty-two pages of the volume are occupied with the minutes of the proceedings, and require no notice here. Then follows the address of the President, Dr. Geo. A. Ketchum, of Mobile, on "The Sanitary Needs of the People of our State, and

the Related Obligations of the Medical Profession." The importance of the subject is well presented; the actual achievements of Sanitary Science in diminishing disease and promoting longevity are forcibly represented; and the plan is recommended of influencing the passage of suitable legislation through the action of the State and County Medical Associations.

Following this address are the papers of Drs. Michel and Cochran, on Yellow Fever, the notices of which were written before this volume was received.

A Report on Dengue is contributed by Dr. Wm. H. Anderson, of Mobile. He regards dengue "as essentially a malady in which the nervous system bears the brunt of the attack, and in which the remedies to do good should always be addressed to the impaired nervous functions. Elsewhere he remarks: "I look upon dengue as an affection of the ultimate filaments of the cerebro-spinal nerves after they enter the muscles." This idea, in his view, is founded chiefly on the general muscular soreness and severe pains throughout the body, and also on the tedious convalescence. It seems to us that a similar reasoning would assign a like pathology to many other specific febrile maladies. In this connection another interesting question suggests itself—whether this special pathology of dengue belongs to the malady as it has manifested itself elsewhere; for he says: "It is indeed strange that dengue should have assumed so many different phases as are mentioned by intelligent authors who have described it in their particular localities. The account of the disease which Dickson gives in such graphic language, does not correspond with the malady as I have seen it in Mobile. The description of dengue given by Arnold, of Savannah, Campbell, of Augusta, and Fenner, of New Orleans, would not enable us in Mobile to recognize our dengue as the same disease which fell under their observation." Unfortunately Dr. Anderson affords no solution to the above question.

The eruption is not regarded as essential to the disease, being in the majority of cases merely a partial efflorescence. This feature corresponds to what was observed of the disease in New Orleans last year.

As may already be noted, the views of the writer are somewhat peculiar; nevertheless the paper is a valuable contribution to the literature of this singular disease.

A brief account of the cholera, as it prevailed at Birmingham,

Ala., in 1873, is contributed by Dr. M. H. Jordan. The infection is traced to Huntsville, through the medium of bedding. The author states that he found the disease amenable to treatment by opium and mercury in the earliest stage. The same tractability was observed of the choleraic disease which prevailed in New Orleans earlier in the season; and indeed it was not supposed to be true Asiatic cholera here until it had extended to more northern localities and committed fearful ravages.

Dr. J. J. Dement gives an account of the same disease, as it prevailed at Huntsville. It is here stated that the first case occurred June 3d, one day after the first case observed by Dr Jordan, of Birmingham; so that his theory of infection derived from Huntsville fails. It is remarked, however, that intestinal complaints were unusually prevalent at Huntsville previous to the outbreak of cholera, and that this pestilence had already ravaged Memphis, Nashville, and other towns in Tennessee.

Here we shall take the liberty to remark that we have found no satisfactory mode to account for the cholera visitation of 1873. From other localities, New Orleans has received the credit of supplying the infection, but the mode of its introduction here has never been accounted for. It had not reached nearer than central Europe, and was never known to take so long a leap in the dark. The Crescent City is accredited with any amount of wickedness, including original sin, and by this time perhaps is sufficiently reconstructed to produce original cholera. As originality is the true characteristic of genius, then let the cholera visitation of 1873 throughout the South and West be attributed to the Genius Loci of New Orleans, without taking the trouble of looking farther.

The most elaborate paper in the volume is entitled "The White Blood-Corpuscle in Health and Disease," by Jerome Cochran, M.D., Professor Public Hygiene and Medical Jurisprudence, Medical College, Alabama. It has been otherwise published, in connection with a paper on the Yellow Fever of 1873 by the same author and already noticed. To any one interested in physiology, and especially this particular topic, Dr. Cochran's essay will be highly interesting.

State Aid to Hospitals is the subject of an able article by Dr. P. Bryce, Superintendent Alabama Insane Hospital. A Reformatory for Inebriates is an object which the author thinks suitable for organization and support by legislative authority. Habitual

intemperance is regarded as a form of insanity, and therefore as demanding restraint and systematic treatment. The necessity of applying suitable methods to subjects not consenting, is squarely recognized, and he would have them committed by judicial process. Such a plan of treating drunkenness would undoubtedly be desirable and useful, provided it could be made practicable; but it is doubtful if civilization has reached a pitch sufficiently high in this, or any other country, to render the necessary legislation operative on the one hand without leading to grave abuses in execution on the other. Intelligent medical men may already be prepared to regard chronic drunkenness as equivalent to, if not actually, volitional insanity; but, before the general public will apprehend this view of the case, it is likely that a more material one will be taken—that the drunkard is robbing his family of the support which is their just due, and inflicting a pecuniary damage on the community through their pauperism. Dr. Bryce would deprive him of his liberty, as a person of unsound mind, for the purpose of effectual treatment, and would put him to work as a therapeutic measure; the unscientific practical reformer would confine him as a malefactor, for the purpose of reformation, and compel him to work for the discharge of his moral and legal obligations. Thus we see that both views and plans, would employ about the same modes of solving one of our most difficult social problems. The moralist hitherto has chiefly carried on the war with drunkenness, in some instances invoking the aid of State legislation, and with rather indifferent success. People are far from being ready to adjudge the drunkard a lunatic, but they know enough of political economy to see that he is a very unprofitable member of society, and enough of plain justice to see that his unchecked career is a constant wrong to both person and property.

The all-absorbing subject of Medical Education is also brought in as a sort of corollary to his general theorem. His plan is to organize and endow a State Medical College, on the principle that it is one of the duties of Government to provide a complete, as well as rudimentary, education for the people. The perfection of this plan would require State aid to theological schools also, but Dr. Bryce would hardly advocate such a measure, in view of the multitude of religious sects all sure to present their claims for support, and another, and probably the largest sect, of no religion whatever, equally sure to oppose the whole scheme. Un-

fortunately, we have sects in medicine also, and the difficulty would be the same in kind, if not in degree. In this happy country the lion is not legally authorized to devour the lamb, and yet they will not lie down together. When the millenium comes, all will enjoy the same religious faith and take the same physic. For the present, large allowance must be made for human imperfections and prejudices.

So much space has been occupied with the papers already noticed, that the others must be dismissed with barely a mention. These are, *Contributions of Physics and Chemistry to Practical Medicine*, by T. O. Summers, M.A S.A.; *Hæmorrhagic Malarial Fever in Alabama*, by E. D. McDaniel, A.M., M.D., of Camden, Alabama; *Puerperal Eclampsia—Chloroform—Forceps—Post Partum Hæmorrhage*, by F. M. Peterson, M.D.; *Anatomy and Diseases of the Cervix Uteri*, by J. S. Weatherly, M.D.; *Pneumogastric Nerve—An Interesting Case*, by W. H. Anderson, M.D., Professor Physiology, Medical College, Ala.; *Report on the Mortuary Statistics of the City of Selma, Ala.*, by Benjamin H. Riggs, M.D.; *Pycanthemum Linifolium*, by E. M. Vassar, M.D., of Dallas Co [This is a new remedy for atonic dyspepsia]; *Report of the Committee on the Contract System; Report of Committee on a Question of Medical Ethics; A Case of Occlusion of the Os Uteri at Term, resulting in Delivery through the Rectum*, by Goronwy Owen, M.D.; *Aspergillus Nigricans*, by W. D. Bizzell, M.D., of Mobile [A fungus affection of the meatus auditorius]; *Thermal Waters*, by A. L. Garnett, M.D.; *Jackson County Diseases*, by J. S. Bankson, M.D.; *Annual Oration—Cell-life the Basis of all Force, both Mental and Physical*, by S. D. Seelye, M.D., of Montgomery.

Twenty-seven years of united effort for the common good and the production of a volume of such extent, filled with matter of such general interest, indicate a spirit of the medical body in Alabama which in Louisiana is conspicuous by its absence. The following is perhaps a fair explanation: Voluntary associations commonly originate in large centres of population, of which there is only one in this State. In New Orleans our population is eminently cosmopolitan, and the Medical element so heterogeneous in its composition as not to admit of fusion. Probably this condition will last through the present generation of practitioners, but our successors may be kindred enough in interest, in opinion and in sentiment, to admit of cordial and harmonious

union for the promotion of the great and good ends which all, even now, agree in professing to pursue. S. S. H.

The Relation of Medical Societies to Progress in Science. Inaugural Address of the President of the Medical Society of the County of Kings, N. Y. Alex. J. C. Skene, M.D., June 16, 1874.

The subject of this address is reform in medical education, about which so much has been said and so little done, that it would seem an excellent case for the Chancery Court or the Circumlocution Office. The ideas of the author are admirable. He would require some preliminary education of medical students; he would have a lengthened period of medical study and graded classes; he would have clinical instruction instead of clinical lectures; the examination and the conferring of degrees should be separated from the schools. These would be excellent features, but unfortunately no effectual mode of bringing them into practice is clearly pointed out. If the schools could agree upon a plan of action and steadfastly adhere to it, it would, of course, be feasible; but the trial and failure of this scheme are familiar to all who are interested in the subject. It has occurred to us that the American Medical Association might adopt and carry out some effective plan to elevate the standard of general and medical education among practitioners of medicine, if they were so disposed; but any such measure is totally impracticable so long as the Association remains under the control of professors in the medical schools.

Another portion of the address is devoted to "The relation of the profession to the education of the public in questions of medical science." He would, to a certain extent, popularize medical science through journals in general circulation, so as to teach the public to shun quackery and enable them wisely to choose their medical advisers. Such an object seems to us a very desirable one; but the difficulty would be, that these popular journals would naturally fall into the hands of such medical men as would use them for their own advertising mediums with a public that does not take the trouble to distinguish between notoriety and reputation.

The discussion of such subjects by medical societies is certainly legitimate. The persistent agitation of a subject is some-

times conducive to the discovery of truth, but oftener to the providing of a way to success. The parable of the woman who gained her cause with the unjust judge by her importunity, and the story of a woman who married her suitor to stop his persecution, should encourage those who desire to reform medical education not to rest themselves, nor allow others to rest, until their point is gained. S. S. H.

Annual Address of Claudius H. Mastin, M.D., with the Proceedings of the Society of the Alumni of the Medical Department of the University of Pennsylvania, at the Anniversary Meeting for 1874.

The greater part of the address is devoted to the history of the Medical School, and the remainder to the subject of reform in medical education. The author recommends the endowment of chairs in the medical colleges; the extension of the term of study requisite for graduation; an examination of students on their general education previous to their admission to the medical schools; the establishment of different degrees of graduation for those who have completed the required term of study, according to proficiency shown at examinations. These are excellent recommendations, and many interested in the subject may remember to have heard of them before. It would be a happy arrangement to provide for medical professors like clergymen, with both a salary and a house to live in; and to furnish students with brains, a preliminary education and pocket money. They will all come in with the millenium. Meanwhile bricks will continue to be made without straw, the demand for doctors will be supplied, and the public will get about as good as they deserve. Nevertheless, we like to see the subject agitated. Stir up the water, or it will stagnate. S. S. H.

History in Disease. By J. J. Speed, M.D., Read before the Kentucky State Medical Society.

The author believes that the natural history of diseases should constantly be borne in mind, in order to avoid attempting too much by means of medication. He also makes a strong plea for professional candor in the relations of the physician with his

patients and their friends. Doubtless it is honorable, and no more than honest, to take a high moral ground and not stoop to deception, whether grand or petty. But the average human understanding is weak and prone to credulity; the appetite for truth must not always be gratified with the undiluted article; babes must be fed with milk; and physicians, like priests, should quality their truth according to the capacity of those who receive it. This is not meant as countenancing falsehood or duplicity in any form. We must not give spurious milk, though it is sometimes necessary to skim it more than people ask or expect. S. S. H.

On Strain and Over-Action of the Heart. By J. M. DaCosta, M.D., Professor Practice of Medicine, Jefferson Medical College, Philadelphia.

This is the third of the Toner Lectures "Instituted to encourage the Discovery of New Truths for the Advancement of Medicine," and is published among the "Smithsonian Miscellaneous Collections." Although it can hardly be claimed that this paper contains new truths discovered by original research, it is a good exponent of the present state of knowledge on a very important subject. S. S. H.

The Hypodermic Use of Quinine: A Dangerous Experimental Medication, and rarely justifiable. By Stephen Rogers, M.D.

The title conveys a clear idea of the contents of this pamphlet. The majority of physicians are ready to assent to the author's dictum, while some are so infatuated with this mode of medication, that no one who falls into their hands ever gets off with a whole skin. To such it is useless to adduce any evidence, or address any arguments, but to any open to conviction the author's views may confidently be recommended. S. S. H.

A New Apparatus for Extensor Paralysis. By John Van Bibber, M.D.

The object of this appliance is to relieve strain on the paralyzed muscles by affording artificial support, and is designed particularly for the affection of the fore-arm caused by lead-poisoning.

Briefly, it consists of an elastic rubber cord fastened to unyielding bands round the arm just above the elbow, and round the hand, with another band round the wrist to serve as an annular ligament. The appliance is intended to be merely supplementary to the usual treatment by strychnine, galvanism, &c., and it appears to us to be reasonable and likely to be useful. S. S. H.

The Annual Address before the Philadelphia County Medical Society. By William B. Atkinson, M.D., retiring president. Delivered May 8th, 1874.

This paper is entitled "Hints on the Obstetric Procedure," and is both sound and judicious. The author is the well-known Permanent Secretary of the American Medical Association.

Observations on Electro-Therapeutics. By A. D. Rockwell, M.D., Electro-Therapeutist to the New York State Women's Hospital.

This consists chiefly of a brief relation of a number of cases treated on the plan above indicated.

New Publications.

The following pamphlets have also been received:

Herpes Gestationis: A rare Affection of the Skin Peculiar to Pregnancy. By L. Duncan Bulkley, M.D.

Albuminuria. By William Bailey, M.D., Professor Principles and Practice of Medicine and Clinical Medicine, Louisville Hospital Medical College.

The Climate and Diseases of the Gulf Coast of the Florida Peninsula, with remarks on the former in Relation to Pulmonary Tuberculosis. By J. P. Hall, M.D., Tampa, Fla.

Transactions of the Medical Society of the State of West Virginia, 1874. S. S. H.

Archives of Ophthalmology and Otology. Vol. iv., No. 1.

We have received the first number of the fourth volume of this valuable publication. It contains an abundance of interesting

matter, especially clinical relation of cases, accompanied with instructive comments and practical deductions. It is idle to attempt to make a review of the number, which must be read in the original to be thoroughly appreciated. Two cases of Glioma of the Retina are reported, illustrating the clinical features of the disease, and presenting also some special interest. In the first case the glioma is accompanied with *metastatic* tumors *between the periosteum and the surface of the bone.* The second case is that of an infant, aged twelve months, the disease showing *hereditary* origin: a large number of relatives have died from exactly similar tumors, which, in every instance, first attacked the eye, and subsequently the surrounding tissues. The microscopical examination goes to show that the glioma originated in the inner granular layer. Two cases of Sarcoma of the Choroid, the first case presenting "a new feature concerning the propagation of intra-ocular sarcoma, viz., the *occurrence of secondary tumors by dissemination of germs from a remote tissue.*" The second case is one of Melano-Sarcoma of the Choroid extending to the Retina and Optic Nerve. The following *remarks* concerning that case are taken from the *Archives:*

1. *The age*, 22 years, of the patient affected with a choroidal sarcoma which showed a marked beginning of pigmentation is unusually young, only a few cases of choroidal sarcoma of that age being on record.

2. The *origin* and early *development* of the growth were free from pain and inconvenience, the patient discovered accidentally that the eye was blind, and had no pain from it until 18 months later. The second, or glaucomatous stage of the disease, set in when the eye had been known to be blind for fully two years.

3. The patient was *tubercuious* and of a tuberculous family, showing that there is no exclusion between tubercles and sarcoma, more especially between tubercles in the lungs and melano-sarcoma in the choroid.

4. The sarcoma *originated in the vicinity of the optic disc, overlapped the disc* in its growth, folded the retina up, and detached it from the choroid in its whole extent, but *remained itself partially covered by it.*

5. *The close contact of the sarcoma with the retina* and optic nerve explains its extension to these parts by *immediate propagation.*

6. It is remarkable that the infiltration of sarcoma cells did not reach beyond the retina proper, the pars ciliaris being free.

7. At the anterior part of the retina, the foreign elements formed *excrescences and little tumors with a free surface.* It appears plausible that the cells on the free surface of these tumors may, by contact, transfer the morbid action to the neighboring parts, as we have seen in the ciliary body of the foregoing specimen, or that such cells may be detached from the surface of the excrescence, fall through the subretinal space on the inner surface of the choroid, and thus form secondary growths. The retina, with its excrescences or small exulcerating tumors, may become the seminium of isolated secondary choroidal tumors, as we have likewise seen in the preceding case.

8. The present case represents many analogies with the former, of which it may be considered as a lower stage of development.

Prof. H. Knapp reports three cases of tenotomy of the superior and inferior recti. The operation for tenotomy of either muscle has as yet been very seldom resorted to, and we may safely venture to say that our knowledge of the history of such cases as reported by Dr. Knapp, is deficient to such a degree as to create great uncertainty as to what should be done in a given case. We earnestly recommend to our readers a thorough perusal of the original article in the *Archives*, if they wish to acquire additional *scientific practical* knowledge in cases of rare occurrence. We shall take no extracts from this article, which must be read throughout and thoroughly thought over.

The clinico-ophthalmological contributions contain the relation of a case of *Corectopia Binocularis*, two cases of Embolism of the central Retinal Artery, one case of Embolism of the lower branch of the art. cent. ret. in the right eye, the left eye being blind from embolic disease of long duration. The fourth case is one of Embolism of a branch of the art. cent. ret. with hemorrhagic infarction in the retina.

Dr. Knapp gives a description of a new ophthalmoscope, with a single disc, which seems to possess some practical advantages. To the instrument a table is added indicating the elongation and shortening of the optical axis, corresponding to the different auxiliary lenses, if the mirror is held 20 min. from the patient's eye. The auxiliary lenses are inserted into one disk, placed under a hole in the mirror, leaving the mechanism of fixation,

rotation and covering of the disk, the same as in the compound instrument.

A very interesting case of Keratitis Vesiculosa, *with secondary glaucoma*, is reported by Dr. Thomas R. Pooley, of New York, which case contains some teaching concerning the *lymphatic channels* of the anterior hemisphere of the eyeball. We shall take the liberty of stating here that a resumé of the recent discoveries in that line—we mean the lymphatic spaces of the eyeball—may be found in many periodicals (Hayem's Review in France, Aumale's d'Oculistique in Brussels, &c.). For the information of the average physician not directly interested in special studies, Dr. Pooley remarks that his case is the third on record, the first case having been observed by Von Graefe, the second by Saemisch: both cases are reported in Dr. Pooley's article.

Dr. D. F. Lincoln, of Boston, gives a translation of a case of great interest observed by R. Wreden—the case is one of phlebitis of the sinuses of the dura mater, caused by otitis; *it terminated in cure.*

Dr. Oscar Wolf, of Frankfort-on-the-Main, writes a very instructive article on the methods of examining the derangements of hearing, as concerns perception of sounds (translated by Drs. C. T. Blake and D. F. Lincoln). The author criticises the methods of testing the hearing heretofore employed, investigates largely into the tests by means of the human voice, relating numerous experiments, and concludes that speech is the most important means of testing the hearing.

Dr. R. Wreden, of St. Petersburg, contributes a most interesting article on Aural Fungi, taking advantage of his personal experience and that of others. He has observed 74 cases of Myringœmycosis Aspergillina, all of which belong to the two forms of fungi which he has described (*A. flavescens* and *A. nigricans*), excepting one case, of which he gives a separate description accompanied with a drawing of the fungus (*otomyces purpureus*). The examination and criticism by the author of the cases observed by others is highly instructive, and worthy of all the attention of the special reader. Dr. Wreden concludes as follows:

1. Myringœmycosis is an independent parasitic disease of the ear.

2. The Aspergillus, with its different forms of fructification and hybridism, must be denominated the specific aural fungus.

3. The ascomycete form (i. e., the form with utricular fomit) of the aural Aspergillus has been found.

Dr. Charles H. Burnett reports a case of Myringœmyeosis illustrative of the features mentioned by Wreden.

A case of pearly tumor (Cholestratoma) in both ears is furnished by Dr. C. T. Kipp, of Newark.

The otologieal review is given by C. T. Blake, of Boston, and the ophthalmological review by E. Gruening and H. Knapp.

Clinical Lectures on Diseases of the Nervous System. By William A. Hammond, M.D., Professor of Diseases of the Mind and Nervous System in the University of New York, etc., etc. Reported, edited, and the histories of the cases prepared, with notes, by T. M. B. Cross, Assistant to the chair of Diseases of the Mind and Nervous System, University of New York. New York: D. Appleton & Co., 549 and 551 Broadway; 1874.

A Practical Treatise on the Diseases of Women. By T. Gaillard Thomas, M.D., Professor of Obstetrics and Diseases of Women and Children in the College of Physicians and Surgeons, New York, etc., etc. Fourth edition, thoroughly revised. Philadelphia: Henry C. Lea; 1874.

Both of the last two mentioned books arrived too late for extended critical notices. Dr. Thomas' work has a reputation co-extensive with enlightened medicine, and it is only necessary to inform our readers that a fourth edition has been issued, "thoroughly revised" by its author, and as thoroughly executed by its publisher. We have looked into Dr. Hammond's book sufficiently to enable us to attest to the interesting, and we presume, instructive character of its contents. It is an octavo volume of nearly 300 pages, printed on excellent paper, and in a perfection of style which the midnight student may well bless as an exceedingly agreeable variety.

Erysipelas and Child-Bed Fever. By Thomas C. Minor, M.D., Cincinnati: Robert Clarke & Co.; 1874.

This is an octavo volume of 131 pages, excellently well gotten up by the publisher. The aim of the author has been to gather facts and figures sufficient to settle the question of connection

between erysipelas and child-bed fever. He has brought considerable industry and application to bear upon his work, and the practitioner will find this compilation to contain the statistics of these two diseases as afforded by the census reports and a portion of the medical history of this country. The general conclusions which the author announces are explained in the following propositions:

"1. *Erysipelas and puerperal fever seem to prevail together throughout all the States.*

"2. *Any marked increase in any one locality of one disease seems to be accompanied by a corresponding increase of the other.*

"3. *Where histories of past epidemics of either disease are obtainable from any of the States, the seeming connection of the two diseases was noticed by physicians at the time of such epidemics, and remarked on.*

"4. *For these reasons we are, I think, justified in concluding that there is an intimate connection existing between erysipelas and puerperal fever.*"

The Complete Hand-Book of Obstetrical Surgery; or Short Rules of Practice in every emergency, from the simplest to the most formidable operations connected with the Science of Obstetrics. With numerous illustrations. By Charles Clay, M.D., etc. From the third London edition. Philadelphia: Lindsay & Blakiston; 1874.

This manual should be kept continually within the reach of every busy practitioner. Especially will it prove valuable to those who, from their isolated situations, do not possess opportunities for obtaining consultations in many sudden emergencies.

Infant Diet. By H. Jacobi, M.D., Clinical Professor of Diseases of Children, College of Physicians and Surgeons. New York: G. P. Putnam's Sons; 1874.

We are indebted to Kain & Co., 152 Canal street, for this book. It is a duodecimo volume of 118 pages. This is a dogmatic little essay, in regard to whose merits we are not very favorably impressed. The reader may find a demonstrative smack of its character from the following quotation of rules in regard to "Summer Complaint."

"III. About Summer Complaint. It comes from over-feeding, and hot and foul air; never from teething. Keep doors and windows open. Wash your children with cold water at least twice a day, and oftener in the very hot season.

"When babies vomit and purge, give nothing to eat or drink for four or six hours, but all the fresh air you can. After that time you give a few drops of whiskey in a teaspoonful of ice-water every ten minutes, but not more until the doctor comes. When there is vomiting give no milk.

"Give no laudanum, no paregoric, no soothing syrup, no teas."

Essays on Conservative Medicine and Kindred Topics. By Austin Flint, M.D., Professor of the Principles and Practice of Medicine, and of Clinical Medicine, in Bellevue Hospital Medical College, New York. Philadelphia: Henry C. Lea; 1874.

These essays comprise papers read before various medical societies at various dates. The last one, entitled "Divine Design as Exemplified in the Natural History of Diseases," was read before the Young Men's Christian Association of Louisville. While they exhibit nothing particularly commendable for originality, either as it respects doctrine or prescript, they are interesting to the medical reader as very excellent arguments against too much medication in the treatment of disease.

The Physician's Visiting List for 1875. Twenty-fourth year of its publication. Philadelphia: Lindsay & Blakiston.

"Tucks, pockets and pencil. Arranged for from 25 to 100 patients a week. Sold by all booksellers and druggists."

A Practical Treatise on the Diseases of Women. By F. Gaillard Thomas, M.D., Professor of Obstetrics and Diseases of Women and Children in the College of Physicians and Surgeons, New York, &c. Fourth edition, thoroughly revised, with one hundred and ninety-one illustrations on wood. Philadelphia: Henry C. Lea; 1874.

The merits of this book are quite well known to the whole

medical profession of this country. We therefore discharge our obligations as Journalists when we inform our readers that a new edition has been published. The publishers have issued the book in their usual unexceptional style.

Surgical Emergencies: together with the Emergencies attendant on Parturition, and the Treatment of Poisoning. A Manual for the Use of the General Practitioner. By William Paul Swain, F.R.C.S, Surgeon to the Royal Albert Hospital, Davenport. With eighty-two illustrations. Philadelphia: Lindsay & Blakiston; 1874.

An extended notice of this work will appear in the January No. The subjects are deeply interesting, and appear to have been very carefully treated.

CORRESPONDENCE.

NEW ORLEANS, Sept. 21, 1874.

Mr. Editor—I believe it is often said that love-letters are subjects for ridicule to all except to the parties interested in their perpetration and reception. As time flows on, and the ardor which lent them their inspiration, with "first fond love grows cool," these effusions become spiritless and insipid to both the first and second persons, whom they once concerned. It is then that they are liable to be pronounced malapropos, both in diction and sentiment. Is not this strikingly true of physician's prescriptions? Who is able to appreciate them except the practitioner who orders them, and the patient who is asked to render the more hazardous proof of their appositeness by swallowing them? The precise aim of the prescriber, the apprehension he takes of his patient's symptoms, or the philosophy which governs him at the moment of prescribing cannot be appreciated by a third person, and are apt to pass from the recollection of the prescriber himself. It consequently occurs that prescriptions regarded as being wise at the time of writing, become afterwards matter for criticism. These reflections came, after having taken from my shelves some old prescription books, over whose pages I have been looking, and with unavailing efforts trying to recall conditions of disease

which could justify some at least, of the dosing recorded upon those pages.

As it respects some of these prescriptions, memory stands ready to vindicate their efficacy; others again, seem to me foolish and absurd. For, at this distance from the time when many of them were written, all that I can perceive is the name of some disease, and the names and quantities of certain drugs which were given for its cure, as these are often the sum total of the record. The circumstances amid which the prescriptions were written; the symptoms present; the precise condition of the patient, physical or mental; the antecedents to the prescriptions, are all lost sight of, and with their disappearance must likewise be lost the indications of cure as they presented themselves to the prescriber on the spot, and at the moment when the prescriptions were made.

But, leaving out of view for the time being, my own attempts to cure, which no doubt may often have been ill-judged and unphilosophic, let us take some prescriptions made in 1856 by one of the leading men in our profession in America. The patient for whom the said prescriptions were made, was somewhat peculiar in his notions, and as his usual medical attendant was absent, he called in the gentleman above referred to, and required the druggist to return to him every prescription which he filled. It has thus occurred that the copies here given are from the original papers, bearing at their upper left hand corners the doctor's split-toed ℞'s, and at the lower right hand side his own autographs. All of them were swallowed by the anxious sufferer, except about one-half of No. 6, which remained untaken when his own physician arrived and took charge of the case. Each reader must conclude for himself, to what further extent a man suffering the terrible pain of violent cephalalgia, would still have followed the beckonings of that cheer-inspiring, but often delusive phantom—"Hope." The disease with which the patient suffered was diagnosed as intermittent hemicrania.

It is my wish to lay these prescriptions before the reader, accompanied by my own efforts at interpretation of the philosophy which dictated them in each instance. Prescriptions No. 1 and 2 were made on the 4th of February:

℞. Extr. colocy, comp.
Pul. jalap - - aa ℈iss,
Sub. mur. mercury - grs. xv,
Pul. ipecac - - grs. iij.
S. Purgative, five for a dose.

℞. Sulph. Quinine - - ℈iss
Divide in nine pills. S. Three pills every 6 hours

It is evident that the prescriber has concluded that the disease is malarial neuralgia, and he proceeds to treat it accordingly, with preliminary purgation followed by quinine. No. 3 was ordered February 6th.

℞. Dover's powder - - ʒj,
Strychnia - - - gr. ⅓ (one-third)
℥. ft. Divide in 12 pills. S. Two pills every five hours.

This prescription was, without question, designed to meet the same indications.

On the 8th February it would seem from the prescription made, that the physician finding his patient not only not relieved, but having exacerbations every alternate day, determined to order up his heavy artillery against the malarial element in the case.

℞. Sulph. quinine - - - ʒj,
Morphia - - - - grs. ij,
Strychnia - - - - grs. iss,
Arsenious acid - - - grs. iiss,
Extr. stramonium - - - grs. iv.
℥. ft. pil. No. xxx. S. One every five hours.

On the 10th of February, the physician continues the prescription of the 8th, but again resorts to purgation. He knows, as man in all probability instinctively knew from the beginning, that "pain is often the prayer of the nerves for healthy blood," and he again endeavors to rectify erroneous secretions by elimination. With this idea ruling, he makes the following prescription:

℞. Sub. mur. mercury,
Extr. colocy, comp.,
Pul. jalap - - - aa ℈iss.
" ipecac - - grs. iij.
℥. ft. div. in pil. No. xv. S. Five at a dose—purgative.

On the 11th of February, the physician seems to have reached a turning point in his ideas of pathology and treatment. Heretofore, there is no evidence that he has ever suspected structural change. To his dismay he finds the energetic treatment he has pursued does not cure.

He perhaps learns, what should have been at first disclosed, that his patient has led an irregular life, and that he had run the

gauntlet of the dissolute as it respects disease. His position now affords a dilemma in which every practitioner is liable to find himself placed. Looking upon the skillful physician as more often the adjuster of disturbed function, than the corrector of structural changes, and yet remembering that cephalalgia may be due to either, and that intractable cephalalgia justifies a suspicion of the latter cause, we may begin to realize one of the principal difficulties which the prescriber had to surmount. Inward questioning brought up to his mind the query, have I to deal with the dynamics of the system merely, in which case disturbed functions and perverted forces constitute the pathology, or is there structural change at the bottom of it?

The result of these deliberations was, to change the treatment to one designed to meet organic alterations, especially those due to syphilitic poison. He therefore ordered the following:

℞. Iod. potash - - ʒiv,
Bichloride mercury, grs. ij,
Distilled water - - ℥viij,
Syrup ginger - - ℥ij,
Spts. lavender comp. - ℥ss.
℥: ft. sol. S. Tablespoonful every five hours.

On February 15th, it seems probable that the periodical complexion of the case became again more marked, since the following prescriptions were ordered.

℞. Sulph. quinine - - ℈ij.
Divide in ix pills. S. three pills every 6 hours.

℞. Sulph. magnesia - - ℥j,
Calcined magnesia - ℥ss.
℥. Divide in two powders.

On the next, and last day of this physician's charge of the case, he seems in utter desperation to have dropped all remedies, except the solution of iodide potash and bichloride of mercury, and the following in addition to it:

℞. Sulph. morphia - - grs. iij,
Water . - - - - ℥j,
Syrup ginger - - - - ℥ss,
℥. ft. sol. S. Tablespoonful pro-re-nata.

While I wish to assure your readers that I offer no unfavorable

criticism upon these prescriptions, and further, that their author is not only a great and good man, but one whom I esteem as a personal friend, there is yet another mode of looking at them which will afford instruction to younger members of our profession, and which may apply to a great majority of practitioners, old or young. Has it ever occurred to one of your readers to take the whole file of prescriptions which he has made for some violent attack of acute disease, and study them cumulatively? Take, for example, as I shall, in regard to the above case, each article of the pharmacopœia which has been ordered, and set opposite to it the aggregate amount given. This method of analysis will often afford curious results. The following list is correctly made out, for the case in question, which covers fourteen days of treatment.

Calomel	45	grains.
Comp. extr. colocynth	90	"
Jalap	60	"
Ipecac	6	"
Quinine	130	"
Dover's powder	60	"
Strychnia	1 5-6	"
Morphia	5	"
Arsenious acid	2½	"
Extr. stramonium	4	"
Bichloride mercury	1	"
Iodide potash	120	"
Sulph. magnesia	1	ounce.
Calcined magnesia	½	"
Syrup ginger	1 fl.	"
Spts. lavend. comp	2 fl.	drachms.

Looking at this list abstractly, one might say that the lamented Brinton would have pointed to such a case with some degree of satisfaction, as affording most extraordinary proofs of his statements regarding the powers of endurance of the human stomach, and afterwards of the human economy. But while I, who continually plume myself on being a most conservative physician, am willing to side with such a view, still a faithful retrospection of certain of my own cases obliges me to confess to hours, or even days, of treatment fully as heroic.

During the month of October, 1872, a woman came under my

care, suffering from an unusually grave attack of sub-acute articular rheumatism affecting a knee-joint. She had not been well cared for, or even well nourished, previous to becoming ill, and had travelled more than one hundred miles subsequent to her attack. The following list shows, with a great deal of exactness the drugs which she actually took while under treatment, and the amount of each respectively. Ths period of treatment covered forty-five days, but during the last week medication of all kinds was greatly diminished.

Chloral hydrate	120	grains.
Sulph. morphia	12	"
Ext. conium	16	"
Extr. hyoscyam	49	"
Pul. opium	135	"
*Pul. digitalis	126	"
Pul. ipecac	60	"
Iodide potash	ʒvi,	
Sulph. magnesia	℥j,	
Bitartrate potash	℥j,	
Syrup iodid. of iron	℥j,	
Cod liver oil	℥vij,	
Comp. tinct. cinchona	℥vj.	

Now as it respects the ultimate issue of these two cases, which have been adduced to show the sum total of drugging actually put in practice, I am able to state that the patient first referred to became insane, and was sent to a lunatic asylum, where, I think, he died. No post mortem was ever published. The second patient made an excellent recovery, and was delivered in the early part of January, 1873, of a vigorous and perfectly healthy child. This addendum is an important one, since the statement has not infrequently been made, that the fœtus in utero experiences its full share of the toxic effects of opium given during pregnancy. While it has not been my lot to be called to treat a case of opium-poisoning in the pregnant female, I have never hesitated to exhibit opiates to such patients for all the thera-

* The following was the combination under which opium was given, and its frequent repetition involved the amounts of digitalis and ipecac ordered.

℞. Pul. opium,
Pul, digitalis, aa - - grs. ix.
Pul. ipecac, - - - " iv.
M. Ft. pil. No. ix, S. One every 4 to 8 hours, as pain required.

pentic purposes to which they are usually applied in the treatment of disease.

Another point of interest, in connection with the practitioner's retrospection of his prescriptions, is rested upon the evidences he is almost sure to find of the influence of habit. At some period, readily discernible by a frequent repetition of particular prescriptions, the practitioner seems to have learned from his own observations, or to have borrowed from other sources, the applicability of certain combinations of drugs for the cure of given diseases. It may indeed, be some very old formula, so long forgotten that the prescriber may suppose it to be either original with himself, or with some friend who has suggested it to him. It has been a very short time since a distinguished physician claimed for his father originality for a prescription, which my own accidental researches proved to have been in use so long ago as the middle of the 18th century. But very little subsequent to the same period, some of the British surgeons, while serving in Asiatic climates, applied nitrate silver for the cure of diseases involving chronic ulcerations of the bowels. In 1847–8 I remember to have had under charge a considerable number of the volunteer soldiers who had served one or more compaigns in Mexico. A great majority of these patients were suffering from attacks of chronic diarrhea and chronic dysentery. My prescription books show the great confidence I had in nitrate silver in the treatment of these affections. Then, I believed that the cures effected were in great part, if not wholly, due to the nitrate silver. It is now my belief that the opium was the chief curative agent in the formulæ employed. The prescriptions under which the nitrate silver was given were the following.

℞. Nit. silver - - - grs. iij,
Pul. ipecac - - - grs. vi,
" opium,
Extr. Gentian - - aa grs. xii.

ℳ. ft. pil. No. xii. S. One night and morning, and oftener if diarrhœa and pain are urgent. Occasionally this prescription was pilled by the addition of pul. gum arabic, grs. xii, and oil cloves, drops vj.

In concluding these desultory remarks, it remains to say that of the great number of prescriptions which are recorded in my old prescription books, but few now remain in customary use.

The most notable exception to this statement is the following prescription, which for full thirty years I have frequently administered for certain dyspeptic conditions, attended with irritability, or pain of the stomach, and calling for an alterative, as well as a soothing course of treatment. I do not know the source from whence the prescription came, but make no claim to originality in its use.

℞. Sub-nitr. bismuth - - - grs. x,
Pul. Rhubarb,
Blue Mass - - - - aa grs. v,
Acetate morphia - - - gr. ss.

ℳ. ft. pil. No. v. S. One each night, or oftener if a speedy effect be desired.

Now that some of our contemporaries are exercising themselves to such a degree about claims of priority for mixing calomel and sugar, there is one other formula I desire to mention as often appearing upon my books, and especially in former years. I learned the use of this prescription from my preceptor, who called the powders "Catholicon powders," and who always enjoined the strictest care in triturating them together. It was as follows:

℞. Calomel,
White sugar,
Gum Arabic - - - aa grs. x,
Pul. Opium,
Pul. ipecac - - - aa grs. v.

ℳ. Ft. chart, No. x. S. One each night, in obstinate malarial attacks, or other forms of continued fever. If this once admirable physician were living now, he would be well advanced in the last decade of years necessary to make him a centennarian. How long he had been using this prescription when I first compounded it for him in 1839 I cannot conjecture. TATTLER.

RICHMOND, VA., October 19th, 1874.

The fifth annual session of the Medical Society of Virginia convened in the town of Abingdon, Tuesday, October 13th. The attendance was quite as large as could reasonably have been expected, the entertainments were as hospitable as could have been desired, and the session itself as harmonious and as profit-

able as any ever held. And it is about this session that I propose to write in this letter.

The meeting was called to order at 11 a. m., by the president, Dr. Alfred G. Tebault, of Princess Anne County. The Committee of Arrangements announced that Dr. R. L. Payne, Lexington, N. C., was present as a fraternal delegate from the Society of the Old North State. He was welcomed by the president, and invited to participate in the proceedings of the session.

After some routine business, the meeting adjourned to convene at night in the Presbyterian Church, the use of which was kindly offered the Society.

At night, the address of welcome on the part of the citizens was delivered by Hon. Wyndham Robinson, and that for the local Society by the President, Dr. E. M. Campbell—both well timed and well said.

Dr. M. P. Christian, of Lynchburg, was then introduced, and delivered "the annual address to the public and profession"—selecting as his subject the *Objects and Aims of Medicine.* There was present to hear the address a large audience of ladies and non-professional gentlemen, all of whom were interested and instructed.

A letter from Prof. Hunter McGuire, enclosing a petition to Congress asking for rank to be given the medical officers of the army, was read. On motion of Prof. F. D. Cunningham, the petition was recommended to the favorable consideration of the members of the Society.

Second Day.—Society called to order at 10 a. m. No report from either of the three Committees on the Prevailing Diseases of Tide-water, Middle, or Western Virginia.

On motion of Dr. G. Wm. Semple, the above committees were abolished, and in their stead six committees of one member each, with authority to choose assistants, were ordered to report on the advance of medical science in—1, Anatomy and Physiology; 2, Surgery; 3, Practice of Medicine; 4, Obstetrics and Diseases of Women and Children; 5, Materia Medica, Therapeutics, Pharmacy and Chemistry; 6, Hygiene and Public Health.

Dr. H. T. Bahnsen, Salem, N. C., having arrived, was welcomed as another of the fraternal delegates from the North Carolina Medical Society.

Election of officers for ensuing term being in order, Dr. S. C. Gleaves, of Wytheville, was chosen president. Richmond was

selected as the place for the next session, at a time to be appointed by the Executive Committee.

On motion, the retiring President, Dr. Alfred G. Tebault, was elected an Honorary Fellow.

At the night session, Dr. Tebault, in accordance with previous appointment, delivered his address as President. He selected *The Mission of the Physician* as his subject, which he presented in a very interesting manner, and showed that he had carefully studied his subject.

The report of the Committee on the *Effects of the Use of Tobacco*, Dr. W. W. Parker, of Richmond, chairman, was, by request of the committee, deferred until the next session, as some further experiments and observations were desired.

Dr. William C. Dabney, of Charlottesville, read a valuable paper on *The Development of Connective Tissue*, which was referred to the Publishing Committee.

The following appointments, by the president elect, for the ensuing session, were announced: *Essayists*—Drs. Wm. White, of Abingdon, and Thomas B. Ward, of Norfolk; *Delegates to the American Medical Association*—Dr. A. G. Tebault and thirty-five others—one delegate to every ten active members. *To the North Carolina Medical Society*—Drs. J. B. McCaw, A. G. Tebault, and eight others. *To West Virginia*—Drs. J. E. Chancellor, of Charlottesville, A. M. Fauntleroy, of Staunton, and Wm. S. McChesney, of Staunton. *To Mississippi*—Dr. E. M. Campbell, of Abingdon. *To Alabama*—Dr. Wm. F. Barr, of Abingdon.

On nomination, Dr. Wm. C. Dabney, of Charlottesville, was elected to deliver the Annual Address to the Public and Profession.

A paper on the *Use of Davidson's Syringe as an Aspirator*, by Dr. J. St. P. Gibson, of Waynesboro, was referred to the Publishing Committee.

Dr. Wm. L. Dunn read the *Report of a Case of Spontanous Expulsion of an Ovarian Cyst*, which was referred to the Committee on Publications.

Dr. Robert J. Preston, of Abingdon, read the *Report of a Case of Rupture of the Uterus during Labor—Death.* The Editor of the *Virginia Medical Monthly* asked permission to publish it in that journal. Granted. It will appear in the December number.

A paper on *Typhoid Fever—some Hints as to its Pathology and Treatment*, by Dr. Samuel K. Jackson, of Norfolk, and another

paper on the *Relation of Cryptogamia and Disease*, by Dr. Wm. D. Hooper, of Lynchburg, were referred to the Publishing Committee. Both are valuable papers, based on personal observation and research.

Anæsthesia, especially in Obstetrical Practice, was announced as the regular subject for discussion. Interesting remarks and personal experiences were detailed by Drs. R. L. Payne and H. T. Bahnsen, of North Carolina, J. E. Chancellor, G. Wm. Semple, and others.

Third Day.—After the morning's session was opened, the discussion on Anæsthesia, &c., was resumed, and participated in by Drs. G. Wm. Semple, Wm. F. Figgat, W. F. Barr, H. M. Grant, and others.

Prof. James B. McCaw delivered a very interesting lecture on *Electro-Therapeutics*. To illustrate his remarks, the speaker exhibited several different instruments made by the Galvano-Faradic Manufacturing Co., of New York, which the company had kindly placed at the command of Dr. McCaw, who explained the uses and the manner of using each of the batteries. After calling attention also to Dr. Garratt's disc, &c., Dr. McCaw exhibited an ingenious instrument of his own invention, by means of which to keep up a constant electric current through the body—which he especially recommended in many of the ovarian troubles. It consists simply of an abdominal supporter with large zinc plates, fitting in front over the lower abdominal wall on each side of the body, connected by metallic wire as a conductor to two similar plates which are placed behind on each side of the spinal column. The constant currents thus kept up through the body, the doctor affirms, he has seen do good in uterine and ovarian trouble; and by placing the supporter (?) higher up on the body and over the stomach, it may be recommended in certain forms of dyspepsia.

At the afternoon session the Treasurer's Report was read, showing the Society to be in a hopeful financial condition. The annual assessment per member was fixed at two dollars.

The Committee on Ways and Means, after due consultation, recommended that the transactions of this session be published as an addition to the *Virginia Medical Monthly*. Adopted. This measure was adopted only to enable the Society to publish its transactions in full at a lessened expense, in the hope that by so doing all financial embarrassments may be removed by the

next session, when the transactions will appear again as a separate volume.

Dr. Wm. H. Bramlette, of Pulaski County, read a paper on the *Etiology of Scarlatina*, which was referred to the Publishing Committee.

At the night session, Drs. Wm. Owen, of Lynchburg, Hugh McGuire, of Winchester, John P. Mettauer, of Prince Edward; and Joseph S. Edie, of Christiansburg—all of Virginia, and Drs. Joseph Jones, of New Orleans, J. Marion Sims, of New York, and Paul F. Eve, of Nashville, were elected Honorary Fellows.

On motion, all members over seventy years of age are exempted from paying the usual annual assessments.

The banquet given at a later hour by the Abingdon Academy of Medicine was an exceedingly *agreeable* part of the session. The ladies had a finger in the pie, and of course everything was as it should have been.

The summary manner in which I have been compelled to report the proceedings of this session forces me to exclude reference to much of interest and to withhold comment.

Yours truly, LANDON B. EDWARDS.

EDITORIAL.

We most earnestly invite the attention of our readers to the following copy of a petition which is designed to be presented to Congress during its next assembly. That a successful result may be secured for those members of our profession in whose behalf we present this petition, two things are necessary to be done. The first is, that the petition be as nearly unanimous on the part of our profession as it is possible to make it. The next is, that it be presented to the coming Congress at the earliest practicable date, in order that opportunities may be afforded our "resurrectionists" to bring it again to the surface after its probable inhumation by our national Solons. Unfortunately for our profession, the stubbs of our check-books pretty generally demonstrate in a quiet kind of way, that we are bereft of the most convincing arguments of the present era. But for all this, our profession has an influence which, when unanimously evoked, is quite sufficient to carry any laudable act of legislation. This

is a move to procure laudable legislative action for the relief of a large and useful section of the medical profession.

We are well aware that now is a most inopportune time to solicit aid from citizens of the afflicted and persecuted South, in behalf of any department of that national arm which, to speak in the mildest terms, has been attempted to be misused for the furtherance of our oppression. In despite, however, of all this—in truth, it would be better to say, *for* all this—we should work with more zeal, and greater unanimity, that we may secure justice to every member of a profession whose great ends are works of beneficence, and whose brotherhood should be unbroken, either in peace or war.

This petition does not originate with any medical officers, but is gotten up under the auspices of the American Medical Association. Physicians desiring to append their signatures to this petition should do so without delay, and forward their lists to Dr. John M. Toner, chairman, Washington city. Printed copies of the petition with blanks for signatures, may be obtained by addressing the Editor of this Journal, or the petition may be copied in manuscript and autographs affixed.

To the Honorable Senate and House of Representatives of the United States in Congress Assembled:

The undersigned members of the Medical Profession, residing in the parish of ————, of the State of Louisiana, respectfully petition your honorable body in behalf of the Medical Corps of the Army, to which we are informed existing laws do not accord, as in our opinion they ought, equality in rank with the other staff corps of the Army, or with the Medical Corps of the Navy. We understand that Medical Officers of the Army may serve continuously for thirty, and even forty years, without attaining a higher grade than that of Major; in fact there are at present among the Surgeons on the active list, several who entered the Corps from thirty-five to forty years ago, and served with distinction in the Florida war, the Mexican war and the late war, and yet whose present rank is that of Major only; while in the other staff corps and in the line of the army, regular promotion by seniority to the grade of Colonel, usually takes place in less than thirty years. Furthermore, we have learned with regret, that during the last session of Congress a reduction in the number of surgeons was made by law, so that the number now authorized for the army is exactly the same as the number allowed to the navy, although the number of men to be cared for in the army, and the number of posts or stations is so much greater, while at the same time the older army surgeons have

less rank than has justly been accorded to their brethren in the navy. In consequence of the reduction, and of the operation of previous laws, the medical corps is now so organized that those who may enter at the present time as assistant surgeons are not likely to become surgeons, or attain even to the rank of Major, in an average life time.

We respectfully represent to your honorable body, that the Medical Corps of the Army has not deserved such treatment, and that the officers who belong to it are not only in every way the equals of their brother officers with whom they share the perils, privations and hardships of frontier life and of Indian campaigns —(deserving, therefore, equal remuneration in the shape of rank and emolument,) but that they obtain their education at their own expense, and that from the high standard required for admission to the Corps, which we hope may never be lowered, they necessarily commence their career in the army much later in life than their brother officers. We would further respectfully represent that a service in which the highest rank that can be expected to be attained by length of service is that of Major, does not offer a fair remuneration to men who have the qualifications which army surgeons should and do possess, and who hold the responsible positions in which they are placed.

Your petitioners therefore entreat Congress to redress these grievances of the Medical Corps, and to put it on a permanent basis of respectability and usefulness, by providing that its officers shall hereafter receive a certain definite increase of rank in accordance with length of service, so that they may look forward to obtaining successively the grades of Major, Lieutenant-Colonel and Colonel, after a reasonable number of years, not to exceed thirty years service for the grade of Colonel.

And your petitioners will ever pray.

Medical Student.

A new medical journal, to be termed the *Medical Student*, is to be started at Wheeling, West Virginia. It will be under the charge of Dr. James E. Reeves, of Wheeling. The first issue will appear January 1, 1875.

Archives of Dermatology.

We are in receipt of the first number of the "*Archives of Dermatology*, a quarterly journal of skin and venereal diseases," edited by L. Duncan Bulkley, A.M., M.D. New York: G. P. Putnam's Sons, 308 Fourth Avenue. This number contains the following original papers:

I. Rotheln or German Measles—J. Lewis Smith.
II. Notes on Urethral Stricture—F. J. Bumstead.
III. Elephantiasis Penis, (with plate)—Robert F. Weis.
IV. Hints about tertiary syphilis—Charles R. Drysdale.
V. Lichen Planus—R. W. Taylor.
VI. Varieties of Urticaria—Howard F. Damon.

Correction.

In our publication of the excellent paper read by Dr. Lœber before the Medical and Surgical Association of this city, the author is made, by a typographical error, to quote "Hunter" as one of his authorities. It should be corrected to read Hueter. The latter has claims to a growing reputation in America, and should be accredited properly whenever reference is made to him by our writers.

Death of Dr. Anstie.

The following notice of Dr. Anstie's death will be read with many regrets by his admirers in this part of America:

The untimely death of Dr. Francis Edmund Anstie will grieve many readers on this side of the ocean also, who knew him well as editor of *The Practitioner.*

On Sunday, September 6th, he wounded a finger of his right hand in a post-mortem examination of a child. He complained of his arm on Tuesday and Wednesday, and poulticed his axilla on the latter day; but, although he had spoken about his arm to two or three medical friends, he never seriously consulted any one till Tuesday. Coma, pleuro-pneumonia, and erysipelatous patches on the pectoral region of the side where the hand was wounded, appeared; and he died at 2:30 on Saturday, with symptoms of clot in the heart, forty-one years of age.

He was the author of works on "Stimulants and Narcotics (1869)," of the articles "Alcoholism" and "Neuralgia," in Dr. Reynold's "System of Medicine," and of "Notes on Epidemics in 1866." These published works, however, represent but a small portion of the literary work which he did. He edited the *Practitioner* from its commencement, and contributed numerous articles to various journals, reviews, year-books, etc. For the last ten years, his labor was great; and he scarcely ever allowed himself rest.

We regret to add that he leaves a young family insufficiently provided for.—*Medical and Surgical Reporter.*

Medical Ethics.

Some person, perhaps the author of the paper to be referred

to, has sent us a copy of the *Utica Daily Observer*, containing an article from Dr. A. K. Gardner, of New York, upon the subject of Medical Ethics. Many of our readers are aware that Dr. Gardner is a man of more than mediocre talents, and also of considerable professional attainments. It may, therefore, be a matter for surprise that he should resort to the secular press to discuss the subject of which his paper treats. Medical journalists never close their columns against correspondents who are members of the profession in good standing, and who employ their pens in promotion of the general good of the professional commonwealth. But there is one feature in the communication to which it is proper to give especial notice. This is, the overstrained, and even venomously invective spirit he manifests towards the conduct of his own profession. We admit that he is apparently only an assailant of irregularity and inconsistency in his profession, but we presume that very few will feel that they do not understand the animus which lies at the bottom of the communication. Even if we take a more liberal view of the matter, and admit that the writer is actuated by the most proper motives, there will yet remain two great objections to his mode of seeking to rectify error. One of these consists in the bad taste he exhibits in bringing all the foul linen he can gather from his own profession, to wash in public; the other is in resorting to the public press, in order that the display should be a popular one. The truth is, that one standing verdict will scarcely ever fail in its application to all those members of our profession who so readily rush to the secular papers, either to vindicate themselves from fancied or real wrongs, or with the avowed purpose of edifying the non-professional in regard to some problem, whether ethical or scientific. This verdict is, that such persons have either already failed before their own professional brethren, or dreading failure, have taken an early appeal to the more ignorant and thoughtless public. The gist of Dr. Gardner's offence appears to have been consultations with homœopaths, or, as the Utica paper naively but rather ambiguously puts it, "with having held a consultation with a homœopathic physician of generous attainments and high standing." Of course, at this distance from the field of action, we can know nothing in regard to the gravity of the offence beyond what is actually stated, but the liberality of the medical profession in New York city appears

to us to border upon the extreme limits which regularity can submit to, and we must suppose that Dr. Gardner would not have been arraigned upon a frivolous or unjust charge. But a few days have elapsed since a highly respectable physician made the following report of his practice in the New York Pathological Society. "A teaspoonful of the following mixture was ordered at bed-time:

℞. Ex. cypripedii fl.,
Ex. asclepiæ fl.,
Ex. dracontii fl.,
Ex. scutellariæ fl. aa.

With this the patient sleeps very well, and can now rest on both sides."

In the West, or South, this kind of prescription would be held a sufficient cause for characterizing the practitioner as a "steam doctor;" in New York no such accusation seems to have obtained. And they are right, for such accusation should not be based upon any single prescription, whatever may be its character. The catholic nature and inclusive scope of the medical profession are the chief characteristics of its truth. It is only when these eccentric prescriptions are shown to be so much the habit and rule of the practitioner that they evidence his exclusiveness, that we have the right to arraign him on charges of irregularity, even though he may profess strict regularity. It is quite different with the homœopath, who not only bases his practice upon the absurd doctrines of potencies and "similia similibus curantur," but also seeks to put the regular profession upon the defensive by a continued repetition of assaults upon its teachings. Consultation with irregularity as manifest as homœopathy is an offence demanding arraignment before the profession. He who holds such consultations injures the profession at large, for he attempts an admixture of error with the truths to whose furtherance he is morally bound.

New Orleans Medical and Surgical Association.

At a meeting of the New Orleans Medical and Surgical Association held on the first Monday in September, Dr. Joseph Holt was elected President, Dr. S. S. Herrick Vice President, and Dr. W. H. Watkins Secretary and Treasurer.

Table I---August--September.

Day of Month.	Temperature.			Mean Barometer Daily.	Relative Humidity—Mean.	Rain fall—inches
	Maximum.	Minimum.	Range.			
Aug.						
26	94	76	18	30.019	62	.00
27	92.5	78.5	14	30.003	67	.00
28	91.5	77.5	14	29.947	66	.00
29	92	77.5	14.5	29.923	67	.00
30	82	73	9.0	29.889	82	.89
31	88.5	73	15.5	29.905	77	.13
Sept.						
1	89.5	77	12.5	29.956	74	.00
2	87	77.5	9.5	29.972	78	.08
3	86.5	77	9.5	29.986	80	.60
4	88.5	75	13.5	29.959	83	.03
5	———	———	———	29.986	76	.01
6	88	75	13	30.003	75	.00
7	89.5	78.5	11	30.077	79	.00
8	85.5	77.5	8.0	30.055	77	.30
9	88.7	77.5	13.2	30.008	78	.00
10	88.8	74.8	.14	29.944	77	.00
11	89.5	73.5	16	29.956	68	.00
12	88	74	14	29.989	66	.00
13	88	74	14	29.993	73	.00
14	87.5	73.5	14	29.976	73	.00
15	86	73	13	30.008	66	.00
16	87.5	74	13.5	29.986	69	.00
17	86	73	13	29.990	71	.00
18	89	75.8	13.2	30.007	67	.00
19	90.5	76	14.5	30.045	68	.00
20	87	76.5	10.5	30.071	63	.00
21	86	71.5	14.5	30.085	66	.00
22	88	76.5	11.5	30.050	50	.00
23	87.5	74.8	12.7	30.010	75	.80
24	88.8	76	12.8	30.000	77	.55
25	83.5	75	8.5	29.997	81	.00
26	86.8	74.5	12.3	29.935	83	.00
27	86.5	66.5	20	29.875	65	.10
28	78.5	61.8	16.7	29.940	51	.00
29	———	———	———	30.084	53	.00
30	75.5	63.5	12	30.124	53	.00
Mean..	86.86	73.97	12.89	30.002	70.6	Total. 2.47

METEOROLOGICAL REPORT FOR NEW ORLEANS.

Table II---October.

Day of Month.	Temperature.			Mean Barometer Daily.	Relative Humidity—Mean.	Rain fall—inches
	Maximum.	Minimum.	Range.			
1	——	——	——	30.084	56	.00
2	——	——	——	30.057	60	.00
3	86	69	17	30.106	64	.00
4	86	70	16	30.189	56	.00
5	87	67.5	19.5	30.135	50	.00
6	84.5	67.5	17	30.096	56	.00
7	84.5	69	15.5	30.051	61	.00
8	80	67	13	30.019	56	.00
9	80	63.5	16.5	29.992	62	.00
10	74.5	63	11.5	29.993	61	.00
11	81	68.5	12.5	30.057	65	.00
12	84	67	17	30.220	59	.00
13	80	52	28	30.338	43	.00
14	68.5	51	17.5	30.307	55	.00
15	69.5	56.5	13	30.250	70	.00
16	75.5	62.5	13	30.141	75	.00
17	79.5	62.5	17	30.099	58	.00
18	72	57	15	30.094	64	.00
19	74	59.5	14.5	30.093	68	.06
20	76	60	16	30.195	70	.00
21	79.5	60	19.5	30.202	72	.00
22	81	66.5	14.5	30.264	74	.00
23	81	66	15	30.136	74	.00
24	80	67	13	30.117	75	.00
25	80	67	13	30.200	77	.00
26	81	67	14	30.238	61	.00
27	79	62.5	16.5	30.155	57	.00
28	81	62	19	30.862	70	.00
29	82	66	16	30.100	70	.00
30	70.5	65.5	5	30.210	46	.00
31	62	53	9	30.305	44	.00
Mean..	78.60	63.29	15.31	30.174	62.00	Total. .00

Mortality in New Orleans from August 24th, 1874, to November 1st, 1874, inclusive.

Week Ending	Total Mortality.	Malarial Fevers.	Yellow Fever.
Aug. 30	129	17	None.
Sept. 6	87	7	1
Sept. 13	108	9	0
Sept. 20	122	16	0
Sept. 27	123	18	1
Oct. 4	132	17	4
Oct. 11	120	16	0
Oct. 18	140	19	0
Oct. 25	138	17	1
Nov. 1	106	14	1
Total	1205	150	8

THE

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ORIGINAL COMMUNICATIONS.

ARTICLE I. *Reminiscences of the Projectors of the Medical College of Louisiana, of the Visiting Physicians to the Charity Hospital, and of Classmates during my residence at the aforesaid hospital, in the years* 1835-6-7. By F. J. B. ROMER, M.D., etc.

"Vigilari et factus sum, sicut passer solidarius in testo."—101*st Psalm.*

Youth is the age of generous impulses, of high resolves, of noble aspirations; it is also the season of confiding friendships, of imprudent confidences, and of marvellous illusions. Endowed by nature with a spirit at once chivalrous and daring, it pants to flesh its maiden sword, and frets because it cannot quench its fiery ardor.

Like the war steed, pawing the ground impatiently while listening to the distant sound of the bugle summoning the host in battle array, youth is eager to participate in the conflict of the battle of life. It chafes at the restraints which hinder its free action. The years of pupilage comprise the most wearisome period of its existence. Time appears to move on at a most provoking slow pace, and independence-day seems to be removed at a fabulous distance.

But all things mundane are fleeting. Youth forms no exception to the law. The so much wished for milestone, marking

the great event which transforms the *adolescent* into the *man*, is at last reached; the toga of virility is assumed. The new-fledged man takes his allotted position in the arena wherein is fought the great battle for existence. Henceforth he is an actor, and plays his part in the exciting drama of human events.

Little did he dream, on entering the lists, of the fierceness of the strife—made all the fiercer because the weapons used by the combatants are none other than the passions of men.

The conflict lasts the better portion of man's life, and the tide of battle ebbs to and fro with varying success. Now *right* is in the ascendant, anon *evil* prevails; the latter perhaps the oftener.

Blows are received as well as given; none can expect to come out of the *melée* unscathed; and thus, to the elastic step of the young man of former years, to his erect and defiant attitude, to the high estimate in his own prowess, have succeeded the unsteady or cautious gait of the old man, a body bowed down with the recollections of past sorrows, a grave and subdued countenance, which reveals to the most unpractised eye the severity of the ordeal undergone, and the meek resignation of the man to a fate he cannot control.

The foregoing trains of thought suggested themselves, as I tried to recall to mind my reminiscences of teachers, friends and associates while a resident at the Charity Hospital of New Orleans, in the years 1835–6–7.

To those who may accuse me of allowing my imagination to soar through the realms of fancy, instead of keeping her within the narrow path of reality, I simply reply—

Of the original projectors and teachers of the Medical College of Louisiana, how small the number still among the living! Of the honored dead, how few who have succeeded in reaching the goal at which they originally aimed! Where is Luzenberg, where Harrison, Hunt, Barton, Powell, Riddell, Stone, Jones and others?

Where shall I seek for my classmates—a Lewis, a Carpenter, a Simeon Koski, a Delavigne, and others still, whose names do not at this moment recur to me? Where but in their graves? My young friends had ambition, the ambition to climb to the topmost round of the ladder which leads to fame. Helas! a relentless death claimed them as its own. Some died in the early part of their career, others lingered a while longer, then followed their comrades to the same bourne. All of them lost their

lives before realizing the programme with which they set out on entering active life, though several (among whom Lewis and Carpenter stand preëminent) would have risen to eminence had their lives been spared.

One consolation remains to soften our regret at their untimely loss. My young friends were at their posts when death called them hence. They died in the performance of their duties. All enjoyed, while alive, the esteem of, and were honored by, their fellow citizens. None, but reflected credit on his Alma Mater.

When I cast my eyes around me in search of the friends of my youth, and instead of meeting their beaming looks, their smiling faces, or feel the hardy shake of their welcome, I behold only their tombs, my heart is ready to sink within me, and it seems as if I were dwelling in a vast charnel house, surrounded on every side by death, and I the sole living occupant left to survey the ghastly picture. With the royal psalmist I may therefore in truth exclaim:

"Vigilari et factus sum, sicut passer solidarius in testo."

It were, however, a sad privilege if memory's office was solely confined to the remembering of past misfortunes, or to the bringing back to us sombre or disagreeable images of former years.

Memory possesses a higher attribute. A merciful God has endowed it with a quasi supernatural power of resurrecting the dead; of reanimating loved forms long since departed, of giving them the expressions, and make them look and act, as when they walked the earth in reality. We can even hold converse with them; nay, memory can bring back to us pleasures well nigh forgotten, and gratify our longing by allowing us to reënjoy them.

In a word, by memory's aid we can live our lives over again, almost to that happy period of childhood when, nestled in a parent's lap, we, for the first time, attempted to lisp the sweet name of mother!

I wish to avail myself of this gift of a beneficent Providence by recording, while life yet lingers in me, my recollections of the birth of the Medical College of Louisiana, and at the same time delineate cursorily the characteristics of some of the venerated projectors of the same. The narrative, however imperfect, may perchance interest some of the younger members of the profession by showing to them what the united efforts towards the accomplishment of an object can achieve, when persistently directed to that end.

The first faculty of the Medical College of Lousiana, who were also its projectors, was composed of the following gentlemen:

Dr. Thomas Hunt,
Dr. Charles A. Luzenberg,
Dr. James M. Mackie,
Dr. T. R. Ingalls,
Dr. A. H. Cenas,
Dr. E. Bathurst Smith,
Dr. Jno. Harrison.

The professors were all young men, with the exception of Drs. Cenas and Luzenberg. They were not, I think, overburdened with practice, but they loved their profession; they were ambitious to carve out for themselves a name which should be engraved in the temple of science, and they possessed, moreover, the *will*, and the *determination*, to push on, and to push up, through the rugged path which leads to success.

It was well for them their hearts were steeled against detraction, and that they had prepared themselves beforehand to accept ill success for years to come as the sole reward for their unremitting labor.

When the first prospectus appeared, announcing a course of medical lectures, to commence in January, 1835, the good people of New Orleans were taken quite by surprise.

Why! said some, such a thing as a medical school in New Orleans has never been heard of; the reputation of the city for insalubrity will always prevent medical students from pursuing their studies here; in a word, it is preposterous to attempt to establish such a school. Others objected, because of the youth of the professors, as well as for their presumed want of experience. Others again were much puzzled to know whence the apparatus would come from, and *who* would provide the halls in which the lectures were to be delivered.

The ancient population was, however, most amazed at the temerity of the young Faculty, and could only reconcile the ambitious daring of the professors by referring it to the proverbial audacity of the Americans.

Dr. Smith resigned before the first session began. He was succeeded by Dr. E. Barton, a gentleman in the meridian of his life, and well qualified to fill with credit the chair of Materia Medica, to which he had been called. Dr. Barton's manners were those of a Virginia gentleman of the old school—gentle,

refined, dignified. If he lacked in depth of thought, and if his diction was rather verbose, he more than made up for these deficiences by his untiring industry in collecting and in collating facts. His statistical tables are an invaluable legacy to Louisiana. New Orleans owes the Doctor's memory a debt of gratitude for his tables; they have contributed not a little to remove the stigma of insalubrity from her fair name; they have moreover demonstrated, that by abstracting the mortality from occasional visitations of yellow fever, New Orleans can compare with any city in point of salubrity.

The first session opened as announced with a class of eleven matriculants, a number which, though small when compared with subsequent classes, was at the time looked upon as a flattering beginning, auguring success for the future.

The second year witnessed several mutations in the Faculty, thus: Dr. Cenas resigned the professorship of Obstetrics at the close of the first session, which he did not resume again till 1839. Dr. Ingalls went also into retirement. He had taught chemistry the first session, and obstetrics a portion of the second. Why he should have left at so unseasonable a time, I cannot surmise; it may have been on account of ill health. Dr. I. had been, I think, a surgeon in the army; he was an excellent naturalist, and possessed great urbanity of manners.

Dr. Powell taught chemistry during the second session, having succeeded Dr. Ingalls. For some cause this chair was doomed to repeated widowhoods. Dr. Powell resigned the chair at the close of the second session.

Though possessed of but medium capacity as a chemist, the professor had the happy faculty of gaining the affection of his pupils. He was an enthusiastic phrenologist, and no ordinary adept in geological lore. His fine collection of fossils from the coal measures of Pennsylvania, made by himself, was proof of his ability and devotion to science.

Dr. Luzenberg retired during the session of 1836 and 7. Dr. Warren Stone succeeded as Lecturer on Anatomy and Surgery, and subsequently as Professor of the same branches, and the writer of this paper was then appointed Demonstrator of Anatomy, filling the place made vacant by Dr. Stone.

Dr. Luzenberg was a man of middle stature, with light hair, blue eyes, receding forehead, and florid complexion. He was, in fact, a perfect type of the Scandinavian race. In character he

was a man of energy and firmness, with a good sprinkling of self-esteem, the latter quality degenerating ofttimes into a "hauteur" calculated to repel. He had, however, strong prejudices, and, according as these dictated, likes or dislikes—he could be a staunch friend or a most bitter enemy.

As a lecturer, he was more discursive than deep, but was nevertheless a pleasant speaker and good teacher. Dr. L. was also a good anatomist, and as an operator he was dexterous and neat. As a surgeon he was bold and venturesome, but formed his judgment frequently too precipitately, causing occasionally difficulties or annoyances which might easily have been averted.

Let me recall an instance. A man is admitted into ward No. 5, during the early part of the second session; he had a large tumor, situated between the posterior margin of the scapula and the spine. Dr. Luzenberg sees the man, examines him, and turning to the students, announces a "malignant tumor, probably a fungus hœmatodes!"

Now it was the duty of the House Surgeon to visit at noon every ward for the purpose of prescribing for the sick who had come in since the morning visit, and whose condition forbade waiting until the evening visit.

Dr. Stone, on feeling the tumor, at once recognized an abscess, which he opened, not knowing at the time of Dr. Luzenberg's examination. When the latter returned in the evening, in company with some medical friends, to whom he had imparted the news of his "*trouvaille*," his consternation can readily be imagined when informed of the denouement in his patient's case.

I might relate other instances, parallel to the above, but refrain; my aim is simply to establish the correctness of my estimate of Dr. L. as a surgeon, not to defame his memory. In fact, I cannot think of Dr. Luzenberg's sad end without feeling the tears of sympathy moistening my eyes. The doctor's star of destiny was at its zenith at the organization of the medical college. He had then a splendid practice, stood highest in the city as a surgeon, was pecuniarily independent, and was with all the cherished head of an estimable family. What more could he desire, and what had he to fear? Lo! all at once, his star is seen to wane, and to take a downward course! What could the omen portend? Helas! human prosperity is the gift of a fickle goddess, of which she may deprive us when least expected.

When last heard from, the poor man had died in a distant city

of disease of the heart. If *that* heart could have been laid bare, we might perhaps have found crushed ambition, blighted hopes, and the sting of humiliation, the proximate causes of the organic lesion. A morally broken heart is a fearful thing to behold.

Dr. Thomas Hunt resigned the chair of Pathological Anatomy and Clinical Practice at the close of the third session. In his retirement the Faculty sustained a loss not easily repaired.

Professor Hunt had much personal dignity, but happily blended with great suavity of manner. Like the other members of his gifted family, Dr. Hunt was naturally eloquent. His language in the lecture room was always elevated, chaste, and appropriate. He was also the friend of the student; ever affable, ever ready to explain, or to help him out of his perplexities.

With the exception of the charter granted by the legislature to the medical college, in April, 1835, this institution received no aid, pecuniary or other, from any source whatever. The Faculty was left to its own resources. It toiled on nevertheless year after year, in the hope the day of prosperity would dawn after awhile, and that their fellow citizens would eventually appreciate its efforts to elevate medical education; meanwhile the Faculty had to defray, out of their private incomes, rents for lecture rooms, cost of apparatus, fuel, &c.

Little by little the classes increased in numbers. The college began to acquire fame; it endeared itself to the people of New Orleans; it made friends, who through their influence obtained the *lease* of a lot of ground from the legislature in 1843, whereupon the Faculty could erect a building for its use. The conditions of this lease were, however, burdened with so many onerous exactions, it looked as if the legislative wisdom of Louisiana was more anxious to stifle than to foster the growth of this infant institution of learning in their State.

The legislature little reckoned on the metal of the men who composed the Faculty; these not only erected a suitable building on the leased lot, but they complied with all the other stipulations imposed upon them. They performed still more, for they extended the fame of the college which their talents and labors had built up, until even enemies had to acknowledge it a success.

The Convention of 1845, appreciating the patriotism and scientific zeal of the Faculty; recognizing moreover the importance of having a medical school in the great emporium of the South, in-

corporated the Medical College of Louisiana into the newly created *University*, as its *Medical Department.*

The world is lavish in its award of praise and in its distribution of honors to the brave and successful soldier. It wreathes laurels around the brows of its warriors and its heroes.

Are the bloody fields of deadly strife the only arenas wherein heroism is displayed? As defined, heroism is synonymous with courage, daring and gallantry. Now, does it require less courage to stem successfully the tide of public opinion—less daring to originate a new and untried enterprise, requiring, to insure its permanent success, the appliance of diversified talents—less gallantry, in endeavoring to endow your country with an institution which will bring honor to it, when the realization of this laudable endeavor is encompassed on all sides with difficulties of every kind, necessitating years of pecuniary and personal sacrifices—years of arduous and unremitting mental toil?

To the original Faculty of the Medical College, Louisiana is indebted for a medical school of which she may well feel proud, because this school ranks second to no other in intrinsic excellence within our broad country, however richly we may suppose others to be endowed, or however hoary with age their walls.

Of the old Faculty, the venerable Dr. Cenas alone remains yet among us to receive our grateful homages. All of his colleagues have passed away, one by one, to a better world, there to reap the reward promised the faithful laborer.

A grateful posterity will, however, continue to cherish their memory, and their names will be encircled with garlands of love and affection, as long as disinterested philanthropy and love of science are appreciated by their countrymen.

During my residence at the Charity Hospital, its medical staff consisted of a House Surgeon, an Assistant Surgeon, and four visiting physicians. With the exception of one visiting physician, namely, Dr. John Ker, the balance of the above officers were members of the College Faculty.

Besides the above staff, the Board of Administrators of the Hospital authorized the appointment of six medical students, who were to reside permanently in the house, the admission being left to the choice of the Faculty.

When I first became an inmate of the hospital, Dr. Jno. H. Harrison was the House Surgeon, and Dr. Warren Stone his Assistant. After the first session, namely, in the spring, I think,

of 1835, Dr. Harrison resigned his position as House Surgeon, but was immediately appointed one of the visiting physicians. Dr. W. Stone succeeded as House Surgeon, and Mr., but subsequently Dr. Simeon Koski, a resident, acted officially as Assistant to Dr. Stone, until April, 1836, when, having graduated, he left the hospital. The writer of this was then officially appointed Assistant Surgeon by the Board.

Owing to his long connection with the Charity Hospital, either as House Surgeon or as Professor of Surgery, bringing him in constant contact with hospital patients, and owing also to his extensive civic practice in the city of New Orleans, it may well be asked, who does not know Dr. Stone? Indeed, it can be affirmed, that for over the third part of a century the name of Dr. Stone has been a household word throughout the Mississippi Valley, and wherever his name is uttered it is pronounced with sentiments of respect and love.

Whence this wide-spread popularity? Others have enjoyed equally as good hospital facilities, and had quite as lucrative practices; but no name, to my knowledge, either in this or other countries, ever awakened the magic thrill of affection in the breast of the masses as does the revered name of Stone. Again I ask, whence the cause for the manifestation of so universal a tribute of love? It was because, under the rough exterior of the man there was hid a "*big heart!*"—A woman's heart in sensitiveness for the misfortunes of his fellow-man; a heart whose every fibre vibrated in sympathy with the woes of the unfortunate!

Dr. Stone was emphatically the friend of the poor. On entering the house of a stricken family, he did not restrict the sphere of his usefulness to the alleviation of the physical sufferings of his patient, but, like the good Samaritan, when he perceived moral distress in his patient's features, he sought to console, and when he discovered want, he became the purveyor of that family.

Need I say it, one of the greatest sources of pleasure in my declining days is to have been the friend of so good a man. In calling him good, I do not thereby mean that he was faultless. His faults were, however, personal; they affected him alone; besides, have we not all of us our faults? The greatest geniuses the world ever saw had theirs. The sun, even, has his spots. Absolute perfection belongs to the *Infinite* alone.

Dr. Stone was not a learned man; he had not the advantages derived from a classical education, but he was an assiduous student, and sought by constant reading, and studying under *private teachers*, to repair the defects of his younger days' training. Dr. Stone possessed also a most tenacious memory, enabling him to retain the knowledge which he gradually acquired. The most prominent characteristic, however, of Stone's mind, was his "*practical hard good sense.*" This was the needle which directed his steps, and which, united to a perfect knowledge of anatomy, raised his name to the highest pinnacle of fame as a surgeon.

Dr. Stone was eminently a conservative surgeon. The object ever present in his mind was to repair, not to mutilate; to save, not to destroy; hence when he judged a limb might be restored to usefulness, there was no amount of labor or trouble which he was not ready to undergo to accomplish his purpose.

But if Dr. Stone was averse to operating on every occasion, or for the purpose of gaining renown, it may be said of him that he knew WHEN to operate—*how to select* the proper time when an operation held out prospects of success. Moreover, when he judged an operation necessary, he performed it at once, giving his patient thereby every chance towards a recovery.

It must be acknowledged, that recoveries from operations are successful the earlier they are performed. By delaying an operation in the vain hope of restoring a patient to health without a recourse to the knife, while his constitution is undermined by suffering, or hectic, or excessive suppuration, and while his recuperative powers are hourly diminishing, no other result can be looked for save disappointment.

The above reasoning applies with still greater force to hospital patients, or in time of war to the soldiers brought to the ambulances. In these two last classes of persons, excessive fatigue, insufficiency, or defective quality of food, moral depression, or else intemperance, have lowered the *vital powers*. Now is it wonderful, that in such men receiving serious wounds, the "vis medicatrix naturæ" should be found unequal to the task of repairing the injury which has been inflicted, and that the sooner the surgeon comes to the aid of poor nature the more promising the prospect of recovery.

But to return from my lengthy digression.

I do not claim for Dr. Stone the reputation of having been

either a *dexterous* or a *neat* operator; to my recollection he was neither, but he was decidedly a *safe* operator.

After an amputation he always delayed the dressing of the stump until all oozing had ceased, when he would wipe off with a soft sponge the smallest clot of blood; he was no less particular to produce perfect coaptation between the internal surfaces of the stump by a methodically applied bandage, and he was just as scrupulous to leave, in the depending portion of the wound, a large drain. By these precautions he generally insured union by the first intention.

I can remember but few fatal cases; these were mostly attributable to the shock of the operation. Let it be remembered, anæsthetics were unknown in 1836; surgeons were obliged to blunt sensibility by opium, which it was not always advisable to exhibit in full doses.

It was in the conservative treatment of operations, however, that Dr. Stone excelled, by his recognizing the necessity of building up the shattered fabric of his patient. Unlike most of his contemporaries, and in opposition to the dicta of authors, he had the courage to carry out in practice his own physiological notions. He believed that the greater the depression of the vital powers of his patient, and the more extensive the injury sustained by him, the more important also that the latter should be properly nourished. Concentrated beef tea, roast beef or beefsteak, wine, ale, or brandy, formed the staple articles of his dietetics.

If, after amputation, fever was kindled, if the patient suffered pain in the stump, and if, on examination, the latter was found red and swollen, the partial union already formed giving way, the wounded surface covered with an unhealthy and abundant pus, the doctor would, in addition to his dietetics, prescribe cinchona and acidum sulphuricum aromaticum in liberal doses.

The wound was closed and covered with a thick layer of lint, constantly moistened with liq. sodæ chlorinatæ.

If sloughing was threatened, poultices of Peruvian bark, sprinkled over with pulverized charcoal, were applied over the wound, and if erysipelas made its unwelcome appearance, it was combatted with the tinct. ferri chloridi, the doctor's favorite chalybeate.

When I recall the observations of this remote period of my professional life, I cannot but admire the sagacity which dictated the treatment of Dr. Stone, when contending against septic

agents always rife in the atmosphere of hospitals, but the nature of which was then but imperfectly understood.

The untoward accidents of septicæmia and of pyæmia, always to be apprehended in the wounded exposed to the impure atmosphere of hospitals and of crowded ambulances, might, I verily believe, be in a large measure avoided by the adoption of Guérin's dressing of cotton wadding. If, as the learned surgeon of the "*Hotel Dieu*" affirms, the exclusion of impure air from the wound prevents the development of *vibrios* and *bacteria* in the pus, and converts the latter into an inoffensive emulsion, then there can be no doubt his method will prove a real blessing, by lessening the danger from septicæmia and its consequences. Dr. Guérin states, the deaths from purulent infection after amputations in the hospitals and ambulances to have reached, in 1870, during the Franco-Prussian war, the frightful number of 29 in every 30!

Even though Guérin's dressing cannot prevent the deposition of the *ova* or *germs* of septic agents on stumps while the operation is performed, still, experiment has proven that the proportion between the quantity of pus formed and the amount of infiltration from the aqueous portion of the pus, by the wadding acting as a porous body, will induce in the pus a physical state incompatible with the development of organisms.

An analagous fact is observed in the alcoholic fermentation of sugar. Thus, *yeast* does not excite fermentation in syrups until the latter have been diluted with water.

No. 6 was in my time the obstetric ward, but owing to want of room, we also admitted into it occasionally cases of syphilis.

A woman near the term of parturition is taken ill with yellow fever, and dies. Her body being claimed by friends is left in the bed she died on. Over forty minutes after her death the nurse heard a noise proceeding from the deceased: affrighted, she came running to inform me of the fact. Examination revealed a *post obit* delivery of a dead but well formed male child, with the placenta lying by its side. Palpation through the abdominal walls discovered a perfectly contracted uterus.

The point of interest in this case consists in that it proves the vegetative functions to subsist for a limited time after death, as is already made manifest by the slow extinction of the animal heat, the growth of the hair, &c. The transition from life to death seems therefore not to be sudden, but would prove the body to retain a remnant of the activity imparted by the soul,

somewhat like a stone thrown, whose motion far from the impulsive activity diminishes little by little, and is extinguished only by gradation.

The above case was destined to furnish other points of interest. I had scarcely time to make the foregoing observations, when another woman is taken with violent labor pains; then a third one follows the example, and finally a fourth—the latter in the fourth month of pregnancy—calls for help. What was the exciting cause of these premature deliveries? Was it fear or was it sympathy?

At the time these women were confined, we had several others in the ward under treatment for lues venerea—one or two with extensive ulcerations. Now of the three women mentioned above, one only made a tolerably fair recovery; the two others had puerperal fever, of which the fourth died.

A woman in the puerperal state, when exposed to a contaminated air, is in the very best condition for the absorption of septic matter. Her depressed state, from the pains of labor and from loss of blood, the still partially engorged womb, the flow of the lochia, the latter being probably the vehicle through which the putrefactive ferments ascend into the womb, are so many causes which favor septicæmia, and consequent pyæmia in her state.

I cannot resist the pleasure of copying a very apposite paragraph from a most interesting article on ovariotomy, published in the *Atlanta Medical Journal* by my friend, Prof. J. T. Gilmore, of Mobile. He says, speaking of the puerperal state. "Pyæmia from thrombosis is fully illustrated in the parturient woman. After every confinement the large sinuses of the gravid uterus become filled with thrombi, and when everything goes well these thrombi become organized, undergo fatty degeneration, and are absorbed, thus obliterating the sinuses by the time the organ has completed the process of involution. Now suppose a lying-in woman is depressed from any cause, either physical, emotional, or by some septic agent, she may succumb to pyæmia from a softening and detachment of the uterine thrombi, developing pyæmia."

In the spring of 1835, an epidemic of purulent ophthalmia broke out in one of the male orphan asylums of the city. Some 30 or more little patients from 2 to 6 years of age were brought to the hospital. It was truly a pitiful sight to see these poor

children groping about the ward, with their swelled eyes, the lids glued together, to hear their moans from the intense suffering, and which they sought to relieve by constant rubbing with their tiny hands. In the greater number of these children the disease was already in the height of the second stage; the lids were tumified and red, the palpebral and the conjunctiva lining the globe of the eye were exceedingly swollen, and of a bright red; indeed the blood-vessels were seen coursing over the mucous membrane in all directions. In most of the cases the cornea had a dusky hue; in some, ulceration of the same had already begun, while in others an interstitial deposit of lymph had taken place.

If the disease was virulent, the treatment was no less heroic. Dr. Stone advised bleeding from the jugular vein, which we performed to "deliquium animi" on all the patients who were in the second stage. To the few in which the disease had not as yet progressed beyond the first stage, the doctor contented himself with the application of leeches to the upper eyelid or outer angle of the eye.

After bleeding, calomel, followed by an aperient, was given; warm emollient fomentations were employed to soothe and cleanse the eyes. When the blood-vessels were much dilated, scarification was had recourse to; next we pencilled the eyes with a strong solution of nitrate of silver. It is not my purpose to enter into a lengthy detail of the treatment from day to day; suffice it to say, all recovered—none lost their sight completely. One had staphyloma in one eye; three or four had moderate opacity of the cornea; some five or six returned with a few specks in the same.

Dr. Cenas' treatment of disease was "*eclectic*," with a decided leaning towards the physiological school; he was, besides, more systematic than any of his colleagues in the management of disease, and his success was proportionately great.

Dr. Mackie's treatment, like Dr. Cenas', was eclectic, but with something of the experimental dash in it. Delicacy forbids me to say more as regards these two worthy physicians, lest I trespass on the rules of good breeding.

Of Dr. John Ker I know comparatively little. He was very reticent with his colleagues of the hospital, though ever courteous in his bearing towards them. His medical opinions were a reflexion of those advanced by Dr. James Johnson; in other words, Dr. Ker was a stanuch mercurialist.

With Dr. J. H. Harrison I was, however, most intimate. I assisted at almost every post mortem examination he performed, and helped to make with him numerous physiological experiments. It may therefore be surmised, I had a fair opportunity to learn his views in reference to medical subjects.

Dr. Harrison was small in stature, his frame was of a delicate mould. He had a very intelligent cast of countenance, was quick of apprehension; always modest in his bearing towards others.

When alone he would lapse into low spirits, from which he roused himself and would even contrive to be cheerful when in public. Poor friend! well mightest thou feel discouraged, being aware the fell destroyer *phthisis* had gained entrance into thy slender frame.

Dr. Harrison was a physiologist of no common order, and as a pathologist, he had few if any superiors on this continent.

As a practitioner, Dr. Harrison gave the preference to an expectant plan of treatment. He was an indefatigable searcher after truth; he was committed to no medical theory, simply because he believed in none of those prevalent in his time. He never lost an opportunity to verify the diagnosis by autopsies, and to learn from them the pathological alterations corresponding with certain groups of symptoms during life; he did, however, not subscribe to the teaching of contemporaries that these alterations constituted *the* disease. With him, all that pathological anatomy revealed was the sequence of diseased action; hence, Broussaisism, organism, and all the other *isms*, found no admirer in him; and hence, also, he did not believe that *life* was merely an organism in action, or that it was simply the result of the play of physical or chemical forces on matter: Dr. H. was a *vitalist.* He felt convinced the *vital force* was of a different order from mere physical force, which it, indeed, often controls or resists; and hence his deference to the "vis medicatrix naturæ," as evidenced by his treatment of disease.

At the time to which these recollections refer, large numbers of cases of sporadic cholera were brought every spring into the hospital. Flatboat men and levee laborers made up the bulk of our patients. These men had seen much hardship, were exposed to all kinds of weather, and many had intemperate habits.

When admitted, the majority of cases were already in the second stage of the disease, and most of the remainder were in a state of collapse.

A few cases originated in the house. The preliminary symptoms were characterized by lassitude, an uneasy feeling about the stomach, a diarrhœa more or less frequent, sometimes accompanied by nausea.

Confinement to bed when the weather was damp, or when the purging was frequent, the administration of a few grains of calomel, of quinine with a vegetable astringent, or of opium when the patient complained of griping, generally sufficed to arrest the disease.

When, however, the disease was in its second stage, then dry frictions, sinapisms to the extremities and to the spine, were in order. Calomel was administered in quantities varying accord ing to the views of the attending physician, from 1 to 2 grains hourly, up to almost as many scruples during the same period. Acetate of lead and opium were frequently given to control vomiting, as well as to arrest purging. Broken pieces of ice were allowed to quench the thirst. Bleeding from the arm, when there existed great oppression in the chest, was resorted to. Scarified cups were often prescribed to relieve a burning sensation in the stomach, much complained of, or when there existed giddiness, or when the breathing was laborious. Stimulants, such as spir. ammon. aromat., sulphuric ether, tinct. capsici, wine, brandy, were given without stint to maintain strength or counteract faintness.

In the collapsed stage, besides the stimulants mentioned above, dry frictions, sinapisms, the hot vapor bath and the hot water bath, temperature of the blood, also frictions over the spine with turpentine, or the blistering of the same with liq. ammon. fortior, were all tried, to produce reaction.

If reaction was obtained, the after-treatment depended on whether there supervened congestion or not. Bleedings, general, local, purging, ice to the head, etc., were employed, according to circumstances, to counteract these accidents.

We made numerous post mortem examinations of subjects who had died of cholera, with but indifferent results.

Thus, the lungs were sometimes found engorged, giving them the appearance or feel of the liver; at other times they were collapsed, and then again they presented a natural aspect, though there had existed much oppression of the breathing during the latter hours of life.

The right cavities of the heart, and the larger blood-vessels of

the thorax, were ordinarily found distended with black grumous blood, but not uniformly so. Clots of black blood were also occasionally found in the left cavities.

Within the abdomen the peritoneum and omentum were generally pale. The stomach and intestines presented exteriorly a natural appearance, except that the latter were frequently distended with gas. Their mucous membrane was pale, or it was congested in patches, sometimes it was considerably softened.

The liver was generally congested, but not uniformly so. The gall bladder was commonly found distended with a thick viscid bile. The spleen presented nothing peculiar. The kidneys were congested. The bladder was ordinarily empty.

In the head we found frequently traces of more or less congestion, and even extravasation in patients who had died in the stage of reaction.

The facts elucidated by our examinations were, as it is readily perceived, in a measure negative. We could find no lesion by which to locate the disease. Cholera, like paludal fevers, was evidently a form of *miasmatic poisoning*, though *generically* distinct; for whereas pernicious paludal fevers generally induce a hemorrhagic diathesis, cholera as invariably favors the transudation of the *serum* through the vascular coats.

Whether cholera is due to the presence of myriads of *micrococci*, the fruit of an ustilago, in the rice water evacuations or in the vomited matter, as stated first by Klob and Thomé, and subsequently by Professor Hallier, of Jena, is not yet determined, but there can be no doubt as to the propagation of cholera by some such cause.

The most prominent feature of cholera is the lowness of the temperature of the body, which ranges from 10° to 15° F. and upward below the normal standard, and which may in part be attributed to an arrest, more or less complete, of the functions of nutrition, in consequence of an impeded circulation owing to viscidity of the blood from loss of its serum.

I believe the opinion has gained ground, which assumes that cholera constitutes a "*stage*" of fever, corresponding with the *algid* stage of intermittents. The primary object of the physician should therefore tend towards bringing about reaction. In its formative stage, quinine, opium, calomel and astringents, may be used; but when we find the diarrhea to increase, spasm to supervene, etc., it is futile to rely longer on their efficacy.

When the constitution is not broken down, bleeding from the arm at the beginning of the second stage of cholera, provided we can obtain a full stream, I have seen to produce a speedy reaction. If this cannot be done, wet cups were resorted to. General or local bleeding, when combined with dry frictions, vapor baths, and moderate doses of mercury, I have seen to succeed oftener than any other mode of treatment.

Of late chloroform has been given with much benefit in congestive chills; it has also been resorted to in cholera to check vomiting. The doses have varied from a few drops to one fluid drachm of the drug.

Were it ever my misfortune to be again thrown in the midst of a cholera epidemic, I should certainly be tempted, by reason of the analogy between the *stage* of cholera and the *algid* period of intermittents, to administer chloroform in doses not less than fʒi every half hour, until reaction was fully established, making use at the same time of frictions, sinapisms, vapor baths, etc.

Owing to its diffusible stimulating nature, the reaction induced by chloroform would scarcely ever assume a serious form.

Fevers constituted the majority of the ailments during the summer months. Intermittents opened the series, then came remittents, and finally, as summer merged into fall, we had pernicious malarial fever, or yellow fever, or both. Typhoid fever prevailed most in winter months, or in early spring.

The management of paludal fevers did not differ much from the treatment pursued at this day, except that bleeding, either general or local, during the febrile stage was often resorted to. Cinchona was also more frequently prescribed than now, perhaps because of the high price of quinine. Fowler's solution of arsenic was the favorite remedy in *tertian* and *quartan* fevers; also as an after tonic to guard against relapses, and to build up constitutions broken down by malarial intoxication.

The winter months filled the wards with numerous cases of inflammatory affections. These were treated by general and local bleedings, by tartar emetic on the Rasori plan, or in acute rheumatism by large doses of nitrate of potassa. Cardiac complications in the latter disease were not unfrequent.

Inflammations were then believed to be produced by an increased action of the capillaries of the diseased part. Swelling, redness, heat and pain, when the diseased action was superficial, and fever with pain, and perverted function of the organ assailed,

when internal, were assumed as positive evidences of the fact. Hence bleeding, by diminishing directly the volume of the blood, and indirectly by lessening the *materies morbi*, was held to be proper and necessary.

Unfortunately for human pride, the facts by which Brousaisism was upheld were falsely interpreted, or were no facts at all. A more attentive study and a closer investigation of the phenomena involved in the question, have conclusively shown that instead of *sthenie* or increased action, there existed in reality an *asthenie* or decreased action in the inflammatory process; that moreover, what was looked upon as a violent combustion resolved itself into a perverted nutrition of the part.

Clinical observations have furthermore shown, that bleeding with a view to eradicate an inflammation rather increases the dangers from the same, by its lessening the *vital* force; that inflammations run through a definite course, modified in each organ according to the tissues involved; that medical interference is powerless to arrest them when once formed, and that the sphere of action of the physician is limited to the duty of conducting them to a favorable termination.

When we consider attentively the various contradictory systems of medicine which have successively supplanted each other, or when we try to analyze the logic of the multifarious theories which have in turn swayed the medical world, we cannot but feel humiliated at the *instability* of its *creeds*, and the diversity of its formulas. Why these incongruities? *Nature* is *one; truth* is *one;* there can therefore be but one true interpretation of a pathological fact—one only true method by which health may be restored.

Why, I ask again, this divergence of opinion? It is because medical science rest now-a-days upon no deeper foundations than *plausible generalizations* of *more* or *less* well-observed facts.

We possess no *medical doctrine*, catholic in that it is founded upon an *universally* acknowledged *principle*.

We have deserted from the traditions of the past; have run after strange gods; have in fact become idolatrous, allowing our minds to become perverted by a materialistic philosophy which teaches that *vital force* is only a form or mode of the physical forces! Strange infatuation for physiologists to fall in, when they may daily observe at the bedside of their patients a death-struggle going on between the physical forces and a conservative

force which we call *vital.* When, moreover, during every moment of our existence this vital force keeps the physical and chemical forces within the limits it has prescribed for them!

Analyses of phenomena and their generalizations are necessary steps towards the acquisition of knowledge; but unless we refer them to a *first principle*, their interpretation will be as changing as the figures seen in a kaleidescope, and as unstable as the surface of the sea when agitated by the wind.

Let us hope that, tired of the constantly shifting foundations of their science, physicians will ere long return to the *time-honored principles* which a false philosophy caused them to abandon. Already we can perceive a glimmering of this auspicious return in a more correct pathology, and also in a more rational treatment.

Diseases begin to be looked upon as *morbid entities* which run definite courses, instead of being symptomatic affections ascribed to fanciful pathological conditions. The practice of *elimination* of *effete* or *toxic* matter through the excretory organs, is another affirmation of the truth of the Hippocratic doctrine—it is a late acknowlegement of the ancient doctrine of *crisis*, which the new practice forestalls, thereby extending a helping hand to struggling nature instead of robbing her of her strength.

Before closing, I would say a few words of my fellow resident students. These were M. M. G. French, Whitman Wilcox, Alex. Hart, and Alber Simeon Koski. Dr. French was a nephew of Dr. Barton; he was an estimable young man, of good princip'es, and evidently devoted to his studies. Unfortunately he possessed a defective hearing, which made him sometimes appear to disadvantage.

Dr. Wilcox, who succeeded me as Demonstrator of Anatomy, was always in good humor, had ever a smile on his lips, was never in a hurry, but who nevertheless, I *guess*, has better succeeded in the world than any of his associates.

Dr. Hart is the third on my list. I have not had the pleasure of seeing him for years, but his portrait has not faded from my mind—his ponderous sensitive nose, his dark keen eyes, his sharp features, are still vivid in my mind. He was a pleasant companion, was always ready with an anecdote, but he could be very serious. I fancy I can see him yet, engaged in a profound argument on some abstruse physiological question, his

index cutting the air like a pendulum, but moving, when he desired to be impressive, with absolute railroad speed.

Dr. Simeon Koski was a pupil of Dr. Luzenberg. He had imbibed from his tutor quite a taste for surgery. He settled in Baton Rouge after his graduation, where he soon acquired a lucrative practice with the reputation of being a good surgeon. Indeed, he was christened by friends as the young *Dupuytren.* Unfortunately, Simeon Koski, who was a Polish exile, had been obliged to perform manual labor for support before he began the study of medicine. Dame rumor reported that Dr. Simeon Koski had been seen working in a bakery at Cincinnati. Gallic maliciousness was satisfied; from that day, le docteur Dupuytren became le docteur Du Pétrin!

Poor fellow! I saw him prostrated by yellow fever in the fall of 1839. He had barely recovered when, on turning a corner his buggy wheel struck the curbstone, and he was thrown against a stone, causing his instant death. Peace be to his ashes.

ARTICLE II. *On Anatomical and Surgical Drawings for Class Demonstrations.* By Edmond Souchon, M.D., Demonstrator of Anatomy, and Chief of Surgical Clinic to the Professor of Surgery, University of Louisiana; Visiting Surgeon to the Charity Hospital of New Orleans.

Introduction.—It is my experience that none of the so-called demonstrative branches of medicine, such as anatomy, surgery, or obstetrics, can be decently and successfully taught to large classes of students, without a good series of drawings made for the purpose. It is likewise necessary that they be made in a very systematic and orderly manner; in such a way as to represent every fact, circumstance or condition, connected with the subject. By this means the students learn with the eye as well as with the ear.

Drawings for class demonstrations should be of very large size, so as to be easily seen from all the points of even a large amphitheatre. They should not be mere sketches or diagrams, but should be shaded and colored, and varnished over, so as to resemble paintings, in order to enable the students to seize rapidly the precise idea of what they represent, thus requiring but few words to make them understand the subject thoroughly.

The drawings should not be few and without any connection with each other, as are the drawings of most colleges, but they should form, as I have said, a complete and gradual series, following and assisting each other.

Drawings made after this manner are of most valuable assistance in lecturing on anatomy, surgery, or obstetrics. In many instances they are much preferable, in a large amphitheatre, than the actual anatomical and pathological specimens themselves; for when these are natural they are usually small, and to describe them to a large class by word of mouth is almost losing one's time, for the students can see nothing, and cannot be made to be so interested as if they had a good representation in drawing to follow with the eye as the words strike their ear. It is like describing a person's face by word of mouth; it will take many words to do it very imperfectly, whereas by exhibiting a good painting of him a glance will suffice, and it will only require a few words to call the attention to the most important peculiarities.

In no instance is the want of a good series of drawings more imperiously felt than in lecturing on structural and microscopical anatomy. This is the bugbear of students, the great majority of whom consider it imaginary and conventional, whereas it is most easy to master if proper pains are taken by the teacher to get up for himself a complete set of drawings (schematic and others), and also a set of sections of tissues and organs to show them under the microscope that the drawings are made after nature and not after imagination. I have heard many students of my private classes express their surprise and gratification at finding out how simple and beautiful these things were, when demonstrated by good and accurate drawings and also under the microscope.

The superiority of such drawings, made by an anatomist or surgeon, or obstetrician himself, over the enlarged wax or papier maché models of museums, is obvious to all those familiar with museums, for they are aware of the constant inaccuracies of those models usually made by uneducated workmen.

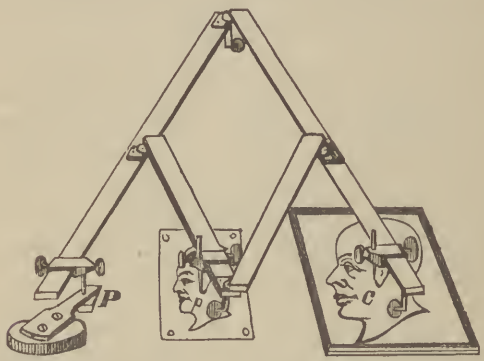
History.—My enthusiasm for drawings does not spring from a natural taste for them or from the natural gift of drawing, for I possess neither, and was driven by necessity to becoming a draughtsman. I can recollect now the time when I was no admirer of drawings, preferring the natural specimens, but that

was when I was quite young and yet a student, making my debut as a private teacher. I was then lecturing to a small number of students who could easily gather around me and have a full view of the dissections. But later, when through the kindness of Prof. Richardson, I was granted the favor to lecture in his stead on the Anatomy of the Eye and Ear, I soon found out that to talk to students about those organs without large drawings to assist them in forming a correct idea of what I was saying was somewhat like talking to them about the man in the moon. I had taken great pains to make a number of fine sections and dissections of the eye, and I circulated them among the students on the benches, floating in water or nicely and laboriously put up in little glass jars. They afterwards told me that those preparations though fine could not be understood by them; that they did not know which was which, for want of some means of forming a fundamental idea of the parts, as it were, on which to build. I took the hint, and understood at once that large drawings for class demonstrations were indispensable to an accomplished and conscientious teacher. During the following summer I tried my hand at it, making sections of the eye, which were easily made with the aid of compasses; then I tried a little shading and coloring with pastels. I would not go to a drawing master, for I knew he would set me to drawing sketches, noses, ear, etc., and I had neither the time nor the disposition to go through such an ordeal and apprenticeship. At first my drawings were very uncouth, and both too small and too fine, but I gradually found out how they should be done and contrived to make a small number that summer. They were of such great use to me in lecturing the next year as to encourage me, and I improved my stock both in quantity and quality. I next made a series of drawings of the female organs of generation, ovology and accouchements, on which I had to quiz and lecture to my private students at the college.

Discovery of the Pantograph.—However, I felt that I was progressing too slowly, and I began to search in my mind for some means that would be quicker than the ordinary process. I first thought of photography, but that was too expensive. Next I tried to get up a kind of dark chamber, and to so arrange it as to be able to enlarge the drawings of books so as to suit my purpose. This was not very feasible. It was whilst talking over the matter with Mr. Duhamel, the Optician on Canal street, that, having told him my

object, he showed me an instrument called the *pantograph*, which he said he thought might do, as it was used by architects and engineers to either enlarge or diminish drawings and plans. The moment I saw the instrument I knew that it was what I wanted. The pantograph is built upon the geometrical principle that in similar triangles the bases bear the same relation to each other as the sides of these triangles.

The French Pantograph (fig. 1). It is composed of two long slips



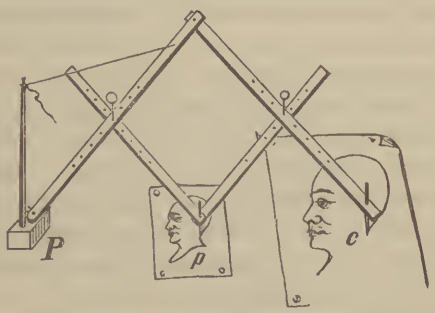
of wood, articulated at their upper ends, which lie on the outside, and of two smaller pieces one-half the length laying inside of the others and articulated together at their lower ends; their other ends articulate each with the middle of the long slips. The lower end of the left long slip articulates with a piece of lead, so as to form the pivot, P; at the lower end of the right long slip there is a piece of lead pencil or crayon, C On the left short slip there is a pointer, p. The pivot, and crayon or pointer, are adapted to brass slides. which can slide up and down the slips of wood so as to enable the operator to modify or adjust the instrument, according to the size of the drawing he is about to reproduce. The apparatus is supported by, or works on, small brass rollers which turn easily and rapidly, and which are placed under the articulation of the two long slips together, under the pointer and under the crayon. It is spread on a suitable smooth table; the drawing which is to be enlarged is placed underneath the pointer, and seizing hold of the pointer, the hand makes it follow all the lines or outlines of the drawing which is reproduced at the other end of the instrument by the crayon, or on a sheet of white paper; it is found to have been enlarged once, twice or three times, etc., according to the size of the instrument and of the drawing which is

being copied, and also according as the instrument is set for once, twice or three times. There are marks on the slips of wood which indicate the points to which the brass slides should be placed, according to the size you wish to obtain. An important rule to be observed is, before beginning, to make sure that the three points represented by the pivot, the pointer and the crayon are all exactly on the same line, which is easily done by stretching a piece of string, or a ruler across from the pivot to the crayon and seeing that the pointer is also on that line. This is indispensable in order to obtain similar images; if those three points were not on the same line, at the start, the image would be reproduced most disfigured; for instance, if you copied a circle, an oval will be produced; if it is a human face, it will be ridiculously elongated. I had a great deal of trouble with this part of the procedure, until I learned better.

When it is wished to have a dimished reproduction of a drawing, it is placed underneath the crayon, which is then replaced by the pointer and itself put in place of the pointer; the pointer is then taken hold of and is worked as said above.

Upon experimenting with this French pantograph, I found that it was very uncouth and very heavy; that it required a remarkably smooth table to allow the roller to work quickly; that when enlarging a drawing to some extent, the least little deviation of the pointer made a marked difference at the other end; and, finally, that for my purpose, which required very large drawings, the instrument should be made of such size as to increase those difficulties very much.

The American Pantograph or Crayograph (fig. 2). I was very much



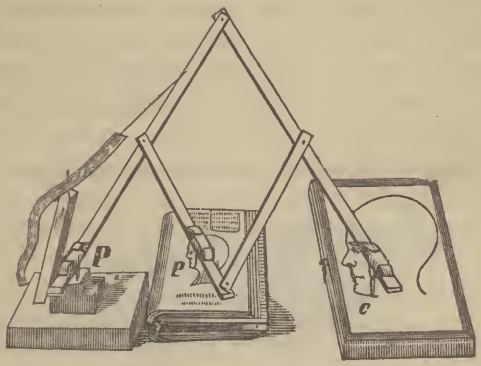
annoyed and rather despairing of succeeding, when I heard that there was passing through New Orleans and stopping at the St.

Charles Hotel a party by the name of Reynolds, of Fall River, Mass., who taught the art of drawing by a mechanical process, without having ever taken drawing lessons, and enabling any one, after some little practice, to reproduce and enlarge photographs, drawings, etc. I had some shrewd idea that the so-called new invention was but an old thing revived, and could not be anything else but the old French pantograph modified and improved. Upon entering the room and glancing at the instrument, I saw that my surmise was correct, but the modifications were such that it rendered it a far superior instrument. It is baptized the *crayograph*. It is built upon the same principles as the pantograph, but is made of very narrow and thin slips of wood, making it excessively light; besides, it did not work on rollers, but was suspended by means of a string starting from the head of the instrument and fastened to a kind of little mast driven into the little block of wood which represented the pivot, which block was provided on its under surface with two sharp steel points, which were driven with full force into the drawing-board and thus fixed the instrument. The slips of wood presented all along almost a series of numbered holes, two of which were traversed by a peg in each slip of each side in such a manner as to articulate them. These holes are to adjust the instrument to the drawing which is to be copied. I remarked also, that in copying a drawing the operator, instead of taking hold of the pivot and following with it all the lines of the picture, took hold of the crayon, and through it caused the pointer to follow the lines of the drawing. Now this requires some practice to do it well, but gives to the reproduced picture a clear and firm outline, which it lacks by the other process.

The Author's Pantograph (fig. 3). Though delighted at the discovery of such improvements on the old instrument, yet I could see that this new one, as it was, could not very well answer my purpose: it was too small and too frail to make such large drawings as I needed, and the adjusting process was rather complicated, I thought, and could be simplified. I therefore declined to buy the instrument, and determined to manufacture for myself one of my own conception, which would combine the advantages of both. Accordingly I bought some laths, a plane and a saw, and went to work during my spare hours.

My pantograph is made of lighter material than the old one, is narrower and thinner also, but is thicker and somewhat wider

than the crayograph. I thought that it should be so, because I found out that too light an instrument, although larger than the one I saw, trembled too much, particularly when working quick.



I retained the same mode of suspending the instrument, but I discarded the holes and the peg, and adopted the brass slides of the French, but modified them in such a way as to make them very much smaller and lighter, and got appended to the extremities of each a brass spring, which presses down on the wood in such a manner as to make them stay firmly just at the point to which they have been glided, thus dispensing with the screw which is attached to the slides of the French instrument. I also adapted a piece of string to the pivot so as to dispense with the ruler, which is recommended to be used when the instrument is being adjusted. With such an instrument, manufactured by myself, I made during the following summer upwards of two hundred drawings for demonstrating obstetrics, and about half that number of anatomical drawings.

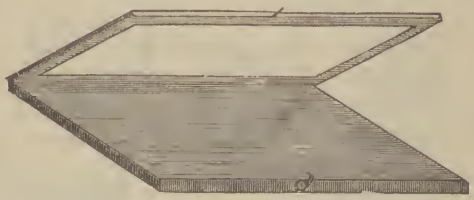
Modus Operandi—the Sketch or Outline.—Small drawings of 2x2 inches, for instance, cannot be enlarged to 30x20 inches at one stroke; it has to be done in two steps.

First the drawing from the atlas or book is enlarged to about 8x6, and then, with a larger instrument, this is enlarged to 30x20, more or less, as desired.

The long sticks of the small instruments are two feet long, not quite two-thirds of an inch wide and one-fourth of an inch thick; the smaller sticks are thirteen inches long, not quite one-half of an inch wide, and one-fourth of an inch thick.

The large pantograph is double the length, but of the same breadth and thickness as the small one.

To enlarge a drawing from an atlas or a book, it is placed under the pointer; the string is then stretched across and the three points (pivot, pointer and crayon) are brought on that same line. Under the crayon is placed a sheet of white paper. In order to keep the paper from moving when the crayon works upon it, I have contrived a little smooth board (fig. 4), 18x13,



with a movable frame along its edges, working on hinges along one border in such a manner as to be readily raised to allow one to place the paper underneath and then come down and be fastened with a hook and hold the paper tightly. Then, taking hold of the crayon, the pointer is made to follow rapidly the outline of the drawing, merely to ascertain if the paper on which the picture will be reproduced is large enough to receive it. If the paper should be too small or too large, the instrument should be adjusted to it by gliding the slides upward to diminish the drawing or downwards to enlarge it. It should be well borne in mind that whenever the slides are moved the line attached to the pivot should always be stretched across, and the pivot, the pointer and the crayon should be exactly on the same line, otherwise dissimilar pictures would be reproduced.

When all these precautions have been secured the right hand seizes the crayon, which is gently pressed down on the paper, and the pointer is made to follow all the lines of the drawing; first the outlines or contours, and then the inner lines: thus we have an enlarged sketch of the drawing. This first sketch is enlarged by the large instrument, which is spread on a large table near by, with also a sheet of large paper under the crayon and on such a board as when using the small instrument, only it is three and a half feet by twenty-seven inches. The paper which I found the best to receive the large sketch, which is to be

shaded, and colored and varnished, is what is known in stationery stores as "*bouquet paper.*" The paper should be, and this bouquet paper is, white, thin, smooth, for important reasons which will be explained further on.

The large sketch is then removed to a very smooth drawing-board suspended in a good light against a wall, or on an easel. This board or easel is also provided with a frame, each border of which can be raised and put down separately, and designed to hold the paper down firmly while it is being worked upon. Then, with a piece of drawing charcoal, (which the French call "*fusain,*") the lines are passed over. This is in order that the next crayon used should "bite," which it will not do over the the lead pencil marks of the instrument's crayon. The crayon which is passed over the charcoal lines is what is known in French as the "*crayons Conté.*" The No. 2 is the one to use for this.

This step of the operation takes about twenty minutes for a moderately complicated drawing.

The Laying of the Colors.—In the beginning I used to shade the drawing and then lay the colors, but I found out that it was best to lay the colors down and then shade, thus saving about ten or fifteen minutes. The coloring materials I use are pastels, of the size of a lead pencil and about two inches long. I had twelve hundred imported from Paris, as they were too expensive here. The most useful colors are flesh color for the skin, and for bones light coffee and milk color, crimson for muscles, vermillion red for the arteries, light blue for fasciæ and tendons, Prussian blue for the veins, light green for the nerves (the traditional white not showing well after the drawings have gone through the process of sizing and varnishing), light yellow for excretory ducts; violet, rose pale, brown, for sundry purposes. The pastels should be what is called half hard; for when too soft they crumble down upon being touched, and when too hard they will not mark easily. The colors being laid down are spread by using the pulp of one or two fingers; this is not easily and quickly done on rough paper, hence the necessity of using a very smooth paper, such as the bouquet paper. I will here remark that as these drawings are to be seen from a distance, and are not to be valued from an artistic but a scientific and practical point of view, it is not necessary at all to attempt to make them fine and perfect. Fine drawings do not show to advantage from

a distance, whereas comparatively coarse ones make a fine effect, as is the case with theatrical decorations.

The shading is done with the crayon Conté No. 3; it is of a beautiful velvet black; it is spread also by using the fingers. Should it be desired to make a black drawing without colors at all, it would be necessary to make first a light shading with the drawing charcoal, for the crayon Conté does not not spread well on the white paper itself. It often happens that the laying of the colors and the shading takes off some of the clearness of the primitive lines of the drawing, and it becomes necessary to run over them again with the Conté crayon No. 2.

The Fixation of the Pastels.—Pastel colors rub off most easily, as is well known, and it becomes necessary to fix the colors in such a manner as to render them able to stand the pressure of the brush when the sizing process comes on. That is effected by means of about one ounce of pure Damar varnish to about one pint of turpentine; the more varnish we put in, the firmer will the pastels be fixed, but the more yellow the paper is liable to become. This yellow coloration of the paper by the varnish is usually very slightly marked by using the pure Damar varnish, but this coloration is horrible when any other varnish, such as copal and Canada balsam, is used. I have spoiled many a fine drawing by using those materials, before I found out this fact. The diluted Damar varnish is laid with one of those flat, broad camel-hair brushes which are used in counting rooms to moisten the paper of the letter-copying book. The drawing should be tacked against the wall with its *back turned towards you.*

As I was making a large number of drawings, I have gotten up a contrivance consisting of a number of laths sliding horizontally through two strong wires fixed upon a frame. Each lath supported a drawing, which was held in place by pressing its upper border down under two wooden screws arranged for the purpose. It is *on the back of the drawing* that the varnish should be laid; if it was placed on the face the pressure of the brush would blot it all. Great care should be taken that the brush should only be dampened as it were by the varnish; otherwise varnish would run down in streaks on the face of the drawing and spoil it. After a few hours the temperature evaporates the moisture, and the varnish remains and fixes the pastels.

Another means of fixing the pastels, consists in throwing over the face of the drawing, with an ether spray apparatus, a spray

of this same diluted varnish; or of a warm solution of most clear and transparent gelatin. This latter has the advantage of never imparting any yellow coloration to the paper. When this process with the spray is used, care should be taken not to throw any more than a light dew over it, lest it will form drops, which will make so many blots.

The Pasting of the Drawings on Bristol Boards.—Previous to pasting the drawing on Bristol boards, the useless white paper around it should be cut out or pinked, by following the contour of the drawing closely either with scissors or the point of a knife. The back of the drawing is then smeared with cooked flour paste. The drawing is then carefully laid on the Bristol board and covered over with a large sheet of paper, on which we should rub in all directions with both hands, so as to spread the drawing well, make it adhere firmly and prevent folds. The laying of the drawing on the board is sometimes a delicate operation when there are many ends and projections shooting off from it, in consequence of the pinking. The boards on which I paste the drawings are known under the name of Bristol boards; they measure 32x28, are not quite one-eighth of an inch thick, are firm, not easily torn, and of a beautiful white. I could find no such large boards in this city, and had to order them from Paris, at the cost of thirty cents a piece. They are provided with two eyelets along one of their borders, so that they can be easily hung.

The Sizing. The next step consists in sizing the drawing and board—that is to cover them over with a substance that will prevent the varnish which is to be laid on from going through the paper and board, without forming any gloss at all and greasing the paper, as it were. In my first attempts I used a slight solution of gum arabic, which I laid over the drawing with a soft, flat camel-hair brush; but in course of time it imparted to the paper such a yellow tint that I had to give it up. I then resorted to gelatin or isinglass, which is capital. It should be used warm though, for it sets when cold; it should be laid on twice, for when only laid on once, small bubbles will form in spite of the greatest gentleness and care in working the brush and in drying, these bubbles burst and leave many spots where the varnish gets into the paper and gives it a grease-spotted appearance. The second coat of isinglass covers these spots and prevents this occurrence. After the isinglass is dry, it

leaves the paper so white and pure that it is difficult to say sometimes whether or not it has been sized.

The varnishing is done with the Damar varnish, to which is added about one-fourth part of turpentine. Thus diluted, this varnish does not impart a marked yellow tinge to the paper, but still in course of time it will show itself to a slight degree. The varnishing brings out the coloring and shading beautifully; besides, it protects the drawing from dust, water, and the friction of one upon another. I have been told that collodion makes a splendid size, but it would be too expensive for my purpose, as I was making a great number of drawings. I built a contrivance by which I could hang some fifty or sixty at one time, to enable them to dry quick. When I lecture I use a frame, made for the purpose, on which I lay the drawings; it will hold about five or six at one time; the following drawings are placed over these as they are needed.

Proceeding as I have described above, I have gotten up for myself upwards of eight hundred drawings on anatomy, microscopy, obstetrics and surgery. It took me four years to make them, only working, though, in the intervals of our college sessions and during the spare moments of my office hours at the college. Without the pantograph I am sure it would have taken me two years more to get up such a number; but with the use of that instrument, and now that I have worked out all the best and quickest rules of this kind of work, it never takes me more than an hour and a quarter, or a half, to go through even a somewhat complicated drawing.

ARTICLE III. *Yellow Fever in Marshall, Texas.* By Jno. H. Pope, M.D.

Marshall is in lat. 32° 29'. Its altitude is 247 ft. above Shreveport, La., and 451 ft. above the level of the sea. The city is built on the dividing ridge between the waters of Red and Sabine rivers. The soil is sandy, with clay foundation. Iron pyrites and conglomerate iron in abundance can be seen on the hill sides and washed places. The timber is pine on west and south, and oak, black jack and hickory, on east and north sides. The water is generally from wells of thirty-five feet average depth, and nearly always contains iron in some combination. The *public*

square (taken as one of the landmarks in this description) is the centre of the town, and the highest point with which we have to do. From here the surface is level toward the northwest, but ravines radiate toward the west, south and east, and broaden and deepen until they merge into a valley some two hundred yards wide, between a quarter and a half mile distant. The stream in this valley overflows with every hard rain, and the marshes hold water until our summer drouths. The natural drainage of the city is good, but in some places has been interfered with by throwing brush into the ditches to prevent washes. We have no board of health nor city physician.

Marshall is not compactly built, and has been described as "appearing rather like a collection of country residences than a city." In 1872 the population was 4000, about half being negroes. It is reputed to be one of the healthiest localities in eastern Texas. Intermittent and remittent fevers have prevailed during the malarial seasons; and they were almost confined to the south and east portions of the town. Up to 1873, no epidemic of any kind had occurred—no extra precautions had ever been taken to prevent it. Shreveport more than once had yellow fever in her midst, and communication open with this place. In 1857 (?) more than one case of the fever was brought here, were sick for some time, and died, the disease never spread among our citizens. Now let us see what were some of the differences between our city during 1873 and before that time, and, at the same time, between the *infected* and other portions. There were about 2000 people added to the population during the twelve months ending September, 1873. A large majority of these came from a northern climate. There was also added a transient population of several hundred, chiefly railroad laborers. Nearly all of these immigrants took up their abode in the same part of the city, to wit, between the public square and railroad depot (which is a half mile to the northeast), and around the latter locality. This created a demand for numerous boarding houses, restaurants and dormitories. Many of these houses were of the lowest class—cleanliness secondary to even the proprietors' godliness.

During the autumn and winter of 1872, about 60,000 cubic yards of dirt were removed near the depot. There were several freezes after this was finished. On the first block northeast of the square was a repository for beef hides, the offensiveness of which was, at times, almost unbearable. This nuisance was

remedied a short time before the advent of the fever. Near this spot was also the city market, which, to all appearances, was neatly kept.

At the outbreak of the fever in Shreveport, hundreds of the citizens came pouring from that city into Marshall, frequently bringing large quantities of baggage, and a greater number of them found accommodations in that part of the city north and east of the public square. No quarantine was established against Shreveport until September 28th, and then it was impossible to make it effective. Many of the Shreveport refugees were sick soon after their arrival, nearly all the cases presenting more or less the symptoms of yellow fever. I select the following as more plainly marked than the average.

Miss G. came from Shreveport on September 2d, taken sick on 3d. Violent pain in head (frontal region), back and limbs; great nervous excitement and anxiety; face flushed; eyes injected; tongue and fauces very red. Urine amber colored and sufficient in quantity—no deposits. Bowels constipated. Gave mercurial purge, followed with large doses of quinine combined with a small quantity of morphine. Gradual subsidence of the fever; discharged convalescent on fourth day. At this time she was at a house two hundred and fifty yards northwest of the square. She was soon removed to one about the same distance due north of the square. We saw nothing more of her, but Dr. Marks tells me that about two or three weeks after the above date he was called to see her for an obstinate bleeding of the gums. He is certain she was laboring under effects of yellow fever.

On the 7th September, Mr. J. D came into Marshall from six miles in the country, where he had been staying since his flight from Shreveport about the last day of August. He had been "feeling badly for a day or two," rode to town on horseback, and took quarters two hundred yards south of the square. Dr. Eads saw him the night of the 7th. He had a wild staring look; face congested; eyes suffused; said he had had a chill; skin very hot and dry; pulse full, strong, and 140 to the minute; pain in head, and in lumbar and epigastric regions; slight nausea; tongue had white centre, and red tips and edges; gums and fauces very red; bowels constipated; kidneys acting well; urine saffron colored. Ordered six ounces cit. magnesia sol., and if don't act repeat in two hours. Follow up with five grain doses of quinine every two hours to cinchonism. Ice freely used. *Second Day.*—Bowels

acted after the second dose of magnesia. Required two scruples of quinine to effect cinchonism. Skin very hot but moist. Pulse 128. Other symptoms much the same as described by Dr. E. on yesterday; eyes have a yellow tinge. Continue ice; apply mustard to relieve pain. *Third Day.*—Skin becoming distinctly yellow. Pulse 110. Patient inclines to sleep, is easily roused, and answers rationally. Kidneys acting well as regards quantity, but no analysis made of urine. Increase of nausea; stomach emits a glairy mucus; mustard relieved pain temporarily. Gave flaxseed infusion and sweet spirits nitre as diuretic, and chicken soup as nourishment. *Fourth Day.*—Skin of deeper yellow; more tendency to sleep; is weaker; nausea increased. Continue treatment of yesterday. *Fifth Day.*—Gradual increase in gravity of symptoms; sleeps a great deal; some delirium; wants to get out of bed; talks irrationally; urine in less quantity; less nausea. Pulse weaker, and 140 to the minute. Died at end of 5th day. No urine passed for eight hours. The disease did not spread from this case. He brought no extra clothing to this house.

(From Dr. Marks' Practice.) C. W. W., Express Messenger between Shreveport and Dallas, made several trips after yellow fever was declared epidemic in former place. No knowledge of exposure by contact to any particular case, or on any particular date. On September 8th, near Dallas, he had a chill without premonitory symptoms. Dr. M. saw him next day. Skin hot and dry; tongue clean and moist, with red tip; nausea and vomiting; expression anxious, eyes suffused, yellow and injected; urine high colored, normal in quantity; bowels constipated; pain in head and back, and thinks he complained of a tired feeling in his limbs. On 6th or 7th day vomited small quantity of black flaky substance in a brown-colored fluid. Convalescent on 8th or 9th day. Pulse became quite weak, skin cool and moist. Gave stimulants and nutritious diet. On —— day had rise of fever, which ran a course simulating typhoid fever. Delirium came on, and was of that peculiar character seen in delirium tremens. Pain in umbilical region; no tympanitis. Convalescence from this fever began on 17th day, and was more rapid than is usual after typhoid fever. There was no spread of the disease from this case that can be traced. The locality was about two hundred yards east of public square.

The following was the first case among the citizens of this place:

I. Wm. Searcy, aged 28, native of Arkansas or Missouri; has lived in Marshall about one year. Tall, muscular, and of bilious temperament. Always enjoyed good health. Had charge of a large billiard hall, in upper story of a brick building on the eastern border of the public square.

This and the case following being the first of our citizens attacked, a committee of Drs. E. P. M. Johnson, Eads, and Marks, were appointed by our medical society to investigate into their origin. They found no evidence to show that Searcy was ever exposed to any other case of yellow fever, or to any infection outside of this city. The evidence bearing on the other case will be given below. Mr. S. lived night and day in the billiard hall and adjoining room. On 19th September he had a severe chill, and a few hours thereafter Dr. Eads saw him with high fever, intense pain in head, back and limbs, and in epigastrium on pressure; expression anxious; eyes red and suffused; skin dry and hot. Pulse about 140; temperature not taken for want of thermometer. Obstinate constipation; only small quantity of urine; no tests applied until 3d day. Good mercurial cathartic, to be followed by quinine.

Second Day.—No abatement of any of the symptoms, pain as intense; constipation as obstinate; skin slightly moist; face has dusky hue; eyes suffused and dark yellow; tongue heavily coated with brownish fur; gums red and swollen. Gave sol. magnes. citr. f℥xij to move bowels. Ice freely given, and face and head bathed in cold water. Five grain doses sulph. quinia every two hours.

Third Day.—Bowels moved by enema; very small quantity of urine of dark amber color, and highly albuminous. Pulse getting weaker and little less frequent. Takes very little nourishment, on account of nausea and vomiting, which get worse each day.

Fourth Day.—Symptoms all aggravated; entire suppression of urine, and black vomit set in, with active delirium.

Fifth Day.—Furious delirium; black vomit continues at intervals. When delirium is wildest, three strong men can not hold him in bed. He died, and when being shrouded, his friends noticed several dark blue ecchymosed patches on neck and shoulders.

II. Henry Rosenbaum, German, 25 or 30 years old, has lived here several years; is very dissipated; keeps a grocery store and bar-room below Searcy's billiard hall (in same building). There is abundant evidence that he was trying to get a conveyance to smuggle lemons, etc., from Shreveport to this place while the epidemic was raging at the former place. Dr. E. P. M. Johnson tells me he has evidence, entirely satisfactory to him, that Rosenbaum did visit Shreveport, and brought from there a lewd woman whom he visited several times after arriving in Marshall. There is no evidence as to what amount of clothing was brought, nor who else visited her. The dates of Rosenbaum's alleged exposure were divers times from four to ten days prior to his attack, which was on 23d September. He had a chill, with fever that lasted forty-eight hours, when it left him with cool, moist skin, clear mind (to all appearances), pulse 70. He expressed himself as feeling well, and did, contrary to cautions of his physician, get out of bed to wait on customers. By advice of "a friend," early on 3d day he ate heartily of Limburger cheese. Soon after he had black vomit. When seen late on previous day, his kidneys seemed to be secreting well. Death at end of 3d day. Thus began the epidemic. I will not attempt to detail each case as I have the above, but will try to trace the advance and recession of the epidemic, by dividing the time into periods of 7 days each. I will give the number of cases first reported during each period, the death rate, and so on; will then give the symptoms we most relied on for diagnosis of doubtful cases; and finally, more particularly notice cases of special interest.

We will say the starting point is Searcy's case, September 19th. From 19th to 26th September is 1*st stadium.* The number of refugees from Shreveport would about balance the number of our citizens that had left. Hence we will put down the population at 6000, one-half being blacks. There were four cases all white, three male; two deaths, both black vomit, and in one it was associated with suppression of urine; two acclimated. All inhabited what will be hereafter known as the "infected district."

Second Stadium.—Four cases, all whites; two females; three acclimated; two deaths. Three of the cases, for several days, wanted the characteristic symptoms of yellow fever, but were afterwards unmistakeable. One case was one block further north, and two cases two blocks northeast, of the billiard hall

We were having occasional showers, with oppressive heat, and cool changes after each.

Third Stadium.—Population of infected district about 1000. Thirteen cases, all whites; two females; nine acclimated; four deaths, two with black vomit, one with suppression, and one unknown. Two of the cases three blocks northeast of the billiard hall, and next door to two of the cases mentioned in second stadium; one case on same block as billiard hall but one hundred yards further north. Two of the cases were near the depot: these had communicated freely with the other cases near the square. Two cases a quarter of a mile north of square: they had been boarding in or near the Bethea House, one block northeast of billiard hall; both were sick at same house in the new locality. The disease did not spread in the latter place, have no positive evidence that they carried their clothing with them; the presumption is they did. During this week the inhabitants of the infected district scattered to various portions of the town and the country, leaving, at end of this stadium, not more than five or six hundred people in that portion of the city.

Fourth Stadium. Thirteen cases, all whites; one female; four acclimated; six deaths—three with black vomit, in one of which there was suppression of urine; one with suppression without black vomit. All the cases but three were north, northeast, and east of the billiard hall. One died in the country, having moved out of infected district from house where case died. He did not take his clothing. No spread of the fever from this case. Two cases were one block west of square; both were daily exposed in infected district. There were a few cases during this stage that have not been reported, coming under head of quacks, whose opinions on medical subjects are worthless.

Fifth Stadium—October 17th to 24th. Population of infected district 250 or 300. Cold, continued rain nearly all of this week. Twenty-nine cases—twenty-six whites, three colored; fourteen acclimated; fourteen deaths—two with suppression, one with black vomit; four with black vomit and suppression combined. All but three of the cases occurred in infected district: of these three, one was half mile northwest, and one a block south of the square. Most of the time of the last one had been spent in the infected district: no spread of the disease where he was sick.

Sixth Stadium.—Population about the same. Seventeen cases,

all whites; two females; eleven not acclimated; four deaths—one with suppression and black vomit combined; two with suppression alone. One of the cases said he had had the fever in New Orleans in 1853. All the cases were exposed, more or less, in the infected district up to day of attack. All but one were sick north and east of public square. There was a white frost and cold rain during this week.

Seventh Stadium—October 31st to November 7th. Some few of the citizens returning to the infected district. Five cases—four whites, male; one black, female; two acclimated; three recoveries, one of which had had black vomit; one death with black vomit. Three of the cases were in the infected district, where all five had been exposed. One case was northwest of square, and one was four miles south of town. There was a freeze on 6th October.

Eighth Stadium—November 7th to 14th. Population about the same. Three new cases, all male—two blacks; two acclimated; two deaths—one with suppression, the other unknown. One of cases (Mr. Fish) was exposed, eight or ten days before his attack, to the case of Bond in the south edge of town. He moved from here to town (not to infected portion), and died of yellow fever. No other case can be traced to Bond or to this man Fish, One case (Henry Faulkner, colored) was only exposed to the clothing of Alice Ross (colored), who had died in town of yellow fever, and her clothing carried to Henry's house.

Ninth Stadium—November 14th to 21st. Four cases, all whites; one female; all acclimated; one death with hemorrhage from mucous membranes. Two of the cases (recoveries) had remained nursing in the infected district during the entire epidemic; one of them had been frequently exposed to epidemics of the fever in Mobile. The female case was six months pregnant; she had a mild case, and did not miscarry. Two of the cases (F. E. Sloan and C. B. Clark) left the city before the fever was bad. Sloan once or twice came to town in daytime, and on the 27th of October he came in and remained until the 31st, sleeping in the infected district in a room rather open, but which had not been specially aired since the cold weather. Sloan was taken sick on the 14th November, and had well-marked case of yellow fever. Clark returned to Marshall soon after Sloan. The place of business of both was in same house, and Clark was frequently in Sloan's bed-room. He drank to excess several days before his

attack. He says he had the fever in Shreveport in 1867. As his present attack developed he had yellowish hue of skin, the tumefied gums that afterwards bled, the black vomit stool, urine almost a solid coagulum under heat and nitric acid, and finally suppression.

Tenth Stadium—November 21st to 23d, after which there was no new case. Three cases, all whites; two male; one not acclimated; all recovered. One of the cases (Dem. King) had been absent from the city until the frost. On his return he occupied a non-ventilated room in the infected district. Another case (E. Baker) had remained in town during the entire epidemic: nursed a case of fever during the 3d, and another during the sixth stadium, both in well-ventilated rooms. He also nursed Sloan seven days, ending November 21st, in a very uncomfortable room, the weather at times very cool, and no fire in the room. He remained with Sloan night and day, and was much fatigued. On 23d he was attacked by the fever, but had a mild case.

The total of reported and non-reported cases will reach at least one hundred and twenty-five. The mortality was heavy, being about thirty per cent. of the cases. The percentage of cases and deaths was greatest among the unacclimated. By unacclimated I mean those who have not lived in this climate two years. Every black vomit case had albuminous urine when that excretion was tested. There were seven cases of suppression without black vomit.

The signs and symptoms most relied on in doubtful cases were the red tumefied gums, that ulcerated and bled; the pulse falling from 120, or 140, in the first stage, to 50 or 60 during convalescence. Several times it went lower than this, and in one case it fell as low as 34 to the minute. This last patient, however, had a very weak constitution; never even recovered his usual health, and finally, five months after his fever, died with symptoms of softening of the brain. There were some cases in which the pulse, in the recumbent posture, never went higher than 80; the skin was cool, the patient felt well unless he stood or sat up, and if he attempted to walk would faint. These were dangerous cases, and required close attention. I know of one case that could not be prevailed on to have a nurse; insisted on sitting up in bed and reading newspapers; would promise his physician to keep quietly in bed, yet the moment he left the room, would sit up. He died with black vomit. *Per contra*, I know of another case,

that his physician considered critical, who got hold of a pistol, drove his nurse off, and went about doing as he pleased and eating what he pleased. He recovered. As for treatment, we tried everything from calomel, quinine and opium, up to a masterly inactivity. Nothing would cut short the disease, but much could be done to ward off its bad results. Opium, and especially morphine, had a bad effect. One physician tells me he had nothing but good effects from paregoric. We tried all the other anodynes; none of them became favorites. Counter irritation or ice would frequently relieve local suffering. Nausea and vomiting were best allayed by opening constipated bowels and giving ice to swallow. I have seen bicarb. sodæ also have good effect on this; and a reliable physician tells me of benefit from oxalat. cerii. In one of the cases of black vomit that recovered, sodæ bicarb. and ice, and in the other, these and afterward plumb. subacet., were given. These were frequently given in cases that did not recover. As diuretics, digitalis and nitrate of potash, and sweet spts. nitre and flaxseed tea, were often used; the last combination was the greatest favorite. There was no recovery from suppression of urine verified by the catheter.

So-called "relapses" were rather frequent, generally following imprudence. Some of them were intermittent or remittent fevers, some gastric fever from indigestion, and some slow form of fever resulting from the profound alterations in blood and important organs consequent upon the yellow fever. In no case did it partake of the character of another case of yellow fever.

There were only two post mortems during the epidemic.

I.—Mr. G., 45 years of age, large frame and muscular. His gray hair and whiskers would indicate he was 60. Taken sick in a house where the fever was very bad; said he was not sick much. He lay in bed with his clothes on nearly all the time; said nothing unless spoken to; answered rationally but in monosyllables. If talked to much, would turn away impatiently. Nothing peculiar about expression of face. Both pupils natural; no *arcus senilis*. Tongue had thick creamy coating, and red tip and edges; bowels constipated; no urine known to have been passed for twelve hours; skin cool, moist. Pulse 80. This was his condition on third day, when I saw him with Dr. F. H. Johnson. That night he was given mercurial cathartic, and further active treatment postponed on account of doubtful character of the case. Dr. J., the next morning, found him "much the same,"

on his first visit. But about eleven o'clock that morning he was summoned in haste, and found that the patient, in attempting to walk across the floor, had fallen, and was now moribund, comatose, cool, and almost pulseless. He gave draught of whiskey and quinine, but the patient died before 12 o'clock. Post mortem four hours afterward. Unfortunately we were very much hurried, as the family wished to bury him "right away." Rigor mortis considerable; no signs of external injury; color of skin natural; tissues cool; muscles red and healthy looking; stomach distended with gas, and a greenish colored liquid, supposed to be quinine and whiskey. The entire liver was decidedly yellow—like I have seen *portions* of this organ in subjects dead of chronic dysentery. Incision with scalpel showed it to be almost bloodless, but on squeezing, the cut surface would show large drops of blood widely separated. The spleen was, if anything, smaller than natural. We were surprised, when opening the abdominal cavity, to see a large dark clot of semi-coagulated blood, lying under the right lobe of the liver. We are sure this was not from any post mortem wound. It extended behind and above the right lobe, between the liver and the diaphragm. We carefully removed this muscle (after taking out the stomach), thus severing attachment by the ligaments between the organ and muscle. In attempting then to raise the liver so as to examine for the ruptured vessel, the portal vein severed connection just at its bifurcation to the two lobes. Certainly not enough force was used to sever them in a natural condition. There was no sign of any thinning of the walls of the vein, nor was there any atheromatous degeneration of any of the vessels as far as examined. The heart showed signs of fatty degeneration of its walls; the valves were normal. One of the kidneys looked pale and flabby. The bladder contained about two ounces of a turbid whitish urine; none had been passed before death. The brain could not be examined.

II.—J. L., died second day of his disease, with black vomit and suppression of urine. Post mortem three hours after death. Skin very yellow. Marked rigor mortis. Tissues very warm; muscles very red and healthy looking; liver and heart of same yellow color that was found in case I. Both kidneys looked pale and flabby; one of them had an ecchymotic spot in its pelvis. Bladder contained very small quantity of turbid urine. A large quantity of black vomit material was in the stomach. Blood

taken from large vessel near the heart was of a scarlet color; kept in a corked vial twelve hours in a cool place it looked slightly gelatinous, but was easily shaken into fluid state again. I put a few drops on filter paper, but could not detect any yellow stain on the border of the drop.

There were some cases presenting points of special interest.

I.—Mrs. Rogers lives a mile south of town. She visited Mrs. Hardin, who was very low with the fever in the infected district; remained not more than an hour; kissed her when leaving; returned home; was not again exposed to yellow fever, but eight days after this exposure, was taken sick and had a mild case of the disease. Other members of Mrs. R.'s family successively had mild fevers seeming more or less like yellow fever, but Dr. Marks, their physician, can not say that any but Mrs. R. had that disease.

II.—Miss M. was exposed in same way about same time to the same case, was taken with the fever about same time, and died of black vomit on 3d day. After her death the family cleansed the house and removed to the country. There was no spread of the fever.

III.—Mrs. P.; history of exposure same as above two, except that she lived in the edge of the infected district. Period of incubation about one week. She recovered from a prolonged attack.

The room in which these cases were exposed was extremely offensive—made so by the exhalations from the patient.

IV.—E. Baker nursed F. E. Sloan in the room in which it is believed Sloan caught the disease after the freeze of November 12th. He nursed S. seven days, and two days thereafter was himself taken with the fever.

I examined chemically between forty-five and fifty specimens of urine from as many different patients; albumen found in 37—generally the worst cases—and the proportion of albumen was in direct ratio to the gravity of the patient's symptoms. Fifty-five specimens of this excretion are noted down as having naked-eye characters of muddy cider when both are shaken up. When allowed to settle, and the sediment chemically and microscopically examined (which was done in twelve of the fifty-five cases), it proved to be almost exclusively urates of ammonia and soda. The phosphates were in *very small* proportion.

The Fords live two and a half miles west of Marshall, in the

pine hills. The locality to all appearances is very healthy. Mr. Ford kept a public stand for entertainment of beeve drovers with their cattle. The beeves were enclosed in pens, or (sometimes) in large pastures. His sons kept a stall in our city market. Their slaughter pen was some hundred and fifty yards east of the dwelling-house. The drainage of the premises is excellent, the drinking water very fine. The house has two front rooms, with a hall between, and a third room running back in an L, with a hall between it and the adjacent room. Plenty of windows for free ventilation. There were several beds in each room, to accommodate as many travelers as possible.

By invitation of Dr. F. H. Johnson (the attending physician), I visited this place several times during the epidemic. I never saw any indications of a want of cleanliness, except on one occasion, when there was a large pile of dirty bedding and clothing thrown up in one corner of the room. Dr. J. tells me that, in the beginning, the sanitary condition was not so good, but still it was no worse than could be seen at most country houses. He had them to take a general cleaning up, and used disinfectants freely.

Early in September Mr. Holloway, wife and son, and Mrs. Keeting and son, ran from the fever at Shreveport and went to Mr. Ford's. Mr. H.'s son was sick when they arrived, and on —— day died with black vomit. Mr. H. himself was next taken with the fever; and so far recovered as to get out of bed, but died, several weeks later, from effects of the disease. These refugees had carried with them a considerable quantity of baggage, which was opened when young Holloway died, and from this time they used their clothing without any caution. Neither of the Keetings was sick until nearly all the Fords had had the fever.

Ten or twelve days after arrival of these refugees the fever broke out among the members of the Ford family. Most of these had associated intimately with the sick Holloways, waiting on them while ill. But among the first to be attacked was an old white man called Buck, who had purposely been kept away from the sick (he being an imbecile and sometimes annoying). He slept in a cabin some fifty or a hundred yards west of the house, and only occasionally was allowed to enter the sick room to make fires and bring water. None of the family had any opportunity of contracting the disease except at this house. Miss

Lulu Ford remained with the family, nursing, several weeks. After the mother died, she went to stay with her brother at a healthy place, a mile and a half distant. She took no change of clothing with her, and had no communication with home. She had yellow fever soon after her removal. Her sister-in-law waited on her. Before this date (latter part of October) her sister-in-law had been exposed at the Bethea House, the centre of the infected district in this city. The date of exposure was October 5th. She was taken sick with the fever a week or ten days after Miss Lulu. It did not spread among the rest of the family, of which there were several members.

The fever at "the Fords" seemed more malignant and uncontrollable than in the worst infected portion of our city, the mortality being fifty per cent. of the cases. I think every member of the family, and three or four of those who went there to nurse, had the fever.

Young Conway and young Jones were refugees from Marshall to healthy places in the country; they took no baggage; both had the fever and died, but there was no spread of the disease in the respective families with whom they were staying.

I am under obligations to Drs. Johnson, Eads, Marks, Thomas and Brooks, for material aid in making up this report of the epidemic.

ARTICLE III. *Bradycrote and Antizymotic Treatment of Typhoid Fever.* By W. HUTSON FORD, M.D., Professor of Experimental Philosophy in Charity Hospital Medical College of New Orleans.

W. Jones, athletic build, aet. 22, sanguine temperament, weighing about 150 lbs, a carpenter, white, first seen at 6 p. m., July 27, 1874; a native of Illinois; had resided in this country some 10 months; never had chills or fever; states that he had typhoid fever in infancy, being so informed by his mother. For several months past he has been employed at his trade in this place (Canton, Mississippi), and has been habitually drinking the water from an old *cistern*, which had not been cleaned out for many years. At his boarding-house also, the water was very bad, owing to seepage with an old and foul cistern. His bowels have been deranged for a week, and he has had light fevers,

daily, however, becoming more severe, without previous chill, or at most only with slight sensations of depression, rawness, and some dryness of hands and feet, every afternoon.

When I saw him at 7 p. m., he presented the characteristic symptoms of typhoid fever so unmistakably, that in connection with the previous detail, I unhesitatingly pronounced his case to be one of that disease. There was very marked gurgling in the right illiac fossa, and some in the left, as well as retraction of the abdominal muscles, pain on pressure, and some tympanitis; considerable heat of the body; pulse 116, and dry skin; tongue red, and heavily coated with a whitish yellow fur, through which the papillæ shone along the edges of a scarlet hue. He was at once placed upon the following powders:

℞. Bismuth subnit., - - ozj,
Hyd. submur., - - - grs. xii,
Camphor, - - - - " x,
Sod. Hyposulphit., - - " cec,
Pulv. Arom., - - - " vj,
Tinct. veratrum (Howard's), - gtt 80.

℔ In ch. xx divide. S.—one every two hours p. r. n. The directions were, that he should have a powder thrown dry into the mouth, and taken down with a gulp of water, every two hours until he became nauseated, or vomited; in which case the powders were to be intermitted for three hours after the vomiting.

He was also to take, in pill form, three grains of quinine with each powder, the belly to be kept covered with a towel constantly dipped in iced water—as much broken ice and ice water as he wished to be allowed, quite regardless of nausea.

His bowels were to be systematically opened with castor oil every other day, which was done until the twenty-first day.

It was soon observed that although the pulse was continually lowered to the neighborhood of 64, there was an exacerbation of *heat* in the afternoon, with some restlessness and confusion of mind, these symptoms appearing at about 2 p. m. To command this exacerbation, three pills of quinine containing together nine grains, were daily given for four or five days, at 10·30 a. m., with the desired result.

The calomel was omitted from the powders on the fifth day, and quinine, in three grain doses, added to each of them. It was

found that after the first three days, eight powders per diem were quite adequate to restrain the pulse.

The diet throughout was wholly of beef-tea, and boiled milk diluted with one-fourth lime water. A teaspoonful of sp. aeth. comp. in peppermint water was given nightly.

The general history of the symptoms under this treatment was as follows:

The tongue was never dry, though red and furred during 21 days; it was always moist at the edges, although a little harsh at first in the centre. This condition partly abated under the influence of the calomel, reappeared again about the 6th day, lasted several days, again disappeared under the renewed use of calomel for two days and a half, and thenceforth did not recur. There was nothing like the dryness of the mouth, cracking of the tongue, or bleeding therefrom, usually seen in typhoid fever, nor was there at any time sordes about the teeth or nostrils.

There was constant thirst, and large quantities of water were taken in the form of ice, iced water, iced effervescing draughts of citrate and acetate of soda, the effervescence being allowed to subside before administration, for fear of filling the intestines with gaseous matter.

The bowels moved usually three or four times a day, of their own accord, and the fœtor of the stools subsided after a day or two almost completely. The oil admixture, as already stated, produced generally two large operations; the patient seemed always notably easier, after the action of the oil, for twenty-four hours.

The pulse was rigidly maintained at from 56 to 64. Only *twice* during his sickness did I find the pulse at 80, and this occurred on both occasions in the morning, owing to an intermission of the powders from 4·30 a. m., until 8·30, the usual time of my morning visit. I saw him every day three times, and frequently four times, once or twice visiting him at 12 or 1 o'clock at night.

In consequence of this steady and very careful maintenance of the pulse at the average standard of sixty, the temperature was always nearly normal. Indeed, his feet and hands in the morning were cooler than normal, and in the afternoon only, as warm as those of a healthy man. There was never any marked coolness about the trunk, however, though the temperature did not exceed 100° or 102° in those parts at any time.

There was no perspiration until the 16th day; afterwards every day a little, usually about 5 p. m., at other times the skin was dry.

There never was any delirium; one night only, about the end of the first week, there was a slight loss of intelligence, and a degree of confusion, which the patient complained of next morning. There was, from first to last, no headache whatever, nor any other pain, except in the epigastrium and in the ileo cœcal region.

There was epistaxis twice, of about 6 ounces, and again of 2 ounces, on the tenth and fifteenth days.

Vomiting occurred on the second, fourth and eighth days; on this last it was accompanied by marked veratrism. After this the quantity of veratrum was reduced to two drops every third hour, and p. r. n.

The time for giving every dose of medicine throughout the disease was specially indicated; such is my invariable practice in heavy cases treated with veratrum.

Under the systematic purgation, the puffiness of the abdomen disappeared about the fifth day, and there was no tympanitis afterwards.

On the 16th day amendment became manifest, and the veratrum was discontinued on the twenty-first.

No alcoholic stimulant *was used until the middle of the third week,* and then in carefully regulated doses of about half an ounce of whiskey every four to six hours, and more frequently after this. Altogether he took 3 bottles of whiskey. The whiskey and veratrum powders were given concurrently. Alcohol is not incompatible with veratrum, provided it be given in moderate doses. Obviously, when alcohol is indicated in *large quantities*, veratrum is, ipso facto, contra-indicated. Upon the discontinuance of the veratrum powders, an alkaline potion containing acetate of potassa, nitrate of potassa, sp. aeth. nitre, tinct. scillæ, was given every third hour. He sat up on the twenty-second day, and thenceforward improved rapidly, and was dismissed convalescent on the twenty-fifth.

In this case the veratrum was given for the purpose of controling the fever, and abating systemic action generally, and of course the febrile motion. Thus the general intensity of morbid nutrition was lowered, and also prevented from rising into a dangerous height. When the pulse becomes weak, the dose

of veratrum, or the frequency of the doses, must be diminished. The bismuth, in such heavy doses (on an average one hundred and fifty grains a day being taken), was designed to act locally upon the ulceration of Peyer's patches—that it did so I have every reason to believe.

The hyposulphite of soda in greater amounts, to about one hundred grains a day, was well borne, and was given for its antiseptic properties. As I believe that the pyrectic movement of typhoid fever is based altogether upon the absorption of lymph of high ferment-potency from the ulcerated patches, as well as upon the absorption of fetid and dangerously fermentative ichor forced out into the cavity of the intestines from the ulcerated surfaces, the hyposulphite was administered, and to a considerable extent the quinine also, with a view to counteract the septic power. It will be recollected that the Brothers Weber have shown that, while pus injected into the veins of animals causes a rapid elevation of temperature, if previously mingled with quinine this elevation does not occur. The hyposulphite and quinine were used consequently as *antizymotics*; the former wholly so; the latter at least partly so; for it cannot be deemed that quinine partly acts as a febrifuge by invigorating the nervous system, in virtue of properties quite independent of the pure anti-fermentative qualities demonstrated by Binz.

The purgation was maintained for the purpose of removing the fetid and dangerous contents of the bowels as thoroughly and as frequently as possible from the economy. It therefore takes rank as an antizymotic measure—indeed, as a neat, successful and important one.

It will be observed that not a particle of any opiate, or other narcotic or hypnotic, nor of oil of turpentine, was given in this case; nor have I ever given any of these remedies, except in a case of perforation of the ileum, during a period of sixteen years, in which I have treated typhoid fever substantially as above.

As regards my success in the treatment of typhoid fever, I must say, that during the above period I have lost but one case, viz., a girl 12 years of age, alluded to above, on the 18th day, or thereabout, of her disease, of bowel perforation. I have, during this period of time, treated perhaps thirty cases of the disease. Once, I think, I have seen the disease cut short on the tenth day.

I wish also to remark that I cannot regard this malady as in

any way allied to the exanthemata; viewing it as a febrile motion, of a sub-pyæmic type, often, when the fever is not restrained, passing into a pyæmic and even into a septicæmic type, as in the state called hyperpyrexia. I believe the febrile movement is solely due to absorption of matter of high ferment-potency produced in the intestinal ulceration which characterizes the malady, however this be itself induced primarily, and that the general grade of systemic action, viz., the grade of the fever itself, is sub-pyæmic, pyæmic, and septicæmic, in bad cases, simply because the fever-ferment alluded to, viz., the lymph derived through the lymphatic vessels from the ulcerated surfaces, and the ichor discharged therefrom into the intestinal cavity and thenceforth taken up by the lacteal vessels, is itself sub-pyogenous, pyogenous, or ulcerative; while at the same time it should be borne in mind, that a very pernicious inquination of the blood must be inaugurated by the absorption from the intestinal canal of fluids distinctly putrescent, if we may judge of this by the usual odor of the evacuations. Absorption of such matter would readily produce fever prone to pass into the septicæmic grade, by a process of putrid infection. Against all these zymogenous influences the treatment, in its antizymotic aspect, was essentially directed. Since treating this case, I have also had another, clearly similar to it, treated in the same way and with similar results, the patient convalescing on the 22d day, and in a week or two returning to his usual business without a single untoward sequela.

Article IV. *Tracheotomy in Croup.* By Dr. J. C. Faget.

"Citiùs et Tutiùs."

Tracheotomy is often called for in cases of urgency, and forced upon physicians even the least familiar with the knife, and oftentimes under the most unfavorable circumstances: at night, in the country, away from all help. Hence the necessity to modify the operation, and leave off the difficulties inseparable from the methods now in general use.

"I am not the only one urging the necessity of tracheotomy, and saying that it is the *duty of the physician* to practise it."—(Trousseau, Clin. Méd., p. 415.) And, if I am not mistaken, it is

Trousseau likewise who expressed the wish that tracheotomy should become as accessible to the physician as blood-letting.

But "*he had performed the operation over two hundred times*" (loc. cit., p. 414), and could well speak of it with easiness; whilst for the great majority of physicians, or even surgeons, we must grant that it is surrounded with difficulties—even in broad day—even by following the method of the illustrious professor of Paris, described in consideration that "physicians oftener than surgeons are called upon to practise it."—(Loc. cit., p. 415.

As to myself, as a physician, I have been compelled to operate eighteen times, several times at night, and I have gained some experience. Well, I now declare, that if at first I went through the hesitations and anxieties which are the habitual accompaniments of this operation as usually performed, I do to-day practise it with as much safety as facility. And I do not exaggerate in saying that I prefer to operate for tracheotomy than to practise blood-letting; because I feel that I cannot fail in practising tracheotomy, whilst no one can be sure that he will succeed in bleeding, even in broad day.

Some fifteen years ago, our venerable confrère, Dr. Lambert, having heard that I had succeeded in two cases of tracheotomy, sent for me, one night, to practise the operation on a child five or six years old, threatened with imminent asphyxia, owing to false membranes obstructing the larynx. Delay was impossible, and the operation was performed immediately, with the assistance of Drs. Blache and Thierry. The child, who was very strong, and in a state of great agitation from impending suffocation and most horrible fright, could not be held still; the light besides was very bad. Under such circumstances we were wrong not to use chloroform; but, perhaps, did we not dare to administer it.

I must confess that I undertook the operation with some emotion. arterial anomalies, the subclavian vein, the brachiocephalic trunk, rose before my mind. Thus, I dreaded to get too near the supra-sternal fossa, and on that account my incision through the skin proved rather high; and the trachea being opened, I found, not without some uneasiness, that the incision through the rings of the trachea was too narrow to admit the canula. Instead of cutting below I boldly cut above, and the cricoid cartilage was divided. A large double canula was then easily introduced, and breathing immediately restored.

The child who was thus operated upon is to-day a very hand some young lady, but her voice is somewhat altered.

Now, will laryngo-tracheotomy account for the alteration of the patient's voice in this case? This question is not devoid of interest. If we consult Boyer, whose "Traité des Maladies chirurgicales," although an old book, is still so useful to practitioners, we find that this illustrious professor preferred laryngo-tracheotomy to tracheotomy in cases of foreign bodies in the air passages, and that the operation was productive of no change in the voice.

Up to that time, however, Boyer, although a surgeon in chief of such a large field as the "Hôpital de la Charité" of Paris, had not yet had occasion to perform the operation. But at last, on the 25th of January, 1820, a child, 9 or 10 years old, was brought to him, who had a bean engaged in his air passages, and he describes the operation he practised as follows:

"I made an incision on the median line through the skin of the neck. Blood flowed abundantly. I tied one vein, then a second, third and fourth one, and I thrust a straight bistouri into the upper part of the trachea. Upon feeling with my finger I found that the incision was too small. In order to make it larger, I introduced a grooved canula into the trachea from below upwards, and used it as a guide for the bistouri. I then divided the upper rings of the trachea, the cricoid cartilage, and the membrane connecting this cartilage with the thyroid. The voice ceased entirely, and air rushed in and out through the incision, but the foreign body did not come out. I made no attempt to extract it. Three or four hours afterwards the child went to sleep, and snuff was introduced into his nose. He woke up, tossed about, coughed, and a white bean, one-third larger than those among which he had taken it, was found under the rag which was over the incision. The wound was dressed lightly, suppuration set in, and adhesive strips were applied. On the first days of February at last, cicatrization took place. *The voice was in no way changed.*"—(Boyer: Traité des Mal. Chirurgicales, p. 142, vol. vii., ed. of 1821.)

Whenever the mode of operating can be selected, the incision of the trachea must undoubtedly be preferred to that of the larynx, owing to the necessity of using a canula for some time in cases of croup. But generally, after five or six days, the canula can be removed definitively. In our case it was allowed to remain in

the trachea for fifteen days. I did not visit the patient except at long intervals, having turned her over to her family physician after the operation. Strong reasons, however, must have existed to justify the keeping of the canula in the trachea for so long, as Dr. Lambert, who took charge of the after-treatment, not only keeps step with science but has also a great deal of personal experience. Furthermore, we have examples of cases of croup followed by alterations of the voice, in which laryngo-tracheotomy had nothing to do, as it was not practised, and in which the damage to the voice was probably due to lesions in the larynx by ulcerations which afterwards cicatrized. One thing certain is that in this particular case there was no further "partial necrosis of the cartilages," as feared by Trousseau.

Here is besides what we can read at page 44 of the most complete and recent work on "Croup in its relations to Tracheotomy, by Dr. Cohen, of Philadelphia."—"Professor Heuter, of Greifswald, strenuously urges the division of the cricoid cartilage, from below upwards, as the safest operation in children. His arguments are that this locality is freer than any other from all anomalous anatomical complications, and that it permits the direct withdrawal of false membranes from the glottis. He has performed this operation many times, and has never encountered any untoward results referable to the division of this cartilage."

The necessity of putting tracheotomy within the reach of all, or rather of practising it quickly, has given rise to Chassaignac's method, about which Trousseau says—"I express myself most forcibly against the expeditious method recommended by Mr. Chassaignac, which consists in steadying the larynx with a tenaculum, in order to penetrate directly into the trachea by puncturing at once, through the skin and the intervening deep tissues."—(Clin. Med., p. 418.)

In my opinion this operation, performed with a plain tenaculum, not even grooved on its convexity, would present not only a difficulty in puncturing, but also the much greater danger of missing the trachea and opening the œsophagus, as it happened to Bérard (Trousseau: Clin. Med.. p. 420).

But, strange! what Trousseau blames the most in Chassaignac's method, is the steadying of the larynx. "There is danger," says he (loc. cit., p. 419), "in steadying the larynx; because, as Dr. Millard judiciously remarks in his excellent thesis, and as Dr.

Lenoir had also said in 1841 (Thèse sur la Bronchotomie), opposing the motions connected with the exercise of a function already so impaired, is running the risk of hastening asphyxia and death."—(Loc. cit., p. 419.) But these motions are controlled only for a few seconds, as it is about the time necessary to enter safely into the trachea when it is steady. Besides, the pressure does not bear upon the trachea, but upon the cricoid, which is more resisting. If, on the contrary, the knife has reached the trachea without its being steady, it may well happen that it be withdrawn before the incision is sufficient. Supposing, however, that the trachea should be sufficiently opened, "all is not done," as Trousseau himself says, p. 421, "and the remaining part, if not the most difficult, requires at least the greatest coolness and presence of mind. And really at that moment blood gushes into the lungs, and respiration becoming more difficult, venous hemorrhage, far from stopping, becomes more abundant. The dilator must at once be closed and introduced into the opening of the trachea. However easy this may seem, it requires some habit, especially if the trachea is not steady, and ascends and descends, and if the incision is small. The operator then, not finding the opening, becomes confused, and the assistants themselves grow anxious. Trousseau confesses that it "happened to himself to find out that the dilator was between the muscles when he thought it was in the opening of the trachea, or that only one branch of the dilator was in the trachea." And, to how many others the same thing must have happened! If, on the contrary, the trachea is steadied during the few seconds necessary to divide the trachea, and to take the dilator, the only thing remaining for the operator to do is to push the closed extremity of the dilator between the thumb and first finger of the left hand holding the upper end of the tracheal incision, and to introduce it easily and safely into the trachea. The handle of the dilator must then be raised, its branches going in, and afterwards opened.

A greater objection to Dr. Chassaignac's method is, that in some cases of diphtheria the swelling of the cervical cellular tissue over the larynx is so extensive that it is impossible to appreciate the relations of the parts beneath, even by the touch.

In such cases, then, a previous incision of the skin and thick cellular tissue is absolutely necessary. In order to prove this assertion, I beg leave to mention in a few words the facts which I published in the "Gazette des Hôpitaux," of October, 1866.

Two brothers, one ten, the other seven years old, were taken about the same time, and both most seriously affected, presenting the most evident signs of general poisoning by diphtheria.

The elder had an immense swelling of the cervical ganglia and cellular tissue, with paralysis of the soft palate, strabismus, albumen in the urine, etc. But respiration was at no time seriously impeded, although the diphtheritic secretions were quite abundant in the throat. Tracheotomy was, of course, at no time thought of in this case. Cauterization with nitrate of silver, insufflations with tannic acid, frequently cleaning the pharynx with a mop, now and then, dipped in dry tannin, constituted the local treatment, whilst the general treatment consisted mainly in the use of Peruvian bark, wine, brandy and broth. He recovered, but slowly, and suffered several months with paraplegia.

The younger was not so lucky; he had to be operated on. The signs of general poisoning were quite as bad with him as with his brother: there was albumen in his urine, paralysis of his soft palate, strabismus, etc. And besides, his cervical ganglia turned into large abscesses, *the cellular infiltration went so far as to produce swelling over the sternum*, and at last the difficulty in the passage of air through the larynx was such, that the trachea had to be opened in order to avoid death by asphyxia. Contrary to all hope, the child recovered, and is to-day a young man, twenty-one years of age, with no trace of this awful disease, except a small cicatrix on his neck. His voice is perfectly clear, and natural. As his brother, he was long convalescing, and suffered for several months with paraplegia.

Am I not permitted to say that both these cases, especially the one that was operated upon, were affected with the "diphthérie maligne" of Trousseau? It is certain that they both showed numerous and evident signs of general poisoning. Yet Trousseau says: "If the diphtheritic infection has deeply penetrated the system; if the skin, the nasal fossæ are affected with the special phlegmasia of diphtheria; if the frequency of pulse, delirium, and prostration indicate general poisoning; if, in a word, you have a case of malignant diphtheria in which the danger resides more in the general condition of the patient than in the local affection of the larynx or trachea, an operation must not be practiced, as it is invariably followed by death," p. 428.

No doubt with both these children the danger proceeded first and above all from their general condition, the poisoning; but at

last, with the one who was operated upon, the imminent and ultimate danger was due to the local affection of the larynx, the obstruction in the air passage. The child was asphyxiated, and was going to die for want of air. He was provided with air, and resisted poisoning until he recovered. "Je le pansay, Dieu le guarit." Never could this beautiful and deep thought of the father of French surgery be better applied than to this particular case.

In résumé, I may repeat here the propositions which I published in 1866, in the "Gazette des Hôpitaux, of Paris."

The main object of tracheotomy, in cases of croup, is evidently to furnish air in sufficient quantity to one who stands in need of it, and also to favor the exit of products, acting as foreign bodies, which encumber the trachea, and to which an easier outlet than an obstructed larynx must be given; then, to check in time the progress of asphyxia, to allow sufficient respiration, and thus afford to art and nature chances to fight anew against the disease; such is the part of tracheotomy. It is all, but it is assuredly a great deal.

These few words show the importance of not delaying, and the urgency to give a large inlet to air, and an easy outlet to the false membranes.

There are cases of croup in which asphyxia constitutes the whole danger; the disease appears as being local, the false membrane acting only as a foreign body. In such conditions the case has favorable chances, provided the operation be resorted to in due time. My first success in tracheotomy was in a case of this kind, which, besides, presented several anomalies.

The subject was a boy, 4 years old, V. C. The outset of the disease was quite violent, because, contrary to rule, it began in the larynx. As in *false croup*, the child was seized suddenly in the night with hoarse cough, wheezing respiration, etc. Nothing could be seen in the pharynx—no trace of false membrane; the cervical ganglia were free. Therefore, my first impression was that I had to do with *stridulous laryngitis*. But, in the morning, asphyxia became so threatening that an operation was seriously thought of. It was only then, for the first time, that small white spots could be perceived on the tonsils, spreading from below upwards, from the larynx to the pharynx. It was clearly a case of diphtheria which commenced in the larynx.

I had delayed long, and I scarcely at last had time to bring

together a few confréres, needed for the operation. About midday, however, Drs. Daret, d'Aquin, and Alain, answered my call. It was full time: the cyanosed face and extremities, cold sweats, weakness of the pulse—everything was threatening imminent death, when I succeeded in performing the operation, which, besides, was very simple and easy. Just as I made a large incision in the trachea, a false membrane escaped from it, and was thrown off at a distance. It was picked up, unfolded, and found to measure nearly one inch in length; but it was thin, and consequently newly formed. At the subsequent dressings, several small diphtheritic concretions were extracted, but each time thinner and thinner, until at last it became certain that no more was formed in the trachea. In the pharynx, on the contrary, an abundant diphtheritic secretion kept on for several days; no treatment was applied there. The cure was rapid and left no trace behind, except the small cicatrix on the neck, which is unavoidable.

But in other cases, as we have seen it, general poisoning is evident; the cervical ganglia are swollen, the urine contains albumen, there is paralysis of the soft palate, strabismus, and paralysis of the lower extremities, etc. In such cases, many practitioners—even Trousseau—are of opinion that no operation should be performed, and pronounce it useless. It is undoubtedly true that death, in such cases, most frequently occurs, even without there being serious uneasiness in breathing. At other times, however, difficulty of breathing is very great, and if it cannot be regarded as the principal cause of death, it largely contributes to it; so that, even in such cases, the operation is indicated, were it only to make the respiration easier, freer, and consequently to put the patient in better resisting condition.

We generally do not consider attentively enough how important it is that the air necessary for hæmatosis should not be diminished in quantity, especially in circumstances where we have already to contend against general poisoning, with great oppression of the radical forces. It is evidently now as urgent to provide the lungs with a sufficient amount of air as it is to introduce tonics and analeptics into the stomach. Indeed, after free and easy respiration has been restored, we yet meet with children that we fail to cure, and that because it is impossible to nourish them. They refuse all kinds of food, without speaking of those in whose cases all the liquids ingested

escape through the incision or the nasal fossæ. A child, upon whom I operated in 1865, was in a state of asphyxia when the operation was performed, it having been delayed in order to wait for daylight. Respiration was perfectly restored, and remained normal till death, but there was no power of nourishing the child; he died of inanition after one week of vain struggle. I have always regretted that I did not, in that case, resort, immediately after tracheotomy, to the stomach tube, to nourish the patient. The case had exhibited all the signs of general poisoning.

Mode of Operating.—Tracheotomy, like the operations for strangulated hernia, has been surrounded by anatomists with dangers more theoretical than real. For instance, have not the chances for hemorrhage been much exaggerated? What is certain is that, in the great majority of cases, the operation is completed without a single ligature being called for. By keeping exactly on the median line, and avoiding to carry the incision too low in the supra-sternal fossa, we are secure against all serious danger. Besides, the operator can always guard himself, against all accident resulting from arterial anomalies, by feeling his way while dissecting. As for venous hemorrhage, the best and quickest way to stop it is to hurry in making a large opening into the trachea. All this is now common experience.

Yet, serious difficulties may be met with in the operation, by those at least who are not surgeons: for instance, the difficulties resulting from the great mobility of the trachea over the vertebral column, and the want of any established point of reference for the operator. Those difficulties vanished, for me, from the day that I selected the anterior projecting part of the cricoid cartilage to serve as a basis, or a point of reference, and using this cartilage to apply and steady the tracheal tube against the vertebral column, in order to make the incision with safety, and introduce the dilating forceps with facility into the trachea.

This is now my method of operating: The head being thrown back over a hard and rounded pillow, placed below the neck so as to cause its anterior part to project forward, I make an incision through the skin, with a straight and quite sharp bistouri, from the larynx to just above the suprasternal fossa. The incision through the skin must start from above the cricoid, but not too high above it, lest the superior border of the plate of the canula should afterwards enter into the incision. Previous to

placing the child on the operating table, it may be well to mark the angle formed by the meeting of the anterior part of the neck and the chin, so as not to cut above that angle.

The subcutaneous areolar tissue being generally more or less pathologically thickened, it is often difficult to feel the cricoid cartilage; but when the incision has been carried through the infiltrated cellulo-adipose tissue, the finger introduced in the incision will certainly feel it.

The cricoid being felt distinctly, I dissect rapidly below it, keeping in the median line, the assistant sponging lightly and quickly after each cut, in order to see the way; if any artery should by chance appear, the eye and touch could make it known, and it would have to be tied.

The trachea being sufficiently denuded, I introduce the thumb and index finger of my left hand, from below upwards into the upper angle of the incision, and catch hold of the cricoid cartilage, which I apply and steady against the vertebral column. Then, and only then, I thrust the point of the bistouri into the trachea, between the thumb and index holding the cricoid, the back of the instrument sliding on the inferior margin of the cricoid cartilage. I then cut, without stopping, five or six rings of the trachea below. This being done, I take hold with my right hand of the dilating forceps, and having introduced it through the incision in the trachea, between my left index and thumb, *still holding the cricoid*, and the forceps being still closed, I raise the patient to a sitting posture, and now only I open the forceps and dilate with it the incision of the trachea, avoiding particularly not to dilate to excess.

All that can be done in less than no time, and with the greatest safety, because the trachea, which was largely opened, has always remained steadily unshaken between the two fingers of the left hand holding the cricoid.

I have sometimes introduced directly the canula, instead of the dilating forceps first; but as a rule it is best to introduce the forceps first, and keep it in place for awhile, because it dilates more, and affords a larger outlet for the exit of the contents of the trachea; and it is a wonder then to see how pieces of false membranes, clots of blood and mucus, are at once thrown off at a distance, and how respiration promptly becomes quiet and easy.

As may have been noticed, my incision of the skin is very large; this affords a great deal of facility for the operation, and afterwards a few *serre-fines*, if necessary, will easily and rapidly

remedy its slight inconveniences. Thirty-six or forty-eight hours after my operations for tracheotomy, there was only a rounded opening, enclosing the canula, and the serre-fines could be removed.

As I have said in beginning, it is so much the more important to make tracheotomy an operation within the reach of all, because it is often called for in cases of urgency, when it must be performed at once, without any possible delay. And it is evidently not in croup alone that it is often necessary to resort to it *immediately*, but in many other affections, as œdema of the glottis, other obstructions of the larynx, as cancers, polypi, etc., and especially where an extraneous foreign body has lodged in the trachea, as a bean, a kernel, etc.

Out of fifteen operations, of my first series, I had three recoveries; those three cases which proved successful had been operated on when in a desperate state, whilst several among the remaining twelve who died were, on the contrary, in conditions apparently much less unfavorable than those who recovered. What conclusion must we draw from these facts? That there is no case, as it were, in which the operation must be refused; because there is perhaps no affection in which the physician is so liable to be deceived in his prognosis as in croup.

Hence the occasion to repeat the old adage—*Melius anceps quam nullum.*

Last Facts.—The few cases of laryngeal diphtheria which came to my observation during the first weeks of November, 1874, confirmed the conclusion I had arrived at in 1866, that there is scarcely any case in which the operation must be refused, and the following still add to this conviction.

The *first* case happened in Dr. Wiendahl's practice. He had left his little patient tolerably well during the day, not thinking an operation necessary before the next day. At nine o'clock in the evening, however, we went to see the child together, and found him in a dying condition. Had we had the necessary instruments it would have been our duty to operate. But it was far, and the child expired fifteen minutes afterwards. There is probably no experienced physician who has not been surprised sometimes by such dreadful aggravation in diphtheria, so that a child, left comparatively well, would be in a desperate state a few hours afterwards.

The *second* case also of Dr. Wiendahl's was a little negro boy 3

years old, sick for a week, whose larynx was affected since three or four days. Dr. W. had seen the patient for the first time on the day of the consultation. This also was a dying case. I operated nevertheless, and death was quite threatening during the operation, which did not last two minutes. Perfect breathing returned, however, and on the third and even fourth day after the operation we had good hope. At one of my visits on the fourth day I found the child suffocating. I removed the inner canula, it contained nothing, and suffocation kept on. I then removed the outer canula, and a large piece of hard and dry mucus, a real foreign body, moving in the trachea, and too large to come through the canula, was immediately thrown off at a distance. Quiet and easy breathing returned again, and my hopes revived also. I allowed the child to remain without any canula the rest of the day, but thought it prudent to put it back for the night, as he was nursed by old women. The next (5th) day, it became evident that exudation had been deposited in the bronchi; there was broncho-pneumonia, and the child died in the night.

The *third* case, again in Dr. Wiendahl's practice, an Irish boy 5 years old, whose larynx was invaded only since the morning, was also moribund. Adynamia and poisoning were extreme, pulse disappearing upon pressure, and asphyxia commencing. Night was near, but delay was impossible. We operated in the dark, and not without some difficulty. The canula in place, respiration had to be assisted by intermitting pressure on the abdomen, and several times putting his head downward. At last respiration returned, and his heart beat with a little more strength. Stimulants were indicated, but it was impossible to get the boy to swallow a drop of liquid, as he would close his teeth as soon as a drink was put to his mouth, although he was conscious. He died three hours after the operation. Respiration and circulation were in full activity when Dr. Wiendahl left him one hour after the operation.

Fourth Case.—A few days afterwards, Dr. Touatre and I assisted Dr. Borde in an operation for tracheotomy performed on one of his patients, aged 5 years, apparently under the most favorable circumstances. This time it was not late; there were only threats of, but no asphyxia, although the child had suffered with attacks of suffocation. It was a case of diphtheria, as shown by deposits in the pharynx, but there was no swelling of

the ganglia. The larynx had been affected only since two evenings before, following deposits in the pharynx, which had been noticed two or three days previous. From the beginning the worst sign was complete anorexia, whilst the child was usually a good eater. He took beef-tea and wine with dislike. Syrup of ipecacuanha, to excite vomiting during suffocation, phosphate of iron and small doses of quinine, were the only remedies exhibited. No blister, and in fact nothing that could either fatigue or weaken the child, was applied.

It cannot be denied, that without the operation the chances of cure would have diminished, as aeration of the blood was decreasing. "To operate as soon as possible is a favorable condition for success," says Trousseau (Clin. Med., p. 427), and we all agreed to perate immediately. The operation was skilfully practised under the most desirable conditions by Dr. Borde. A little chloroform was given to diminish sensibility, and prevent the child from moving during the operation. Hemorrhage was trifling; no ligature was needed.

It is useless to say, that the most assiduous and intelligent care was given to the patient after the operation. Dr. Borde advised inhalations with carbolic acid, and the next day all was going on most favorably, and the greatest hopes were entertained. Yet, in the middle of the third night after the operation the child died with suffocation, and it is probable that the diphtheritic exudation had extended into the bronchi.

Fifth Case.—In the afternoon of *Thursday*, November 26, 1874, Dr. Gaudet brought me to see a little boy, 4 years old, with some obstruction of the larynx, with commencing asphyxia. All had been anomalous from the beginning, and kept on to the last, in this case. The larynx was affected fifteen days before, but without any deposit in the pharynx, or ganglionic engorgement, or any sign of general poisoning. It was the outset of stridulous laryngitis; the attacks of suffocation occurred at long intervals; but the voice remained hoarse and more or less diminished. Infiltration, with thickening of the vocal cords, was supposed to exist. Yet days passed, and the embarrassment of the larynx remained the same. On the 25th and 26th, hive syrup was given to excite vomiting, and at last, in the afternoon of this day (26th) asphyxia was threatening, and it became urgent to operate.

Chloroform was used to facilitate the operation, because night was coming. The incision in the trachea being too small, I

divided the cricoid, according to Heuter's method. No ligature was necessary. The dilator first, and a large Trousseau's double canula (8½) afterwards, restored free respiration. But the child being yet under the influence of the anæsthetic, the opening of the trachea was not accompanied by the usual intense coughing and efforts at expulsion which are generally so useful to throw off all foreign bodies obstructing the air passage, such as mucus, blood, or false membranes. This might perhaps be an objection to the use of chloroform in cases when there is much asphyxia.

One of my sons, a resident student at the Charity Hospital, sat up with the child that night. The child was restless, but the only attendance necessary consisted in giving him now and then milk and brandy toddy, and cleaning the inner canula of bloody mucus firmly adhering to it, but without false membranes. Febrile reaction was somewhat intense.

All next day (*Friday*) thick mucus, gradually getting free from blood, but adhering strongly to the canula, in spite of the vapor of water inhaled through the canula, was thrown off. I was commencing, indeed, to think that we had not operated upon a case of diphtheria, when the incision assumed through the day a grayish aspect, requiring to be touched with nitrate of silver, and the cellular tissue all around began to swell. At the same time ronchus could be heard over the whole chest, showing that there was narrowing in the bronchial openings. The second night was worse than the first, and yet no trace of false membrane.

On Saturday morning (about forty hours after the operation) the child was in great agitation. I removed the canula, and put the dilator in its place. A violent spell of coughing followed, and, at last, a piece of false membrane was thrown off; it measured one and a quarter inches in length, one-sixteenth of an inch in thickness, and was bifurcated at one extremity, which I have no doubt corresponded with the bronchi. I did not put the canula back, in order to leave a freer outlet for any false membrane that might become loose, inasmuch as when the neck incision was closed respiration did not seem to be very bad, showing that the larynx had become less obstructed. A few hours after I found the child sitting in his bed and amusing himself with engravings. Now and then he drank milk, beef-tea and brandy toddy, but rather reluctantly. He had thirst, but no appetite. Nevertheless I entertained some hope, whilst the parents thought their child saved. In the evening I applied the canula again,

thinking it more prudent that he should be with it during the night. But I was soon after called back in haste, the child was suffocating.

The canula being removed and the dilator applied instead, it became evident to me that there was a false membrane adhering probably by the bronchial bifurcation, free at its upper extremity, and at each inspiration producing clapping, like a valve closing. I imagined it might be the longitudinal half of the fragment that had been expelled in the morning, and I introduced a feather surely as far as the bronchi, with the hope of loosening it; but the same valvular clapping kept on, and the child was asphyxiating.

During this struggle which seemed to me so long, I was tempted to introduce a blunt pointed bistouri through the incision, and divide the tracheal rings to the sternum. But I was alone, and the thought of instantaneous death withheld my hand. Dr. Jacobi, of New York, said on an analogous occasion: "the child was strangled in the attempt to save her life," and I fell back before the possibility of having to say, in my turn: the child was stabbed in the attempt to save his life. Gradually the efforts to get air grew less and less, and the child died quietly in the night.

I might have resorted to aspirations and suctions through a large india-rubber catheter; but I had not yet read page 46 of Dr. Cohen's "Croup in its relations to Tracheotomy:" "Another element of danger is the descent of loosened membrane upon the bronchi. Heuter strongly urges in every operation for membranous croup that, immediately after the opening made into the trachea, an elastic catheter (from 8–12) should be introduced down to the bifurcation, and suction be made, as first recommended by Roux."

Last Conclusions.—What conclusions must we draw from the facts above, so different from one another, so varied, so contradictory? That we must never be discouraged, and that we must assist nature to the last. As long as there is a shadow of hope we must give air to the poor children a prey to diphtheria, and afford them a chance to struggle against death. To the last we must open the air passages, as well as feed and stimulate the patients, using the stomach tube to that effect if necessary. Dr. Millard says: "The fear of shortening the list of successes must not influence the practitioner in giving up an operation too readily" (Trousseau, Clin. Med., p. 428). In fact, in the present state of

medical science, the prognosis of tracheotomy is impossible: children in the best conditions have died, and others operated upon under the worst circumstances have recovered. Hence the duty to operate, unless all hopes have really vanished away.

But it is not only the fear of a fatal result that keeps back most of the practitioners, but really the operation itself. Whereupon, they hesitate, delay, and at last give up an operation which, if performed in time, might have resulted favorably.

This accounts for my publishing, and publishing over again, a few details on the operation for tracheotomy, which I believe are calculated to put the operation within the reach of the least experienced physicians in the use of the knife:

1st. Making an incision exactly on the median line, from the larynx to the supra-sternal fossa.

2d. Feeling for the cricoid, and holding it between the index and thumb of the left hand, and pressing it against the vertebral column, in order to steady the trachea.

3d. Cutting into the trachea, immediately below the cricoid cartilage, and dividing the trachea sufficiently low, in one cut and while it is steady.

4th. Carrying the closed branches of the dilator between the index and thumb, still holding the cricoid, and pressing the dilator into the trachea at the same time that the handle is being raised.

By this process it is impossible to miss the opening in the trachea, or to fail introducing the dilator into it.

5th. Raise the patient to a sitting posture and open the dilator.

It is always time to introduce the canula, as the dilator kept open for a while will favor the freeing of the trachea, even better than the canula, of the blood, or even of the false membranes, if any have already formed below the larynx.

The after-treatment, no doubt, is of the utmost importance: "A great deal of the success to follow the tracheotomy for croup will depend upon the after-treatment of the case" (Dr. Cohen, p. 55). I know it; but it does not here fall within my province to speak of it. My object in this article was only "*the operation for tracheotomy*," with the hope that it might be practised by all, "*Citiùs et Tutiùs*," that is, more rapidly and more safely than it is generally performed.

ARTICLE V. *The Cause of Anæsthesia and Death Produced by Chloroform and Ether. My Method of Resuscitation, with an Analysis of Dr. M. Sims' Address, Published in the November Number of this Journal.* By M. SCHUPPERT, M.D., Professor of Operative Surgery, Charity Hospital Medical College.

In the following pages I will offer the proofs that anæsthesia produced by chloroform and some other substances, like ether, ammonia, carbolic acid, etc., as well as death, and the symptoms resembling death, following the inhalations of the vapors of chloroform and ether, are caused by a globulary stasis and embolism of the blood in the cerebral bloodvessels; that the method of resuscitation I have pursued in such "cases of death by chloroform," and which has been published in several medical journals, has not, as Dr. Sims asserts, been copied from the late Dr. Nélaton, and that the said Dr. M. Sims, in his unwarranted and unproved accusation of plagiarism, has provoked and deserves the severest criticism.

The subject under consideration comprises the local and general disturbances of circulation, which are produced by an altered form of the red blood corpuscles, that altered form being the result of the influence of temperature, or of the action of different substances, as, for instance, the so-called anæsthetics: chloroform, ether, alcohol, carbolic acid, etc.

C. Hueter, Professor of Surgery in Greifswald, Prussia, and some of his scholars under his direction, have made extensive researches with those different anæsthetics on animals, and I intend to give them the full credit they deserve for their investigations, a portion of which have been published in the Deutsche Zeitschrift fuer Chirurgie, 4th volume, under the heading "Globular Stasis and Embolism, by C. Hueter," reference being had to the red blood corpuscles, though stasis and embolism may also be formed by white blood corpuscles.

Hueter does not intend to imply thereby a stand-still of the blood corpuscles in general, but an arrest in consequence of their own alterations. The first question then, is, by what means are we able to change or alter the forms of the red blood corpuscles in the living body, and what kind of disturbances do the altered forms produce in the circulation? Hueter says: We have observed the living circulation in the web skin, the mesentery, the tongue and the lungs of the frog, but these parts cannot be used

for the purpose of observing the first disturbances which are produced on the red blood corpuscles by those different substances; for that purpose the skin of the abdomen and of that part between the toes of the extremities are the best. We observe the skin to become *red* under the application of ammonia, chloroform, glycerine and carbolic acid (3 per cent). That redness does not depend upon a simple dilatation of the vessels, but on the overfilling of the vessels with dense masses of red blood corpuscles; the vessels being at the same time dilated. What is the cause thereof? Paralysis it cannot be; since in that case no complete obstruction in the circulation could have taken place. Paralysis of the tunics of a vessel would, moreover, be followed by an increased current of the blood. If we put a few drops of liquid ammonia on the abdominal skin of a frog we will soon observe one, two, or more red blood corpuscles, with an altered form, adherent to the wall of the vessel. We must therefore conclude that alteration of the form of the red blood corpuscles is the cause of that phenomenon. In using glycerine the absorption of water may also have an influence on the alteration of the stroma of the red blood corpuscles. With carbolic acid and chloroform the influence is probably of a more complicated nature, though, at present, we do not know much about it. The intravascular alteration of the form of a red blood corpuscle we must consider, so far as the real cause of the globular stasis.

The epidermis of our skin is much thicker than that of a frog, still not thick enough to protect the papillary body with its nerves and capillaries against the influence of any of those mentioned liquids. We all feel a numbness in our fingers from contact with even diluted carbolic acid, an irritation of the sensitive nerve fibre being first experienced, which, when augmented, ends in a more or less complete anæsthesia. It is not impossible that the carbolic acid by imbibition has the same effect on the red blood corpuscles of man as we have observed on those of the frog. The effect of ammonia on the skin is of a cauterizing nature—it kills the tissues. The globular stasis has therefore to be considered as a factor in causing necrosis. Let us now inquire in what connection inflammation stands to the cauterizing influence of ammonia and to globular stasis. It seems, says Hueter, that the action of most all cauteries has to be considered less as causing inflammation than producing globular stasis. With the exception of dilatation, which we might con-

sider to be of a passive character, all the criteria of inflammation are absent. But we cannot deny that with an additional action of other irritants (monads), the process of inflammation may take place at a later time. Such a process would have as a base globular stasis; the globular stasis, not the inflammation, being a consequence of canterization. The action of canteries is different. Some produce a moist, others a dry scab. The latter sort are to be preferred, because with them the process of putrefaction cannot be developed on account of want of water. Compare, for instance, the scab produced by caustic potassa. The more extensive the diffusion of the cautery in the tissues, the greater is the reaction (globular stasis, inflammation by nomads). Like the named canteries, we observe also the effects of a high or of a low temperature causing a globular stasis. The red blood corpuscles will thereby attain a variety of forms. The stasis produced in the vessels of the skin by those canteries, chloroform, carbolic acid, glycerine or ammonia, are only of short duration. The disturbances caused by the last mentioned substances are greatest. We do not imagine that the red blood corpuscles, which have once changed their forms, will again return to their original shape, but they will enter the circulation and produce there different disturbances.

All the named canteries act also as anæsthetics, producing at the same time paralysis of the muscles. It will probably astonish many to hear of such effects produced by glycerine and ammonia, or by an immersion of parts of a frog in water of 40° C., or in ice. Immersion in vapor of chloroform of a part of a frog, will act even more energetically than if inhaled. This anæsthetic and paralyzing effect of agencies, so different in their pharmacological character, speaks much in favor of a mechanical explanation. The red blood corpuscles, altered in form, which after their arrival in the capillaries of the centres of the brain have formed there globular embolism, are to be considered as the representatives of the anæsthetic effects. The globular embolism, which follows globular stasis, does not present itself in the formation of distinct cuneiform embolic masses, but in a diffuse multiplication of globular stasis through the whole area of the circulation of the blood.

The importance of the globular stasis in regard to the teachings of physiology cannot be appreciated too highly, since the relations between the walls of vessels and the circulating blood

are illustrated by the globulary stasis in a singular manner. At the first view, says Hueter, it became doubtful, if the stasis was more the result of an alteration of the walls of the vessel, caused by the acting substances, than an alteration of the elements of the circulating blood. We might have reasoned, that a dilatation of the vessels had been brought on by a paralysis of the vaso-motor nerves, simultaneously with a histological alteration of the internal coats of the vessels, producing a rugged condition, to which the red blood corpusles became adherent, interrupting the circulation by filling up the vessels. But with these speculative ideas experience could not be brought to harmonize, though a change in the contractile and mechanical relations of the walls of the vessels cannot well be denied, since it has been proved by microscopical examinations. The direct observation of the origin of the globulary stasis with the help of the microscope, shows it to be intimately connected with the transformed blood corpuscles.

Pharmacologists have endeavored for a long time to find out the action of certain poisons on the human organisms, especially to classify them as operating on the nerves, or acting on the blood vessels. We have in this manner learned to discriminate between nerve and blood poisons. The action of poisons on the circulation of the blood we have generally considered as an action on the peripheric vaso-motor nerves and nervous centres, and we know of only a few poisons which act directly on the blood. The researches of Hueter go to prove, that some poisonous substances alter the form of the red blood corpuscles in such a manner, that the latter remain adherent to the walls of the smallest vessels; and this happens not only at the place of application of these poisons, but, through the process of globular embolism, the circulation of the blood may be interrupted temporarily even in the remotest regions of the body. One of these poisons—chloroform—can change, with even a moderate influence, the forms of the red blood corpuscles to such a degree, that thereby great disturbance in the circulation may be effected. In regard to the so-called blood poisons it is therefore important to know, whether they not only act chemically, in changing the molecular composition of the blood, but also mechanically, in altering the form of the red blood corpuscles, through what we call globular stasis; yet we may assume that the latter will play the more important part in the act of poisoning. No doubt

we have to discriminate between the action of chloroform on the abdominal covering of the frog and its application in man; still, we can hardly doubt, that in man the introduction of the vapors of chloroform, during the act of inspiration, will act on the blood of the thin capillaries of the lungs in the same way, as on the blood of the cutaneous capillaries of the frog. By the introduction of cauterizing poisons (for instance, of sulphuric acid) into the stomach, there will originate in the periphery of the mucous membrane, destroyed by the action of the sulphuric acid, processes resembling those of the globular stasis caused by ammonia, which will develope by globular embolism a secondary effect on the general circulation. From all this it becomes apparent that, in the action of poisons we have to consider foremost the *mechanical momentum* represented in the effects of the changed form of the red blood corpuscles upon the circulation, and that in such poisoning actions, the *chemical* effects comprise only the *form* of the red blood corpuscles.

To return now to the anæsthetic effects of certain substances.

Hueter does not hesitate to declare, and here he is supported by his researches, that the anæsthetic effects of chloroform, ether and alcohol, are based upon an alteration of the form of the red blood corpuscles. He considers, as the representatives of the anæsthetic effects the changed red blood corpuscles which, from the place where their form has been altered, have been transported to the capillaries of the centres of the brain, causing there a globular stasis. He finds for this hypothesis a support in the fact, that the different degrees of anæsthetic power, which exists between alcohol, ether and chloroform, runs parallel with their power to alter the form of the red blood corpuscles. Chloroform causes the most important changes, whilst the effects of ether and alcohol are of smaller intensity. The observations made on the frog show, that by painting its abdominal coverings with ether and alcohol, not only a globular stasis but also anæsthesia is produced.

Hueter continued his experiments on rabbits. It is known, he says, that these animals very rarely survive the narcosis by chloroform, but usually die during the first 24 hours after their recovery from the narcosis. In the blood taken from these animals during the narcosis produced by chloroform, or soon afterwards, the red blood corpuscles show a very irregular form; their outlines

are spiked, pronged, often presenting two great prominences in the form of a club (the author states here that he is well aware of the frequent natural occurrence of pronged red corpuscles in the blood of the rabbit, but with which these had nothing in common). If the narcosis has been produced by ether, the red blood corpuscles present more the form of the mulberry. Corresponding to these slight alterations in the form of the blood corpuscles is the narcosis. It lasts a shorter time and is commonly very well endured by these animals.

Experiments made with alcohol, injecting four syringes full (of Pravas syringe) hypodermically, resulted in drunkenness; the rabbits presented unsteady running, falling on their sides, which was followed in about 20 minutes by deep sleep.

Blood taken from such animals presented a characteristic alteration of the red blood corpuscles. The outlines of the circular form of these corpuscles had about 20 very fine short spikes, looking as though they were fringed. These changes of the blood corpuscles lasted even 24 hours after the time intoxication had set in. It is singular that the red blood corpuscles of men, during the period called in German parlance "katzenjammer," presents the identical alterations as in rabbits. Who can deny the practical importance which these experiments have in reference to the narcosis produced by anæsthetics, for the purpose of performing surgical operations. The results, says Hueter, obtained with different anæsthetics, force me to hold with those surgeons, who regard ether as a less dangerous anæsthetic than chloroform.

It cannot be denied, that the narcosis produced by ether, requires a longer time and is of shorter duration, than when caused by chloroform; but that should not induce us to give preference to the more dangerous chloroform; yet, we will soon show, that we also possess means to deprive the narcosis, produced by Chloroform, of its danger and I am therefore not disposed to decide yet the question of ether *contra* chloroform, in favor of the first. It may be of interest to state here, that the method used by Hueter in narcotizing frogs, consisted in injecting hypodermically ½–2 syringes full of ether into the muscles of the thigh; the narcosis produced in this manner, lasted from one to several hours. The researches in regard to the globulary stasis produced by chloroform, which have been made by one of the scholars of Prof. Hueter, Mr. Witte, will soon be published. The following is an

extract from these taken for the present purpose. The method of producing narcosis by chloroform in a frog, consisted in the following proceeding. At the bottom of a common test tube a small portion of cotton wadding, saturated with chloroform, was pressed down. One of the hind legs of a frog was now inserted in the tube, so that the upper portion of the thigh nearly closed the opening. A few minutes after the test tube had been filled with the vapors of chloroform, the frog had become perfectly narcotized, through the absorption of the chloroform by the vessels of the skin. These vessels showed now the highest degree of globular stasis, the skin of a deep red color, by the complete filling up with red blood corpuscles; the circulation was interrupted and a complete tetanic condition of all the muscles of the narcotized extremity observable. A microscopical examination revealed globulary stasis in the exposed extremity, whilst in the other an uninterupted circulation of the blood could be seen.

It would undoubtedly diminish the danger of narcosis produced by chloroform, if we could narcotize man in a similar manner. The importance of an organ like the lungs, which supply the blood with the necessary oxygen, is obvious, and it is not impossible, that a globulary stasis produced in the lungs, may, under certain circumstances, produce death by suffocation. That would be obviated by the vapors of chloroform being absorbed through another capillary system. At the time when we were engaged with our researches in regard to the action of chloroform, says Hueter, I received the interesting communication of my friend Schuppert, of his method of resuscitation from apparent death caused by chloroform. After Brown had already recommended the same treatment,* Schuppert has also published his good results and with a more correct interpretation than Brown, of the effects of his method. We undertook to prove the value and importance of the treatment on our frogs and rabbits. In general the results of our investigations speak in favor of his views. I will not anticipate Mr. Witte, but make use of so much of his experiments as the importance of the subject in question will justify, since a subject which is so intimately connected with the

* It would appear from the statement of Hueter, as if Brown had anticipated me in the method mentioned; that is not what Hueter intended to insinuate, he only has reference to *the time of publication* of Brown's paper. From the publication of Brown's cases in the *British Medical Journal*, 1871, it is clear that I employed my method several year before him. My first case happened in 1864.

saving of the lives of men, the direct removal of danger to life from the narcosis produced by chloroform, cannot be published too soon.

Hueter describes then the following experiment, appertaining to the question involved in my method. On an oblong piece of a glass plate, which on both sides carries a piece of wax in the form of half a ring, a frog narcotized with chloroform, either by painting the skin of his abdomen, or by inhalation, is fastened in such a manner, that on one ring his tongue is fastened with needles, on the other ring the spread out web skin; the leg being in a straight position. The glass plate is now held during some minutes in a vertical position, with the head of the frog uppermost, the legs downwards. When the tongue of the frog is then brought under the microscope, there will be observed considerable disturbance of circulation, which we have to bring in connection with the process of globular embolism. Sometimes there is seen a complete standstill of the column of the blood in the vessel, whilst the skin of the web-foot shows hardly any interruption in the circulation. Let the glass plate now be turned and held during some minutes in a reversed position, the head downwards, the web-feet uppermost, and the microscope again be brought into action, we will then observe considerable disturbance of the circulation, up to a complete stasis in the vessels of the skin of the web-foot, upon which the process of globular embolism has now reacted to its full extent; whilst on the contrary, the tongue presents a free circulation. But this turning of a frog upon his head often results in the complete recovery of the frog from his narcotism and the frog will try to free itself from its fetters, before the experiment is finished. In such a case the trial often succeeds in bringing the frog back into its narcotic condition, by reversing the position, head uppermost. It may be, that the pressure of the arterial column of the blood, between the heart and the capillary territories, will assist the action of the heart, in pushing the red blood corpuscles, adherent to the walls of the capillary vessels, forward into the veins. The lower half of a frog, in an erect position, will therefore become free from globular stasis and we will find it to accumulate in the head, whilst, when the position of the frog is reversed (turned upon its head), the embolic stasis is dissolved in the head and the frog escapes from its narcotism. Whatever the correct explanation may be, this much is certain, that we are most positively author-

ized to avoid the dangers of narcosis produced by chloroform, by a lowering or turning of the head. The attentive reader of my method and of these experiments cannot fail to come to the conclusion, that in my propositions I have been fully sustained by the ingenious experiments of Hueter, and not less does it seem true, that in case of affection of the right heart, with the administration of chloroform, we have to use the utmost caution.

Let us now recapitulate.

Though we cannot give at the present time a proper definition of the action of chloroform, we observe that the form of the red blood corpuscles is changed; that in consequence of that alteration of the form, the corpuscles become adherent in the small capillary vessels; that the walls of these vessels become paralyzed; that the vessels are dilated, often to the double of their calibre, and that synchronous with the process of stagnation narcosis also takes place. With the circulation of the blood the red blood corpuscles, altered in form, are driven into the veins, the right heart, and from there to the different parts of the body. When the capillaries of the brain are filled with stagnating blood, it is obvious, that severe alterations of innervation of the nutrition of the nervous centres will take place. With a renewed action of the heart, a propulsive movement of the stagnating blood and a diminution of the calibre of the extended vessels has been observed. Schueller, experimenting on rabbits, found that after 8–10 inspirations of chloroform vapor, the arteries and veins of the pia mater began to contract with a retardation of the pulse, but soon to be followed by a paralysis of the walls of the vessels and dilatation, and subsequently with a complete stagnation of the blood. From this we may conclude, that the red blood corpuscles, altered in form by the action of chloroform, are the real carriers of the anæsthetic effects and the cause of the stagnation of the blood, of the globular stasis.

Following up the hints given by Hueter to produce anæsthesia by external application of chloroform, I have tried on two occasions the anæsthetic influence of chloroform vapor on the prepuce of the penis. After the exposure during a quarter of an hour of the penis, hanging in a cylinder of glass at the bottom of which was put some cotton saturated with chloroform, the cylinder being placed in warm water, the skin had become so insensitive, that its subsequent incision did not produce any pain. In another case the penis remained a quarter of an hour in the cyl-

inder, without the temperature of the chloroform having been raised, and though the quantity of the chloroform vapor was less than in the former case, patient did not feel the cutting as intensely as a few days before, when I operated on the same parts without the application of chloroform. I shall repeat the experiment on a greater scale on the extremities, as soon as the opportunity presents itself.

In closing herewith the interesting and most valuable researches of the eminent German surgeon, I am forced to turn to a less pleasant subject, to the address of Dr. Marion Sims, of New York, published in the last number of this Journal, "On Nélaton's Method of Resuscitation from Chloroform Narcosis, read at the 42d Annual Meeting of the British Medical Association, held in Norwich, August, 1874, by J. Marion Sims, M.D., Surgeon of the Woman's Hospital, of the State of New York, etc."

With candour and liberality the distinguished editor of this Journal has offered its pages to those "touched in the article of Dr. Sims;" remarking at the same time, with great propriety, that, "in further discusssion of that important matter, the question of usefulness ought to be superior to the less important one of priority." In giving publicity to the contributions contained in the foregoing pages, illuminating the hitherto dark and obscure action of anæsthetics, I hope I have succeeded in showing that I fully concur in the views expressed above by the editor; but when claims to priority are touched with unclean hands, when they are encircled with a net of suspicion, when they are attacked with the unfair weapons of sophistry and hypocrisy, those claims have ceased to be an unimportant matter, and I hope to be equally successful in refuting the insinuations of the article, whose title I have just mentioned.

Dr. Sims, in that very remarkable address, which he published in several medical and lay journals, simultaneously in England and in this country, in the *New York Tribune* and in the *Scientific American* (and in how many others I do not know; in the *Tribune* he appears even as the co-inventor of Nélaton) impeaches two American physicians, Dr. Holmes of Chicago and myself, of *plagiarism!* Not knowing Dr. Holmes, nor his article in the *Chicago Medical Journal*, I must leave it to him to defend himself against that disgracing accusation.

Sims, in publishing that "address," written in his well known egotistical and conceited style had, as it seems, two material

points in view; *first*, his own eulogistic self-glorification, and, *second*, to try and defraud me of the modest claims I had asserted to a method, original I believed with myself, but too important to hurry into print before I could support my ideas by the weight of experience. Modesty and doubt had prevented me from publishing the first case, which happened in 1864, though the result of my treatment seemed to verify the idea I had already previously formed, that chloroform did produce anæmia of the brain. Fortune, which attended me during nearly ten years of my administering chloroform, had deprived me of the opportunity of again testing the value of the method. Never, from the first application, till the publication of my method of resuscitation, had I heard or seen any remarks having reference to it (with the exception of Brown's paper, in 1872, mentioned in my article; and though I thought his views erroneous, still, in the *modus operandi* it corresponded with mine), nor have I since been able to learn from any source, that the method referred to was not original. Besides this Journal and the *Zeitschrift für Chirurgie*, it has been published and been mentioned in several medical papers. Only a few days ago, the editor of the *Revue de Therapeutique Medico-Chirurgicale*, le Docteur A. Martin Langer, in Paris, sent me a copy of his paper (for which I tender him herewith thanks), and though published in the city of M. Nélaton, not one word was said that the method had already been known there. In order to plead my defence in a proper manner and to prove, that my assertions and expressions are well supported, I have to beg the kind reader, to go with me in a cursory manner over that mysterious document, the address of Dr. Sims to the British Medical Association.

In the beginning of that address, we are made acquainted with a new kind of fistulas—"fistulas in high life." "The patient," says the author of "Silver Sutures, the Greatest invention of the 19th Century," "belonged to one of the oldest and best families of France, and the operation was one of the most difficult I ever performed on any one in *her station of life*."

He cured the fistula, though Nélaton had pronounced it incurable, testifying thereby to his superiority of judgment and operative skill. A friend and colleague of mine, and a scholar of Nélaton, who, moreover, had been very intimate with the French surgeon, when I showed him this passage, pronounced it "a fiction." My friend, though having daily frequented the lectures

of Nélaton, does not remember to have heard Nélaton say anything of the method of resuscitation, as Sims asserts.

Sims, in examining the case, discovered that the *vaginal* portion of the *cervix uteri* was destroyed, nevertheless he afterwards dissected it off from the posterior wall of the vagina! After he has given a lesson in geography and told us, that the operation was performed at St. Germain and that St. Germain is about an hour's distance from Paris by rail, he says, that Dr. Nélaton with some other doctors were present; that one of them, Dr. Campbell, was selected by the family, by Dr. Nélaton and himself (Sims), to give the chloroform; that he (Sims) had had entire confidence in him (Campbell), in his caution, skill and judgment; that he frequently stopped during the operation to ask Dr. Campbell if all was going on well and that at the end of 40 minutes, the sutures, 12 or 13 (he is not quite sure of their number, though of the minutes he is), had all been placed, when all at once he discovered an *unusual bluish, livid* appearance of the vagina, as if the blood were stagnant; he felt rather uneasy about it and asked Dr. Campbell, if all was right with the pulse, when Campbell replied, "All right, go on; but stop! stop! no pulse, no breathing," and, looking at Nélaton, he (Campbell) said, "*Tête en bas, n'est ce pas;*" and Nélaton replied, "Certainly, there is nothing else to do." "Immediately the body was inverted, the head hanging down, while the heels were raised *high in the air* by Dr. Johnson, the legs resting one on each of his shoulders." Sims tells us here, that he many years ago had imbibed the convictions of his countrymen against chloroform in general surgery, always giving ether the preference and having never felt the dread of danger from it under any circumstances (this proves how little Sims knows of the history of ether inhalations—vide below). Sims continues: "whilst Dr. Campbell supported the thorax, Mr. Hebert held the patient's jaw open with a spoon, and Dr. Nélaton pulled the tongue out with a tenaculum." (Why, when *tête en bas n'est ce pas* was the "nothing else to do?") If Nélaton had felt that security in the application of "his method," *that* pulling was not necessary at all! and equally so Dr. Bazard's efforts at artificial respiration. But what did Sims? He tells us: "I stood aloof and watched the proceeding with the most intense anxiety," and "so I stood about 20 *minutes*, though Dr. Campbell, in his report, says 15 minutes. Be this as it may, the time was so long that *I thought it useless to*

make any further efforts, and *I said, gentlemen, she is certainly dead*, and *you might as well let her alone.* But the great and good Nélaton never lost hope," etc. That means, the man Nélaton, whose superior judgment Sims had just before tried to detract from, did not care for his (Sims) palaver, "and she recovered." After the resuscitation the patient was laid on the table horizontally, and at once died again; yes, according to the verification of Sims, was "more dead than before." But this time it took not quite so long to bring "her to life again under the same "tedious, painful, protracted and anxious efforts:" "She is all right this time." She had produced in Sims a "*tension of heart, mind and soul.*" But the agonies of our man with a "mind and a soul" were not to be ended yet; "the best blood of France" had to die a third time and this time "she looked the perfect picture of death," and our hero again "gave up all as lost." Notwithstanding the two preceding recoveries—the double proof that the syncope was caused by anæmia of the brain—Sims thought that "the blood was so poisoned, so charged with chloroform, that it could no longer sustain life." "But Drs. N. and C., and J., and B. and H. (all but Sims), quickly inverted the body a third time, thus throwing all the poisoned (?) blood possible into the brain, and lo! there was a spasmodic gasp, another and another, and a third inspiration far between; then we (Sims?) watched, and waited, and wondered if there would ever be a fourth; at length it came, and with it the pulse, and the natural color of the tongue and lips, and even vomiting. She moved her hands and pushed even against Campbell." And what thought Sims during these anxious minutes of reanimation? "I was," he said, "by no means sure that these movements were not merely signs of the last death struggle (hear!) and so expressed myself." (O si tacuisses!)

Sims does not tell us what the other doctors answered upon such a keen diagnosis. Dr. Johnson did not respond with "see here, Dr. Sims, what a confounded ass you are," but, "see here, how she kicks," and very soon after this all said, "she is safe at last!" "*For heaven's sake keep her safe!* I replied," and with a *naïveté* without its parallel, he adds to the narration of this serio-comic history, "*this was the first and only suggestion I made during all these anxious moments, and it was acted upon.*"

What shall we say to such self-incrimination of incompetency and pusillanimity—to such a spectacle of timidity and cowardice!

Instead of lending a helping hand, yes, being the first to suggest, to act, in such a case of emergency, he folds his arms and cries "she is certainly dead, and you might as well let her alone!" and such a man, the representative of a noble profession whose shibboleth is bravery, intrepidity and daring, calls himself *a surgeon!* It is fearful to contemplate, that such a faint-hearted person should invite human beings to intrust their lives into such hands.

"I have detailed this interesting case," continues our hero of the needle, "at great length (and *ad nauseam*), because it establishes a principle of treatment as any one case ever did or possibly can" (if it was true), "when life was again and again reëstablished by a position that favored only the gravitation of blood (poisoned as it was thought to be?) to the brain." But I will close here this just if bitter criticism of the narrated case and turn to the main object—the impertinent allegations uttered by Sims in the following sentence, copied in full:

"The *New Orleans Medical and Surgical Journal* for November, 1873, says: In the course of an extended experience in the administration of chloroform, it has happened three times to Dr. M. Schuppert that to all appearances the narcotized subject died—that is, respiration ceased, the heart stopped beating, and muscular contractility became extinct. The method he adopted for resuscitating these patients consisted in reversing the body, either by hanging them up by the feet or laying them over a bed or table; so that the greater part of the body with the head hung down. In that position, artificial respiration was also tried. In one case, five minutes elapsed before there was a natural inhalation. All of them recovered. Dr. Schuppert believes that in cases of death from Chloroform, the primary cause of the cessation of the respiration and circulation rests in anæmia of the brain, and not in impregnation of the blood with carbonic acid.

"Another American authority, Dr. E. C. Holmes (*Chicago Medical Journal*, September, 1868), says that whenever there is any failure of the heart's action, as is nearly always the case, the body should be laid at an angle of 40° with the head downwards, so as to favor the passage of arterialized blood to the brain.

"*I take it for granted that Dr. Schuppert and Dr. Holmes must*

[*Continued on page* 589.]

EXCERPTA.

Suppositories of Chloral.

Suppositories of chloral have been recommended by Dr. Const. Paul in cancer of the uterus. They are made from cacao butter 11 grams, white wax 7 grams, and chloral hydrate 6 grams, to be divided into six suppositories.—*Gaz. Chim. Ital.*, iii.

Improved Formula for Charta Sinapis.

A. W. Gerrard, in the *Pharmaceutical Journal and Transactions*, gives the following formula as one which, in his hands, has stood the test of experience:

Take of caoutchouc,	- - -	1 part,
Benzol,	- - - -	49 "
Black mustard, in powder,		q. s.

Dissolve the caoutchouc in the benzol; then stir in the mustard till of a proper consistance for spreading on paper.

New Preparation of Ergot.

A very active preparation of ergot, which is particularly adapted for subcutaneous injection, is suggested by Dr. Wernich, of Berlin, who proposes to exhaust the drug with ether, strong alcohol, and finally with water. The infusion is then dialyzed through parchment paper, and the solution evaporated. This extract, after acidulation with sulphuric acid, was mostly soluble in alcohol, and when again carefully neutralized by soda, yielded all its active properties to weak alcohol.—*American Journal of Pharmacy from Apotheker Zeitung.*

A Test for Blood in the Urine.

The following is given by Professor Almen, as a simple means of detecting blood in the urine: Mix a drachm of guaiacum with an equal volume of oil of turpentine in a test tube, and shake together till an emulsion is formed. Add the urine to be tested carefully, so that it may fall to the bottom of the tube and form a distinct layer. As the urine and emulsion come in contact, the guaiacum resin separates as a fine white, dirty yellow, or green precipitate. If blood is present in the urine, the resin will assume a more or less intense blue color—often almost indigo. In normal urine, or that containing only albumen or pus, the blue color does not make its appearance.—*Journal Applied Chemistry.*

[Abstracts of Papers read in the Obstetric Medicine Section.]

Application of the Galvanic Cautery to Gynecology. By J. Braxton Hicks, M.D., F.R.S.

After describing the mode of action of the various forms into which the platinum wire is arranged, he explained their adaptation to the various diseases met with in obstetric practice; comparing it, as he proceeded, with the other forms of cautery, with the knife and écraseur. He pointed out the special advantage of the wire loop in cases of malignant disease of the cervix, which were too sessile for the écraseur. He also alluded to the great advantage it was, in this form of cautery, that the loop, needle, and conical forms could be applied cold; and that complete and careful adaptation to the end intended could be effected. The paper was illustrated by Messrs. Krohne and Sesemann with apparatus (slightly modified, after Dr. Middeldorff's) in action. The battery was Benson's combination four cells; the carbon plates immersed twelve inches by eight.

On Dysmenorrhea. By C. R. Drysdale, M.D.

The object of this paper was to state the conviction of the author that over much attention had of late years been paid to the mechanical treatment of painful menstruation, to the exclusion of hygienic and therapeutic remedies of less heroic but no less beneficial nature. This opinion was given after a trial made for some years of the various methods recommended by Sir J. Simpson and his able followers with very doubtful success. In some instances cases of dysmenorrhea, treated by dilatation by means of sea-tangle tents, had become more intractable from the the superaddition of some other uterine disease, such as pelvic peritonitis, or even abscess, to the former severe sufferings of the patient. Again, in some cases of incision of the cervix uteri, it was questionable that damage of a severe nature had been done to the patient, and that, above all, when the internal mouth of the cervix was divided. Several instances of this kind had come before his notice where, indeed, no relief had been given, after all, to the painful and distressing malady, which was certainly one of the most distressing diseases that members of the human race were subject to. Dr. C. R. Drysdale thought that the surgical school had of late laid far too much stress on the supposed necessity of cutting operations in dysmenorrhea, and asked whether sufficient attention had been given to the use of warm baths, quinine, and iron, Indian hemp, chloroform, and other remedies; the use of a pessary, like those called Hodge's, was often of service; and catheterization with the sound was sometimes of use; nor did he entirely object to incisions being made use of in those very rare cases where there is conical cervix with pin's-head os uteri. But, in the immense mass of cases of painful menstruation coming before the practitioner, there is no very

great narrowing of the canal of the cervix. In all such cases he had come to the conclusion that it is unadvisable to have recourse to any surgical operation, but that palliative and hygienic remedies should alone be had recourse to.

On Maternal Impressions. By J. Clapperton, Esq.

Mr. Clapperton read a paper on the effects produced upon the fetus by some fright experienced by the mother in the early months of pregnancy. In a large proportion of cases the object of fright was a natural object, and the results of the fright were various. In some, a nevus, or mother's mark, bearing a striking resemblance to the object which excited the mother's alarm, was found upon the skin of the fetus; in others, a graver impression was made, and there was a serious modification or arrest of development induced. In order, however, for this to take place, a certain progress only must have been made by the embryo; a little further development protected it from any very grave modification. In some instances the changes produced by fright were recurrent in the same individual, the object being different in the separate cases. Mr. Clapperton had met with one of these recurrent cases; in the first instance the object was a rat, the second a dog. Other observers had had similar experiences. The actual means by which such modification was produced were not quite determined on, as there had not yet been demonstrated any nervous connection between the mother and her child. Further research, Mr. Clapperton thought, might succeed in demonstrating some nerve communication to exist. After showing that there were some grounds for supposing that these impressions upon the mother affecting the embryo were not confined to human beings, Mr. Clapperton discussed the importance of these malformations from a diagnostic point of view, and stated from his own experience the puzzling character of the presentation in these cases. The paper was illustrated by a specimen.

The Rectum in its relation to Uterine Disease. By Arthur W. Edis, M.D.

The author was desirous of drawing attention to the important help afforded us by the rectum, both as to the diagnosis and treatment of uterine disorders. Although known to the few, exploration by the rectum seemed scarcely as generally known as its importance would suggest; and, for the relief of pelvic symptoms, there were numerous cases where sedatives applied *per rectum* were of far greater service than when taken in the ordinary way. As regards the diagnosis of uterine disorders, there were several methods of rectal exploration: viz., the ordinary digital one—useful in cases of retroflexion, fibroid, and

ovarian tumors, &c.; eversion of the rectum through its sphincter, like the finger of a glove, by pressure from the vagina; manual exploration, as suggested by Dr. Gustave Simon. In reference to the treatment of uterine disorders by the aid of the rectum, the cases were very numerous. Reposition of the retroflexed gravid uterus could readily be accomplished by pressure *per rectum*. The relief of imagined uterine symptoms might often be effected by a proper attention to the condition of the bowels. The question of absorption by the rectum was too well recognized to need discussion. Pessaries and suppositories made of gelatine and glycerine were far more useful than those made with cocoa-butter, absorption being more uniform and certain. In menorrhagia, enemata of ice-cold water, acting by reflex action, proved of great service. In dysmenorrhea, enemata or suppositories of morphia, belladonna, and other agents, were very useful, and too little resorted to. In the morning-sickness of pregnancy, the most intractable vomiting might often be relieved by opiate enemata or suppositories, and life sustained for many weeks by nutrient enemata. In cancer of the uterus, suppositories were far more useful than pessaries in allaying pain. In chronic constipation, small suppositories of soap or extract of belladonna were of much service. As to diseases of the rectum itself being mistaken for uterine disorder, the mistake was not unfrequent; fissure causing vaginismus, ulcer causing dyspareunia, menorrhagia, &c.

On the Tensile Strength of the fresh Adult Fetus. By J. Matthews Duncan, M.D.

He began by pointing out the very wide room there was for advancing midwifery by laboratory experiments of varied character. Without many such, indeed, progress in many practical matters was impossible, because without them we could not reach a solution of many questions which were fundamentally important in the settlement of what was right practice. One of these questions was that whether delivery by podalic version or by forceps in cases of contracted pelvis was the better proceeding. After a hundred years of mere discussion the question was still unsettled. The present paper gave a few simple results of an inquiry as to how much force it was possible to get applied to the fetal head by pulling on the child, and Dr. Matthews Duncan had made out the following points:

The child's neck is the part that gives way first, and under a force of about 120 pounds. Before the whole neck gives way the spinal column snaps under a weight of about 105 pounds. One leg is enough, so far as mere strength of materials, to pull by, for pulling by one leg dissevers the child at the neck. The Prague seizure is not required, at least as an aid to the leg. As in the neck, so in the leg, the bones give way before the soft

parts. By the forceps a greater extractive force can be used than by podalic extraction, for the purely extractive force by the latter cannot be above about 120 pounds. Much greater force might be exerted by the accoucheur than 120 pounds, but no greater force than about that amount could be exerted in simply dragging the head through a contracted brim, the body being delivered, for if more were applied the neck gave way. As the spinal column yielded under about 105 pounds, the force applicable with a view to delivery of a living child was further restricted to within that amount. Many other important points were dwelt upon in the paper, with the object of introducing exactness, where hitherto there was great want of this desirable precision.

On Decollation as a Mode of Delivery in Shoulder Presentation. By Fred'k W. Wright, M.D., Derby.

After referring briefly to the opinions entertained by numerous writers on this subject, the author recorded his method of procedure, which consisted in passing a simple blunt hook, pierced with an eye carrying a piece of twine or tape, to which are attached half a dozen thin wires made of copper, over the fetal neck. The presenting fetal arm is, meanwhile, seized by the right hand of the accoucheur, and considerable traction made upon it until the left fingers are passed over the neck, when the blunt hook is inserted, and traction made so as to enable the operator to withdraw the twine. A leash of copper wire now attached to this, and traction made at the end of the twine, thus drawing it out of the vagina, while the wire will be drawn in and over the child's neck. By pulling the opposite end of this wire-saw rapidly backwards and forwards, with a saw-like motion, the head of the fetus may be severed in five seconds. The body was then extracted by the presenting arm; or, if this had been amputated to give room to operate, the body might be readily extracted by means of the crotchet hooked over the clavicle or over one of the ribs. If the decollation were made close to the shoulder, there will be less difficulty in delivering the head, as the whole length of the neck, with its firm groundwork of vertebra, thus left attached to the head, makes an excellent hold for appropriate instruments. The head is then fixed in the pelvis by external pressure, the neck if possible being made to present; this is then seized by a pair of strong forceps with horned teeth on the inner aspect of both blades, like craniotomy forceps, and traction made upon it till the head escapes externally. Should the neck give way under the efforts at traction, the head may be delivered by (*a*) the forceps, (*b*) the crotchet hooked into the lower jaw, or (*c*) the posterior fontanelle may be perforated and the crotchet introduced within the cranium, and traction made upon one of the cranial bones. The author thus briefly summed

up the comparative advantages of the two operations—evisceration and decollation. Evisceration is not generally completed under an hour; decollation is accomplished in a few minutes. In the former, anesthesia has to be kept up for an hour or more; in the latter, it is either not required at all, or not required for more than a few minutes. If the former, the soft parts of the mother are liable to be lacerated by removal of the bony covering of the viscera; in the latter the liability stands at zero. In evisceration, the operation is prolonged, bloody, repulsive, and abominable to the last degree; in decollation, the operation is speedy, is not bloody, and is not repulsive in the least degree. In the former, the instruments required are numerous, and complicated as well as dangerous; in the latter, the instruments are never more than three—viz., the harmless blunt hook, the simple wire saw, and the toothed forceps for delivery of the head. In the former, there is no reduction of bulk, delivering a fetus doubled in itself, even though deprived of its viscera; in the latter, the bulk is not increased. Perforation and evisceration are not always sufficient to bring relief to the mother; decollation never fails to bring instant relief and safety to the mother, and is unquestionably simpler, safer, and speedier than evisceration.

The Long Forceps, the Short Forceps and the Vectis. By Edgar G. Barnes, M.D., Eye.

The author recorded his experience in twenty-six cases. In twelve, he employed the long, in fourteen, the short forceps. All the mothers recovered; twenty-four of the children were born alive. He preferred the long to the short forceps, and thought the former, in the majority of instances, far superior to the vectis, which latter was only adopted to remedy malposition of the head, whereas the long forceps accomplished this, as well as remedied slight disproportions between the fetal head and pelvis, and augmented the deficient *vis a tergo* by supplying a *vis a fronte*. The object of his paper was to promote a discussion on the relative merits of the vectis and forceps.

Accidents that may happen to Pregnant Women suffering from Disease of the Heart. By Michel Peter, M.D., Paris.

Women suffering from an organic disease of the heart, who become pregnant, are exposed to accidents that may strike the lungs, impede gestation, and aggravate the state of the heart. The pulmonary accidents are the production of an extremely rapid double congestion of the lungs, with spitting of blood, and asphyxia, or capillary bronchitis, or lobular pneumonia, or double pleurisy. The accidents of gestation are miscarriage, with the death of the fetus. The cardiac accidents are the acceleration or aggravation of the general disorders attending organic diseases of the heart—namely, dyspnea, painful palpitation, visceral con-

gestion, anasarca. Pulmonary accidents and miscarriage happen ordinarily towards the middle of the gestation, and especially in the course, or towards the end, of the fifth month. These results were derived from eight cases, four observed by the author, and eight by Professor Sée, at La Charité, in Paris; M. Budin, house-physician at the Hospital St. Antoine, in Paris; and M. Senvre, house-physician at La Maternité, of Paris; each of those physicians having observed their cases after their attention had been called to these facts by the author. All these women but one were not at their first gestation; they had their attacks, one at her second pregnancy, three at the third, one at the sixth, one at the fifteenth; and one of them, after having miscarried at her third gestation, miscarried again at her fourth and fifth; another, who miscarried at her sixth gestation, had again two successive miscarriages; at length the one who miscarried at her fifteenth gestation had also two successive miscarriages. The meaning of these facts is, that the heart was a fatigued one, both by the increase of work caused by pregnancy, and the more remote date of the disease of the heart. The production of the pulmonary accidents is due to the augmentation in the total mass of the blood, which increases necessarily as increase the wants of the fetus. Thence the occurrence of these accidents, not at the beginning of the pregnancy, but after some months. The augmentation in the total mass of the blood, caused by pregnancy, produces physiologically plethora in the lungs, and, in some women, dyspnea, and even spitting of blood. The disease of the heart involves morbid congestion of the lungs, hence a pregnant woman with disease of the heart is doubly exposed to pulmonary accidents. The gestation produces an hypertrophy of the left ventricle, so that morbid regurgitation of blood in the lungs is increased by the state of heart. In seven cases the organic disease of the heart was an insufficiency of the mitral valve (complicated, in two cases, by a stricture of the orifice). In one case only the disease was a stricture of the aortic orifice. Two of the women died in consequence of their pulmonary accidents and miscarriages, and the six who recovered were afterwards rendered miserable by the severe and permanent aggravation in the disorders of the disease of their heart. The recovery was due to medication both energetic and rapid. The practical consequences of these facts are the following: 1. A woman diseased at the heart should not be a mother. 2. If she become pregnant, the physician must attentively survey her respiratory functions, and intervene energetically as soon as pulmonary disorders begin. 3. If the woman be safely delivered, she must not nurse her child, in order not to fatigue more her diseased heart. 4. Reciprocally, when a pregnant woman suffers towards the middle of her pregnancy very severe pulmonary disorders, or when she miscarries by them, the physician must consult her heart, and perhaps he will thus discover a disease of the heart till then unknown or mistaken.

Sir John R. Cormack remarked that renal congestion is a common cause of albuminuria, and of the various associated dangers of toxemia. This toxemia takes place from the pressure of the gravid uterus, when there is neither disease of heart or kidney, particularly in *Primiparæ*, in plural pregnancies, and in women of extreme muscular development, whose rigid fibres do not readily yield to the augmenting womb. Of course, with disease of the mitral valve, or structural disease of the kidney, the toxemic dangers incident to the pressure of the gravid uterus are much augmented. He thought that Dr. Peter's paper was of great practical importance, as was the whole subject of the dangers of pregnancy originating in the diseases or structural peculiarities of the individual.

The president thought the albuminuria of pregnancy was often of a reflex nature.

Dr. Peter, in his reply, said that, in the cases he had observed, there were no symptoms of toxemia.—*Obstetrical Journal.*

Treatment of Prolapse of the Umbilical Cord.

Dr. G. J. Engelmann observes that there are cases of prolapse in which it is not desirable to leave the progress of the labor wholly to the powers of nature—cases in which interference is necessary yet no indications for operation exist. Now the first and most simple assistance that can be rendered is to properly direct the patient's voluntary efforts; either, as the state of the case demands, keeping her quietly in one position, refraining from pressure with the abdominal muscles, or, when labor is far advanced, to encourage her to aid the passage of the head by the exertion of all her energies. 1. Postural treatment.—Equally simple, and on that account neglected probably in clinical teaching as well as in the text-books, is the treatment by position, which is a valuable aid to the practitioner. It consists in placing the patient on the side opposite to that on which the funis has prolapsed, so that the cord may be relieved from pressure, when it may perhaps glide back into the cavity of the womb. When the prolapse takes place in one or the other of the sacro-iliac fossæ, the patient should be placed on her hands and knees in the knee-elbow position. This position, however, is unfortunately very tiresome, and if too fatiguing, the patient must be placed in the corresponding side position, on the left side if the cord has prolapsed into the right sacro-iliac fossa. Dr. Engelmann has achieved good results by this method. Position alone, as Thomas some time ago remarked, will rarely if ever cause the return of the cord without the aid of manipulation, unless the bag of water is unbroken; and even then it may not. 2. Reposition of the cord.—The carrying back of the prolapsed loop into the cavity of the womb beyond the presenting part is a treat-

ment that has been given up as ineffective by some, whilst it is most warmly recommended by others. In Engelmann's cases reposition was accomplished in only seven of the eleven cases in which it was attempted; and though apparently successful in these seven cases, the cord not reappearing, only four of the children were saved. In the out-door department the results were but little better, reposition of the prolapsed loop having been practised in thirty-two cases, and, notwithstanding that the operation seemed to have succeeded in twenty-six of these, not more than sixteen children were saved—in fact, by reposition of the cord alone only thirteen, as delivery was hastened by operation in three other cases. The life of the child was saved in fifty per cent. of the cases in which reposition was apparently successful, and in forty per cent. of all the cases in which it was attempted; and as it was only resorted to in the more favorable cases, with well-pulsating cord and normal pelvis, the plan does not seem to afford much encouragement. Reposition is justifiable in many cases, but it has its strictly defined indications. With few exceptions, it must be confined to cases of prolapse with head presentations, as it is only with the rounded and resistant head that, when the loop has been carried back beyond its greatest circumference, the uterus can by its contraction prevent the immediate return of the prolapse. Not unfrequently a life is lost by too obstinate adherence to this method of treatment, the continued pressure and traction required proving fatal to the child; and in the same way, even when apparently successful, pressure at a higher point may have arrested the circulation in it. It should only be undertaken when the os is so far dilated that the escape of the waters is no longer to be feared, that, in case of necessity, delivery by forceps or turning can be immediately resorted to. The best instrument for the purpose of reposition is Robertson's funis replacer, and when apparently accomplished the fœtal heart must be closely observed, as it is by this means alone that it can be ascertained that it has been really effected; the fœtal pulse becoming strong and regular, continuing so after several pains. 3. Anæsthesia.—The use of chloroform was frequently resorted to, and proved a valuable adjuvant in achieving reposition of the cord. 4. Forceps.—The forceps were resorted to about as often as the reposition of the cord. In fifteen of the thirty cases in which it was applied the child was saved. 5. Extraction by the feet.—Extraction by the feet, simply not following version, was practised in sixty-five cases, in forty-seven of which (72·3 per cent.) a living child was developed. The success naturally depends upon the favorable prognosis offered by breech-first labors, in which alone it can be resorted to, and the treatment is mainly a postural one. Extraction by the feet was practised in fourteen of the lying-in house cases, and in only one was the child delivered dead, putrid—a case which should justly be excluded. The results were less favorable in the out door cases, the accoucheur not unfrequently being called in too late. The

patient should be so placed that a return of the presenting loop may be facilitated, all muscular strain must be avoided, the membranes must, if at all possible, be preserved intact until the os is sufficiently dilated, and when this is the case, the parts being yielding, we must not wait for threatened signs on the part of the fœtal pulse, but at once deliver by version. The operation which was most frequently resorted to and which proved, comparatively speaking, most successful, was turning by the feet, immediately followed by extraction. Of the 125 cases so operated on, seventy-two were favorable, 57·6 per cent. of the children were saved; and this result holds good not only for transverse and shoulder presentations, but also for head presentations. 7. Cephalotrify.—Craniotomy can certainly not be classed among the operations called for by prolapse of the funis, yet Engelmann makes mention of this operation, as it was so often necessitated for the preservation of the mother, and as the large number of these operations, twenty-five amongst 365 deliveries, complicated with prolapse of the cord, most forcibly proves the frequency of the highly contracted and the distorted pelvis as cause of the prolapse.—*American Journal of Obstetrics*, August 1874.

Effect of Warmth in Preventing Death from Chloral.

Dr. Brunton, in experiments with chloral, confirms the observations of Liebreich and others, and finds that the subcutaneous injection of a solution of chloral induces sleep, which is light and easily broken if the dose be small, but passes into coma if the dose be large. In dogs, considerable restlessness was observed before sleep came on, and the respiration was at first rendered rapid, but subsequently became slow. A remarkable diminution of temperature was observed, which appears to be partly due to greater loss from the surface, caused by the vessels of the skin becoming much dilated under the influence of the drug, and allowing the blood to be cooled more readily by a low external temperature. It is partly due also to the diminished production of heat which cessation of muscular action always induces. Dr. Brunton found that an animal wrapped in cotton-wool may recover perfectly from a dose of chloral which is sufficient to kill it when exposed to the cooling action of the air, and that recovery from the narcotic action is much quicker when the temperature is maintained in this way, and still more rapid when the animal is placed in a warm bath, providing this is not excessive. The bearing of these experiments on the treatment of persons suffering from an overdose of chloral is obvious. The patient should be put to bed, and the temperature of the body maintained by warm blankets and hot-water bottles to various parts of the body, and especially to the cardiac region. Warmth over the heart is an excellent stimulant to the circulation, which, like the

respiration, is enfeebled by chloral. If respiration threatens to fail, it should be maintained artificially so as to allow time for the chloral to be excreted and the normal functions to be restored.—*Humphrey and Turner's Journal of Anatomy*, No. 14, May 1874.

New Researches on Podophyllin.

In a note on the results obtained in the clinique of M. Demarquay from the use of podophyllin, M. Gerard Marchant remarks that the recent memoir of Dr. Paul on the treatment of habitual constipation by this remedy has anew called the attention of practitioners to its value. Podophyllin is the resin obtained from the root of the *podophyllum peltatum*, a wild plant of North America, which is sometimes called the Carolina Ipecacuanha. It belongs to the class Berberidaceæ. Numerous experiments undertaken by M. Delpech demonstrate that podophyllin is composed of two resins, one of which is soluble in chloroform, ether, and alcohol, while the other is insoluble. It may be administered in the form of a pill combined with ginger or extract of hyoscyamus. Of forty patients to whom it was administered, three only were refractory to its action. One of these cases suffered from cancer of the uterus, a second from fibrous tumor of the uterus, and a third from stricture of the rectum. The effects of the administration of podophyllin are felt in about twelve hours. Its action is not accompanied by pain like that of aloes, nor by nausea like that of jalap, but by tickling sensations, gurgling, &c. The motions are semi-fluid, usually moulded, normal in color, and contain a considerable quantity of bile. Podophyllin may be employed for a long time without producing any secretory disturbance of the digestive tube, providing it is not given in too large doses. In the cases under M. Demarquay, which were chiefly women, the dose was never more than a grain, and no ill effects ever followed; but the dose of half a grain is preferable.—*Bulletin Générale de Thérapeutique*, Aug. 30, 1874.

Philadelphia Hospital, Philadelphia—Reports of Practice and Peculiarities in Treatment—The Treatment of Chronic Bright's Disease.

The endeavor in treatment is to restore the kidney to the normal condition. The condition of the blood is therefore of the first importance. The remedies regarded as pre-eminently serviceable for the general constitutional treatment of this disease, are iron and quinine. Basham's mixture, or liq. ferri per acetatis, uniformly is used, and Dr. Tyson is of the opinion that the addition of strychnia in 1-24 gr. doses adds very much to its

efficacy. The mixture is administered in tablespoonful doses three or four times a day. Ten grains of quinine are given daily in divided doses.

Milk diet is regarded as specially useful, and a due proportion of meat may not be improper.

In the treatment of the dropsy incident to this affection, it is thought that digitalis will not help so much as in the dropsy developed in the acute form of the disease.

The salines are regarded as better for overcoming the dropsy. Digitalis is not to be discarded, but such remedies as bitartrate of potassa or acetate of potassa are decidedly preferable. When dropsy is present the diuretic treatment, which is chiefly relied upon, is combined with the general constitutional treatment. The idea is to improve the condition of the blood, and hope through this improvement to affect the dropsy. Hence, diuretics form no part of the general plan of treatment, except the diuretic effects obtained by administering the ferruginous mixture referred to. The formula for Basham's mixture can be found in connection with the report from the Pennsylvania Hospital.

ACUTE BRIGHT'S DISEASE.

Dry cupping over the loins and digitalis. The digitalis increases the blood pressure, causing more water to pass through the kidneys, so as to flood them. If symptoms of incipient uremia are present, or perhaps the patient has had one or more convulsions before admission, hydragogue cathartics, diuretics, and dry cupping to the loins, are the chief measures for the relief of the convulsions or symptoms of convulsions. Hot-air baths are occasionally employed. Acetate of potass, in thirty-grain doses, is a diuretic very commonly employed.

Hypodermic injection of morphine, as recommended by Prof. Loomis, of New York, has been resorted to in one case for the purpose of controlling such convulsions. In that single case the convulsions were completely controlled. Two cases of acute Bright's disease had been treated with ergot. The symptoms were at once relieved, and the treatment regarded with favor.

CHRONIC CYSTITIS.

A case of this kind was doing very well under the following treatment. Injections three times a day of a tablespoonful of the following mixture to a pint of water:

℞. Borax - - - - - ℥i,
Glycerine - - - - - ℥vi.
M.

The patient was also drinking a simple infusion of triticum repens and buchu.

CHRONIC ORCHITIS.

When the patient was admitted the testicle was very much enlarged, and exceedingly hard.

A local application of hydrochlorate of ammonia of the strength of an ounce and a half to the pint of water had been kept continually applied, and the testicle at the end of a week or ten days was quite soft and natural to the touch. It was next ordered to be strapped.

CHRONIC ULCERS.

In general these cases are treated by strapping with adhesive plaster. Three or four cases of large ulcers upon the legs, having heavy indurated edges, were doing very nicely under the local influence of tr. iodine applied to the indurated parts. The ordinary tincture is painted freely over the induration, and then covered with adhesive straps in the usual manner. Dr. Housekeeper instituted the plan of treatment and had obtained results eminently satisfactory.

BED SORE.

One was healing kindly under the influence of the compound resin ointment.

GONORRHŒA.

In both acute and chronic gonorrhœa, injections of sulphate of zinc are employed, varying in strength from one grain and upwards to the ounce of water; and in acute cases the injections are used from the beginning.

Internally, oil of sandal wood is the favorite remedy, administered in doses of gtt. xv. t. i. d.

SOFT CHANCRE.

Caustics first. Acid nitrate of mercury is the favorite caustic, and a very favorite dressing to follow the cauterization is aromatic wine applied upon scraped lint, being careful to dress each sore separately.

INDURATED BUBO.

Cover the part freely with mercurial ointment, and keep up constant pressure by means of a *hot brick*. Better results were claimed for this method of treatment than any other that had been adopted.

PHAGEDENIC CHANCRE.

One of these was seen, which had been met with the more

than equally energetic treatment of having all the dead tissues removed by the knife, and the operation followed by cauterization with acid nitrate of mercury. The phagedenic process was at once arrested, and the sore now presented a healthy appearance—the slough having been removed—under the influence of aromatic wine dressing.

SUPPURATING BUBO.

Free incision, and pack the cavity with lint wet in a lotion composed of one drop of sat. sol. of carbolic acid and six drops of sweet oil.

SYPHILITIC LICHEN.

The case was exceedingly well marked, and was at the 43d day from the exposure.

The treatment which the patient was under was as follows:

Good food and porter; vapor bath not lasting more than ten minutes, once or twice a day; and the following drugs:

℞. Potassio tartrate of iron - - - ʒvi,
Aquæ - - - - - - - - ℥viii.
M.

Teaspoonful of this mixture with from 1-8 to 1-16 of a grain of bichloride of mercury, t. i. d.

The mercury is combined, not for the purpose of producing its specific effect, but simply for its tonic and alterative influence. Salivation is not desirable.

In connection with the above, the patient was also receiving Fowler's solution, gtt. xxv. t. i. d., administered in milk and after meals.

The arsenic was commenced in doses of five or six drops, and increased to ten as soon as possible, and from that the remedy was to be carried on until *some nausea* was produced. The twenty-five drops were being taken without inconvenience.

The idea entertained was that arsenic is not specially serviceable in the treatment of this class of affections unless carried up to the point of producing *some nausea*. Prognosis guarded with regard to perfect cure in this case.

ANTE-SCROTAL FISTULÆ.

Introduce a sound into the urethra; determine the track which the fistulæ take to reach the urethra, by introducing a small probe. When this is determined, melt a small quantity of nitrate of silver in a test-tube over a spirit lamp, and coat the end of a probe with the silver by simply immersing it in the molten mass. When sufficiently coated, introduce it into the fistulæ, and repeat every third day. On the days between, throw into the fistulæ

by means of a hypodermic syringe a concentrated tincture of cantharides.

The concentrated tincture can be obtained by evaporating the ordinary tincture to one-third or one-fourth its natural bulk.

TONIC PRESCRIPTION.

The following prescription is regarded specially beneficial as a general tonic in syphilitic patients, and has been found to act exceedingly well in those cases in which the functions of the stomach have been almost destroyed by the use of alcoholics.

℞. Quinia sulph. - - - grs. xxx,
Tr. ferri chlorid. - - - ʒiii,
Strychnia sulph. - - - gr. ¼,
Syr. zingziberis et,
Aquæ ad - - - - ℥iii.

Teaspoonful every four hours.

PHAGEDENA.

The case was one of sloughing upon the foot, dependent upon syphilis. Bristow's solution of bromine was employed in this case to destroy the phagedena, and the applications followed by yeast and charcoal poultice. The solution is made as follows:

℞. Bromine - - - - - ʒi,
Bromide of potassium - - - ʒii,
Aquæ - - - - - - ℥ii.
M.

It is claimed for this solution that the appetite is almost invariably improved within twelve or twenty-four hours after its application to the affected part. This was said to be the peculiar effect produced by the application.

REMOVAL OF BRACHIAL PLEXUS.

The patient was a man 71 years of age, who had a painful neuroma of the skin for ten years. Dr. Maury had dissected and removed the entire brachial plexus of nerves, and the operation was attended with complete recovery so far as pain was concerned. Motion and sensation were lost. A more perfect report of the case can be found in the *American Journal of Medical Science* for Oct. 1873.

SPRAINS.

Foot bath, hot as can be borne, for half an hour, followed immediately by free rubbing with liniment composed of soap liniment with a small quantity of aqua ammonia and oil of rosemary, and then a snugly applied bandage.

ACUTE SYNOVITIS.

Treatment, general antiphlogistic, associated with local counter-irritation in the form of tr. iodine or blisters.

CHRONIC SYNOVITIS.

The aspirator has been used somewhat frequently, and with very good results.

ESMARCH'S APPARATUS.

The tourniquet is preferred to the rubber tubing, for the reason that all danger of the tubing getting loose is avoided, and the tourniquet can be easily loosened to facilitate finding the arteries for purposes of ligation.

FRACTURE OF THE FOREARM AND COLLES FRACTURE.

For the treatment of these fractures Bond's splint and Nélaton's pistol-shaped splint are the ones chiefly employed.

FRACTURE OF THIGH.

The wire-gauze splint of Dr. Nathan R. Smith is usually used.

FRACTURE OF THE LEG.

The book-back splint is the favorite.

The idea is to nearly enclose the limb in some unyielding material, and yet leave the posterior portion sufficiently flexible to permit of easy removal. This can be easily accomplished by reinforcing a light plaster dressing with lateral strips of cloth, pasteboard, leaving the back of the splint more or less flexible.

"Permanent," or "immobile," or "mobile immobile" dressings are but little employed in the treatment of fractures in this hospital.

ACUTE ARTICULAR RHEUMATISM.

The full alkaline treatment is commonly adopted in the treatment of this affection. Dry cotton is the most commonly used local application. Hop poultices are occasionally employed.

If the temperature becomes *high*, demanding active measures for its reduction, the patient is placed in a bath of the temperature of 60 F., and permitted to remain there until the temperature of the body has fallen within reasonable limits. Dover's powder is given to relieve pain, and, as soon as the acute stage has passed, quinine gr. xv. per diem.

PNEUMONIA.

In the main, the treatment of this disease is expectant. Rarely is local blood-letting resorted to. It is occasionally employed

for the purpose of relieving local pain. Sometimes digitalis is used; sometimes veratrum viride; but these remedies are administered only in a small proportion of cases. Quinine, ten grains a day, is quite commonly administered with a nourishing liquid diet. What is called the "jacket-poultice" is, as a rule, applied. The jacket-poultice is simply a linseed meal poultice sufficiently large to cover the whole chest, and usually has a little powdered mustard sprinkled over the surface, or mixed with it. Most of the patients recover. The application of cold to the chest has never been adopted. Quinine in *large doses* has been used for the purpose of reducing high temperature, but it does not enter into the plan ordinarily adopted in the treatment of the disease. Carbonate of ammonia is used when an active stimulant expectorant is desired, or its general stimulating effect.

SUB-ACUTE PLEURISY.

Diuretics (iodide of potassium, acetate of potash); tonics and good food; and moderate counter-irritation. Paracentesis thoracis is performed rather late in the progress of the case, remedies having first been faithfully tried. To relieve the pain of the more acute form of pleurisy, and also of this form, the local application of *cold* has been resorted to with good success.

DELIRIUM TREMENS.

Bromide of potassium and hydrate of chloral are the remedies commonly employed. The potassium is administered in doses varying from 20 to 60 grains, and the chloral may be given perhaps twice during one night in doses varying from 15 to 20 grains.

In one case chloral, bromide of potassium, and morphine had failed to produce sleep, but whiskey, in half ounce doses every half hour, accomplished the desired effect. When nervous symptoms dependent upon alcohol are present, a dose of whiskey is ordinarily administered before resorting to any medical treatment whatever. The hospital staff are about equally divided upon the question of bromide of potassium being able to produce sleep.

ACUTE PERITONITIS.

Opium is the chief remedy employed.

NIGHT SWEATS OF PHTHISIS.

Sulphate of atropia is regarded as an excellent remedy for their arrest. It is usually administered in a simple dose at bedtime of 1-60 of a grain.

Aromatic sulphuric acid.

Niemeyer's pill. Niemeyer's pill and atropia used in conjunction. Oxide of zinc and hyoscyamus were said to be efficient with their cases.

The pill called Niemeyer's, or the one he ordinarily prescribed, properly known by the name of Heim's pills, is in general prescription. The formula for the pill is as follows:

℞. Pulv. herb. digitalis - - - ℈ss,
Pulv. rad. ipecac,
Pulv: opii puri, aa - - - gr. v,
Ext. hellessii, q. s, u. f. pl. - No. xx,
Comp. pulv. rad. irid. flor.

S. A pill three times daily. The addition of a scruple of quinine to the above prescription constitutes Niemeyer's modification, and is added for its antipyretic effect. This pill of Niemeyer's is one very commonly prescribed in this hospital, in cases of phthisis, and is regarded as a very valuable combination of remedies.

HÆMOPTYSIS.

Hypodermic injections of ergotine were mentioned with special favor. The solution used is of such strength that ♏xv. contain one grain of the ergotine.

Inhalations of the oil turpentine are frequently employed.

As good success has attended the use of the above remedies as any which have been employed.

DIARRHŒA OF PHTHISIS.

Among the many remedies and combinations of remedies that have been used, the following prescriptions have obtained some reputation for good:

℞. Opium - - - - ½ to 1 grain,
Bismuth subnit. - - - gr. xv,
Ipecac - - - . ½ grain.
M.
Taken p. r. n.

℞. Nitrate of silver - - ¼ gr. to ½ gr.,
Opium- - - - - ½ gr. to 1 gr.,
M.
Ft. pil.
To be taken p. r. n.

℞. Strychnia sulph. - - 1-24 gr.
Bismuth subnit. - - - 15 to 20 gr.
M.
Taken p. r. n.

TYPHOID FEVER.

Treatment, in the main, expectant. Some of the visiting physicians prefer to have administered about 18 grains of quinine daily for the purpose of preventing tissue waste. The acid treatment is also regarded with favor; that is, the administration of

some one of the mineral acids in moderate doses from the beginning.

Glycerine and water has been employed for the purpose of reducing the temperature when it becomes moderately high, and it was thought with more satisfactory results than sponging with simple water. The proportions used are—

℞. Glycerine - - - - - - - - ʒiv,
Water - - - - - - - - Oi.

When *hemorrhage from the bowels* occurs, the chief reliance is upon cold applications and the use of suppositories containing various astringent remedies, but especially opium. All necessary measures also must be adopted to sustain the patient until the crisis has passed.

ERYSIPELAS.

The case was an ordinary one, in which the disease affected the head and face.

Tr. iron and quinine were being used as internal remedies.

These remedies are uniformly employed in these cases.

For external applications the lead-water with tr. opium is a favorite, although not especially new.

Oxide of zinc ointment is sometimes used. Hyposulphite of soda, combined with some mucilaginous mixture, is a favorite application with some of the visiting physicians.

SCARLET FEVER.

Treatment of the fever is mainly expectant. Stimulants are of benefit in the class of cases seen in this institution.

For the dropsy which sometimes appears, diuretics are used, and if the children have some age, purgatives are administered. When throat symptoms are prominent, a very common prescription is a saturated solution of chlorate of potash in tr. of iron and well sweetened with honey. The dose is to be regulated according to the size of the dose of iron proper for the age of the child. The rates of mortality are low.

DIPHTHERIA.

Constitutional treatment by means of quinine and iron.

Local application, most commonly of dilute muriatic acid, sometimes of tincture of iron. The resident physician had but little faith in the power of lime-water to dissolve the membrane, for the reason that he had kept for a long time a specimen of membrane that had been preserved in lime-water.

Alcoholics are not administered until the system needs them. It is believed that this is the better plan, for the reason that, if commenced early, they are liable to produce more or less nausea, perhaps vomiting, which may prevent their use at the time when

they are most needed. Local applications are made to the throat if the throat is very much inflamed.

Treatment with calomel and bicarbonate of soda had just been adopted in a couple of cases, but the results were undetermined.

WHOOPING-COUGH.

Hydrate of chloral is the common remedy, and gives very satisfactory results. Sometimes belladonna is combined with it.

The remedy is pushed as much as possible; commencing, in a child from one to two years old, with one to two or three grains t. i. d.

Sugar had been found in the urine of all these patients.

The co. tr. benzoin is sometimes added to the chloral to stimulate the kidneys to increased action, when the chloral has rendered them somewhat inactive.

FACIAL ERYSIPELAS IN PREGNANT WOMEN.

Erysipelas was developed in three pregnant women a few days before confinement. Their labors were easy and they recovered without a single bad symptom. The erysipelas passed away with the most favorable termination. Every other woman, however, who was in the same ward was affected by something which caused the temperature to rise upon an average to 103° the day after confinement. Many other symptoms of equally unfavorable import were developed in connection with the rise in temperature. After the second case opium and quinine were administered for a few days previous to confinement in anticipation of serious puerperal trouble. There were no deaths.

PUERPERAL FEVER.

Opium in sufficient quantities to calm all local irritation; quinine in tonic doses; sometimes in large doses, grs. xxx. a day, to reduce the temperature; veratrum to control the pulse; fair amount of liquid nourishing diet; and alcoholics if necessary. If veratrum produces vomiting, aconite is substituted for it. Care must be exercised in the administration of aconite lest its depressing effects be too markedly produced. It is much easier to bring up from the depression of veratrum than from the depression of aconite. Turpentine stupes are applied to the abdomen if tympanites is present.

One case, probably puerperal septicæmia, was treated by giving nitro muriatic acid dilute in doses of twenty drops every two hours; lead and opium lotion to the abdomen; opium simply to quiet; quinine grs. xxx. daily for three days. The bowels moved several times each day, and the discharges were exceedingly offensive. No measures were taken to arrest the discharges, and they did not produce exhaustion. The patient recovered.

QUININE AND MORPHINE.

It had been noticed in several cases that, when one-quarter of a grain of morphine would not produce sleep, if ten grains quinine were administered a short time previous to administering the morphine, the morphine would almost invariably act efficiently. This fact was noticed in connection with puerperal cases.

MAMMARY ABSCESS.

Quinine in full doses as soon as chill occurs. Cease nursing at once, and remove the milk by hand rubbing, covering the parts with warm lard, and rubbing from the base of the gland towards the nipple.

QUININE OF GLYCYRRHIZA.

To disguise the taste of quinine administer it in fld. ext. of glycyrrhiza. If fld. ext. is not at hand, the powder can be used with nearly the same result.

INJECTION OF PULMONARY CAVITIES.

In two cases in which there were well marked signs of cavities in the lungs, from 15 to 20 drops of a 15 per cent. sol. in water of Trousel's solution were thrown into these cavities by means of a hypodermic syringe plunged directly through the walls of the chest. This was done with the result of producing an improvement in the general symptoms, and very much less expectoration. The measure was suggested for the relief of hemorrhages. —*The Medical Record.*

Ante-Uterine Sub-Peritoneal Hæmatocele. By Henry O. Marcy, Cambridge, Mass.

Mrs. B., aged thirty years, a strong, well-developed woman, was delivered of her first child on the 18th of May, after a somewhat prolonged but not especially severe labor. The only complication was a rigid condition of the perinæum, which resulted in a partial laceration. This was immediately brought together with silver-wire sutures. Complete union resulted, and the stitches were removed on the tenth day. Lactation supervened normally, and there were no marked uterine or abdominal pains. More than usual restriction of motion was observed, because of the ruptured perinæum. Fifteen days after delivery (June 3d), she was suddenly seized with a sharp pain just to the right of the median line above the pubes, of a most excruciating character, followed by partial faintness. No unusual exertion had preceded the suffering, except slight straining at stool. I saw the patient soon after seizure, when, at the point of pain, there was considerable tenderness, but no especially abdominal

fullness was then noticed. The patient was anxious and depressed, the pulse small and rapid. A full opiate, hot fomentations, absolute quiet, etc. Some hours later Mrs. B. was quite pallid and faint, pulse very small and weak, extremities cold, perceptions clear, and complained of considerable pain in abdomen, with feeling of weight and fullness.

An ovoid-shaped, fluctuating tumor, nearly or quite on the median line, distended the abdomen to a point somewhat above the umbilicus, the patient being nearly as large as before delivery.

Was it an over-distended bladder? A male catheter was introduced with difficulty, and only by carrying the curve directly backward. Urine had frequently passed during the day in small quantities, and the bladder scarcely contained an ounce. Was it the uterus? Anteriorly the floor of the vagina had a soft, fluctuating feel, evidently transferred from the superincumbent mass. The uterus could be felt in nearly its whole outline, retroverted, and lying in Douglas's fossa. The patient was altogether too seriously ill to allow of a more extended examination at this time. She slowly rallied from her condition of extreme exhaustion; there was only a very slight elevation of temperature, and no inflammatory symptoms supervened.

June 5th.—Dr. W. W. Wellington and myself made a more thorough examination. The patient was extremely weak, and complained much of local heaviness and weight. The uterus was retroverted, well contracted, and measured scarcely more than three inches in depth.

The tumor retained the same regular outline as at first, except being perhaps a little flattened in its antero-posterior diameter, and there was very little change in position by turning from side to side. Fluctuation was undoubted, but less distinct than upon the first day. The patient bore every appearance of excessive loss of blood, and our diagnosis was extra-peritoneal perimetric hæmatocele.

At the end of a week the tumor seemed something firmer, probably due to more or less complete coagulation.

30th.—Patient is considerably improved in general condition, but still confined to bed. No inflammatory symptoms have at any time appeared.

The tumor has not much decreased in size, the patient measuring at umbilicus thirty-eight inches in circumference, and, four inches below, forty and one-fourth inches. Fluctuation clearly defined.

So much time having elapsed, and no absorption taken place, the contents softened. I thought it wise to give the patient the benefit of operative interference, deeming a rupture of the sac imminent as well as dangerous from the possibilities of septic poisoning, etc. It seemed hopeless that this immense mass should be removed except by spontaneous or artificial discharge. Much to my disappointment Dr. Wellington counselled

delay, owing to the improvement in the general condition of the patient.

July 9th.—The circumference was thirty-eight inches.

August 3d.—Thirty-six inches. Mrs. B. continued to slowly improve, could walk with some difficulty; complained of soreness and tenderness. Spent August and September in the country. During the autumn the improvement was more rapid, and convalescence was fairly reëstablished in early winter. April following I find the following notes:

General health good; menses regular and of normal character. No leucorrhœa. Bowels regular. Has at times sharp pelvic pains on walking. Uterus is movable, slightly retroverted, deflected to the right, and two and a half inches deep. About an inch to the right of the median line, and anterior to and loosely connected with the uterus, lies a hard, flattened mass, about the size of a small orange. Some months later the patient remained much the same, except that this hard mass had diminished in size, perhaps not larger than an English walnut.

Extra or sub-peritoneal hæmatocele is certainly rare. Its occurrence has even been denied by Aran, Voisin, and others. Bernutz contends that the extra-peritoneal effusions are thrombi, and only result from labor. Aran bases his objections upon a clinical distinction, and affirms that there are no sub-peritoneal perimetric blood-tumors at all important in size, so as to become worthy of consideration with intra-peritoneal tumors. They cannot become large, because they are limited within the fibro-cellular layer, covered in by the peritonæum.

Nonat, Becquerel, Huguier, and Robert, all maintain that extra-peritoneal hæmatocele may occur. Prof. Simpson, in his work on "Diseases of Women," gives notes of a *post mortem* examination with a diagram. He say: "On dissection, I found the reflexion of the peritonæum between the uterus and rectum raised up, and a large mass of broken coagula of blood formed the tumor, having been extravasated behind the peritonæum, forming the posterior covering of the broad ligaments, and, as it accumulated, having separated and pushed before it that portion of peritonæum, and the utero-rectal fold of this membrane.

Prast cites two cases, in one of which the blood was effused between the layers of the broad ligaments, and in the other it dissected up the connective tissue behind the uterus.

Dr. Barnes quotes a case from Becquerel in which more than two pounds of blood were found outside the peritonæum, the blood having pushed its way between the different organs and displaced them all.

Ante-uterine extra-peritoneal hæmatocele is yet more rare. Dr. Thomas ("Diseases of Women," page 477) says: "Nonat dogmatically announces that the uterus is never found between the tumor and rectum, that is to say, behind the mass of blood; but Chassaignac reports a case in which the sanguineous collection existed entirely between the bladder and uterus, and conse-

quently must have forced the organ backward." Dr. Barnes reports two cases in which the tumor was the size of a small orange, throwing the fundus uteri backward. They subsequently entirely disappeared.

In *résumé* of our case above reported, we submit that the general symptoms—sudden appearance, pallor, prostration, and slow absorption—prove that this enormous tumor could have been no other than that of blood-character, while its defined outline, encysted from the first, non-development of peritoneal inflammation, so uniformly present in intra-peritoneal effusion, absence of fluid in the retro-uterine space, and subsequent history, are in evidence of its being of extra-uterine character; while the depressed bladder and retroverted uterus, with the position of the resulting cicatricial tissue, mark its point of origin as ante-uterine. Its large dimensions were rendered possible by the relaxed conditions of the connective tissue incident to the recent delivery of the patient.—*New York Medical Journal.*

Belladonna in Spasmodic Asthma.

Dr. George G. Wood, in the *Philadelphia Medical Times* of September 19th, gives the result of his experience with large doses of belladonna in the treatment of asthma. He says:

"I usually employ the tincture of the United States Pharmacopœia, in doses ranging from twenty to sixty drops. The strength of the tincture differs so much, as commonly kept in shops, that the size of the dose must be lost sight of, and the quantity given be regulated by the effect produced. It may be given during the paroxysm with great advantage, but it acts best when given before the attack commences. For example, if the patient has nocturnal attacks coming on after midnight, as is usual, give him a dose just before going to bed, and repeat it if necessary to produce sound sleep. He fails to awake at the usual time for the attack to commence, and sleeps on, awakening in the morning very much refreshed and strengthened. This treatment may be repeated night after night, until sufficient time has been had to remove the tendencies of the disease to return, either by changing his location or adopting other requisite treatment, as the case may call for. I could relate several cases to prove the above statements, but will have to omit them for want of space.

"Sometimes, but not often, belladonna produces dryness of the fauces, and delirium. These are indications which show that it should be discontinued, and hydrate of chloral should be employed in its stead. It may be used on the same principles as belladonna to produce sleep and thus ward off attacks. For the past two years I have been treating spasmodic asthma on these

principles, and with most satisfactory results."—*New York Medical Journal.*

The Action of Veratrum Viride.

Dr. H. C. Wood, Jr., assisted by Dr. Joseph Berens, publishes in the *Philadelphia Medical Times* (August 22d and following numbers) a series of elaborate investigations into the action of veratrum viride upon the circulation. The following results have been arrived at regarding veratroidia:

"The action of this alkaloid upon the circulation is altogether subordinate to its influence upon the respiration.

"In minute doses it stimulates the cardiac inhibitory nerves or nerve-centres, but when given in sufficiently large doses it finally paralyzes the peripheral inhibitory cardiac nerves.

"It exerts some action upon the heart-muscle or the contained ganglia; this action is probably a sedative one, but it is very feeble, and is only distinctly perceptible when the drug is precipitated at once upon the heart, or when the dose given is much above that required to arrest respiration. To kill the heart-muscle very large amounts are required.

"Upon the vaso-motor system veratroidia acts as depressant, but its influence is feeble, much less intense than its action upon the pneumogastrics. When artificial respiration is maintained, it can be given in such doses as to paralyze the vaso-motor centres."—*New York Medical Journal.*

Byrd on Cholera Infantum.

Dr. Harvey L. Byrd writes as follows in the *Philadelphia Medical Times.* This terrible scourge of infancy and childhood is carrying large numbers of the young and tender ones of this community to their long homes, and such is the extent of its ravages that it might be said with propriety of language to prevail at this time as an epidemic in our midst.

Baltimore, hygienically considered, is probably equal in all, or at least very many, respects to her most favored sister-cities; but, while this is the case, the hand of the destroyer occasionally falls heavily upon her, and she is then called upon to mourn the loss of those she cannot rescue from the embrace of death. Since the advent of summer the mortality has been considerable among infants and children one to three and four years old, but it is chiefly within the last three weeks that our mortuary tables exhibit a fearfully large proportion of death from cholera infantum. Within this period there has been not only a steady but an alarming increase in the death statistics from this generally intractable and fatal malady. After resorting to the remedies

most in vogue in the treatment of summer-complaint, such as calomel in minute and moderately large doses, alone and in combination with Dover's powder, chalk, charcoal, etc., bismuth, magnesia, pepsin, tannic and gallic acids, acetate of lead, alum, nitrate of silver, creosote, pyroligneous acid, laudanum, etc., alone and in various combinations and mixtures with indifferent and unsatisfactory results, even when strict attention was given to diet, fresh air, bathing, stimulants when called for, etc., it was finally decided to adopt a plan of treatment with special reference to an alterative action on the blood; at the same time giving strict attention to the skin with a view to the elimination of the poison, as far as might be, by this organ.

Accordingly, with the leading object in view, namely, an appeal to the blood primarily, sulphite of sodium and aromatic sulphuric acid were prescribed internally, and tepid or cold alkaline baths, according to indications, ordered externally; to which whiskey or brandy was added when required.

One grain of the sulphite, with four drops of paregoric, was given in gum-water every two hours, to a child one year old, and the dose doubled for a child two years old, increasing or lessening it according to age and the anodyne effects of the paregoric, thus:

℞.	Sulphite of sodium - - -	grs. xvj,
	Powdered acacias - - - -	grs. xij,
	Camphorated tincture of opium -	fl℥j,
	Water - . - -	℥ij.—M.

Sig.—One teaspoonful every two hours, to a child one year old, shaking the phial before using. One drop of elixir of vitriol in three spoonfuls of iced water was given, three times a day, to a child one year old; and the dose was increased one drop for each year, and lessened to one-fourth or one-half drop when below one year of age. A tepid or cold bath, rendered alkaline with an ounce or more of carbonate of sodium, or potassium, or common salt, was used morning and night. In addition to the foregoing remedies, aromatic cataplasms were ordered, and kept applied over the entire stomach and abdomen. Cow's milk and farinaceous articles of food were not allowed, and scraped or finely chopped beef, or lamb, raw or but partially cooked, or essence of beef (to which a small portion of brandy was added when required by the feebleness or prostration of the patient), was used as much as practicable as nourishment. Wine whey was allowed freely in the second stage of the disease, when it agreed with the patient. Infants were allowed the mother's milk, or that of a healthy wet-nurse, and fifteen to twenty drops of lime-water three or four times a day when the milk disagreed. This plan of treatment has been pursued for the past two weeks, with complete success. In a small proportion of cases, quinine, in appropriate doses, was also administered when a tendency to periodicity was observed.—*London Medical Record.*

Digitalis in Puerperal Cases.

Dr. Winkel, who has made various experiments with the different preparations of digitalis, does not hold it to be a specific in puerperal fever, but administers it as a prophylactic against the phlegmonous process. When disturbance of the digestion results, he discontinues its use. Dr. Winkel found the hypodermic use of digitalin, dissolved in equal parts of alcohol and water, in the proportion of .001 gramme of digitalin in 3 grammes of fluid, to be the best method of administration. The mean requisite dose is .005 grammes. The advantage of this preparation is found in the smallness of the dose and the ease with which its action may be regulated. The effect follows in one to three days generally, a simultaneous sinking of the temperature and pulse being observable. Elimination cannot be absolutely stated to take place through any organ. Digitalin appears to act upon the brain—producing rest, better sleep, and enlargement of the pupil. It appears also to act as a styptic, recommending itself in this respect after severe hæmorrhage. The question as to the tolerance by puerperal women of large doses cannot as yet be answered, the assertion of its only partial absorption through the altered condition of the intestine being unsupported by proof. The favorable action of digitalis is attributed by Dr. Winkel to its effect upon the circulation; for, as in the chills of the puerperal condition the prognosis is more favorable when the pulse is not over 100, it is evident that a remedy which will prevent such increase must be beneficial. The observation of after-pains following the continued use of digitalis, together with its styptic action, has given rise to the conjecture that its effect is upon the unstriped muscular fibres of the uterus. By its contraction of the capillaries it prevents exudation of the colorless blood-corpuscles.—*New Remedies*, July, 1874.

Heiberg on a New Method of Treating Dangerous Symptoms presented during the Administration of Chloroform.

Dr. Jacob Heiberg, of Christiana (*Berliner Klinische Wochenschrift*, no. 36, 1874), describes certain manipulations for the relief and removal of disquieting symptoms often presented during the administration of chloroform. These consist especially in noisy and retarded respiration, congestion or pallor of the face, and a small pulse. This condition, frequently met with in hospital practice, is usually treated by forcible separation of the jaws, and by dragging the tongue forwards with large forceps or sharp hooks. To these proceedings the author raises the objections that they interfere with the course of the operation and distract the attention of the surgeon; that the tongue is often wounded by broken or sharp teeth; and thirdly, that the quan-

tity of chloroform used is often necessarily increased, and the subsequent ill effects of the narcosis rendered more intense. The manipulative proceeding, recommended by the author, consists in depressing the lower jaw *in toto*. When the respiration becomes noisy and disturbed, and in all cases where hitherto the teeth have been separated and the tongue pulled forwards, Dr. Heiberg would depress the jaw in the following manner. The chloroformist, standing behind the recumbent patient, places both thumbs on the symphysis of the lower jaw, passes the second segment of the flexed index finger behind the posterior margin of the ascending ramus of the lower jaw on each side so as thus to hold fast the whole bone between both hands, and then with some force drags the jaw directly forwards. The whole jaw may thus be made to glide forwards, so that the lower dental arch stands in front of the upper arch. This proceeding, which may be carried out with special facility in children, is at once followed by deep and perfect respiration. So long as the jaw remains dislocated, so to speak, respiration is unimpeded and quiet, and just the same result is obtained as from traction on the tongue. Dr. Heiberg states that the anatomical reason for this favorable result is not quite clear to him, but he thinks it probable that the epiglottis lying over the rima glottidis may be thus elevated. The following are supposed to be the chief advantages of Dr. Heiberg's proceeding. 1. The operator can do his own work with less disturbance, and the operation can be performed quietly and with more safety. 2. Lesions of the teeth and tongue are thus avoided. 3. Less chloroform is used, the after-effects of the narcosis and the dangers of chloroformisation being thus lessened.—W. Johnson Smith, in *The London Medical Record.*

Hysteria Simulating Hydrophobia. By Dr. M. L. Humston, of Monroe, Indiana.

Believing the case given below is not devoid of general interest, I send you the record of it from my notes.

On the evening of the 27th December, 1872, I was summoned with Dr. Joseph McClain, of this place, in great haste, to see Mrs. C., who the carrier said was suffering with hydrophobia, and he added quite a graphic account of the patient's biting, barking, spitting, growling, etc.

The distance from my office was ten miles. On arriving we met J. W. Deaver, M. D., who finding the case to be urgent had proceeded to examine the patient, and found her frothing at the mouth, snapping, biting at everything that came within her reach, shaking the bed-clothes with her teeth, growling like a dog, and tearing up handkerchiefs, etc. He then administered the following at one dose, and repeated within an hour—

℞. Valerian fl. ext. - - ʒij,
Bromid. potass. - - - ʒss.

After which he proceeded to prepare the following:

℞. Chloral hydrat. - - ʒj,
Potass. Bromid. - - - ʒij,
Syrup Simplic - - - ʒviij. M.

And ordered a tablespoonful every hour until the patient went to sleep, which she did in the course of a few hours.

About ten o'clock, p. m., of the same evening, Dr. McClain and myself arrived. Her parents informed us that some three or four months previous she had been chased and bitten by a dog supposed to be mad, and the mark was yet on her arm. She then being under the influence of chloral hyd., I examined the arm carefully, and while doing so she aroused and began to talk, and tried to show me the scar, but I could detect nothing. The temperament of Mrs. C. is nervo-sanguine, the nervous temperament predominating, both parents being quite nervous. The patient kept calling for water in her quieter moments. When it was brought she would immediately go into spasms.

As the physical symptoms of hydrophobia, as laid down in the authorities, were wanting, we had no trouble in diagnosing hysteria. We informed the family of this opinion, which they did not accept, and to this day believe it was a curable case of hydrophobia.

The patient not having passed any urine for some time we gave her

℞. Buchu fl. ext.
Spirits ether nitrici - - aa ʒj. M.

and ordered a tablespoonful to be taken every two hours until it had the desired effect, which was soon accomplished. About 12 o'clock at night we gave a cathartic, and about 2 o'clock, a. m., on the 28th, Dr. Deaver left, with the promise to be back that day. Dr. McClain and myself stayed until eight o'clock, a. m., same day, when we left her sleeping soundly under the influence of the chloral.

January 1st. Patient convalescent; entirely rational; drinks water; eats anything edible. She was then discharged, with the instruction to use the chloral and bromide on the least sign of the return of the symptoms, especially the bromide.

There has been at this writing no return of the hydrophobia. She now has a fine healthy boy, about eleven months old. Mother and child both doing well.—*Medical and Surgical Reporter.*

Contribution to the Therapeutical History of Calomel.

The instability of some mercurial compounds, under certain conditions, is a subject of grave importance, both to the practice

of medicine and to medical jurisprudence. Prof. Bellini (*Dublin Medical Journal—American Journal Medical Science*, January, 1874) has given an able review of this subject. The following are some of his conclusions relating to the possible change of calomel into corrosive sublimate within the living body, based upon both clinical experience and experiments in the clinical laboratory.

(1) Calomel, introduced by the mouth into the fasting subject, is in a very small portion of the stomach, and a greater part of the small intestine, changed into a soluble mercurial compound. In the stomach, this change is effected by the lactic acid and the alkaline chlorides, and in the small intestine by the alkaline carbonates of the alkaline fluids.

(2) Calomel, introduced into the stomach which is digesting protein aliments, is either wholly or almost decomposed within that viscus, and changed to metallic mercury and a soluble mercurial compound.

(3) Acid drinks and fruit should be withheld during a course of calomel, but magnesia in any form promotes the local action of the mercurial.

(4) The use of mercury may be injurious when given with ammoniacal salts or in certain diseased conditions, *e. g.*, in those suffering from ammoncima, uro-ammoncima, cholera, typhus, etc.

(5) Calomel might cause poisoning when administered contemporaneously with hydrochloride of ammonium, aqua laurocerasi, with alkaline or metallic iodides and bromides, the alkaline sulphides, alkaline hydrobromates and hydryiodates.

(6) Opiates render its effect less perceptible by their control of the sensibility.

(7) Calomel applied externally may be rendered soluble by the alkaline chlorides of the organic fluids.

(8) Such external use may produce severe local lesions if at the same time there be given internally alkaline iodides, bromides, or sulphides, or even sulphur itself.

These results are supported by the combined testimony of clinical observation, and chemical and physiological research.—*Detroit Review of Medicine and Pharmacy.*

Inversion of the Uterus from Short Cord. By D. F. Woods, M.D.

Inversion of the uterus is, fortunately, a rare accident. On account of its infrequency the following case is of value, and its history may not prove uninteresting to the readers of the *Medical Times*:

About a year ago I attended Mrs. B. in her first confinement. When I arrived at her house, I found her in the first stage of labor. There was nothing worthy of note in this dilating stage. Soon after the commencement of the second stage, the patient

complained of a dragging or tearing pain in the region of umbilicus, which pain was greatly intensified during the contraction of the uterus. Each bearing-down effort was preceded and accompanied by a feeling of dread, and as labor progressed the sensation of dragging or tearing pain became more and more agonizing. The woman described it as "a feeling as if her whole insides were tearing to pieces."

The child presented in the second position (right occipito-anterior) of the vertex. This dragging or tearing sensation lasted through the whole of the second stage, which was prolonged about eight hours.

As soon as the head was delivered, and I could reach the cord, I cut it, applying the ligature some time after to the child. The cutting of the cord did not, however, arrest the already partially-inverted uterus, for a heavy pain stopped by the contraction of the diaphragm brought down the whole contents—the uterus completely inverted, with its placenta firmly adherent at its fundus. Hemorrhage occurred in great profusion from the inverted organ, which seemed to extend to the patient's knees.

With considerable effort I tore the placenta from its firm plastic adhesions, and afterwards endeavored to restore the uterus to its normal position. This at first was no easy matter, as I found the more I attempted to restore it, the more contracted its mouth became.

Finally I resorted to the following method. With the hips of my patient considerably elevated, the thumbs of both hands applied to the fundus, by degrees I was able to overcome the resistance, and make sufficient indentation to push the fundus with my right hand through the os uteri, which gradually relaxed as it had contracted By degrees I restored the organ to its normal position in the abdomen. When this was accomplished, my right hand was introduced into the cavity of the uterus, where, holding it, I ordered three drachms of wine of ergot to be given, at the same time making slight friction over the abdomen with the other hand. I soon felt the uterus contract firmly upon my introduced hand, which I gradually withdrew.

The hemorrhage, which to this moment was very profuse, ceased. I applied a bandage with a firm compress to hold the uterus contracted, gave a large anodyne, and ordered two drachms of the wine of ergot to be continued every half hour until four doses were given, and after that gradually discontinued. The ergot was kept up for ten days, in doses of a drachm every three hours. My patient was considerably exhausted from the excessive hemorrhage, and the next day complained of considerable soreness over the abdomen. Independent of these, she gradually recovered without any further serious untoward symptoms.

The child (a female) was slightly asphyxiated at birth, but soon recovered under the simple treatment of alternate cold and hot douche, and she is now a large child, about a year old.

An examination of the placenta showed signs of former inflammation at its uterine connections, evinced by an excess of what appeared to be fibrinous coagula of considerable hardness. The cord was bound round in a ball with plastic exudation, holding it firmly and moulding it closely to the amniotic layer at the fundus where attachment took place.

The whole length of the cord was not six inches. The woman had a number of falls at various intervals during her pregnancy. At one time, the pavement being slippery, she fell on her doorstep, striking her stomach. The injury at no time gave her severe annoyance, except muscular soreness the next day—not sufficient to call in a physician.

Since the occurrence of the above, I have attended this same patient in her second confinement, the interval being a few days over thirteen months. This last is a large, well-formed male child, and presented in the left oblique of vertex (left occipito-anterior). The mother had a quick delivery, and no difficulty whatever. Both mother and child are doing well.—*Philadelphia Medical Times.*

Treatment of Spasmodic Asthma.

Dr. Julio J. Lamadrid recommends the combination of chloral hydrate with the bromide of potassium in the treatment of spasmodic asthma. The following is the formula which he employs:

℞—Chloral hydrat..... ʒv
Potassi bromide..... ʒijss
Syr. flor. aurantii,
Aqua dest., aa..... f. ʒi. M.

Sig. A teaspoonful in half a wineglass of water every two hours, until sleep is induced or dyspnœa is relieved.—*Philadelphia Medical Times*, August 29, 1874.

Pills of Sulphate of Quinia.

Editor American Journal of Pharmacy:—The existing formulæ for making quinia pills have probably proved unsatisfactory to many of your readers. The process of the U. S. P. yields a rather bulky and unsightly pill. With glycerine alone as an excipient, one obtains (if fingers and tools are clean) a white, but large pill, and the mass is apt to be either crumbly or flabby. Made with dilute sulphuric acid the product is small and solid, the mass hardens so quickly when it begins to set that it can be worked only in small batches. Recent experiments have led me to the adoption of the following method, which, after an experience

of three months, and the making of some thousands of pills, I pronounce unexceptionable:

Take of	Sulphate of Quinia - - -	600 grs.
	Tartaric acid - - - - -	100 grs.
	Glycerine, pure - - - -	75 minims.

Rub the quinia and acid together in a mortar to a fine powder, till no appearance of crystals remains, add the glycerin—just 75 minims, no more nor less—and continue the trituration till the powder becomes adherent, when it should be beaten into proper form for handling and divided into the required number of pills. The mass is firm, solid, rolls well, does not set for some hours, is, in fact, a "beautiful mass," and the pills will be found quite small for their weight, very white, if rolled in starch powder, and however old or dry they may become, they remain perfectly and entirely soluble.

Let me repeat that the quantity of glycerin is just right, though it seems at first insufficient for such a bulk of quinia, but in connection with the tartaric acid it does the work, though requiring patient trituration. Even a very few drops of glycerin more than the proportion given will render the mass inconveniently sticky. H. P. Reynolds.

Dyspepsia and the Use of Pepsin.

The views of Dr. Schacht concerning digestion have been confirmed by Professor Leube (The inventor of Leube's meat solution) of Jena, in a lecture just published on stomach diseases. He says: 1. No condition of the stomach has yet been observed in which pepsia is altogether absent. 2. The cause of indigestion is generally the absence of sufficient acid. 3. The action of pepsin in a solution of albumen resembles that of a ferment, and it will continue so to act without end, merely by the addition of more acid. 4. Alcoholic solutions, especially wine, on account of the tannin it contains, should be avoided as vehicles for pepsin. Finally, he recommends, in case of indigestion, a solution of chopped meat with water, adding a small proportion of pure muriatic acid, and some thickening. He finds such a solution very nourishing, and reports excellent results. These views and experiments are not novel, but exhibit the old doctrine as to digestion, and it appears to be the sound one. The secretion of the pancreas is now thought necessary to the digestion of fatty substances; and where these are used to any extent—as in cod-liver oil—it would be best to take the new medicine pancratin, which acts best with an alkali instead of an acid, or to use a little of the solution of the pancreas of freshly killed animals. *Scientific American.*

have obtained their knowledge of this method of resuscitation, either directly or indirectly from the teachings of Nélaton; for he had for years been in the habit of explaining his method in his lectures, and at his clinics, and Dr. Johnson published an account of it in the American papers in 1861." To which, subsequently, Sims makes the following further remarks: "As the facts now laid before you fully explain themselves, it is unnecessary for me to indulge in any lengthened remarks on the subject. In my own country the accoucheurs often use chloroform, and the surgeons mostly use ether. "Whether death from chloroform is due to cerebral anæmia or not, it is at least safe to adopt Nélaton's method in all cases of supposed or threatened danger; but the safest plan is to relinquish the use of chloroform altogether, except in cases of obstetrics. The frequent cases of death from the use of chloroform in surgical operations that have occurred amongst us, should warn us to give up this dangerous agent, if we can find another that is as efficient and, at the same time, free from danger. Ether fulfils the indications to a remarkable degree." Herewith ends that remarkable address, a concatenation of repulsive elements, read at the 42d Annual Meeting of the British Medical Association, and the effect it must have produced can only be imagined.

Let us now inquire into, and try to analyse the assertion of Sims, that I must have stolen the method, he calls so repeatedly and conspicuously *Nélaton's method of resuscitation*, either *directly* or *indirectly* from the teachings and writings of Nélaton. Why is it that Sims, in the face of so many deaths which have occurred almost monthly, if not oftener, according to the reports in the different Journals, since he met for the first time, as he asserts, with the application of the method, in that case of of vesico-vaginal fistula in "high life," I say, why is it that he has observed so long a silence and has not given to the world the benefit of the knowledge he derived from Nélaton, when the profession seemed to stand so badly in need of it? Why did he not portray that picture, he has now given us in such vivid colors, at the time Dr. Holmes, of Chicago, published his article in 1868, but was first reminded of the importance of that method after I had given publicity of it in this Journal? I cannot therefore help inquiring, what amount of truth there is in that Nélaton story. Had it a real existence, or has it sprung up in the fertile brain of a man, so rich in inventions? I cannot too strongly lay emphasis upon this fact. It is of great significance

to me, that he first becomes aroused to its full importance after he has read my article; he neither inquires nor investigates before, nor does he even think it worth while to as much as mention this valuable treatment in his publications, when he certainly has had sufficient opportunity to do so. The man who could conceive and publish the idea, that I must have purloined my invention from the teachings of Nélaton *directly or indirectly*, cannot have any respect for justice or truth. Some people attain the comfortable stage of reposing full faith in their own inventions and tell stories so often, that they have grown to believe them to be realities. So I should not wonder if Sims, in the *New York Tribune* or the *Scientific American* were to tell us by and by (if he does not already), that he considers himself as the real benefactor of mankind.

It is not the first time that I have had occasion to lay the dissecting knife at Mr. Sims' qualifications as an inventor and a man of truth. It was once my painful duty to defend an absent author against being robbed of an invention, which had become by right his property, and now I am informed that I must have taken my method from M. Nelaton. The first case in which I applied my method of resuscitation happened in 1864, and Dr. Holmes, as Sims asserts, published his case in 1868; Holmes could not well be informed of my practice, and I do not know if he ever heard of Dr. Nélaton's method, but I believe that, like me, he never had had any knowledge of the celebrated Frenchman's method of proceeding, and that for the simplest reason in the world, because Dr. Nélaton never spoke, nor wrote about it. That belief I am forced to sustain, till Dr. Sims brings other and better proofs, than he has offered in his address. Dr. Sims reminds me of a friend of mine, an excellent man, but a great story teller. When he had told a story several times he believed so earnestly in its reality, that when in company with strangers and one or the other showed a doubt, he could get so angry, that he even would fight a duel to prove his sincerity in the facts, as he had related them, though his friends were aware, that they never happened. Such phenomena may be considered psychological curiosities. Having never been in Paris, the *direct* manner by which I could have come into possession of the doubtful method of Nélaton, is out of question and all other sources, which would belong to the *indirect* manner, have so far proved abortive. Whosoever of those who had been in Paris and had heard the lectures of Nélaton, even

his intimate friends, did not know anything about it. The only book in my possession, and the only one I have ever seen from the authorship of Nélaton, his clinical lectures on surgery, from notes taken by W. F. Atlee, of Philadelphia, does not contain even an allusion to it. In no other work or handbook of any other author, throughout a library of over 1000 volumes on medicine, have I found any mention of it. The editor of the *Zeitschrift für Chirurgie*, Professor Hueter of Greifswalde wrote to me, November 26th 1874, "of Nélaton's merits in this question I know absolutely nothing, though I have been studying the literature of that subject thoroughly. I have never met with anything having reference to it. Besides, during four months, 1861–62, I have been at Nélaton's clinic, but do not recollect of ever having heard anything about it." The editor of the *Revue de Therapeutique Medico Chirurgicale*, who sent me a copy of his Journal, in which he had taken notice of my article, is silent about it and I would consider it strange, that a Frenchman, who had been educated in Paris, would not claim for his celebrated countryman a method of so great import, if such was really the case.

In the face of such indisputable facts, can any reasonable man blame me, if I ask for more and better proofs before I believe the fistula story of Dr. Sims. Most willingly will I cede all rights of priority to the celebrated French surgeon, if he has a right to them and the more willingly, since I consider all such priority claims a ridiculous undertaking.

Is Dr. Sims aware, that the ruins of Pompeii have lately brought to light a speculum, of which the editor of *Harper's Weekly* remarks, that it is exactly the counterpart of that of Dr. Sims? the same speculum, of the invention of which I once proved that Dr. Sims had been anticipated as well, as in the whole of his method of treating vesico-vaginal fistula; that the invention of that speculum had been published by Dr. Metzlar, of Prag, just before the time when Dr. Sims began to study the literature of the operation for vesico-vaginal fistula, about which he felt so worried and sore, that even in his clinical notes on uterine surgery he gives us a detailed description of the circumstances by which he came to his (?) invention; that by some it was called after the name of another man who had nothing to do with it, except to hand it to the instrument maker to be copied; though that is another error, since Dr. Metzlar not only published it

before Sims, but has never been in this country.* Evil minded men might as well claim that Sims had copied not only that speculum, but also his mode of operating from Metzlar, since Metzlar had already cured six cases before Sims began to "think of it," and the more so, since both methods corresponded with each other to the smallest item; and what were my remarks—I said, "Metzlar may thank the prejudice of his countrymen, that the triumph of having initiated so valuable an improvement, has been carried off by others."

When Dr. Bozeman published, in his modest way, his treatment of vesico-vaginal fistula, in 1856, with a new mode of suture, who does not remember the blind fury, illiberality, and intolerance with which Sims persecuted that man, going even so far as to accuse him of having stolen his operations from him, and dragging him through the mire of calumny; and that is the Dr. Sims who, in 1858, before the New York Academy of Sciences, used language, never heard of since the days of Bombastes Theophrastes Paracelsus, declaring silver sutures "the greatest invention of the Nineteenth Century," who now with effrontery and arrogance declares, that I must have my method either *directly* or *indirectly* from Nélaton; *indirectly*, probably because Dr. Johnson had published an account of it in the American papers in 1861. But why not be more explicit, and say in what papers? I have searched for it without satisfaction. I have gone through the *American Journal of Medical Sciences* for 1861, and the following years, without finding even as much as a note of Nélaton's method of the anæsthesia, caused by chloroform. But what I did find, and which settles some other untenable assertions in Dr. Sims' address, is laid down in the following article, contained in Vol. xli, page 596, of the year 1861, the identical year when, according to Sims, Dr. Johnson published Nélaton's method in "the American papers." "*On the Nature of Death from the Administration of Chloroform and Ether, as observed in Hospitals.*" "*Dr. Chas. Kidd*, read an interesting paper on this subject before the Physiological Sub-section of the British Association for the Advancement of Science. From data collected and tabulated, it appeared not difficult to explain and to offer some new directions, as to the nature of death from anæsthetics, a subject of serious

* In my pamphlet on vesico-vaginal fistules I committed the mistake of giving the year of Metzlar's publication 1846, instead of 1845.

importance in medical practice and the author having collected and tabulated 109 deaths from chloroform, and 24 *from ether*, believes himself," etc. 24 *deaths* from the innocent *ether!* from which Sims "never felt the least dread of danger under any circumstances." This is nearly one death of ether to five of chloroform, which comes pretty reasonably near the proportion in which ether and chloroform are given, according to the statement of some authors, yet I believe that the proportion is even a greater one; from which we might have to judge, that ether is more dangerous than chloroform. But let us further hear what Dr. Kidd has to say: "Of these 133 deaths, 90 occurred in males, 43 in females. There have already from 250,000 to 300,000 operations of various kinds been performed under the influence of anæsthetics, chiefly under chloroform, without there having been any well attested instances of death from stoppage of the functions of life, or narcotism of the system, by chloroform. A law of tolerance of chloroform has long been established in hospitals—one lad took 62 ounces in 12 days, for intense pain," (and that is not as much as I have observed; the son of my friend Shelly of this city, had inhaled about 5 pints in succession, to kill an intense chronic headache; and I have administered myself to my wife, during a spinal irritation of so intense a nature, that I had to keep her constantly during ten days and nights under the influence of chloroform, over 7 pints; the only consequence in both cases was a kind of delirium tremens potatorum). "Chloroform, and this may be well considered, has now to bear all the obloquy for all the consequences following its application, even for such which happened before chloroform was known, or used." Dr. Snow counts 6-7 cases, which died on the operating table, from fright. Looking to the facts, Dr. Kidd says: "Many of the deaths from chloroform were sudden, of the the nature of a fit." He further thinks chloroform to have a *powerful action on the pneumogastric in the lungs, and stopping the action of the heart.* "Chloroform," he says, "as a general fact, has had a good effect on the general result of operations in surgery. Probably ten per cent. of lives are still directly saved by chloroform yearly." Dr. Kidd speaks of the experience of the chief recorders of death from Chloroform, amongst whom he mentions Denonvilliers and Robert, of Paris, and the conclusions they arrived at were: "There is little if any benefit in choosing ether in preference to chloroform as an anæsthetic. (How is this, Dr. Sims?) There

is a very remarkable law of tolerance of chloroform observable in all bad surgical cases. A patient may have had administered chloroform a hundred times, but die of its effects on the next administration. There are two modes of death: probably in one-half of the deaths from chloroform, the action is like an *irritant* on the *laryngeal recurrent* and *pneumogastric nerves;* the other half are instances of *syncope*, *convulsions*, *fits*, etc. In all cases we are justified in having recourse to such means as *artificial respiration*, *tracheotomy* in cases of spasm or asphyxia, a *sudden dashing of cold water* to *wake up the respiratory nerves*, fresh air fanned on the face. *Acupuncture* of the *muscles* of *the neck* is also recommmended, so as to irritate the spinal accessory and phrenic nerves, and last, *warm water* in *the rectum*, and *inhalation of oxygen*."

Herewith ends the whole arsenal of remedies, of treating death from anæsthetics, of the most experienced surgeons, the French not excluded, and not a word of *anæmia*, not a syllable referring to "Nelaton's method of resuscitation." In no text book, in no handbook, in no journal, do we meet with a passage or a single word, having reference to such a highly important treatment; but still I must have stolen my ideas from Mr. Nélaton, *either directly or indirectly.*

When Wheatstone, by his ingenious and difficult experiment, measured the velocity of the galvanic current, he had been, unknown to him, anticipated fully seven years by a poor German student, who had discovered, in the quiet retreat of his laboratory, "what astronomers had searched heaven and earth to find out and had failed." When Adams calculated the existence of Neptune, Leverrier was after him with his powerful telescope, unconscious of the calculation of Adams. But why enumerate such facts, where inventions have been made simultaneously, and independent of the labors of others? Nothing is more barren of fruit, than questions of priority. It has never been my habit in making use of even a sentence uttered by another, without giving due credit for it, not to speak of the impropriety of adopting the labors of others, and pronouncing upon them from selfishness, as the fruits of a cultivated mind and profound studies. Herewith I dismiss the unproved accusation of Dr. J. Marion Sims, that I must have my method of resuscitation, either *directly* or *indirectly*, from Nélaton, and Mr. Sims may thank the editor of this Journal, that all the stronger reproofs such an unwarrant-

able abuse of facts and violation of historical truth deserve, have been eliminated.

Postscript.—When this article had been written, and was ready for printing, I received a paper "On Chloroform and its Use in Surgery," written by W. Koch, in German, and edited 29th October, 1874. Koch has thereby increased the already great number of treatises on chloroform, by one more. The writer of this pamphlet says near the end of it: "Of late a remedy much favored in the middle ages against all kind of diseases has been recommended against the apparent death from chloroform: placing the head of the patient in a recumbent position. Mr. Schuppert, of New Orleans, is still more radical; he has his patient hung up by the feet." Koch does not wonder to find such remedies recommended in England and America (and he might also include France, since in annotation he mentions, that Nélaton had already made use of such before me), when "he saw in his own country the narcosis produced by chloroform explained by embolism of the cerebral vessels, and as a remedy against it, recommended hanging up by the feet." The few casualties that Koch can find are of sufficient proof to him, that persons near death from asphyxia will get well, notwithstanding the most "adventurous play," if only the cardinal point, *artificial respiration*, is used. He further ridicules the idea, that the rest of the arterial blood should be driven from the left ventricle to the brain, and says that the advocates of the "method" had lost sight of the only advantage that the position could give, namely, the disencumbrance to the right ventricle and auricle of blood, which according to the laws of gravitation, might flow back to the vena cava and thereby cause contraction of the right ventricle."

It cannot be my object to enter into a controversy with Mr. Koch, otherwise I might be induced to question for what new ideas, remedies, or methods, in the treatment of the apparent death from chloroform, we are indebted to the eminent writer of this compilation. The answer would probably be a barren one, a *testimonium paupertatis*. Artificial respiration is his panacea, and a small dose of galvanism applied to the heart; a doubtful proceeding to the initiated; but as I said before, a controversy with Mr. Koch is out of question. A man may be wise, still it shows ill-

breeding and a want of proper education, to underrate another country and more so when that country is England, the mother of modern surgery. When in time Mr. Koch gets older, and probably wiser, he will also look upon America with a different eye. To despise a remedy, because that remedy has been already recommended in the middle ages, is also a novel idea. The hanging up by the feet, since the writer will have it so, as recommended by me, is after all not so strange a proceeding; it has been advocated not long since and moreover by a prominent German surgeon, for a reduction of incarcerated hernia, and the remedy does not seem to me half as barbarous as the manner, I have just been obliged to reflect upon. If Mr. Koch had read my article with more care, he would probably have met with a statement, that I recommended less the hanging up by the feet than generally the recumbent position and further, that only in my first case artificial respiration and here even in an incomplete degree, had been tried and that I gave it as my opinion, that the artificial respiration had nothing to do with the act of resuscitation; his cardinal point, artificial respiration, falls therefore to pieces—becomes non est.

In reference to the researches and experiments of Hueter, which have astonished and irritated Mr. Koch so much, I will only remark, that I believe Hueter too good an observer, experimenter and judge, to place the least doubt in what he saw; and the observed facts do hardly allow another explanation, all the reasoning of Mr. Koch to the contrary notwithstanding. If Mr. Koch intends to interpret my views as if I had advanced the idea, that in the reversed position the rest of the arterial blood would run back from the left ventricle to the brain, he utterly misstates a fact. Equally untrue are his remarks in reference to the losing sight of the only advantage of that position; he is equally unfortunate here, as his anatomical wisdom goes begging the question. All his interpretation he might have found alluded to, in my calling attention to the *danger of dilatation of the right ventricle.*

Why I have taken any notice of the article of Mr. Koch, was less on account of his irrelevant remarks, than the notice he appended to the method of resuscitation, that Nélaton had already made use of it; but he doubtless copied this from Dr. Sims' address.

ARTICLE VI. *Special Report of Quarantine Operations at the Mississippi Station*, 1874. By Dr. ALFRED W. PERRY.

During the quarantine season, from June 16th to October 13th, a new plan of disinfection was practised at the Quarantine on the Mississippi River, below New Orleans, on vessels from ports infected with yellow fever. This disinfection was more extensive and complete than has ever been performed anywhere in the world; and I think that the evidence which will be adduced will demonstrate that the successful result was not a mere sequence, but was strictly dependent on what was done before, to get this result. Soon after being assigned to quarantine duty, apparatus was prepared to carry out my ideas in regard to the disinfection of vessels; this apparatus was brought into working order June 20th, 1874. It consisted of a sheet iron cylinder 3 feet high, 3 feet in diameter, with numerous adjustable air-holes near the base; into this was put a charge of 30 to 100 lbs. of sulphur, which was lighted by a few burning shavings; an 8-inch iron pipe connected the upper part of this sulphur burner with the entrance opening of a No. 2 Sturtevant fan-blower: to the outlet was connected a short upright iron (8-inch) pipe, and to this was fastened an 8-inch rubber hose. The fan of the blower was put in motion by three pulleys of increasing speed, and made 2500 revolutions per minute; the main driving pulley was turned with a crank by two men; the whole apparatus was mounted on a small, stout, flatboat, which was towed to the vessel lying at anchor near the Quarantine Station, the fire lighted, and the blower started, until the shavings had entirely burned out and the sparks had ceased; one end of the rubber pipe was then connected to the blower outlet, and the other end was introduced in the vessel. It was found that large steamers have dead-lights just large enough for the 8-inch pipe; in small steamers a side port was opened, and closed tightly with boards and a hole of the proper size cut in the boards: for sailing ships an 8-inch hole was cut either in the hatch cover or in a false cover made for the purpose. The smallest hatch was used to make the opening in; the pipe being arranged, the sulphurous acid vapor was pumped into the vessel for a time varying from a half hour to two hours—the sulphur vapor pumped in the fore hatch permeated every crack in the vessel and made its way to the stern, and always

came in the cabin through the minute cracks in the wood-work. Although introduced on top of the cargo, the vapor penetrated to the lowest part of the lower hold. To those acquainted with the laws of the diffusion of gases this action is evident. Only the holds of vessels were disinfected in this way; the forecastles and all small compartments were disinfected by chlorine gas, generated in the usual manner. Wherever yellow fever had existed on the vessel, either while at her port of last departure or on the voyage, additional measures were adopted. All the clothes of all the men were soaked in a dilute solution (1–30) of white carbolic acid, and the decks, forecastle, walls, bunks, washed with the same solution. Even this disinfection was not as thorough as could have been done with more apparatus to use other disinfecting agents, viz., hot air, carbolic acid vapor. During the quarantine season, there arrived from yellow fever ports 21 steamships and 43 sailing vessels; all of these were disinfected as above: 5 of these sailing vessels had had yellow fever either in the port of departure or on the voyage, and were *ipso facto* infected.

Yellow fever was introduced from Havana into Key West, Pensacola, Pascagoula, and all ports on the gulf, and into New York, and in some other Atlantic port. Yellow fever has been introduced into New Orleans through a so-called strict quarantine (i. e., detention),

In 1868, by Peter Lablasse.	In 1872, S. S. Havana.
1870, uncertain.	1873, Bark Valparaiso.
1871, Brig Hope.	1874, Bark Queenst'n, Oct. 16th,

after disinfection had ceased and after my term of service had ended.

1st. We see by this table, that since 1869, the date of the first exact report, that yellow fever has come through a "time quarantine" almost every year.

2d. We find, that yellow fever had a great tendency to be imported this year—case Mobile, Pensacola, Pascagoula, etc.

3d. That 5 infected vessels came to the quarantine.

4th. That no yellow fever came through the quarantine during the period of disinfection—June 23d to October 13th.

5th. That it passed through the ordinary quarantine (without disinfection) this year, bark Queenstown.

I am aware that this is all negative testimony, but there is a

good deal of it, and the testimonies agree, and all scientific probability favors my claim.

The above statistics show that in 40 sailing vessels there were 7 infected; in — steamers there were none infected. The experience of this year and previous years, at this port, shows that the liability of steamers, which stay 48 hours, or less, at anchor in the harbor of an infected port, to introduce yellow fever, is only 1-6th to 1-8th as great as the liability of sailing vessels, which always stay a much longer time. This shows that the detention of steamers at quarantine which have made a short stay in an infected port can be lessened to one or two days, with equal safety as at present, after being subjected to a complete disinfection as above.

I do not maintain, that any kind of a quarantine will keep New Orleans free from yellow fever, because the number of cases that originate here are too numerous and well established to be denied. But the apparently indigenous fever starts too late in season to develope to any great extent, while the foreign yellow fever can be imported and begin to develope in May and June. All of the serious epidemics in New Orleans have originated from early importation of yellow fever. If the sanitary authorities can prevent the importation of foreign yellow fever in May, June and July, they can practically abolish yellow fever epidemics, and quarantine will be worth to the city and State the small direct cost and the greater indirect cost in loss of time to commerce.

PROCEEDINGS OF THE NEW ORLEANS MEDICAL AND SURGICAL ASSOCIATION.

Abscess of the Prostate. By J. Dell'Orto, M.D.

The abscesses of the prostate are generally the consequences of the acute inflammation of that gland.

The diagnosis of these abscesses is very easy. After several days of intense suffering and fever, with symptoms of prostatitis and cystitis, suppuration in the tissue of the prostate presents itself with the following symptoms: a feeling of heaviness in the perineum, and deeply seated pain in the region of the rectum, accompanied with pulsations isochronous with the beatings of the heart; great difficulty in the evacuation of faeces, and in voiding urine, amounting sometimes to a total retention; finally, by introducing his finger in the rectum, the surgeon recognizes the

purulent focus on its surface soft and resistent. When the abscess has arrived to such a point, the general symptoms are gradually decreasing, and its rupture must be expected at any moment, and the pus may take different routes. This is the practical point of great importance in this disease, to which I want to call especially your attention to-night.

When we study the anatomy of this gland, its situation between the pelvic fascia and the median perineal aponeurosis, in that space which separates the bladder from the inferior portion of the rectum, we can easily understand that the pus formed in it cannot take any other routes but the following four:

1st. Either the perforation of the purulent focus takes place on the side of the bladder or urethra, then the pus comes out with the urine, and the patient feels immediately relieved. In this case the prognosis is, no doubt, very favorable, and the recovery almost sure in a few days Unless, when by some internal constitutional causes, the prostate continues to suppurate, and the abscess increases to such an extent as to produce a large purulent cavity resembling another bladder; then the prognosis must be very unfavorable and the disease may end fatally. Velpeau relates three cases in which *post mortem* examination proved the existence of such cavity.

2d. The perforation takes place on the side of the rectum, and the pus will come out through the anus. This accident, according to my opinion, is the most favorable of all, because the pus finds a ready outlet, and recovery is made easier

3d. When the perforation is made inferiorly, under the median perineal aponeurosis, the pus is forced outwards to the skin of the perineum, where a purulent tumor is felt, which must be immediately operated upon. This case is more complicated and dangerous than the preceding two, and, though it may be liable to recovery, requires a long and tedious treatment, and when it happens in a patient of debilitated and tainted constitution, it may prove fatal, by the abundance of suppuration and mortification of tissues. Such a case I saw a few months ago in consultation with my distinguished friend Dr. Castellanos.

4th. The pus, instead of verging towards the bladder, the rectum, or perineal integument, infiltrates between the fascia of the pelvis, and the middle perineal aponeurosis, in which case it causes such a purulent inflammation of all the tissues in the lower portion of the pelvis, as to jeopardize the life of the patient, and when the abscess opens itself through the recto-vesical sac into the peritoneum, death is inevitable.

In the case which I am going to relate the abscess opened into the rectum, and the patient made a quick recovery.

A. C., a stout, strong man of bilious-sanguine constitution, aged 45; had been following his occupation of fishing at Lake Pontchartrain for several weeks in the months of July and August, 1871, and had been much exposed to the cool, damp air of the lake shore, where he had slept several nights consecu

tively. He had enjoyed fair health all his life, until the 1st of September, when he began to have chills and fever. On the 3d of September he came in town, and at 7 p. m. of the same day I saw him for the first time. He had had a strong shaking chill a few hours before, and had a very high fever, complicated with neuralgic pains all over his body; tongue very much coated. I made diagnosis of intermittent fever. I prescribed a warm foot bath and a bottle of citrate of magnesia.

September 4th—*Morning Visit.* No fever; he had been freely purged by the lemonade. In the evening fever returned, with chill at the same hour. Administered 24 grains of quinine in twelve doses.

September 5th—Fever dissappeared, but the neuralgic pains continue, especially in the face.

September 6th—Passed a good night. He is not quite relieved of the neuralgia, which is fixed on the left side of the face. I applied an ointment made with the extract of belladonna and opium.

September 7th—Neuralgia has disappeared completely, and I believe he will soon be convalescent. In the evening he complains of a light pain at the hypogastric region, with a little burning in voiding urine. I make diagnosis of irritation of the neck of the bladder, and neuralgia of the same, produced by rheumatic cause. No fever. Prescribed general warm bath, camphor pommade, with belladonna, flax seed poultice, and emollient enemata.

September 8th—He is not better: he is costive, and complains of something heavy about the anus whenever he wants to evacuate. Ordered two ounces of castor oil, and continued the same treatment.

September 9th—He passed a very restless night; great difficulty and pain in making water. There are some symptoms of urethritis, as burning sensation all along the urethra, and running of a gonorrhea-like matter (he never had any venereal disease). A little fever in the evening. I introduce the catheter into the bladder. No difficulty in the introduction of the catheter; no strictures in the urethra. Continue the same treatment, to which I added injections of infusion of belladonna into the urethra.

September 10th.—He is worse. Pain at the anal region increasing. High fever. I suspect the inflammation of the prostate, and by introducing my finger in the rectum, I feel the gland very much swollen and hard. I applied twelve leeches over the perineum. Since this date I introduce the catheter twice a day.

September 11th—Leeches drew plenty of blood, but without any improvement; the inflammation is progressing rapidly, and diffusing itself over the wall, or muscular membrane, of the bladder, and peritoneum; abdomen is swelling; great suffering, and tenesmus in defecation, which causes distressful pains and spasm. I fear peritonitis.

September 12th—Applied a dozen of leeches at the hypogastrium, and administered the following cathartic pills: blue mass and calomel aa gr. x., extract hyosciam. gr. v., for three pills; frictions of mercurial ointment over the abdomen several times a day.

September 13th—Pills did not act. Ordered a bottle of citrate of magnesia, after which he had several good passages, and felt easier. Prescribed tincture of belladonna, 6 drops every three hours. In the evening the fever had disappeared, and the abdomen was less swollen. He looks better. Peritonitis is cut short, and everything leads me to believe that he will pass a good night.

September 14th—Passed a very restless night; slept none. At twelve o'clock (midnight) fever again with, chills. Called in consultation Dr. Coursault, who advised to give some more quinine, 30 grains, with 8 grains of hyoseiamus, for 8 pills, to be taken, one every two hours, and continue the same treatment of general baths, mercurial friction, belladonna drops, etc. The urine drawn by the catheter is always natural, and very little reddish.

September 15th—Getting worse; fever continues; pulse over 100; abdomen more swollen; no sleep, no rest; strains very much at stools, with unbearable pains. In the evening applied another dozen of leeches at the hypogastrium. In the evening, at 9 o'clock, I injected in the rectum 10 drops of tincture of belladonna, with 15 drops of laudanum and paregoric each.

September 16th—The enema was kept, and produced a good sleep of three hours. Fever disappeared; abdomen softer; bowels freely open; but the difficulty in making water, and the strainings at stools are so strong that the nervous system is shaken. Pulse is falling; tongue is getting very dry; there is a kind of bronchitis, as in typhoid fever.

September 17th, 18th, 19th, 20th—Nothing of importance during these four days; no improvement, but the pulse kept good and strong. Continued the same treatment with belladonna drops, mercurial ointment, etc.

September 21st—Worse than ever. Pains are so intense that he prefers to die. Pulse is getting weaker. I examined the prostate, and felt a little fluctuation. I proposed to perforate it with a *trocar*. Dr. Coursault prefers to wait one or two days longer, but we agree to apply another dozen of leeches to the perineum, and, in order to relieve the vital force, and present an adynamic condition of the system, we prescribed at the same time carbonate of ammonia, grs xx., infus. digital., ℥jv., syrup flor. aurant., ℥jj, to be taken by one tablespoonful every two hours; and this mixture was continued for several days. In the evening he feels a little better. Dr. Coursault suggests to give him small enemata of about one ounce, made with a strong infusion of belladonna leaves, laudanum and paregoric, of each a tablespoonful, and half a teaspoonful of tannin, these enemata to

be repeated every two hours, in order to produce narcotism, and act on the tumor as astringent. There is total retention of urine. I introduced the gum elastic catheter, and kept it in the bladder for forty-eight hours.

September 22d—Five enemata were administered, which were retained, and produced sleep all evening and night. In the morning, at 3 o'clock, the abscess opened itself into the rectum, and four tablespoonfuls of a purulent, thick, milk-like matter were brought out.

September 23d—Since this date he was greatly relieved. Pulse is good; skin is moist, and he is all the day under the influence of narcotics. Withdrew the catheter; ceased all medication, with the exception of carbonate of ammonia, and ordered good nourishment.

September 24th, 25th, 26th,—Improving rapidly. Pus from the rectum comes out freely, plentifully, and of a good nature.

September 27th—He is convalescent; the purulent discharge has ceased to flow, and the prostate is in its natural state. He has good appetite. I ordered enemata of cold water every morning for ten or twelve days, after which lapse of time he was perfectly recovered.

The facts which I think worthy of notice in this case are the following:

1st. It was an *hydiopathic* prostatitis, produced by a rheumatic and malarial cause.

2d. The happy result was due a great deal to the patient's strong, sanguine and healthy constitution, which allowed us to use an energetic antiphlogistic treatment, with repeated bleeding by leeches, and thus was prevented two or three times the diffusion of the inflammation to the peritoneum.

3d. The benefit obtained by the belladonna, which, though administered in large doses, and for long time, was tolerated, without ever having produced any physiological or poisonous effect.

Clinical Cases. Read before the Association, November 23d, 1874. By Dr. A. Pettit.

Mr. President and Gentlemen of the Association:

I regret to say, that I have allowed circumstances to prevent me from preparing such a paper for your consideration, as I should and would like to have brought before you. In its stead, however, I have made a brief report of some cases, possessing interest either from their own novelty or the novel treatment practiced.

I will premise that hypodermic medication is a favorite method with me, and though I have used but few agents in this way, I am strongly impressed with its advantages, and intend to practice it more extensively in the future.

Case First. Was called at 10 a. m., August 18th, to see a negro girl, 17 years of age. Found her having occasional convulsions; feet swollen and had complained of headache. She had arrived at near full term of pregnancy. Thinking the convulsions were puerperal, I demanded a vaginal examination, which she positively refused. I wished to procure some of her urine for examination; but she refused to pass any, or allow it to be drawn by a catheter. So, becoming disgusted with the woman's conduct, I ordered a cathartic and warm bath, which were to be followed by a good dose of chloral, and discharged the case. In the evening I was sent for again, and knowing the woman's life to be in imminent danger, I went. Found that the convulsions were recurring with greater frequency, consciousness not returning during the intervals. My former directions had not been carried out, and the patient was now unable to swallow. On making a vaginal examination, I found the os a very little dilated, but the membranes were ruptured and head presented. The uterus was making no efforts to relieve itself, and I was unable to procure any urine by the catheter. To control the convulsions I administered chloroform, and kept her considerably under its influence most of the night. The frequency of the convulsions was much diminished, she only having them when the effects of chloroform subsided. To hasten uterine action I administered the warm douche to the uterus, and repeated it three or four times. It did not seem to do much good, for uterine action was very feeble. The general condition of my patient seemed better, and she could now swallow fluids, so I gave her a good dose of chloral, and ordered it to be repeated whenever she became restive; also ordered 30 drops of Bat's ergot every hour and a half until I returned. At breakfast next morning I engaged Dr. Watkins to see my patient with me at 10 a. m. Arriving a little before the Doctor, I found she had had few convulsions, the uterus was acting much better, and os sufficiently dilated for the use of the forceps, but she was still unconscious. Skin cold, and heart acting very feebly. I ordered Simpson's friction, and the application of dry heat, also that a hot toddy be prepared for her. Before these directions could be carried out the Doctor came in, and I remarked to him, that my patient was much exhausted, and her heart was failing so rapidly, that I feared she would die. He immediately suggested that I would give the whiskey hypodermically, and deliver at once with the forceps. He prepared the whiskey, diluting it with equal parts water, and I administered several syringefuls, inserting the syringe deep into the muscular tissue. I now put on the forceps, and in a very short time delivered her of a live child, but before the delivery was accomplished she had reacted finely and had a good pulse. I examined her urine next day, and found it albuminous. This woman made a good recovery. I had no more trouble with her or the child. The hypodermic injections produced no abscesses.

Case Second. Was called at 3 p. m. (Oct. 20th), to see an old negress, whom I found in an extreme state of collapse from an acute attack of diarrhœa: her skin was cold and bathed in a profuse perspiration; her voice was a feeble whisper, and she was almost quite pulseless at the wrists. I ordered sinapisms, dry heat and surface frictions; also a hot toddy, which was immediately ejected. I now gave her, hypodermically, 5 or 6 syringefuls of whiskey and water, equal parts of each; administered it in both arms and one thigh; also ordered a bismuth and chalk mixture, each dose containing an eighth grain of morphia. I had no idea that my patient would react, but as I had to visit the same neighborhood that evening I called at the house, and to my surprise, found patient not only alive but slowly reacting. Feeling somewhat encouraged, I repeated the hypodermic injections of whiskey and water, giving 7 or 8 syringefuls, as before, and continued other treatment. Next morning found patient much improved, and in two days I discharged the case.

In neither this case or the preceding one did any worse results follow the injections than a slight tenderness of the limbs injected.

Case Third. Was called, Aug. 2d, to see a large fat woman, who had arrived at, or about, the seventh month of pregnancy; found her very much jaundiced, and her urine seemed highly charged with bile, though I did not examine it with reagents. She complained of much pain in right hypochondrium and some pain in hypogastrium; bowels constipated and abdomen tympanitic; had a feeling of coldness and heaviness about the hypogastrium, and had not felt movements of child for some time. Vaginal examination revealed nothing, except an abnormal heat about the parts. Relieved pain by hypodermic injections of morphia and atropia, and gave a purgative dose of calomel and soda, which was followed by a dose of oil and turpentine. I thought the fœtus was dead, but not being sure, concluded to keep patient under the influence of opiates for awhile to avoid miscarriage. She got along more comfortably, but still complained of some pain in right hypochondrium and hypogastrium. Being called from the city I instructed my patient, in case she did not get along well, to send for Dr. Watkins. On my return in two days, I found that the doctor had delivered her of a still-born child, which was much macerated and decomposed. As the fluids discharged were of an offensive odor, he had ordered carbolized vaginal injections. I ordered a continuance of the same, and as she seemed to get along well, paid her only two visits and discharged the case. In eight days I was called to her again, and found her suffering from very severe tetanus. Vaginal examination revealed nothing except an abnormal heat of parts; treated her with chloral and hypodermic injections of morphia and atropia, but she died in a few days. I was unable to procure a post mortem examination.

Case Fourth. Was called to see an Irish woman on morning of the 8th October; found she had been in labor for 36 hours; was very much exhausted and was becoming feverish; waters had ruptured and head presented; pains were producing much suffering, but were not effective. Head was not larger than usual, and her pelvis was roomy, so I put on the forceps and delivered without any trouble. There was no more hemorrhage than normal. Visited patient in the evening and again next morning; found her doing well; visited her again on 12th and 15th, and found her, at each visit, doing finely. At my last visit she was in exuberant spirits, and wished to get up, but I ordered her to remain in bed until I saw her the next week. On the morning of the 19th, being in her neighborhood I called in to see her, expecting to find her doing so well, that I would discharge the case. You may imagine my surprise, when on entering the house I found her dying, apparently of heart paralysis, or heart clot. On inquiring into the circumstances of the attack, I learned, that she had been up two days, and that on the day before she had cooked breakfast and dinner, entertained considerable company, and taken several toddies with her friends; during the night she had an attack of cholera morbus, which prostrated her very much, nevertheless she got up next morning and cooked breakfast; after her husband left the house she went out in the yard to have an action on her bowels, during which operation she fainted, and was carried in the house. It was at this time that I happened in and found her cold and pulseless; face much cyanosed; respiration suspirious and voice a whisper, but she was conscious and rational; the heart's action was so feeble and frequent, that I could discover nothing by auscultation; the face expressed a *great deal* of anxiety, and she was very restless, tossing about continuously in spite of my instructions to lie down. I ordered sinapisms, frictions and applications of dry heat; administered Hoffman's anodyne and aromatic spirits of ammonia. She died in less than a half hour after I saw her. I afterwards regretted that I did not try the hypodermic injection of atropia or whiskey. Her death occurred on the 10th day after delivery, but I do not think it attributable to any accident of labor, but rather to over-exertion during the prostration resulting from the choleraic attack.

Case Fifth. Was called, October 26th, to see a negro man, whom I found to have well pronounced tetanus, but not of exaggerated severity. He was a steamboatman, and the only injury he had sustained was a contusion of one foot from the falling on it of a piece of iron; there was little evidence of the injury when I saw him. I gave him cathartics from time to time, as he seemed to require them, and administered chloral and hypodermic injections of morphia and atropia pro-re-nata. He is now convalescent and walking about, though he still has difficulty in opening his mouth.

I will state in concluding my report, that I have recently had several cases of continued fevers, in which I have administered carbolic acid with apparent benefit.

My first case was that of a man whose fever continued its regular course with daily exacerbations, in spite of quinine, arsenic, mineral acids, digitalis, and all ordinary anti-periodics and febrifuge mixtures. I finally resorted to the administration of quarter-drop doses of carbolic acid, once a day, by hypodermic injections; he immediately began to improve, and in less than a week was discharged convalescent. My next two cases were children, to whom, having exhausted anti-periodic and febrifuge mixtures, I administered two-drop doses of carbolic acid in combination with mineral acids, repeating dose three times a day; patients very soon began to improve and were discharged convalescent. I can not say that the carbolic acid was curative, but it looked so probable that I shall continue to use it under similar circumstances.

NOTICES OF NEW BOOKS.

Annales d'Oculistique: Tome lxxii, 11e série, T. 2, 1m and 2l livraisons. Juillet et Aout 1874.

Mr. Giraud Teulon contributes an interesting and instructive article on *symptomatic attitudes* in cases of paralysis of the muscles that move the eyeball. The article is no less than a *mémoire* read by the author at the Paris Academy of Medicine on the 31st day of March, 1874. The disturbance resulting from double images in cases of paralysis of the ocular muscles is so great, that the patient will submit to any inconvenience in order to get rid of double vision. He will contrive to take such a posture of his head and body as will enable him to bring that part of his field of vision in which movement is not impaired to bear on the object he wishes to see. It follows therefore that his *attitude* presents a peculiar appearance in each form of paralysis, such attitude being necessarily identical in all cases of the same kind, and different as regards all other cases of paralysis—a *typical attitude* obtains in each form of muscular defect, and constitutes a differential diagnostic sign. The patient will very naturally find out what part of his field of vision is free from double images. If rotation of the eyeball to the *right* be impaired, double images will obtain in the *right half* of the field, the left half being entirely free from the inconvenience; for, as says the author, double images appear only when the deficient muscular power *should*

take an *active* share in producing movement; the patient will then turn his *face* to the *right*, in order to bring *immediately in front of him* the *left half* of his field of vision, which alone admits of simple binocular vision. If rotation of the eyeball *downwards* be impaired, double images existing *below the horizontal line* or lower half of the field, the patient will keep his face bent *down* towards the ground in order to bring objects *in front of him* within the range of the *upper* half of the field of vision where single binocular vision exists. "When the motor power of the eyeball is deficient, the various positions of the head of the patient arise from the necessity of bringing into use that part of the field of vision in which simple binocular vision is preserved, which result obtains when the field of simple vision is brought directly in front of the patient. The field of vision being necessarily connected with the head of the patient, said head will have to rotate around an axis parallel with the limit of the diplopia, and in the direction of the diplopia."—Grafe.

The *attitude* of the patient in a case of paralysis is then only intended, as says Giraud Teulon, to compensate the deficiency in the movement of the eyeball; the best illustration of such compensation is furnished in the case of the movements of the head and neck of birds, whose eyes are nearly motionless. Now, let us suppose that a patient is unable to rotate the *left eye* to the *right*. In such a case the field of vision is divided into two halves, *left* with simple vision, and *right* with double images: in order to put the left half of the field, where single vision obtains, in relation with the median line, i. e., in front of him, the patient will have to turn his head to the right.

Let us now examine the cases of paralysis of motor power for rotation of the eyeball *directly* upwards or downwards. Giraud Teulon here recalls the following laws: 1st, the vertical meridians must remain and be maintained vertical; 2d, the locomotion of the pupil is procured by the action of one of the recti muscles (superior or inferior) combined with that of the *contending* obliquus muscle. Each of the muscles constituting the power acting on a crooked lever, its action results from three components, vertical movement, horizontal and oblique. When rotation upwards is deficient, double images obtain with the following characters: 1st, unequal height (vertical component); 2d, images apart from each other horizontally (horizontal component); 3d, images inclining towards each other (oblique component). Now,

the *attitude* of the patient, intended to neutralize diplopia, will be the result of the various actions directed to compensate the three deficient powers, the compensating powers being the muscles of the eyes and of the neck acting in such a way as merely to *complete* the movements that should have been performed by the paralyzed muscles. The patient will turn his face upwards, in order to supply the deficiency in the rotation of the eyeball upwards; the deficiency in the horizontal component will be supplied by the abducting and adducting powers, so that one of the eyes will be placed in abduction and the other in corresponding adduction. The patient will furthermore lean his head towards one of his shoulders, striving to correct the inclined position of the images: here again, the movement of the head is intended to *complete* the suspended action of a deficient muscular power, the head being directed in such a way as to effect parallelism of the cardinal meridians, which condition is altered by the paralysis.

Physiology teaches, that in oblique or intermediate direction of associated vision, the cardinal meridians in both eyes are maintained exactly parallel with each other, such meridians being moved simultaneously and to the same extent in the oblique direction of intended binocular vision. Now, in the above case, one of the muscles intended to rotate one of the eyes upward is deficient; it follows that the physiological rotation of the cardinal meridians in that eye is impaired; the unequal height and horizontal separation of the images will be accompanied by the inclined position of one of them, on account of the deviation of the meridian *missing;* the head of the patient supplies the movement by rotation on the antero-posterior axis, in such a way as to obtain parallelism of the meridians; it accomplishes the work that should have been performed by the deficient component power (that of the physiological deviation of the meridian).

We can therefore sum up by saying, that in the new attitude presented by the head and the eyes, the associated muscles of the neck and eyes have merely been called to *complete*, to *continue* the movements that were deficient through the muscular paralysis, and thus to neutralize diplopia.

In a case of paralysis of the levator muscles, the head of the patient is turned upwards, and bent on one shoulder; but furthermore *the face is turned to one side.* This additional feature in the attitude of the patient is necessitated by the fact that simple

binocular vision has been restored *in one direction only* by the supplementary actions above described, i. e., to the extreme right or extreme left. It follows that the patient is in a condition of paralysis as regards one of the lateral directions; the direction of simple vision no longer corresponds with the median plane of the body. The face of the patient must be brought in a direction contrary to that of his line of vision (direction of the object to be seen) in order to bring the look of the patient to correspond with the median plane. It will be observed that the lateral deviation of the head is in a contrary direction to the lateral movement of the optic axis brought on by the action of the ocular muscles.

Another symptom in such cases is a peculiar stiffness of general attitude in the whole body (Grafe), which we can easily account for by the struggle the patient has to go through to maintain his complicated attitude.

Conclusion.—The attitude presented by the patient, in non-complicated cases, points out the kind of paralysis.

The first thing to do is to make out the paralyzed eye; the patient should try to walk with one eye covered; when the sound eye happens to be covered, giddiness follows and walking is disturbed to a more or less extent. Suppose the left eye to be paralyzed, and the head of the patient thrown backwards, we shall conclude that rotation of the eye upwards is deficient. Then if the *left* eye (paralyzed eye) is brought in *adduction*, such position of the eye teaches that the horizontal component power residing in the paralyzed levator muscle should have acted as an *adductor*, for we have shown that the adduction of the paralyzed eye is produced by a *supplementary* action of the associated non-paralyzed muscles. Now the *superior rectus* muscle being the only levator muscle provided with an adductor component power is the paralyzed muscle (the one whose action is supplied).

The position of the head would likewise lead to the same diagnosis, the movement of the head being intended to supply the deficiency of the paralyzed muscle as to its action on the meridian of the eye. In our case (left eye paralyzed), the position of the head teaches that the upper extremity of the vertical meridian *should* have been inclined to the right by the *physiological* action of the muscular powers of the eye. The only muscular power possessing such an action on the meridian, being the *rectus superior* muscle, we again come to the same diagnosis,

V. Grima, M.D.

A Manual of Surgical Emergencies.

This is the title of a book of one hundred and eighty-three pages, recently offered to the medical profession by William Paul Swain, F.R.C.S., Surgeon to the Royal Albert Hospital, Davenport, and republished in this country by Lindsay & Blakiston, Philadelphia.

"Surgical emergencies" is a title calculated to make a book popular and sought after. It sounds well, and for the general practitioner to be prepared for such difficulties, is what "is devoutly to be wished for." Therefore if it sustains what it lays claim to, it will be a valuable addition to any medical library.

In the preface it says, "It is intended to supply what the author thinks is a widespread want, viz., a small book containing directions for the immediate treatment of all the various emergencies with which the general practitioner may be called upon to deal at any moment." It also claims "to be little more than a compilation from the best and most recent works on surgery."

It remains to be developed as to whether, in a careful examination of the work, it will sustain the author's pretentions. We will proceed to notice the leading points in each chapter.

Chapter 1 is devoted to wounds of the scalp, fracture of the skull, trephining, foreign bodies in the ear, nose, etc.

The directions in the treatment of scalp wounds are generally good. Dr. Swain advises that the silver wire sutures be used in bringing the flaps together. It may be that the silver wire sutures possess some advantage over the silk, but I have invariably used the silk in such wounds, and have not in a single case had bad results from it. The comments on fractures of the skull are well-timed, and the distinction between those cases that justify operative interference, and those that do not, are in keeping with sound surgical rules. Dr. S. states, "that simple fractures with depression, and comminuted factnres unaccompanied with a wound of the scalp, without brain symptoms are not to be interfered with." "In children, compound depressed fractures, if no brain symptoms are present, should be let alone." "In compound comminuted fracture with depression, the bone should at once be elevated, and if needful the trephine should be used."

In the management of wounds through the thickness of the lip, Dr. S. advises the hare-lip pins. My custom is not to use the pins in treating wounds through the lip, or in the operation for

hare-lip. I use simple silk sutures. Prof. Frank H. Hamilton states that his success in the operation for hare-lip has been better with the silk sutures alone, than with the pins. In the treatment of troublesome epistaxis, the introduction of an India-rubber bag through the nasal cavity, and inflating it with air, or injecting it full of ice water, is recommended. This is a good suggestion. I once introduced the œsophagus of a sheep, and injected it full of cold water (tying both ends), which succeeded in arresting the epistaxis, after plugging and all the ordinary means had failed. It was in country practice, and India-rubber bags were inaccessible.

Chapter 2 treats of injuries and operations of the eye and orbit. Dr. Swain states that he is under obligations to that distinguished ophthalmologist, Mr. G. Lawson, for assistance in preparing it. The fact that Mr. Lawson supervised the preparation is a proof of sound advice in this especial department.

Chapter 3 gives an account of foreign bodies in the larynx and trachea, with operations on same. Mr. Guersant, for twenty years surgeon in charge Children's Hospital of Paris, states that the success in tracheotomy for pseudo membranous croup and diphtheria under two years old, has not been as great as where the children were over two years old. This fact is not mentioned by Dr. Swain, or any other author whose writings I have examined. But as this suggestion comes from such high authority, I mention it. To induce artificial respiration, Sylvester's method is recommended.

In chapter 4 is given an account of fractures of the ribs, wounds of the chest and lungs, and in the treatment of fractured ribs good suggestions are given; the broad bandage, or broad pieces of adhesive plaster, is the dressing. In commenting on the treatment of wounds of the lungs, Dr. S. states: "The treatment must be perfect rest of the parts by strips of plaster from the spine to the sternum, arrest of all hemorrhage externally, immediate closure of the external wound, application of ice to the chest externally and exhibition of it internally." So far so good, but no mention is made of the free use of some opiate. A patient with a penetrated wound of the lung should be kept positively under the influence of an opiate for twenty-four or thirty-six hours. Dr. J. M. Keller, Louisville Medical College, made an interesting report of a case of wound of the lung, in the *American Medical Weekly*, showing the great advantage of the

free use of opium as above described. There is much valuable information in this chapter. Next we come to a consideration of the management of fractures and dislocations. Dr. S. has shown good judgment in preparing most of this chapter. Some of the apparatuses recommended for fractures are unnecessarily complicated. Prof. Gordon's appliances for fracture of clavicle and collar fracture, Dr. Swain very properly states, that drawing the shoulders back with a figure of eight bandage is inadequate to keep the fractured ends of clavical in place. No treatment yet invented or recommended has proven successful in preventing deformities following this fracture. As successful as any dressing, and by far the most comfortable one, is a proper axillary pad, the arm confined to the side of chest with a bandage, and forearm supported in sling.

Chapter 6 is an account of wounds of the abdomen and abdominal viscera, and the different kinds of hernia. The rules given for the management of these difficulties are generally endorsed by surgeons. The recommendation of the aspirator as an aid to taxis, in the treatment of strangulated hernia, as was first suggested by the inventor of that instrument, Mr. Dieulafoy, is the most recent improvement mentioned in the chapter.

In chapter 7, Dr. S. treats of injuries of urethra, bladder, retention of urine from stricture and enlarged prostate gland. Correct advice is given for the use of the aspirator in puncturing the bladder over the symphisis pubis in retention of urine from stricture and other causes. No mention is made of Nélaton's flexible catheter for the relief of retention of urine from prostatic enlargement. I am satisfied that it is the best instrument known to the profession for the management of retention due to that difficulty. It is so soft and flexible that it can be tied in knots. It can be introduced with more ease than any other instrument in prostatic enlargement, and with less pain to the patient. No practitioner ought to be without a set of these catheters.

Next is chapter 8. It is rather lengthy, and contains much valuable compressed truth. His recommendation for the management of incised and punctured wounds of the knee are conservative, and generally endorsed by the profession. Dr. S. recommends Lister's long splints in the treatment of fractures of shaft of the femur in the adult to the exclusion of all other dressings. No mention is made, in the treatment of this facture, of the extension by Swinberg's plan, the plaster of Paris and sand bags.

The remarks on dislocations and their treatment are generally received by the profession. In compound injuries of the knee-joint, especially gun-shot wounds, "if the fracture is confined to the joint ends of the bone," Dr. Swain says excision may be performed. Experience in resections of the knee joint for acute injuries, does not in my opinion justify a resort to it. I am satisfied that the success of excision of the bones of the knee joint for compound injuries with many of the late Confederate surgeons, was anything but encouraging. Amputation in such cases would be proper.

In referring to the treatment of fracture of patella, no mention is made of the iron ring which was given to the profession by Dr. Gibson, of St. Louis, Mo. The use of this ring was publicly brought before the profession several years ago. Therefore Dr. Swain cannot plead ignorance of this mode of treating this fracture. Our European brothers are most generally characterized by a love and admiration for things that are peculiar to their country, and none more so than the English members of our profession. The claims of American Surgeons are entirely overlooked in this work.

The 9th chapter gives directions for the management of the emergencies in parturition. The author acknowledges himself indebted to Dr. Alfred Meadows for assistance in preparing this chapter. The fact that Dr. Meadows had anything to do with the preparation of the chapter is sufficient endorsement of it to the profession.

In chapter 10 is an account of poisons and their antidotes. There is nothing new given here, but the advice for the management of such cases is good.

In chapter 11, we find a very intelligent history of Lister's antiseptic treatment, by Dr. Bishop. Carbolic acid, boracic acid and chloride of zinc, are the three antiseptic remedies given by Prof. Lister. There is only passing notice ot the two latter, but rather a full account of acid carbolic and his manner of using it. The theory of Lister is, that carbolic acid acts by destroying the lower forms of life (animalcules), therefore it must be a good cleansing and healing remedy for wounds. I am satisfied that its indiscriminate use has caused many members of the profession to form unjust prejudice against it. As a general rule, the solutions applied to wounds are too strong, and therefore prove too much of an irritant. Prof. Lister uses many different strengths

of carbolic acid solutions, and he states: "It is desirable to protect the healing parts from the irritating influence of the antiseptic itself. * * * * This is done by interposing a protective layer consisting of some material unirritating in itself, and as far as possible impervious to the antiseptic agent. The protective commonly used is composed of thin oil silk, varnished with copal and then coated with a layer of dextrin." I never met with a surgeon who observes Prof. L.'s rules in the application of carbolic acid to wounds. I am of the opinion that when it is properly used, it is an excellent remedy in the treatment of wounds and ulcers.

Chapter 12 brings us to the close of the volume. Dr. S. here gives us the various dressings for fractures, the use of the aspirator, and the modification of Esmarch's blood bandage. The dressing recommended for fractures are those generally used by modern surgeons. The descriptions of the use of the aspirator are very plain and simple. The modification of Esmarch's bandage is that described by Mr. Cripps, in *Lancet*, Oct. 11th, 1873.

After an examination of this little book, it is my opinion that it is well compiled, containing much compressed surgical information, and that it will prove a valuable aid to the general practitioner in dealing with emergencies in surgery and parturition. I can therefore recommend it to the profession as a reliable practical work.

I know of no class of surgeons whose experience so well fitted them to write a work on "Surgical Emergencies," as those who served in the Confederate army. During the last two years of that war, they were cut off from the rest of the world both by land and sea, with none of the usual surgical resources within their territory. The unjust and oppressive political ruling of their country has so much impoverished it, that they have been unable to write and publish works on this and other subjects, in which their experience would be valuable to medical science.

J. W. T.

Croup in its Relations to Tracheotomy. By J. Solis Cohen, M.D., Lecturer on Laryngoscopy, and Diseases of the Throat and Chest, in Jefferson Medical College. Philadelphia: Lindsay & Blakiston; 1874.

This essay, referred to the Medical Society of the State of Pennsylvania, was ordered to be printed in their Transactions

for the current year. It is based on a careful study of the published records of more than five thousand cases of tracheotomy in croup, performed in various portions of the world. It would be impossible to read anything newer on the interesting subject of croup and tracheotomy, or in a form so concise, to find anything more complete than this pamphlet. The author has in these few pages evidenced a vast amount of information on this subject as a specialty. At the foot of each page notes indicate the original authors consulted, with the exact page for each quotation. We are truly startled when we realize the labor involved in this simple essay. There is presented, indeed, nothing original—nothing which may be considered as belonging especially to the author. But it is the fruit of patient researches, executed by a mind more than ordinary in calibre, accurate in judgment, and undaunted in confidence, when this confidence is sustained by wisdom or commanded by zeal.

It seems that in Philadelphia tracheotomy has heretofore been less successful than in other places. "Its results in Philadelphia * * * have been so disheartening that many practitioners refuse to sanction tracheotomy in croup under any circumstances. This radical feeling is wrong * * * Early failures may be followed by ultimate successes" (p. 5).

In proof of this, the author devotes about twenty pages to studying the statistics of tracheotomy published in different parts of Europe, from the time when this idea prevailed (that the surgeon's duty ended when he had opened the trachea) until the present epoch, when the "after-treatment" has obtained for itself so important a place. A favorable age, a good selection of cases, skillful operations, assiduous after-treatment—these are the elements of success, and it is in a measure to assist in a better appreciation of these elements of success, that this paper has been compiled.

Attention is invited to four main topics to be discussed in succession; these are—

1. The indications for the operation.
2. The points of importance in connection with the operation itself.
3. The after-treatment of the disease and of the surgical wound.
4. The casualties which prevent recovery.

It is impossible to analyze separately these fundamental and

fourfold parts of the work. So concise and complete is it, we should need to reproduce it entirely; consequently it must be sought in the book itself. Besides, the author himself has perfectly condensed it in his conclusions (page 78):

1st. That there are no insuperable contra-indications to tracheotomy in croup.

2d. That the administration of an anæsthetic for the purpose of controlling the child's movement, is admissible in performing the operation, but that it should be used with great caution.

3d. That a careful dissection should be made down to the windpipe, and hemorrhage be arrested before incising it, whenever there is at all time to do so.

4th. That the incision should be made into the trachea as near the cricoid cartilage as possible, to avoid excessive hemorrhage and subsequent accidents, which might occasion emphysema.

5th. That a dilator should be used, or a piece of the trachea be excised, whenever any difficulty is encountered in introducing the tube.

6th. That the tube should be dispensed with as soon as possible, or altogether if the case will admit of it.

7th. That assiduous attention should be bestowed upon the after-treatment, especially of the wound, and that a skilled attendant should be within a moment's call for the first twenty-four or forty-eight hours immediately following the operation.

J. C. F.

History of the American Ambulance established in Paris during the Siege of 1870-71, *together with the Details of its Method of Work.* By Thomas W. Evans, M.D., D.D.S., Ph. D., President of the American International Sanitary Committee, Commander of the Legion of Honor, Grand-Croix of the order of St. Stanislaus of Russia, and Commander, Officer and Member of various orders. Large 8vo. Pp. xxxviii; 694. London: Sampson Low, Marston, Low, and Searle; 1873.

If the subject of this ponderous volume were merely what is stated in the above title, we might conclude at once that it is another of those big works on little subjects which an indulgent public are too often invited to pay for and perhaps expected to read. An examination of its contents will show, however, that by far the largest and most important part of the book has no direct relation to the Franco-German war nor the American Am-

bulance. In the preface, Dr. Evans informs us that this may be considered the first volume of his "General History of Sanitary Associations during the Franco-German war of 1870–71." He does not hint at the number of volumes that are to succeed this; but, if we may judge by the foretaste, the entertainment is projected on a most liberal scale.

The "General Subject Headings" are as follows:

I. An Account of the Formation of the American International Sanitary Committee of Paris, together with the the History of the American Ambulance. Pp. 1–93. By Dr. Thomas W. Evans, President of the Committee.

II. On the Establishment of Army Hospitals. Pp. 93–251.

III. On Tents and Tent-Barracks. Pp. 251–445.

IV. On the Special Organization of the American Ambulance. Pp. 445–575.

These three articles by Dr. Edward A. Crane, Secretary of the Committee.

V. On the Surgical History of the American Ambulance. Pp. 575–681. By Dr. John Swinburne, Chief Surgeon to the Ambulance.

VI. On the Medical History of the American Ambulance. Pp. 681–689. By Dr. Wm. E. Johnston, Physician to the Ambulance.

VII. Appendix. Pp. 689–694, together with a number of plates illustrating the construction of the Ambulance.

In the beginning of part first, Dr. Evans tells us how he called a meeting of the American residents in Paris, and how he was chosen President of the International Sanitary Committee. The following quotation indicates that his object was rather to afford an example of what might be done, than actually to achieve great results.

"And he particularly insisted on this consideration, viz., that one of the chief objects with Americans, under the circumstances of the case, should be to make known to European sanitarians by practical examples, those systems of hospital construction, transportation, and appliances for the care of the wounded, which had received the sanction of American experience as most suitable in war; observing, that such an addition to the sanitary knowledge of Europe would be far more valuable and useful than any mere giving of material aid to either French or German ambulances, even though it were possible to collect millions of francs for that purpose; and that, therefore, a special aim of the proposed action on the part of the Americans in Paris should

be the organizing of one or more model Field Hospitals, with their accessories, on the plans which had been found so satisfactory during the American war, for the purpose of demonstrating the superiority of the methods, adjuncts, appliances, and arrangements that had been suggested by the experiences of that great struggle, over those which were still employed in the official ambulances of Europe."

Owing to various circumstances, among which may be mentioned the rapidity of the military operations, and defects in the organization of the medical department of the French army, it was determined to abandon the original plan of forming and administering an ambulance, in the true sense of the term, and decided to establish a stationary hospital at Paris. This was pitched chiefly under canvas, but was not therefore an ambulance in reality, though capable of ready removal, had occasion required.

Dr. Evans tells us, with evident pride, how the fugitive empress fled to his house for protection, and how he conducted the "illustrious lady" to the fortunate shores of Albion. This terminated his immediate participation in the work of the hospital, for he did not return to Paris till after the end of the siege.

A large portion of his contribution to the volume is occupied with quotations from medical journals and daily newspapers, and the opinions expressed by various individuals, medical and non-medical, all highly laudatory of the "American Ambulance." Dr. Evans himself is not at all backward in praising its excellencies and commemorating its achievements. His own instrumentality in organizing the enterprise, his efforts in setting it on foot, and his liberality in sustaining it with his own means, are told with a pride not obscured by any veil of modesty; and the candid reader will look upon this volume as a monument contrived, though but very partially executed, by Dr. Evans for his own special glorification.

The contribution of Dr. Crane forms the largest, and undoubtedly the most valuable portion of the volume. The section "On the Establishment of Army Hospitals" takes up the subject from the earliest historical records pertaining thereunto, beginning with Herodotus, and by a rapid view descends to our own times. Great industry and extensive research are indicated, which reflect credit on the author, and will prove interesting and valuable for reference.

A fact noted by this writer, that the French ambulance sys-

tem has undergone scarcely any improvement during the present century, is dishonorable to their national military system. It appears to have some radical defects, among which may be mentioned that the *personnel* of each company is largely in excess of the *materiel;* and, being organized as a unit, it lacks flexibility and adaptability to the varying wants of field service. Moreover the medical staff of the French army has no separate organization of its own, but is merely an appendage to the quartermaster's department and dependent upon it. The diet list and issue of medicines are limited to specified forms, thereby seriously curtailing the ability and effectiveness of the service. Such a belittling and cramping of an important branch of the service must greatly damage both the self-respect and the usefulness of those who belong to it. Its effect was, in fact, to lead the medical staff of the army, at the outbreak of the war, tacitly to resign the ambulance system to a voluntary civil association styling itself "*La Société de Secours aux Blesses.*" This society, though founded in 1864, was never thoroughly organized, had no official recognition and no fixed revenue. Consequently, when the time of need came, they were not ready; and the history of the several ambulance companies sent out by them exhibits a general tale of failure to be where they were wanted, and of inability to perform the work needed. Besides, to illustrate how much importance may attach to a name, the projectors and supporters of these voluntary organizations restricted their attentions almost exclusively to the wounded, ignoring the fact that the diseases incident to the hardships of camp life are far more destructive to soldiers than the casualties of battle. How much more effective might have been these contributions, had they been turned over to a regularly organized and working branch of the public service—a medical department of the army, in the true sense of the term—as was the case in both sections of the country in our late civil war.

We have seen above how much may be in a name; by contrast we shall now see how little there may be also. The term ambulance originated in France, and signified a hospital and its service accompanying the army in its movements. Latterly its signification has there been extended to all military hospitals, stationary as well as travelling. On the other hand, in this country the use of the word ambulance has become restricted to a particular feature in the establishment—the wagon; and, in

like manner, we may eventually find the use of the word *chamber* restricted to a familiar utensil appertaining thereto.

On the subject of ventilation of hospitals, Dr. Crane thinks that too much importance is attached to cubic space, as compared with the renewal of the atmosphere within. In this he is right theoretically; but practically considerable space is required, in order to allow the air to be renewed without producing dangerous or disagreeable currents. A draughty atmosphere is only less objectionable than a close one.

Commencing with the idea that the primary and chief requisite of a hospital has been to afford shelter to the infirm, he makes a brief review of different systems of military hospitals—such as permanent structures, temporary ward-pavilions, ward-barracks, huts, buildings erected for other purposes—and insists that pure air is of more importance than shelter. This statement may be true under most circumstances, but we can easily imagine such conditions of temperature, rain and wind, as would make shelter of essential importance. It is plain and easy to decide that certain conditions are preferable, and preparations should be made in season to secure these conditions; but, when the time of need comes, it rarely happens that they are all practicable, from defects of organization or unforeseen accidents, and then the plan must be selected which is most available and promising for useful results.

Part II. of Dr. Crane's contribution is "On Tents and Tent-Barracks," and extends from page 253 to page 447. The subject of tents takes us back to the earliest historical ages, and he commences with the first authentic records. A large amount of information is given, most of which, though interesting in an historical sense, is only incidental to their use in hospitalization. In fact he avers that little was thought, and still less systematically done, for hospitalizing the sick and wounded in armies as late as the end of the seventeenth century; and that the first specific recommendation of tents for this purpose, within the scope of his research, was in the year III. of the French republic.

Without dwelling at length on separate points, much space is occupied in describing the shape, materials, and mode of construction of the various tents now used throughout the world, and their adaptedness to their special purposes. This branch of the subject closes with a description of the tent designed by Dr. Thomas W. Evans, to which, of course, preference is given.

Part III. of Dr. Crane's contribution is "On the Special Organization of the American Ambulance." The description is very minute, and incidental topics are also touched on, so that 130 pages are occupied with this part. The superiority of tent hospitals claimed over others must consist in more rapid renewal of the contained air, which is readily effected through the canvas covering, supplemented during hot weather by openings as free as may be required. The great difficulty has always been to regulate the temperature in cold weather, owing to the rapid dissipation of heat through the canvas covering, and the absence of any available means for distributing and storing heat, so as to obviate sudden changes of temperature. The grand feature of this hospital undoubtedly is the simple and effective plan adopted to secure a uniform temperature within a large extent of canvas, a description of which is quoted at large from the work.

"Beneath the first tent, at the northern end of each pavilion, a cellar was dug about 6 ft. deep and 6 ft. wide; its length at right angles to the axis of the tent was 8 ft. at the bottom and 15 ft. at the top; the walls were vertical on three sides; on the fourth side, as the dimensions given will indicate, the wall had the form of an inclined plane. This inclined plane was *outside* of the tent, and was furnished with steps leading to the bottom of the cellar; it was moreover covered, to keep the rain out, with a wedge-shaped wooden roof. The cellar space beneath the tent was therefore represented by an oblong solid 6 ft. high, 6 ft. wide, and 8 ft. long, and its position under the tent may be said to have been central and lateral; that is to say, a section halving the tent transversely, if prolonged, would halve also the cellar on a line corresponding with its long axis, while the cellar extending 8 ft. under the tent, projected 6 in. beyond a section halving the tent longitudinally. From this cellar, and along a line 18 in. distant from and parallel with the long axis of the pavilion, a trench was dug to a point about 2 ft. beyond the last tent, a distance of 76 ft. in the pavilion of six tents, and of 62 ft. in the pavilion of five tents. The trench was 3 ft. 3 in. deep where it entered the cellar; at the distal end it was 16 in. deep; its bottom thus formed a slightly ascending inclined plane. At the top it was 20 in. wide, and at the bottom 8 in. wide; it therefore represented in section a blunt wedge. Having thus prepared the cellar and trench, the heating apparatus was introduced.

"This consisted of a stove and certain accessories. The stove was very simple in its construction; it was of cast iron, 28 in. high, 18 in. diameter, and arranged for burning either coal or wood. The stove was placed on the floor of the cellar, with its back directed towards and against the open end of the trench;

an elbow of 6½ in. pipe was adjusted to the chimney, the under surface of the horizontal branch of the elbow resting on the bottom of the trench; straight sections of 6½ in. pipe were now laid the whole length of the trench to the point where it terminated—2 ft beyond the last tent; here an elbow was attached to the pipe, which rose perpendicularly out of the ground to the height of 13 ft., where it was crowned with a water cap. The apparatus for heating the pavilion having been established in the way described; a fire might have been built in the stove; the draught would have been found excellent, and the amount of caloric thrown into the tent altogether sufficient. The heat, however, would have been very unequally distributed, as also subject to great waste. To regulate its distribution, as also to economize it, something more was necessary. The first thing done was to cover over a considerable part of the trench, beginning near the stove. This was accomplished by inserting thin plates of iron into the walls of the trench about 6 in. below the surface of the ground, and covering them with earth; 45 ft. of the trench were thus covered over. The effect of this operation was naturally to create an air-current, drawing most of the heat from the stove into and *through* the trench, to the extreme end of the pavilion, where it arose into the tents from the uncovered portion of the trench In order, however, that the heat might be introduced directly into the tents (the three) nearest the stove, a grated opening (register) communicating with the trench was placed in the floor of each; this could be opened or closed at pleasure; when open, a portion of the hot air arose from the trench directly into the tent; when, closed, the hot air was forced on to the next tent, where, if the register was also closed, it passed to the next—until, in fact, it found a free opening from which it might escape into the pavilion.

"By covering a portion of the trench (the whole might have been covered, but it was not really necessary), and by the introduction of registers, a proper *distribution* of the hot air coming from the furnace was provided for. To economize all the heat generated, and to secure a constant draught of hot air along the trench, and in the right direction, brick walls were built up around the stove, completely enveloping it except on the lower half of its front, and except also, I should say, behind, where it was backed against the trench. This envelope formed a sort of air-chamber, and its object, as M. Bréhan has very well said, 'was to force the exterior air to lick the metallic surface of the stove, and take up a part of its heat, and thus send it into the trench, where along its whole length the hot air is constantly in contact with the stove pipe, which also adds to its temperature.' To the mouth of the brick envelope was attached a sliding iron door, by which the air supply was regulated. To prevent the tents becoming at any time over-heated by the current of hot air, a cut-off, or damper, was placed in the trench 4 ft. from the stove; it was simply a long thin plate of iron, attached to a

perpendicular spindle; when open, its planes were parallel with the long axis of the trench; on turning the spindle one-quarter of the way round, the plane of this plate being at right angles with the axis of the trench, the trench was closed and the current of hot air cut off. That the heat of the stove in this case might not be wasted, in pavilion No. 2 an 8 in. earthen pipe had been let into the top of the brick envelope, and laid in the ground outside of the tent; this pipe ran under the nearest barrack, into which it opened through a register placed in the floor; the hot-air chamber surrounding the stove was thus connected with a barrack in such a way that whenever the heat generated by the furnace was more than sufficient for the pavilion, the damper being turned so as to prevent the hot air entering the pavilion, on opening the register in the barrack the current of air from the furnace was let into that building. Our heat was thus economized, and a considerable saving of fuel effected."

The completeness with which the heat was utilized is apparent from his statement, that the cellar where the stove was placed was never hot, while the vertical smoke-pipe beyond the last tent was cold at the surface of the ground.

Considerable importance is attached by Dr. Crane to the advantages possessed by tents in exposing their occupants to the influence of direct sunlight. It is evident that tents receive more light through the canvas than houses do through their openings; but whether direct light, as it enters tents, has different influences on animal and vegetable life from reflected light, such as chiefly enters windows, must be still an open question.

A very important feature in all hospital arrangements is, of course, the cost; and a comparison of this with other temporary establishments shows a great difference in favor of the former, where it was at the rate of 126.82 francs for each of 150 beds; while in the tent-barrack hospital established in the park of St. Cloud, in 1871, the cost was more than 1000 francs for each of 200 beds.

The Surgical History of the Ambulance gives first a statement of the general principles of treatment. The conservative plan was preferred and practised to an extraordinary degree. As only 247 surgical cases were treated at this Ambulance, of which 126, occuring in 114 individuals, were compound and comminuted fractures, the result can only be considered a contribution on the subject of conservative surgery. Comparisons are drawn between the results at this and some other ambulances and hospitals; and considerable testimony, medical and non-medical, is adduced to show that recoveries were more frequent in the Ameri-

can establishment than elsewhere. These points are taken up in the parts contributed by Drs. Evans and Crane. Admitting the truth of their claims, the reader must judge for himself how much to attribute to the plan of construction, and how much to plan of treatment. Allowance should be made for the bad sanitary condition of Paris after the beginning of the siege. During the whole six months the ratio of mortality for the aggregate population, as compared with that of two years previously, was as 3.48 to 1; though it is to be remembered that the population was considerably augmented by the troops for defense and fugitives from the surrounding country. This increase of mortality is attributed more to bad quality of provisions than to diminished quantity on the whole (fresh vegetables and fresh meats soon almost disappeared); and it is shown that scarcity of fuel led to greatly increased death rates during the prevalence of cold weather.

The plan for disinfection practised is thus described by Dr. Swinburne.

"The barracks and tents—in order to carry out as far as possible all hygienic measures—were from time to time emptied of their occupants, and thoroughly fumigated with chlorine gas, disengaged on an extensive scale, and somewhat in the following manner:—Several large vessels or dishes were placed at equal distances along the centre of the tent or barrack, as the case might be, containing equal quantities of peroxide of manganese and common salt moistened with water. The tent or barrack was then closed, and a sufficient quantity of *strong* commercial sulphuric acid was thrown simultaneously into the vessels. Immediately the ward was filled with thick vapors of chlorine gas.

"After the chemical action had fully subsided, the board floors were taken up, the ground cleaned, swept, and covered with sulphate of iron, and the floor replaced.

"The floors, when swept, were from time to time sprinkled with a mixture of sulphate of iron and sand, which was allowed to sift through the crevices to the ground; occasionally a solution of permanganate of potash was used instead."

This practice we think highly commendable, and believe that still more important results might reasonably be expected in permanent hospital structures. An access of hospital gangrene, erysipelas, or puerperal fever, should be the signal for vacation of the ward and resort to thorough fumigation, followed by cleansing and ventilation.

Only 24 cases were treated in the medical department of the Ambulance, and its history presents nothing of special interest.

In addition to the ten plates illustrating the plan of construction of the Ambulance, there are about sixty wood-cuts descriptive of tents and tent-barrack, and of the results of surgical treatment. Some of the latter represent individuals entirely nude, except the single covering of a suspensory bag. The effect is not artistic, but rather suggestive of the uniform of a Georgia major. S. S. H.

Transactions of the Medical Society of the State of Pennsylvania, at its Twenty-fifth Annual Session, held at Easton, Pa., May, 1874. Vol. X., Part I. Pp. 454.

The volume contains a great variety of matter and numerous contributions, most of which would not repay complete perusal by a busy practitioner, or any one enjoying a large range of medical reading. After a somewhat careful examination, some points have been selected for notice.

Following the address of the President, Dr. S. B. Kieffer, and one on Surgery, neither of which presents anything new or striking, is an address on Obstetrics, by Dr. Wm. B. Atkinson, well known as the Permanent Secretary of the American Medical Association. It is a *résumé* of a year's progress in this branch of medicine, carefully collated from a large number of authorities, and valuable for reference.

Dr. Lawrence Turnbull contributes a brief and interesting paper on "Deaf-Mutism, and the Method of educating the Deaf and Dumb." The important point in this paper is the relative merits of the two methods of expression taught to deaf mutes—that of visible signs and that of articulate speech. The following quotations contain the writer's ideas, in which, we presume, our readers will concur.

In an admirable letter from B. St. John Ackers, of England, in the American Annals of the Deaf and Dumb, April, 1874, he gives his conclusions, after examining the systems all over the world, in the interest of a deaf and dumb daughter: "*Our conclusions are in favor of the 'German System' for all who have once heard, for most of the semi-deaf, and a large majority of the toto congenital.* For the remainder, I cannot imagine any system more appropriate than the 'French,' only recording our opinion in favor of fewer signs, and increased use of dactylology, and writing in the more advanced pupils." What struck him most

was the contrast in the French and German systems in the love of home. Those taught under the "French system" care comparatively little for the "holidays" for home and relations. Why? Because the institution is their home, the principal, matron and teachers, their parents and relations. "Let them be taught by the 'German System,' and this will enable them to think in the written idiom of the language of their country; will enable them to hold conversation with hearing persons, to understand much that is spoken to them, and will make them figuratively less deaf and truly less dumb; indeed, not dumb at all."

There ought to be a commissioner in every State to examine and classify the deaf and dumb, where all who are found to possess any degree of hearing or any remnant of speech (having lost hearing after learning to talk), or any who manifest a marked facility in vocal utterances, should be assigned to the articulating schools, while all others should be placed in the older establishments, where the language of signs is made the basis of instruction.

Dr. Wm. H. Pancoast gives "A New Operation for Ununited Fracture of the Tibia," occasioned by the separation of the fractured extremities by the unbroken fibula. The method is highly ingenious, and interesting enough to justify description here.

I etherized the patient, and then with a strong gimlet, pierced the integument on the outside of the leg, and screwed it into the fibula, about three inches above the external malleolus, where this bone is still superficial. I withdrew the gimlet from the bone, but not from the integument, and slipping the point a little further upwards, I bored a second and a third hole in the same way. The fibula is narrow and does not give you much surface to work on, requiring for this purpose a gimlet, with a hard sharp point, that must be firmly and adroitly applied upon the bone. At the point where I had perforated and weakened the fibula, I now broke it, seizing the leg above and the foot below, and striking the leg at the perforated point on the edge of the bed. The bone broke with an audible snap. I then pushed up the lower part of the leg, endeavoring to make the bones overlap, avoiding too much force so as not to lacerate the arteries. The fragments approximated nicely, but did not overlap as much as I had hoped, owing to the amount of bone resected from the tibia, making the large gap, and from the pinching of the muscles, as I forced the fragments together. I decided, however, to wait, and see how much benefit would be derived from what had been done. I placed the leg in a fracture-box, to obtain from its use, what is a disadvantage in its employment, when we wish to cure fracture of both bones of the leg, without much shortening; as by its weight, it holds the lower part of the leg and foot fixed and immovable, while the body above, unless prevented, sinks down in the bed, causing the fragments to override each other, producing a tendency to great shortening as the

fragments unite. I placed the patient in bed, supporting her back with pillows, and elevating the fracture-box also, so that between the two pressures of the fracture-box below, and the weight of the body above, the bones might be made to approximate as much as possible. She suffered no inconvenience from the operation, ordinary antiphlogistic treatment prevented any fever, and I soon gave her good nourishing food.

Union, however, did not take place rapidly, and motion could still be made at the point of fracture; so at the end of about four and a half weeks, I took the patient again before my class, etherized her, and broke up what adhesions had been formed, forcing the bones up yet more together, so as to make the fragments overlap, and then taking a long, thin, and highly-tempered steel gimlet, made for the purpose, and with a movable handle, I pierced the integuments over the seat of fracture, screwed the fragments together, and removing the handle, let this pin remain for the purpose of exciting inflammation, so as to cause bony deposit, and at the same time to hold the fragments in as close juxtaposition as possible. The patient bore this operation well. I replaced her in bed as before, watching the inflammatory process which was now frankly excited in the leg, keeping it within proper limits so as to avoid erysipelas, with ointment of oxide of zinc and applications of lead water and laudanum. When the pin, at the end of ten days, became loosened in the bones, I removed it. I kept the patient in bed about six weeks, having the gratification of seeing the limb becoming stronger daily. At the end of that time, I let the patient get up on crutches, and in eight weeks from the last operation, she walked into my clinic room without any other support than a cane, and I then discharged her from the hospital.

The same surgeon contributes another paper on "A New Method of treating Intra-capsular and extra capsular Fractures of the Femur and its upper third." The plan is thus described:

This consists in using the Charleston reclining chair, which is so made, with joints at the points where the hips and knees are supported, as to imitate a triple inclined plane, with the additional advantages of movement in an up and down or vertical direction, if passive motion, or change of position is required, as it is mounted on little rollers or wheels to move from one place to another. I modified this chair so as to make it a very comfortable fracture-bed, and one that can be arranged to suit the size of the adult patient. The chair is lined with some cheap material, and well stuffed; with a hole in the seat, filled by a cushion, which can be removed from below, and is retained in position by buttons. Two strips of wood on the under part of the seat are arranged so as to form grooves, in which the edges of the tin, or earthenware bed-pan can slide. When in use, the bed-pan is slid into position after the cushion is withdrawn, and the patient can use it without being disturbed.

The foot-board is movable, and can be pushed down or up, and then fastened by the thumb-screw, so as to make the bed longer or shorter to adapt it to the length of the patient's limbs. On each side of the seat of the chair, and fastened to the corresponding arm is a bar and ratchet, so arranged as to retain the chair in any position desired. The chair is dressed as a bed, with blanket and sheet, each cut so as to have a hole corresponding in size and position with the cushion which fills the opening used for the bed-pan. The patient is carefully placed in the chair, surrounded and padded with as many pillows as are needed, according to the size of the individual, to make the chair snugly support the broken thigh. When the fracture is through the neck of the thigh-bone, an additional support is advantageous, by means of a broad strap and pad, the latter being placed behind the trochanter major, and retained by the strap which buckles around the pelvis. By this arrangement the pad supporting the trochanter, and pushing it forwards, keeps the femur below in a right line with the upper fragment, and assists the action of the chair. If the upper third of the bone is broken, the fragments may be kept in close apposition by means of short splints retained by strips of bandage. The advantage of this method of treatment may be illustrated by recalling the fact, that when one is lying recumbent on a bed, the external rotator muscles have the fullest action, and the thighs, aided by their weight, roll outwards, everting the knees and feet. If this position is imitated in a chair, so that the body is only supported at the shoulders, and on the tuberosities of the ischia, the same eversion of the lower limbs takes place; now let the body be drawn slowly up into the chair in a sitting position, and the eversion of the thighs, legs, and feet is corrected. This little manœuvre shows the value of the chair, as a means of retaining a fractured thigh in the best position for coaptating the fragments, thus facilitating the cure, and as the chair acts as a triple inclined plane, the proper amount of extension is made. There is another great advantage in the use of this chair, which induced me to adopt it in the treatment of the fractures mentioned. As fractures in the neck of the thigh-bone generally occur in the aged, it is of the utmost importance that they should be submitted to a treatment as little wearisome and exhausting as possible.

Dr. J. Solis Cohen furnishes a contribution on "Croup, in its Relations to Tracheotomy. A Paper read, January 14th, 1874, before the Philadelphia County Medical Society." This occupies nearly 80 pages, making drafts on numerous authorities, and containing all that a resonable person could desire to read on the subject. In fact, it is exhaustive of the subject, as it must have been of the endurance of those who took it all at one dose on that memorable 14th January.

Dr. R. Davis relates a case of "Ovarian Tumor Removed

Through the Vagina. Recovery." This, he avers, is the only case on record, except one of a small tumor similarly removed by Prof. T. Gaillard Thomas.

The reports of county medical societies occupy the volume from page 229 to 389. Much of this matter consists of accounts of the diseases prevailing in special localities, and little of it has much interest or value to the general reader.

Dr. James Y. Shearer, of Berks Co., speaks favorably of his experience with the use of tincture of iodine to prevent relapse of intermittent fever. He gives gut. j–x in aquæ ℥ss ter die ante cibum.

Dr. M. L. Herr, of Lancaster Co., reports on Cerebro-Spinal Meningitis. His views on treatment are briefly indicated as follows:

> The treatment with which I succeeded best was quinia in large and frequent doses, persistently continued until the retraction of the head yielded, and after that such tonics and stimulants as were best borne. I, however, did not confine myself to the quinia treatment, but in some cases pursued others, and regretted it in every instance.

The following is a tabular statement of the methods pursued—

Treatment.	No. Treated.	No. Died.
Quinia disulph	51	18
Sod. hyposulphite	3	2
Potass. bromide	3	3
Opium and hyposulph. sod	2	2
Chloral hydrate and potass. brom	2	1
Stimulants and narcotics	2	2

The *rationale* of this plan is not stated. It may be supposed, however, that malaria was an important factor in the ætiology of his cases.

Dr. P. R. Palm, of Lehigh Co., reports a case of abortion caused by the use of Asarum Canadense. It appears that this agent is largely resorted to in that locality. "When I asked her who advised her to use it, she said the girls use it, and that one of her female companions had used it for the same purpose, and with the same results. * * * * I also ascertained that large quantities of the asarum Canadense are sold to women, especially to girls."

> Shameful! Shame the necessity, mother of such an invention:
> Could the contrivance die with them, well that they never be mothers.

Dr. Ross, of Mercer Co., is responsible for the following:

Since I entered the practice of medicine, twenty-five years ago, I have had trouble with the iodides. So many stomachs will not tolerate them; and moreover, my patients were complaining of enteric flatulence, and the bad smell from all the emanations from the body. I speak, also, from personal experience. As a substitute, I have been using the alkaline carbonates and bicarbonates, with good success, and more satisfaction to my patients. Those who were troubled with glandular enlargements and indolent ulcers, and whom I had treated with iodide of potassium as the *sine qua non*, are now relieved quite as promptly by bicarbonate and carbonate of potassa and soda. I think we made a mistake when we accredited the cure to the iodine instead of the alkali. I give this as the result of observation and experience, and also because it was Professor Nathaniel Chapman's favorite remedy. He used salts of tartar or carbonate of potassa.

We always give the iodides on a full stomach, and find little of the trouble mentioned. In scrofulous complaints we value iodine about as much, or rather as little, as the alkalies; but in tertiary syphilis find nothing to compare with large doses of iodide of potassium—15 to 40 grains.

Dr. G. W. Brown, of Schuylkill Co., has a very different idea of the pathology of cerebro-spinal meningitis from the one previously mentioned.

Dr. G. W. Brown reports:

In the early part of last year there prevailed in my neighborhood, and, indeed, in many parts of the country, an epidemic of cerebro-spinal meningitis confined to children from a few months old up to that of puberty. Fully 33 per cent. of my cases proved fatal; some of them succumbing to the disease after a few days or weeks, and others again after months. Believing the disease to be caused by rheumatic poison, I have adopted that plan of treatment which I have found to be most effectual in inflammatory rheumatism, viz., the steam bath, alkalies, purgatives, and opiates in the early stage, with tonics, stimulants, and a good diet in the latter, and my success, as compared with what it formerly was under a different view of the cause, has been such as to encourage me to continue it.

The relations between acute rheumatism and meningitis are still so unsettled that medical authorities are at great variance on the subject. A comprehensive view of the whole matter would indicate, in our opinion, that the rheumatic poison may or may not—oftener not—lead to meningitis; and that meningitis may or may not—oftener not—be due to the rheumatic poison.

S. S. H.

Transactions of the Medical Society of New Jersey, 1874. Pp. 270.

This is the oldest of the American State medical societies, boasting an antiquity of 108 years.

As usual in such reports, the first formal paper is the President's annual address. The subject of Dr. Thomason's address is *Medical Truthfulness.* Avarice, selfishness, indolence, vanity, and mere theory are the antagonists to this quality, against which he warns us. Of course they are to be shunned. He does not tell us, however, whether truthfulness is always appreciated by the public. In fact their fondness of quackery would indicate otherwise. Nevertheless it is our duty to do right, whether people deserve it or not. But the *whole* truth must not always be told. Instance, a case in actual experience. A wife presents herself with unmistakable symptoms of constitutional syphilis some months after the return of her husband from a prolonged absence. Both wonder at the eruption and rheumatic pains and are inquisitive to have them accounted for. It is clear that neither must be accused of incontinence, or there would be trouble all around, and both would blame the doctor. He is fortunate if they are satisfied with attributing the complaint to *impurity of the blood*, but may be under the necessity of resorting to prevarication. A lady complains of pruritus vulvæ, which is rebellious to treatment, until mercurial ointment is given a venture. It wont do to tell her she has crab-lice, but it may be necessary to satisfy curiosity in some way.

It appears that extensive inquiries have been instituted among the profession in New Jersey in regard to hypodermic medication, the use of hydrate of chloral, and the tendency of medical advice to induce drunkenness through the medicinal use of alcoholic preparations. The general testimony is favorable to the first two and negative on the last point. Abscesses are mentioned as occasional consequences of hypodermic medication, even with morphine. None appear to have learned to thrust the needle deep into the muscular structure, as is practised by adepts here, using even equal parts of whiskey and water with impunity.

A large part of the volume is occupied with reports from the county societies, in which there is very little of special interest to one outside the State. The most remarkable case related was one of atrocious trifacial neuralgia, related by Dr. J. B. Mattison, of Morris Co. Morphine and other remedies had lost

their effect, and at last one-third of a drachm of chloroform was thrown by the hypodermic syringe through the upper lip onwards toward the eye. A temporary exacerbation of pain ensued, which was followed by complete relief, but no inflammation. Several short paroxysms of diminished severity recurred within a short time. Sleep was secured by free use of chloral, and complete recovery ensued without repetition of the chloroform injection.

The following plan of treatment for cerebro-spinal meningitis is given on page 213:

In a warm room, a wooden-bottomed chair is placed near the bed (but not so near as to prevent the passing of attendant), a board is fastened in front so as to close the space from seat to floor (to prevent legs being burned in subsequent part of operation), under the centre of chair a tin vessel, in it a teacup containing ℥jss. alcohol. Blankets sufficient to envelope patient and chair from neck to the floor, so as to rest closely there. Have three or four reliable assistants, with *muscle*, who will be promptly obedient to directions. A pillow placed in the back of the chair to support the spine. The patient is carefully taken from the bed and placed on the chair, the blankets being well secured around the neck, and allowed to flow over the chair and rest in ample folds on the floor; bladder of ice on the head. All being ready, ignite the alcohol, from the rear, with a taper; drop the blanket, admitting only sufficient air to maintain combustion. In ten or fifteen minutes the patient will be in a profuse perspiration. Immediately preceding the sweat an "alarm stage" will be experienced by the patient, often requiring the united efforts of physician and assistants to retain the person and coverings in position. When sweating is thoroughly established patient will experience relief, become docile, and allow himself to be wrapped in the blankets and placed in bed, where, not unfrequently, he falls into a pleasant slumber. The case under treatment, after use of the bath, experienced almost immediate relief from pain, the muscular rigidity gone, the skin returning to and exercising its normal functions. Bromide of potass, quinia, and occasional doses chloral at night, to induce sleep, were continued throughout until convalescence, which was established in a month.

An external treatment for small-pox is presented by Dr. Sherman Cooper, of Westfield:

I have treated eight cases of small-pox in the last few weeks—three unvaccinated and confluent, and five of modified variety—by bathing the patient with a solution of potassium sulphuretum, with the effect of arresting the eruption and eliminating the poison, and leaving no marks or traces of the disease behind. I called my friend, Dr. Gillette, in consultation in these eight

cases, and showed him all stages of treatment. He pronounced them all well defined cases of small-pox.

There is considerable difference in the amount of bathing required in different cases. The more severe and inflammatory the case, the more the bath has to be applied. If it is so faithfully applied that suppuration is prevented, no secondary fever follows, and the tubercular masses scale off and disappear in a few days, leaving no marks.

If the efficacy of this plan be confirmed by further trial, it will prove a valuable contribution to therapeutics.

S. S. H.

The Legal Relations of Emotional Insanity. By E. Lloyd Howard, MD., of Baltimore. Extracted from the Transactions of the American Medical Association, 1874. Pp. 12.

The main objects of this paper have been to expose the fallacy of the law which holds that, "the insane cannot commit *crime;*" to show the true cause of the misunderstandings of the legal and medical professions, growing out of expert testimony; and the injustice of the charges so commonly heard against the latter, of shielding the criminal by the invention of "new forms" of insanity. It often happens that the medical witness must testify that a criminal is *insane*, and so secure an acquittal, while his opinion, that he should nevertheless be held amenable for his actions, cannot be received.

The discordance between the legal profession and the medical on the subject of insanity grows out of the fact that the latter has advanced out of sight of the former in an understanding of it. It is natural, therefore, that lawyers should be at fault where insanity is in question. On the other hand, medical men are eqally liable to err in matters of law. For example, the following opinion of the author is quite open to legal criticism:

The only way, in our opinion, to practically reach the difficulty, is to render *all* criminals, whether insane or not, eqally *liable* under the law; leaving the court, and jury, free to determine the kind, and degree of punishment, in each individual case; taking into consideration all the features of the case; investigating the facts of the insanity, as part of the facts of the case, and as *palliative* circumstances only.

It seems to us that so unlimited discretionary power in courts and juries would naturally lead to serious abuses. The restraint of an imperfect law is safer in questions of crime than none at all.

S. S. H.

The Medical Use of Alcohol; and Stimulants for Women. By Jas. Edmunds, M.D., M.R.C.P., London, &c. 12mo., pp. 96. New York: National Temperance Society and Publication House; 1874.

This little book consists of three lectures, delivered at Association Hall, on invitation of the National Temperance Society. The first is on the Medical Use of Alcohol. Dr. Willard Parker, of New York, in introducing the lecturer, made some remarks, among which occurs the following statements about alcohol: "It is not a food, nor should it be used as a common beverage. It has been proved, also, that when taken into the system it diminishes the temperature, lessens the strength, and, by about 40 per cent., shortens human life." The preponderance of medical authority inclines to the belief that alcohol has some value as a food, and this is even admitted by Dr. Edmunds; but let this go, as it is a matter of dispute. Alcohol, he says, when taken into the system, shortens life by about 40 per cent. This is stated without qualification, and coming from a man of Dr. Parker's reputation, must reasonably excite surprise as an extremely loose and unscientific proposition.

In this lecture, as in the other two, Dr. Edmunds is like the temperance agitators in general: in attempting to grapple with a great subject, instead of mastering it, the subject completely gets the mastery of them and carries them into very absurd positions. For instance, on page 19 we read—"I cannot but think that alcohol has a partly narcotic influence upon the body, and that its real uses in medicine are those of the narcotic, and not those of a stimulant nor those of a food."

Again on page 23:

It would then come to this—that whenever a man thinks he is taking a little stimulant, he is simply taking that which stupefies and fools him; and I believe that we have labored, and that the general public have labored, under a great delusion, not only with regard to alcoholic beverages in daily life, in supposing that they had enabled them to do more and better work than they would have been able to accomplish without them. I believe, in cases of sickness, the last thing you want is to disguise the symptoms—to merely fool the patient; that if alcohol were a stimulant, that is not the sort of thing you would want to give to a man when exhausted from fever. If your horse is exhausted, do you want to give him food, or would you give him rest and food? So, if your patient is exhausted by any serious disease, surely it would be the more rational thing to let him rest

quietly, to save his strength, and in every possible way to take care to give him such food as will be easily absorbed through the digestive apparatus and keep the ebbing life in the man.

We think that not even Dr. Edmunds would consider alcohol a narcotic remedy for the bite of a serpent or for syncope, if the question were plainly put to him; and we believe that alcoholic preparations are now generally used rather as food than as stimulant in continued fevers. Moreover, to reason as he does on page 77, that the effect of large and small potations differs only in degree, and not in kind, leads to manifest absurdity. Granted that a large dose produces stupor, and is therefore a a narcotic; we must not infer that a smaller dose will produce a similar effect necessarily, though it may in some individuals, whose first stage of drunkenness is quiet and amiable. But it is not necessary to extend one's observation to the wild Irishman or savage Indian, to find people "crazy drunk."

In the second lecture, on "Stimulants for Women," addressed particularly to women, we find scarcely anything to find fault with, and much to commend. The following advice on diet is especially sensible and good.

I generally say to ladies, if you will take warm milk in the morning flavored with a little tea as your drink; if, directly upon waking, before you rise, you will let a servant bring you up half a pint of milk thickened with a little oat-meal gruel, or anything that is equivalent to the coarse oat meal well boiled and then thinned out with about an equal quantity of milk—if you will take that alone every day, a month's time will make all the difference in the world between your being in good health or your being in weak or feeble health. The best breakfast a lady can take is to eat a good deal of breadstuff and a rasher of bacon, or something of that kind, and then to drink plenty of warm milk with a slight mixture of tea, cocoa, or any simple liquid of that kind at a temperature which will enable her to easily digest it. At dinner-time she will take care, instead of eating a double quantity of flesh, to have rice, milk, or potatoes; in short she will live in the simplest possible way. I observe that in this country a great variety of foods is eaten at the same time, which has a most mischievous action upon the stomach, imposing upon that organ a combination of duties that it never was intended to discharge; and the habit which I have also observed here of drinking ice-water, followed alternately with hot coffee and hot meat, is of all devices that I have ever seen the one most calculated to destroy the teeth and stomach. The habit of taking iced fluids at meals is a most injurious and destructive one. The full measure of food cannot be properly digested if you pour into the stomach half a pint of water, for

that arrests the action of the gastric juice and takes out of the stomach heat, which is its force and power. That is the kind of dinner I would suggest; whether you call it lunch or dinner, I would say that the mother should have a substantial, comfortable meal of that kind in the middle of the day. At tea-time she should have a meal containing a great deal of farinaceous food and a fair quantity of sugar, unless she finds a tendency to become too corpulent, in which case the sugar should be omitted and the amount of pure farinaceous matter should be lessened. Then at supper-time I would have another pint of milk thickened with a little well-boiled oatmeal porridge. During the night I should say that a pint of milk gruel ought to be standing by the bedside. Anything that a mother can do in the way of diet for her child will be done upon that kind of food.

The following passages from page 51 were addressed to a popular audience, and evidently were intended for a dreadful warning. On the same principle precisely we sometimes hear fire and brimstone proclaimed to transgressors of other doctrines, and the effect is believed to be exceedingly wholesome.

The result is that the babies of the present generation are never sober from the earliest period of their existence until they have been weaned.

When I hear a mother telling me that whenever she takes a little whiskey and water, or brandy and water, because the child is fractious, and she finds that her milk agrees with it better, I am obliged to ask her if she knows what she is doing? If she knows that she is simply making herself the medium for distilling into her babe's system almost the whole of that spirit which she takes into her own, and whether she is aware that that soothed condition of the child is really the first stage of drunkenness.

The third lecture, on "The Dietetic use of Alcohol," is, of course, directed against such use. The arguments are good for general application, but are not universally true. In an economical point of view, and for a person capable of assimilating suitable food, alcohol is vastly at disadvantage; but there are conditions of general debility or prostration, and loss of digestive power in particular, when a substance of moderate force value, but of ready assimilation, may turn the scale between life and death.

As these lectures were addressed to popular audiences, we may suppose that they were not intended for scientific readers, nor to be submitted to strict criticism; still, as the book has been sent to a journal which aims rather at scientific accuracy than moral reform, we have thought proper to adhere to our general

rule. Whether such doctrines best answer the purpose, we shall not undertake to decide, though ready to admit, in a general way, that "truth is not to be spoken at all times"—simply because people are not always prepared to appreciate it.

S. S. H.

Hand-Book of Practice; employing Concentrated Medicines as prepared by B. Keith & Co. 18mo., pp. 143. Jersey City: Pangborn, Dunning & Dear; 1874.

This little book consists of two parts: (1) a materia medica list; (2) a list of complaints with statement of appropriate remedies. The remedies are all from the vegetable kingdom, and many of them not included in the U. S. Pharmacopœia. It is stated in the preface that "The publication of this hand-book has been prompted by repeated calls from the medical profession for something more definite on the use of Concentrated Medicines, as prepared by B. Keith & Co." We presume that the call comes from that portion of the profession styling themselves "Eclectics," and are willing to agree that the work is suitable to their wants, including probably all the information desired by them on materia medica, therapeutics, and practice of medicine. S. S. H.

We have also received Prof. Stillé's work on Therapeutics and Materia Medica; fourth edition, in two large and splendid volumes. The book will receive attention in our next number.

Clinical Lectures on Diseases of the Urinary Organs. Delivered at University College Hospital, by Sir Henry Thompson, Surgeon Extraordinary to His Majesty the King of the Belgians, Professor of Clinical Surgery and Surgeon to the University College Hospital. Second American from the third and revised English edition, with illustrations. Philadelphia: Henry C. Lea; 1874.

Our readers may accept this work as among the very highest authorities in regard to the class of diseases upon which it treats.

Essentials of the Principles and Practice of Medicine; a Hand-Book for Students and Practitioners. By Henry Hartshorne, A.M., M.D., Professor of Hygiene in the University of Pennsylvania, etc. Fourth edition, thoroughly revised, with one hundred illustrations. Philadelphia: Henry C. Lea; 1874.

Clinical Lectures on various Important Diseases; being a collection of the clinical lectures delivered in the medical wards of Mercy Hospital, Chicago. By Nathan S. Davis, A.M., M.D., Professor of Principles and Practice of medicine, and Clinical Medicine, in Chicago Medical College. Edited by Frank N. Davis, M.D. Second edition. Philadelphia: Henry C. Lea; 1874.

Former editions of these two books have received extended and favorable notices in this JOURNAL. The speedy demand for new editions is proof of their popularity in the profession.

Orthopædia; or a Practical Treatise on the Aberrations of the Human Form. By James Knight, M.D., Member of the Medico-Chirurgical Faculty of Maryland, etc.; Physician and Surgeon in charge of the Hospital of the New York Society for the Relief of the Ruptured and Crippled, New York City. New York: G. P. Putnam's Sons, 23d Street and Fourth Avenue. 1874.

We are indebted to Robert Eyrich, 130 Canal street, for this work, which is an octavo volume of 364 pages, printed on good paper, and containing numerous illustrations. The author has treated his subjects very elaborately, as the following titles of its various chapters will prove.

CHAP.

I.—Remarks on Defective Physical Formation.
II.—Impairment of Tissues Resulting in Contortions.
III.—General Remarks on the Treatment of Talipes.
IV.—Infantile Paralysis.
V.—Electricity as a Therapeutic Agent in the treatment of Paralysis.
VI.—Contraction of the Hands, Fingers and Toes.
VII.—Lateral Curvature of the Spine-Torticollis.
VIII.—Rachitis.
IX.—Hernia—Procidentia Uteri—Ectropion Vesicæ—Relaxed Abdomen.
X.—Varicose veins—Bursæ—Ganglion.
XI.—Pathological Consideration of Diseases of the Joints.
XII.—Diseases of the Bones—Necrosis.
XIII.—Tonics, and their Effect Upon the System.

The Type and Specificity of Yellow Fever, Established with the aid of the Watch and Thermometer. By Dr. J. C. Faget, of the Faculty of Paris, Ex-interne of Hospitals, Member of the Anatomical Society and of the Medical Society of Observation, Corresponding Member, medalled (gold medal) by the Academy of Caen, Chevalier of the Legion of Honor. With 109 tables of the Pulse and Temperature. Paris: J. B. Ballière & Sons, Booksellers and Publishers, 19 rue Hautefeuille. New Orleans: Am. Sutton, Family Library, 167 rue Royale; 1875. For sale by the American publisher, and by James Gresham, 92 Camp street, at $1 per number, to be sent postpaid to the purchasers.

In this pamphlet, our learned and excellent confrére has produced the very highest authority now extant in regard to the diagnosis of yellow fever. His deductions are based upon a large amount of experience and upon numerous carefully recorded observations. The separate publication of these observations, accompanied as they are, by more than one hundred lithographed charts of temperature, has occasioned a large outlay, and the publisher should certainly be secured from loss.

EDITORIAL.

The Journal Outlook.

Thanks to both the number and steadfastness of its patrons, the JOURNAL yet sustains itself and has cheering prospects of its future. It is consoling to find that we are able to make such an averment, situated as we are under a seemingly unbroken and rayless cloud of financial and political misfortunes. Yet a little longer, and the sunshine of prosperity will surely once more gild our land. It would be believing that the days of unrestrained demon-ambulism had come upon the earth to hold to a different opinion. We trust that our readers will grant that these anticipations are sufficiently sustained to justify us in urging a continuance of their support and substantial aid. We are striving to merit such consideration at their hands, and endeavor to devote the JOURNAL to the purpose, singly and solely, of advancing all the great aims of the medical profession. In this connection we call attention to the offers to subscribers made by the proprietor, and published upon the cover of the JOURNAL.

METEOROLOGICAL REPORT FOR NEW ORLEANS.

Table I---November.

Day of Month.	Temperature.			Mean Barometer Daily.	Relative Humidity—Mean.	Rain fall—inches
	Maximum.	Minimum.	Range.			
Nov.						
1	57	41	16	30.341	54	.00
2	61	43	18	30.305	56	.00
3	67	45	22	30.237	60	.00
4	74	50	24	30.180	67	.00
5	78	60	18	30.205	82	.00
6	79	60	19	30.229	80	.00
7	78	61	17	30.160	80	.00
8	78	68.5	9.5	30.103	87	.02
9	77	67	10	30.084	85	.00
10	80	65	15	30.100	85	.00
11	83.5	68	15.5	30.107	79	.00
12	74	68.5	5.5	30.163	83	.00
13	62	56	6	30.292	80	.00
14	70	56	14	30.291	80	.00
15	74	62	12	30.251	82	.00
16	75	64.5	10.5	30.180	83	.37
17	77	68.5	8.5	30.103	78	.00
18	74	64	10	30.119	76	.00
19	67	62	5	30.097	81	.00
20	67	57.5	9.5	30.052	78	.00
21	74.5	54	20.5	30.025	70	.00
22	79	67	12	29.930	82	.30
23	74.5	66.5	8	30.054	54	.00
24	76	56	20	29.856	65	.00
25	58	57	7	29.985	55	.00
26	59	39	20	30.155	62	.00
27	70	50	20	30.225	83	.00
28	70	60.5	9.5	30.234	81	.00
29	56.5	39.5	17	30.418	63	.00
30	62.5	43	19.5	30.420	61	.00
Mean..	71.08	57.13	13.95	30.163	73.73	Total. .69

Table II---December.

Day of Month.	Temperature.			Mean Barometer Daily.	Relative Humidity—Mean.	Rain fall—inches.
	Maximum.	Minimum.	Range.			
1	68.5	48.5	20	30.430	71	.00
2	71.5	52.5	19	30.339	77	.00
3	74.5	56.5	18	30.241	76	.00
4	72.5	56	16.5	30.220	74	.00
5	68	57	10	30.131	84	1.27
6	62	52.5	9.5	30.001	85	.02
7	62	51.5	10.5	30.160	56	.00
8	57	42.5	14.5	30.337	62	.00
9	63.5	43	20.5	30.323	60	.00
10	66	46.5	19.5	30.305	73	.00
11	67	48.5	18.5	30.311	84	.00
12	67	49	18	30.280	72	.00
13	73.5	56	17.5	30.187	80	.00
14	71	61	10	30.226	76	.00
15	64	48	16	30.295	75	.00
16	73	54	19	30.191	88	.01
17	65	60	5	30.203	85	.04
18	56	50	6	30.214	74	.03
19	69	48	21	29.966	81	.11
20	54	42.5	11.5	30.160	68	.00
21	61 5	42	19.5	30.150	78	.07
22	61.5	52	9.5	30.114	72	.00
23	58	49	9	30.177	81	.42
24	56	47.5	8.5	30.355	54	.43
25	54	45.5	8.5	30.175	74	.45
26	75	53	22	30.095	85	.00
27	76	69	7	30.126	85	.00
28	78.5	67.5	11	30.146	80	.00
29	78.5	67	11.5	30.213	83	.00
30	63	60	3	30.259	91	.00
31	64.5	56.5	8	30.219	—	.46
Mean..	66.18	52 69	13.49	30.214	76.13	Total. 3.31

Mortality in New Orleans from November 8th, 1874, to December 27th, 1874, inclusive.

Week Ending	Malarial Fevers.	Diarhœa and Dysent'y	Small-Pox.	Diph-theria.	Pneu-monia.	Total Mortality.
Nov. 8.....	8	7	0	5	2	135
Nov. 15.....	10	8	6	5	3	128
Nov. 22.....	10	7	2	3	5	150
Nov. 29.....	8	7	2	1	2	114
Dec. 6.....	14	4	2	1	7	123
Dec. 13.....	3	9	2	2	8	135
Dec. 20.....	9	4	5	3	11	130
Dec. 27.....	6	9	4	5	13	140
Totals.....	68	55	23	25	51	1055

THE

NEW ORLEANS

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ORIGINAL COMMUNICATIONS.

Article I.—*Review of Yellow Fever in New Orleans*, 1869-1874.

By S. S. Herrick, M.D.

The *materies morbi* of yellow fever, not having been brought within range of our outward senses, has still to be studied solely by its effects. Accumulated observation of its mode of action tends to the belief that it exists in living germs, propagated rather without than within the human body. Evidence of this supposition is afforded by certain facts: (1) the disease is certainly transportable, through fomites, from an infected to a healthy and distant locality; (2) individuals alone and apart from fomites fail to spread the disease in a healthy locality; (3) the disease is known to extend gradually from existing foci of infection, like the army-worm on cotton plantations; (4) testimony has been for years accumulating to the conclusion that the disease may be arrested and extinguished in infected localities by the use of agents destructive to low forms of life. On the last point particularly reference may be made to the records of our Board of Health for the last six years.

The following table shows at a glance the most important features of the fever, as it prevailed during the years just mentioned.

Year.	Cases.	Deaths.	Date of first and last Case.	Prevailed Where.	Supposed mode of Origin.
1869	6 known.	3	July 17. October 8.	All but one in connection with shipping.	4 occurred on vessels direct from Liverpool.
1870	About 1700 (?)	587	First, May 26. Next, Aug. 14 Last, Dec. —.	Chiefly in a limited portion of Second District, among Italians.	Indigenous, so far as known.
1871	104 known.	54	July 30. December 18.	Chiefly in Fourth District, between river and Magazine street.	Through "Mary Pratt" and "Hope," from Cuba.
1872	83 known.	39	August 28 November 30.	Mostly in Fourth District, but less restricted than in 1871.	Indigenous, so far as known.
1873	388 recognized.	226	July 4. November 19.	In most parts of the city.	First case on "Valparaiso," from Havana—thence spread to river steamers lying near.
1874	Not exceedi'g 19	All but 3	August 19. Novem'r 28.	Much scattered.	All but 4 indigenous.

It should be noted that the observations made by the Board of Health, through their officers, have grown more extensive and accurate during the above years. It would be interesting, if we could obtain the previous history, for a few years, of the two British ships, coming here in 1869 direct from Liverpool, on which cases of yellow fever originated. It seems impossible that these individuals could have contracted the disease ashore, as the first one of the four sickened September 30th, while only two previous cases had come to light in 1869, and the preceding year is accredited with only five deaths from yellow fever.

The first case in 1870 had recently arrived on a steamer from Honduras. The premises were disinfected, and no more cases occurred in that vicinity during the year. The origin of the subsequent outbreak is involved in great obscurity, as it prevailed among a class of people living under bad sanitary conditions, having no social intercourse with other classes, and extremely averse to apply for medical relief.

It will be observed that the disease obtained a decided occupancy every one of these years, except the first and last, and yet no general prevalence occurred. In 1870 there was a severe local epidemic, which was attacked by prompt and vigorous means of disinfection under direction of Sanitary Inspector Albers. A striking example was afforded at 230 Chartres street, which is a tenement house, containing 30 rooms, occupied by as many families, consisting of 183 persons, all Italians. Of these, 44 took the fever, but only two cases occurred after the process

of disinfection. Of the 139 who escaped, 92 were unacclimated, having arrived since 1867.

The two following years the fever was mostly confined to portions of the Fourth District near the river. In this part of the city there are no large tenement houses, crowded with a degraded and unacclimated population, as is the case about the French market.

In 1873 the fever appeared so early (July 4th), and there were so many foci of infection, that it is still more remarkable that we escaped a general epidemic. The "Valparaiso" is commonly accredited with the introduction of yellow fever in 1873. It is certain that the first case broke out aboard in the person of the mate, after the opening of her hatches; and there is evidence that a ship's crew may live with impunity aboard while the hatches are closed, while the infection confined for a length of time in the hold seems to acquire increased virulence. But it is possible that the first cases, which broke out along the river front, may have contracted the disease from infection which wintered over, under the wharves. At any rate the dissemination of the fever over the city did not occur by gradual radiation from this point, as would be expected from a single source of infection. Great uncertainty of the true value of disinfection in that year exists, on account of the general epidemic of dengue which prevailed at the same time. Cases occurring at a distance from the river were regarded as yellow fever, when fatal; while, in the event of recovery, they were generally reckoned as dengue, and no disinfection was practised. Either this principle of diagnosis was correct, or the yellow fever infection failed to spread when allowed full scope; for, in absence of dengue, these cases would have been pronounced unequivocally as yellow fever.

Undoubtedly the fever manifests different tendencies one year with another, and this was observed before efforts were made to restrain its progress by disinfection. It may be attributed to varying activity of the morbific agent and varying susceptibility of its subjects. How much allowance should be made for these considerations can not precisely be determined; but it is an interesting and important fact, that New Orleans has suffered less with the yellow fever during these years than several of her sister cities in the South, while the most obvious factor that varied, as affecting its general prevalence, was the use of disinfectants in this city.

The history of yellow fever in New Orleans during the year 1874 corroborates the foregoing views. Until October the quarantine in the Mississippi River was under charge of Dr. A. W. Perry, who put in practice his new plan of thorough fumigation of vessels with sulphurous acid. During the whole time of his administration no yellow fever infection reached the city through the shipping. The practice of fumigation was discontinued by his successor, and in a few days two cases broke out on the bark "Queenstown," from Havana, while lying at her wharf in the Second District. The vessel was fumigated at the wharf, and no more cases occurred. These cases were treated at Charity Hospital, together with several others carried in from other localities, but none originated in the house this year. Such has been the usual experience heretofore, until the neighborhood outside had been reached in the progress of infection.

For the following statement I am indebted to Dr. N. Hudson, Surgeon U. S. Navy, which strikingly illustrates the tenacity of yellow fever infection in the hold of a ship, and the futility of simple quarantine detention.

"The U. S. S. 'Ticonderoga' arrived at Key West on January 8, 1874, directly from the coast of Brazil, where she had been stationed for three years. Her officers and crew were relieved soon after her arrival, so that the whole ship's company was changed for a new one. The ship remained at Key West for several months, taking part in the exercises of the fleet. In March her holds were thoroughly broken out, and shortly afterwards she went to Norfolk, Va. She returned to Key West about the 1st of June, and in the latter part of that month the hold was again broken out and whitewashed. Some repairs to the boilers were going on at this time, but the general condition of the ship was clean and creditable. On the 9th of August, the captain's clerk, Nathaniel White, was admitted to the sick list with a suspicious fever which developed into an unmistakable case of yellow fever, and on the 12th he died of black vomit. No other cases appeared at this time, and shortly afterwards, about the 20th, the holds were again broken out and whitewashed. On the 26th of August two more cases appeared, and two also on the 27th, when the ship sailed for Portsmouth, N. H. The four cases which had appeared were landed at the Marine Hospital, Key West, and three of them died subsequently with well marked symptoms of yellow fever, while the fourth made a good recovery.

"The Ticonderoga encountered cool and pleasant weather, and during her passage north five new cases appeared, all of which recovered.

* * * * * * * *

"I may add that no previous history of yellow fever on the

ship could be obtained, and there was no appearance of that disease during the season, either on the other ships or the Island of Key West."

The accompanying table gives the leading features of all the cases that have come to light this year. Only four can be traced to foreign infection. The *materies morbi*, supposing it to be living germs, must have wintered over here, as has occurred repeatedly before. The extraordinary mildness of last winter gave room for apprehension of a more serious visitation this year, and failure in fulfillment may be attributed to one or more of the following causes: (1) to almost total destruction of the germs last year by disinfection; (2) to degeneration of disease germs; (3) to diminished susceptibility of the population at large to their morbific influences. How much value attaches to each of these considerations is an open question. As regards the last, it is quite as reasonable to look for varying susceptibility to yellow fever as to any other known infectious disease. It is generally admitted that liability to these maladies varies exceedingly, both as regards individuals and seasons. In other words, the power of resistance to morbific causes varies in different individuals, and with the same individuals in different seasons. But when we find liability to a certain infection to be generally reduced in a population, it seems probable that its virulence has declined.

Here occurs the interesting question, whether the yellow fever germs would finally become extinct in this country, if not reinforced by importations from localities where the fever is endemic and perennial. It is averred by many that we live in the true yellow-fever zone, because a year seldom passes without manifestation of the disease. To this view I can not subscribe, for two important reasons: (1) the disease was never known here before 1796 or '92; (2) our annual winter temperature always arrests its prevalence, and there is reason to believe that the infection has been quite killed out, from the fact that the city has been exempt from the fever certain years (1861 and 1862 entirely so). There is therefore ground of belief that thorough disinfection of suspected vessels at the quarantine, and prompt and vigorous action with infected localities in the city, might in a few years exterminate the *materies morbi* in our midst, and effectually prevent its obtaining another foothold. Of course the price of permanent exemption, like that of liberty, would be eternal vigilance; and it is too much to expect that this would never be re-

laxed under a system of government marked by rapid rotation in office, on the theory that the spoils belong to the victors. It is needful and proper that the sovereign people should be scourged occasionally, to remember that they are human, and to pay some respect to science and sanitary laws.

The plan of disinfection practised this year, under direction of the Board of Health, is substantially the same as that in use for several years previous, and is as follows. Wherever a case of yellow fever occurred, the streets surrounding the square were sprinkled with Calvert's No. 5 carbolic acid diluted with about 50 parts of water. For the roadway a large sprinkler on wheels was used, and the sidewalks were sprinkled by hand. The ground in the neighboring yards around the infected house was similarly treated, and the privy vaults were disinfected with the zinc iron chloride solution. After the termination of the case by death, recovery or removal, the infected apartments were fumigated with sulphurous acid or chlorine. In some instances the walls and furniture of the rooms were treated with the spray of dilute carbolic acid, by means of a steam atomizer. The infected clothing and sheets were disinfected either by boiling water or by soaking in dilute carbolic acid. The extent of ground disinfected was according to the length of time elapsed since the appearance of the fever. It is supposed that the infection extends along the ground at the rate of 40 or 50 feet daily, and that this is the usual mode of its spread, except when it is conveyed in fomites. Accordingly it is thought sufficient sometimes to disinfect the street only partly around the square, and one or two neighboring yards on each side, together with that in the rear; while, after some days' delay, the whole square, or even more, must be surrounded with a disinfecting band and the enclosed surface then be sprinkled.

It will be observed that the earliest cases in 1874 appeared on squares visited by the fever last year, while no such connection exists with subsequent cases. In fact, the mode of infection with eight of the cases can not be traced at all. Another point is the fact that no cases sprang up along the river front from local infection surviving since 1873, although that year disinfection of the wharves and ground underneath was limited to the Third District. It is possible, however, that the very high stage of the river during the following winter and spring may have swept away or destroyed the infection along the whole river front.

Record of Yellow Fever Cases, New Orleans, 1874.

Case.	Occupation.	Age.	Sex.	Nativity.	Time in City.	Residence.	Date of Attack.	Result.	Remarks.
1	....	6 yrs.	M	Ireland.	6 mos.	470 { Decatur, Third Dist. Victory,	Aug. 19.	Died Aug. 22	{ Though black vomit occurred in both these cases, they were not fully recognized as yellow fever till the appearance of the third.
2	....	9 yrs	M	"	6 "	" " " "	Aug. 19.	Died Aug. 22.	
3	Mother of the 2 above.	30 ys.	F	"	6 "	" " " "	Aug. 25.	Died Aug. 30.	Black vomit 12 hours before death. Several cases last year on Spain street, same square.
4	Saddler.	24 ys.	M	France.	2 years.	Lodged rear of 47 Peters street; boarded 102 Front Levee.	Sept. 21.	Died Sept. 26.	Case occurred on same square where he lodged, last year. Admitted to Charity Hospital Sept. 24
5	Sailor.	24 ys.	M	Germany	9 mos.	Resided and died at 459 Julia street.	Sept. 22.	Died Sept. 27.	Disease contracted at Pascagoula, whence he shipped to Galveston; thence reached home September 26. Well marked case.
6	Mariner.	30 ys.	M	"	.....	Near Pascagoula; came to the city September 23.	Sept. 23.	Died Sept. 29.	Contracted at Pascagoula. Well marked case.
7	Servant.	14 ys	F	France	2 mos.	27 Dumaino street.	Sept 26.	Died Oct. 1.	Admitted at Charity Hospital September 27. No case near residence last year.
8	....	10 ys.	M	Norway.	1 year.	619 { St. Claude street. Good Children.	Sept. 29	Died Oct. 3.	No yellow fever on that square in 1873.
9	Consumptive inv'lid	27 ys.	M	Indiana.	9 mos.	390 Magazine, corner of Melpomene street.	Oct. 4.	Died Oct. 11.	No yellow fever on same or adjoining squares in 1873.
10	Laborer (?)	30 ys.	M	Ireland.	1 "	169 Baronne street.	Oct. 7.	Recovered.	Taken sick at Halfway House. Admitted to Charity Hospital October 12. Albuminuria. Convalescence slow. Discharged Nov. 7.
11	Consumptive inv'lid	31 ys.	F	Louisia'a	11 "	412 Magazine street.	Oct. 8.	Died Oct. 14.	On same square as case 9, above.
12	Laborer (?)	24 ys.	M	France.	7 "	Unknown.	.....	Died Oct. 13.	Admitted delirious to Ch. Hosp. Oct 9 { Had been working in rear of Barracks. *Doubtful*
13	Laborer (?	37 ys.	M	"	6 "	Homeless.	.....	Died Oct. 15.	Admitted to Ch. Hospital Oct. 3. Had { been working at Pass Manchac. *Cases.*
14	Drayman.	23 ys.	M	Ireland.	4 weeks.	15 Rousseau street.	Oct. 15.	Died Oct. 19.	Black vomit and almost total suppression of urine. Case opposite side of street in 1873. Came from New York.
15	Laborer (?)	29 ys.	M	"	12 days.	95 Spain "	Oct. 17.	Recovered.	Lived about 400 yards from case 8. No yellow fever on square in 1873. From Iowa.
16	....	37 ys.	M	Italy.	1 year.	163 Orleans "	Oct. 21.	Died Nov. 1.	No case very near in '73. Case somewhat doubtful.
17	Sailor.	19 ys.	M	Ireland.	15 days.	Bark "Queenstown."	Nov. 2.	Died Nov. 7.	Admitted to Charity Hospital November 4th. "Queenstown" left Havana October 11. Sickened 4 days after opening hatches.
18	Sailor.	18 ys.	M	Corsica.	17 "	" "	Nov. 3.	Recovered.	Discharged from Charity Hospital November 20.
19	....	32 ys	M	France.	Unknown.	Chartres street. (?)	Abont Nov. 28	Died Dec. 9.	Entered Charity Hospital December 4. Well marked case.

As is usual with sporadic cases, the rate of mortality has been very high. This is attributable to the fact that such cases are generally not recognized early, and consequently fail to receive proper treatment. But sporadic visitations are not always marked by such fatality. A notable exception to the rule has been the experience of Pascagoula in 1874. There the first cases were not recognized, and a large proportion of them recovered; still the fever never became a general epidemic.

What is most remarkable of the fever of 1874 in New Orleans, is the fact of its remarkable fatality, coupled with the fact that it never gained headway, although cases occurred at no less than twelve separate and independent localities, so far as we can judge. In every instance thorough disinfection was practised as promptly as the circumstances and nature of the case admitted; but we must not be positive that a general outbreak would have followed the neglect of this measure. In 1873, as already mentioned, it is probable that a number of cases of yellow fever that recovered were not reported to the Board of Health, being regarded as dengue. The prevalence of the latter fever also afforded facilities for the concealment of mild cases of the other, when it was desired to escape the annoyance of disinfection with crude carbolic acid. Consequently the most prudent judgment would still keep disinfection in yellow fever an open question, though testimony to its value has accumulated sufficiently to justify and demand its continuance on the general plan now practised.

ARTICLE II. *On the Importance, in the Chemical Examination of Cases of Poisoning, of Determining the Quantity of Poisonous Material Detected.* By J. W. MALLET, Professor of General and Applied Chemistry, University of Virginia.

It has been noticed that in legal trials for poisoning, when the results of a chemical analysis of the contents of the stomach, or of the substance itself of this or other organs, is given in evidence, lawyers and jurymen seem sometimes to establish in their minds an unfounded connection between the amount of the poison stated to have been obtained and the minimum dose of the same substance necessary to destroy life. Hence, in most of the standard works upon legal medicine, the attention of the reader

is drawn to the distinction between the quantity of poison taken and the quantity remaining in the body and detected after death. By some writers this point is discussed at considerable length, and dwelt upon with emphasis.

It is obvious that of the poison originally swallowed, or otherwise introduced into the body, a part may be rejected again by vomiting or purging without having undergone absorption—that of the remainder a part may be absorbed and the rest retained in the cavity of the stomach or intestines, to be there found after death, and that of the portion absorbed some may be eliminated with the urine, bile, etc., previous to death, while some may remain and be detected in the various tissues, as those of the stomach, liver, kidneys, etc. So that no immediate and necessary connection exists between the quantity of poison brought to light by the analyst—whether he have worked upon the contents of the alimentary canal, or upon one or more of the organs themselves of the body—and the quantity which may have been swallowed or which may have caused death. The amount found is but the residuum of possibly a much larger dose after a greater or less proportion of this has been carried off in the various ways suggested above.

So fully and forcibly has this fact been stated by some of the generally referred to authorities, that, in spite of other passages in such works mentioning the desirability of determining, as nearly as possible, the quantity of poison found in an analysis, the reader might well be pardoned were he to acquire the impression that the question of quantity is of very secondary importance—in fact, little more than a mere matter of curiosity—and that the all essential point is simply the establishment beyond doubt or cavil of the presence of poison in the body, since the discovery of any, however little, of the noxious substance, forms an important positive link in the chain of evidence, while the fact that but a minute quantity has been left to tell the tale, or even the entire absence of any such residuum, does not negative the assumption that the man has been poisoned.

That, however, this question of the amount of the poisonous material obtained in a pure state by chemical analysis may become—and sometimes in very unexpected ways does become—a prominent and natural feature of the expert evidence, and that hence it is the duty of the analyst to bestow the same pains upon this as upon all other parts of his responsible work, is

shown so clearly by the history of a case of supposed poisoning tried a year or two ago in Virginia, that it may be worth a brief notice in the pages of a medical journal for the sake of the lesson which it teaches. The main points were the following:

Mrs. E. E. L., a widow, was charged in the Circuit Court of Loudon County, Va., sitting at Leesburg in the autumn of 1872, with the murder of her little daughter (three or four years old) by poison. Evidence as to the deaths of other children of her family was ruled out by the court. The symptoms presented by the child, previously in good health, during her sickness of about forty-five hours preceding death were consistent with the supposition that she had been poisoned by arsenic. Purchase of arsenic by the accused a short time before was established. No intelligible motive for the destruction of her own child was assigned or proved; on the contrary, at least the usual and natural affection of a mother for her offspring appeared to have existed. The body, which had been interred, being exhumed, certain organs—liver, spleen and kidneys—were examined by a chemist of Baltimore, and a quantity of arsenic found representing eighty-six hundredths of a grain of arsenious oxide.

The stomach had been removed at a post mortem examination prior to interment, and in it arsenic was also found, but evidence on this point was ruled out on account of the death of the witness who had carried the stomach to Baltimore, and by whom the identity of the material analyzed should have been established. During the child's sickness preceding death, the attending physician had prescribed three doses, of ordinary amount each, of sub-nitrate of bismuth, though there was no evidence to show that any of these had been actually taken. On the part of the defense, it was proved that subnitrate of bismuth taken from the same bottle used in putting up the prescription contained traces of arsenic, as has not unfrequently been found to be the case in reference to this medicine. The amount of arsenic present in the bismuth was really very minute, and the contents of the bottle in question had been used in the treatment of several other cases without any ill effect.

The theory of the defense, however, consisted in admitting that arsenic had been found in the body on analysis, and attributing its presence to the assumed administration of sub-nitrate of bismuth—not to any criminal poisoning by the mother. In support of this view, the fact of the detection of arsenic in the

remaining bismuth obtained from the drug store was brought prominently forward, while any estimate of its amount was studiously avoided, the prosecution being precluded from offering any testimony on this point—which would have been of the nature of new evidence—after that side of the case had been closed. On the other hand, the accuracy of the determination of weight of the arsenic obtained from the portions of the body of the child examined was vehemently called in question, partly on the ground of the difficulty of weighing such small quantities as those spoken of (the point that it is impossible to make accurate weighings upon a chemical balance *in a city* on account of the tremors caused by passing vehicles (!), which the celebrated Wharton trial has the credit of originating, being revived in full force), and partly in reference to the possibility of the arsenious sulphide weighed having retained moisture, free sulphur, or some other impurity. In other words, the arsenic in the medicine was made to account for the arsenic in the body by exaggerating the one and reducing the other—in both cases by rendering obscure and uncertain the *real quantities* in question. A very little accurate application of figures was sufficient to show that the explanation was untenable, conceding that the bismuth had been actually swallowed, *provided the quantitative determination of the poison found in the course of the analysis could be sustained*, or it could be shown that the margin of possible error in this determination did not exceed, say ten or even twenty per cent. of the whole quantity weighed. This might be considered as fairly established, although the process used was certainly in some respects open to criticism. But it no weighing at all had been made, a quantitative result being deemed sufficient, or if the weighing had been made so roughly and carelessly as to be clearly unworthy of reliance, the theory of the counsel for the defense would obviously have been calculated to tell greatly in their client's favor with a jury.

As it was, a verdict of acquittal was rendered on the general ground of insufficient evidence to connect the prisoner with the administration of the poison, while the newspapers stated that the members of the jury expressed themselves satisfied of the fact that the child had actually been poisoned by arsenic.

The moral of the story seems to be plainly, that in every case in which it is possible, and to the full extent of such possibility, the chemist who is entrusted with the analysis of any tissue or

material supposed to contain poison should prepare himself to testify, not only as to the presence or absence of such suspected constituent, but as to the exact amount which he finds if any be present. Very often such additional information may have no real bearing upon the legal aspects of the case, but sometimes it may prove unexpectedly valuable and important.

ARTICLE III. *On the True Composition of the Sub-nitrate of Bismuth Used for Medical Purposes in the United States.* By J. W. Mallet, Professor of Chemistry, University of Virginia.

Two or three years ago I gave to a student in elementary qualitative analysis in this Laboratory a specimen of sub-nitrate of bismuth which I believed to be pure, requiring him to examine for and report the acid and basic constituents respectively. He found bismuth united to *chlorine*, and on being told to try again came to the same conclusion. I then examined the material myself, and found it to be in fact an oxy-chloride of bismuth.

On enquiry it appears that two varieties of so-called sub-nitrate are in use in this State and others further north—the one in powder and small lumps of irregular shape, said to be of American manufacture, the other in little conical "drops," sold at a higher price as prepared in France. I have procured several specimens of each kind, and in every instance have found the former to be really a basic nitrate, with some hydrate and traces of carbonate, while the latter consisted of oxy-chloride.

How generally this may be the case for the whole country, and what difference of therapeutic effect may be found traceable to this difference of composition, may be worth enquiry. In one instance, occurring in my own family, the true sub-nitrate was taken with no unfavorable symptom, while the French oxy-chloride (sold as the dearer, because better, preparation) produced a good deal of uneasiness and finally vomiting.

ARTICLE IV. *Scarlet Fever with its Sequelæ. A Case of Dropsy with Eclampsia.* By Dr. E. GOLDMANN, Galveston.

Among the many cases of scarlet fever which come under the

treatment of physicians, it is fortunately not more often that they are followed by serious sequelæ. Yet, with all, it is a wise plan on the part of the practitioner never to be too confident, as far as the termination of a case is concerned. One needs but to look through the wards of the large children hospitals of Europe to convince himself, that hardly any disorder of childhood is so apt to leave the most lamentable consequences in its wake. Nor is a case to be considered as particularly mild, because the eruption is so insignificant as scarcely to be perceptible, or because it fails to appear at all (scarlatina sine exanthemata). I have had in my practice cases of this kind repeatedly, and in one of them one of my confreres (as I was informed) declared that it was no scarlatina, while it really was a sufficiently formidable case of it. It would be a mistaken view to look upon the eruption as the principal, while it is only an accessory to a disorder which extends over more parts than the epidermis, or rete malpighi. If I may permit myself the comparison, certainly no one will consider the roseola of typhoid fever, or the herpes labialis of pneumonia, the principal part of these diseases. And we might do so almost with the same right as in scarlatina, where affections of mucous membranes never, and those of the kidneys and other glands rarely, fail to make their appearance.

Epidemics may vary in malignity, but I feel no hesitation in saying that I hardly ever have seen a case in which, during the first days after the invasion of the disease, albumen could not be found in the urine, and, in many of these, the microscope showed casts, which either consisted of epithelium in a swelled and granulated state, or of hyaline fibrinous exudation. This is certainly proof enough that more was involved in the disease than the skin. Beside a number of cases, that I have met and in which scarlatina wrought the most lamentable results, I saw, some years ago at Heidelberg, one which terminated in necrosis of the petrous portion of the temporal bone, subsequently thrombosis of the sinus cavernosus and death. But, though it is well enough to bear these occurrences in mind, it is fortunately but seldom that such a frequent disease of childhood tends towards such fearful complications. In my own practice, I am glad to say that I have almost never met with them. I can not determine whether this was owing to the nature of the cases, or whether really the treatment of Schneemann, which I followed, has led to these happy results. I have my doubts about the

latter, because Schneemann seems to base his treatment on the hypothesis, that the parenchymatous nephritis in scarlatina is caused by the loss of epidermis and the subsequent sensibility to outward influences. The affection of the kidneys, however, generally precedes the desquamation, which emanates from the presence of epithelial and fibrinous casts in the (albuminous) urine, which I have found to appear before that period. Nevertheless, empirical as the treatment is—it is an old and favorite remedy of nurses everywhere—the inunction of lard every day continued for three weeks, has proved itself successful in my experience, and I shall always follow it for that reason. The anasarca, which supervened in a few of my cases, was consequent upon a continuance of the nephritis and to be attributed to the state of anæmia and hydræmia produced by it. I succeeded by means of a tonic and astringent treatment, together with the free administration of milk, eggs, and a generous diet, all through, in rapidly overcoming that trouble. I avoided the use of scilla and digitalis, which are recommended by some of the best authorities, for the reason that I considered it safer not to run the risk of increasing the hyperæmia of the kidneys. But it is not always that these cases of anasarca will pass so kindly, as will be shown by the following case, which came very near ending fatally.

A poor family moved from the interior of this State to this city, and had arrived here about three weeks when the mother came with her two girls, aged respectively 8 years and 6 years, and her little boy aged 2 years, to consult me at my office. Her history was, that she and her husband had suffered for a long time with chills and fever; that the children had been always well until about five weeks ago, when one after the other had been attacked by a fever which lasted several days; that ever since that time the children had never been quite well; that, in consequence of it, they had resolved to leave their home in the country to come to this town, which they had been told was free from malarial affections, that she had, since her arrival, earned her livelihood by going out washing, and her husband by working at his trade; that thus her children were mostly alone, and that only lately she had observed that all three had swollen faces, distended abdomens, and suffered with shortness of breath. A simple, unsophisticated woman, she related her story in a manner somewhat affecting. Her husband and herself had suf-

fered long without murmuring, because they earned the daily bread for their healthy and thriving children, who were all blessed with a most excellent appetite. But then, even the children fell sick, and did not evince their usual cheerfulness and youthful hilarity; and now it was time to leave their comfortable home, which they had built up with every care and industry. Upon questioning, I learned that the children had had an eruption, which, however, had passed away within 24 hours, and that the skin on the soles of the feet and palms of the hands had peeled off. Examining, I found, as could be expected, that all the children had ascites. The pleural and cardiac sacs were free from any accumulation of liquid; only the pericardium of the eldest girl contained a quantity of such, the dull sound extending to a finger's breadth of the right margin of the sternum; the cardiac sounds were muffled, the pulse quick, soft and quivering. There could be no doubt that scarlatina had preceded these symptoms, and that I had no trifling cases before me. Explaining to the mother the full extent of the sickness of her children, I sent her home, enjoining upon her to put them to bed forthwith, and promising her that I should soon call and attend to them regularly. I ordered frictions (a composition of ol. juniper, ol. terebinthia, ol. camphorat., and aq. ammon.), and gave inwardly a mixture of acetate of potass. with succ. juniperi. Besides this, I ordered a tepid bath daily and subsequent packing in woolen blankets. The third day, I found in the morning that the cardiac sounds of the eldest girl had become clearer and, on percussing, that the extent of the dull sound had diminished. I viewed this in a favorable light, but was soon disappointed. Three hours later I was summoned to come as quick as possible, to see the girl again who, as I was informed, had "a bad spell," In a few minutes I drove to the house of the poor family, where a general consternation prevailed. About the cause of it there could be no doubt, for the moment I entered the room an epileptic convulsion began, and the handsome face of the little girl was contracted in a frightful manner, while the bloody froth accumulated on her purple lips. In a case of this kind, where evidently œdema of the brain had ensued, venesection can not be circumvented, or substituted by any other means. Cold had been applied already, to cut short the convulsions, but without any effect. I consequently made at once a free opening in the media basilica, and let a small quantity of blood escape, upon which

the attack subsided almost at once. I then bandaged the arm carefully, so that if a convulsion returned, the vein might not be torn open by the violent muscular contraction connected with it, and then made arrangements to have a steam bath improvised, which I had in a similar case seen applied with good effect. Vapor is one ot the most powerful stimulants for the glands of the cutis, and its action much more certain than the dry hot air bath, which by many authors is considered as best adapted to promote the "evaporation of liquid" from a body affected with serous effusions. It is not evaporation that is to be produced, because that effects itself without any further help from the part of the physician, but an increase of activity of the glands of the cutis, which, in these cases, do not compensate for the suspension of the functions of the kidneys. The child remained in the steam bath until perspiration had become thoroughly established, and then was (with the blanket which had been used to cover her and the chair on which she had been seated) lifted, brought to bed and covered with one more blanket. I ordered infus. senn. comp. in order to relieve the intestines, and, as time was precious, had two enemata given to expedite matters. The patient was all the time in a comatose condition—that is to say, the pupils reacted slightly, the respiration was stertorous, she swallowed after a little while if anything was given by the teaspoon, and betrayed no sign of consciousness. When I saw her three hours later, the purgative and enemata had acted, the passages being discharged on pieces of cloth which had been placed under her. The pupils now reacted, and when called aloud she made attempts to open her eyes. Her pulse had increased but little in volume, its frequency remained the same (98); no convulsion had occurred since, and the breathing had become easier. The diaphoresis had gone on all the time and to such an extent, that beneath the bed the floor showed a place of about one foot diameter which was wet from perspiration that had dripped through bed-clothes, blankets, etc. I therefore had the child removed to another bed, in which previously warmed blankets had been spread, and in which she was, as quickly as could be done, wrapped up again. Inwardly I now ordered a decoction of cinchona and matico, with a little ether, and as nourishment, milk and raw egg. The next morning I found the patient still improved. Some urine had passed, which contained considerable albumen, but no convulsion had taken place; the child was

conscious, and complained of painfulness of the tongue, which had been badly wounded in the several convulsions she had passed through. A steam bath was again given, and the tonic mixture continued. On the 5th day the urine increased considerably in quantity, although no diuretic, strictly speaking, had been administered. The quantity of albumen diminished a good deal—the casts were less numerous, the epithelial cells bearing the appearance of a coarse, fatty granulation. The treatment was followed for ten days, when the child had so far improved as to justify me in leaving out the steam baths. In speaking of improvement I mean, that neither the chemical nor microscopical examination divulged any considerable quantity of albumen or epithelial cylinders; that the urine flowed freely, and the general condition of the girl showed a gradual convalescence, The other two children were during that time recovering slowly, without any intercurrent trouble, under the tonic treatment together with the acetate of potass. and succ. juniper. At the end of the third week the effusions in all three cases had disappeared. Lately I saw the eldest girl while making my daily rounds. Inquiring how she had gotten along since I had not seen her (five months), she told me that she had enjoyed good health; and her appearance testified confirmation of her statement.

I should hardly have written such a long note about a disease so well known to every practitioner as scarlatina, were it not for the sake of exhibiting some of the serious sequelæ of it, and to impress my younger professional brethren with the importance of viewing the disorder, not as a simple affection of the skin, but one of a very complicated kind. Venesection in diseases of children is almost never to be resorted to, because our little patients can not bear the loss of blood. In the case before us the child was 8 years old, and the value of the operation proved itself in a striking manner. The Sangrados of all times have done their share to bring it into disrepute by abuses which have produced in many minds the conviction, that it would be better to strike it out of the list of remedies entirely. The truth lies, as in many controversies, in the middle. *It is sometimes of value, but not often.* For that reason we should not entirely abandon it, and thus deprive ourselves of a means of assistance that in a given case may be of the greatest value. The use of tonic and astringent remedies, together with a generous diet, I consider indicated in these cases, because the effusion of serum into the

different cavities (exosmosis) can best be corrected by causing an endosmosis back to the circulating blood from which it emanated. The experiments of the late illustrious Liebig evince this in a beautiful manner. The blood, getting richer in substance, causes an endosmotic current towards itself, and thus the absorption of effusions. Scilla and digitalis, the favorite remedies of many high authorities, increase the quantity of urine by increasing the pressure of the blood current in the kidneys. Their use is indicated where no albuminuria exists; but wherever that is present, they will only cause the loss of more parenchyma by the mere force of pressure which is exercised by the turgescent blood-vessels and the increased force of impulse of the heart.

ARTICLE V. *Vital Statistics applied to the "Military-Reconstruction" Politics of Louisiana.* By STANFORD E. CHAILLE, A.M, M.D., Professor Physiology and Pathological Anatomy, Medical Department, University of Louisiana.

Long a student of vital, not of political statistics, I was forced incidentally to examine the latter as illustrative of the number of the population,—a number indispensable to mortality statistics. As a student seeking the *whole* truth, I have recently investigated all the statistical facts, known and accessible to me, bearing on the political statistics of Louisiana in 1874. I have carefully refrained from imitating politicians, lawyers, or other partisans, who too often urge such statistics as appear to favor, while suppressing those which refute their side of the mooted question. Fortunately, the correctness of every statement to be made can be readily and rigidly tested, for all of my statistical facts have been derived from well known public documents. And since these facts, without exception, have emanated from officials of the republican party, they can be distrusted with less reason by it, than by its opponent.

A full statement of the political statistics of Louisiana, so as to correctly compare the different data with each other, is unusually difficult for the two following reasons: (1). Since 1868, there have been constructed out of the 48 old parishes, nine new ones, to the vexation both of the statistician and the people. Four of these nine parishes have been contrived since 1870, and the boundaries, population, and other statistics of *sixteen* parishes did not in 1874 correspond with the data of 1870.

Negligence of this fact has caused, and will cause others serious errors. (2). In addition, the various official reports of this State, are almost invariably defective as to one or more of the parishes, or as to some particular; thus, as to the latter defect, forcing an occasional omission, which must not be supposed to be a suppression, and, as to the former rendering a comparison of sum totals dependent in part on estimates. Forced, therefore, to make these partial estimates, I have in every case based them on the actual data, supplying omissions by pro rata estimates of the figures furnished in the official reports. For omissions as to the votes, I have made the estimates for each party, so as to correspond to the *claims* of said party; giving to the republicans, in the parishes omitted by them, the exact proportion claimed by them in the parishes they do report; and the same course has been pursued as to the conservatives. In no case have I favored either party one jot; and to silence all suspicion, I have appended to every estimate the actual figures given for the parishes, which are officially reported.

In order that the official data may be more readily appreciated, I have arranged them in five tables. Of these, the first is the most important,—the remaining four are added as tests of the first, and as illustrations of such statements therein as are most disputed. All five bear on the important question of the relative proportion of the white and colored people, who *live*, *die*, *register*, and *vote* in this State, and City. The first table presents all of these facts generally,—the second and third present the facts, specially as to the white and colored registration,—the fourth, as to the native and foreign born registration,—and the fifth presents the statistical facts as to the votes cast.

To correctly interpret these statistical tables, no one is called upon for any more credence than is required,—as to all statements, whether numerical or verbal,—by the laws of probabilities, coincidencies, and circumstantial evidence. It is presumep to be unquestionable, that the living, dying, registering, and voting populations must be proportionate in some degree to each other. It is claimed, that if the reports from different sources vary in their statement of these proportions, then that credence should be given to such of these reports as are most free from suspicion of partisan design or other sources of error; and it is further claimed, that agreement between the non-partisan statistics derived from different sources must constitute the strongest circumstantial evidence in favor of the comparative correctness of said coinciding statistics, and in refutation of any contradicting partisan statistics.

[The reader is notified that the Tables with the Notes, which immediately follow, are presented, solely as vouchers and for reference; and, therefore, that he can omit these, and safely transfer his attention at once to the "comments" thereon.]

TABLE I.

Political Statistics of the total 57 (a) Parishes of Louisiana, and of the Parish of Orleans, i. e., New Orleans with Carrollton (annexed in 1874) included; giving the number of the white and colored populations only (640 Indians and Chinese excluded) who live, die, "cannot write," register, and vote in said State and city.

	Totals of the 57 Parishes of Louisiana.			Totals of Orleans Parish, or New Orleans,	
	Total of White and Colored.	White.	Colored.	White.	Colored.
U. S. Census. 1870,					
Total Population	726,275	362,065	364,210	144,076	53,837
" " of White and Colored Males	361,815	183,031	178,784	(n)70,279	23,371(n)
" " " " " " 21 years and over	173,979	87,066	86,913	36,198	13,341
Census 1870 for La., and Board of Health 1871-4 for N. O.					
Total Deaths	14,233	7508	6725	(b)4173	2334(b)
" " of White and Colored Males	8,206	4463	3743	.....	.....
" " " " " " " 21 years and over.	4,155	2368	1707	(b)536	186(b)
Census 1870.					
Total Males 21 years and over, who "cannot write"	88,650	12,048	76,612	1947	8154
State Registrar of Voters.					
Number of Voters registered in 1874	167,604	76,823	90,781	28,054	18,145
Supervisors of Election, and Returning Board of 1874.					
Vote as cast (Supervisors of Election)	(c)146,523	(c)75.136	(c)71,387	25,741	13,281
Vote as counted (Returning Board)	(d)143,305	(d)71,253	(d)72,052	26,204	14,062

TABLE II.

Statistics of Males 21 years and over, and of Voters Registered in Louisiana and in New Orleans, 1868, 1870, 1872, and 1874.

	The 57 Parishes of Louisiana.			Orleans Parish, or New Orleans.		
	Total.	White.	Colored.	Total.	White.	Colored.
Males 21 years and over	173,979	87,066	86,913	47,708	35,310	12 398
" " " in New Orleans in 1874 (Carrollton included)	"	"	"	49,539	36,198	13,341
Registered Voters; white and colored, in 1868 (f)	146,398	Not Stated.	Not Stated.	(e)41,733(?)	(e)25,679(?)	(e)16,054(?)
" " " " " 1872 (f)	187,226	88,179	99,047	54.882	34,501	20,381
" " " " " 1874	167,604	76,823	90,781	46,199	28,054	18,145
		Signers.	X Markers.		Signers.	X Markers.
Registered Voters, Signers, and † markers, in 1868	146,398	58,229	88,169	(e)41,733(?)	(e)25,913(?)	(e)15,820.(?)
" " " " " " 1870	153,360	(g)54,648	(g)98,712	36,057	18,720	17,337
" " " " " " 1874	167,604	(h)71,391	(h)96,213	46,199	29,958	16,241
Males 21 years and over, who "cannot write"	88,650	12,048	76,612	10,101	1947	8,154

TABLE III.

Statistics of Males 21 years and over, and Registered Voters in Eleven (i) *only of the Wards* (15 *in* 1870, *and* 17 *in* 1874) *of New Orleans. For these only are the Official Reports complete, and their boundaries not been changed. Said eleven wards are the 2d, 3d, 4th, 5th, 6th, 7th, 8th, 9th, 10th, 11th, and 15th.*

	Eleven (i) Wards of New Orleans.		
	Total	White.	Colored.
Total Population in the 11 Wards	167,175	122,081	45,094
" " of Males in the 11 Wards	78,856	59,965	18,891
" " " " " " 21 years and over	41,700	31,727	9,973
Registered Voters, White and Colored, in 1868, in the 11 Wards	38,603	23,449	15,154
" " " " " in 1872, " " "	47,541	30,072	17,469
" " " " " in 1874, " " "	38,252	23,414	14,838
Registered Voters, signers (White), † markers (Colored) in 1868, in the 11 Wards	38,603	23,961	14,642
" " " " " " in 1870 " " "	31,207	16,445	14,762
Males 21 years and over, who "cannot write"	8,831	1,652	7,179

TABLE IV.

Statistics of the Native and Foreign born White and Colored Males 21 years and over, and of the Registered Voters of New Orleans, in 1868, 1870, 1874, *with Mortality Statistics of the Native and Foreign born Population of NEW ORLEANS.*

	Sum Total of Native and Foreign Born.	Native Born.			Foreign Born.		
		Total.	White.	Colored.	Total.	White.	Colored.
Males 21 years and over in 1868, in Eleven Wards (i) of New Orleans,	41,700	21,910	.....	.....	19,790	.....	.....
Registered Voters " " " " " " "	38,603	25,017	.....	.....	13,586	.....	.....
Males 21 years and over, in 1870, in the total 15 Wards of N. Orleans.	47,712	(k)25,069	(k)12,884	(k)12,185	22,643	22,426	217
Registered Voters " " " " " " " "	36,057	(k)26,861	.....	.....	(k)9,196	.....	.....
Males 21 years and over, in 1874, in the total 17 Wards of N. Orleans,	49,539	26,433	13,322	13.111	23,106	22,876	230(?)
Registered Voters " " " " " " " "	46,199	32,831	.....	.....	13,368	.....	.....
Total Population in 1870	.....	140,923	.....	.....	50,495	.....	.....
Mortality—Annual Average of 3 years, 1868-1870	.....	(l)4010	1-35 of 140,923=4026		(l)1500	(1-34 of 50,485=1485)	
" " " of 4 years, 1871-1874	.....	(l)4830	1-29 of 149,923=4860		(l)1660	(1,30 of 50,495=1683)	

TABLE V.

Statistics of the Republican and Opposition (Democratic, Fusion, Conservative) Vote in Louisiana, and in New Orleans, in 1870, 1872, *and* 1874.

	The 57 Parishes of Louisiana.			Orleans Parish, or New Orleans.		
	Total.	Opposition,	Republican	Total.	Opposition.	Republican.
Vote of 1870	106,544	41,013	65,531	30,400	12,046	18,354
Vote as cast (Forman-Mitchell Board), of 1872	(m)125,611	(m)66,384	(m)59,227	35,807	22,110	13,697
Vote az counted (Lynch-Bovee-Hawkins Beard), of 1872	126,969	54,079	72,890	34,580	20,537	14,043
Vote as cast (Supervisors of Election) of 1874	(c)146,523	(c)75,136	(c)71,837	39,022	25,741	13,281
Vote as counted by the Returning Board, of 1874	d)143,305	(d)71,253	(d)72,052	40,266	26,204	14 062

NOTES ON THE TABLES.

(*a*). Total Parishes in 1874, 57; in 1870, 53. The four additions are Webster, constructed (1871) out of Bienville, Bossier and Claiborne ; Red River, (1871) out of Bienville, Bossier, Caddo, DeSoto and Natchitoches ; Vernon, (1871) out of Rapides, Sabine and Natchitoches ; and Lincoln, (1873) out of Bienville, Claiborne, Jackson and Union. Besides these changes of 14 Parishes, Carrollton, in Jefferson, was added in 1874 to Orleans Parish.

(*b*). The annual average deaths in New Orleans for the four years, 1871–4, illustrate the facts for every year since 1867. The deaths of "536" white, and "186" colored males, 21 years and over, occurred during the four months, September, October, November and December, 1874.

(*c*). The votes of 1874 are the only figures *estimated* in Table I. The estimate (c) is solely for Carroll Parish, not reported. Deducting Orleans Parish, the estimated vote in the Table for the remaining 56 Country Parishes is 49,395 Conservative, plus 58 106 Republican votes ; while the figures actually given for the 55 Country Parishes, alone reported, are in fact "48,929 Conservative, plus 56,438 Republican" votes. The increased numbers in the Table are due to estimates for Carroll Parish, based on the number of registered voters reported for said Parish. The true data for the vote cast, as actually reported in the 55 Country Parishes, are : 50,221 white males 21 years and over, registered 48,315 white voters, and *cast* "48,929" Conservative votes; while 71,475 colored males 21 years and over, registered 70,550 colored voters, and cast "56,438" Republican votes.

(*d*.) The vote *counted* by the Returning Board was for Orleans, and *only* 52 Country Parishes ; and the vote actually reported for said 52 Parishes was in fact "42,380 Conservative, plus 56,382" Republican votes. To these figures estimates were added for the omitted four Parishes of Bienville, DeSoto, Grant and Winn ; estimates based on the proportion of white and colored registered voters in said Parishes, and much more favorable to the Republicans than the actual vote cast. The true data for the vote counted by the Returning Board in the 52 Country Parishes actually reported by it, are : 47,709 white males 21 years and over, registered 45,870 voters, and cast "42,380" Conservative votes ; while 71,340 colored males 21 years and over, registered 70,519 voters, and cast "56,382" Republican votes.

(*e*). The numbers given in Table II for the registration of New Orleans in 1868, should be considerably increased, in order to be fairly comparable with the data of the census of 1870 ; for these numbers do not include the registration of the Twelfth, Thirteenth and Fourteenth Wards, annexed after 1868. These three Wards contained 10,836 total population, viz : 7,710 whites, plus 3,126 colored, of the 140,923 total white, and 50,536 total colored population of the New Orleans of 1870. Therefore the figures in the Table require considerable additions.

(*f*). The white and colored registered voters in the State were not reported for 1868 ; and, in 1870, they were not reported for either the State or City. The registration of 1872 was not made de novo, but was only a revision of and addition to the registration of 1870, hence, allowance should be made for the larger numbers reported for 1872, than for any other years.

(*g*). The registration of 1872 reports "the signers and X markers" actually as "49,448 plus 89,335," instead of the 54,648 plus 98,712 given in Table II; which numbers are given because said registration omits to report the signers and X markers of five Parishes, in which are reported "14,577 registered voters." The figures in the Table are an increase of the figures reported by pro rata estimates of the 14,577 registered voters in the five Parishes omitted.

(*h*). The registration of 1874 actually reports *the signers and X markers* as "67,859 plus 91,052," instead of the 71,391 plus 96,213 given in Table II. This discrepancy is due to the addition to the figures of pro rata estimates of "8,693 registered voters" in these Parishes, as to whom the Official Report omits the specification of "signers and X markers."

(*i*). New Orleans had in 1868, 11 Wards only ; in 1870, 15 ; and in 1874, 17.

The Official Reports as to the First Ward, are not all of them complete, therefore it is excluded : all are complete as to the Fifteenth Ward, (Algiers), therefore it is included in Tables III and IV, though Algiers was not (in 1868) a part of New Orleans.

(*k*). In 1870, there were about 18,500 whites registered in New Orleans. From these deduct about 9000 foreign whites registered, and the result is that only about 9500 *native white* males were registered. If these 9500 whites be deducted from the 26,861 total natives registered, the result is that about 17,361 native born colored males were registered—this, though the census reports that there were in 1870, only 12,185 native born colored males 21 years and over in New Orleans. A like gross discrepancy between the number of native colored males living and *registering* in New Orleans can be similarly proved in the registrations of 1868 and 1874. I have been unable to obtain the registration statistics of the foreign and native born registered in 1872.

(*l*). During the three years 1868-1870, there were 28.5 deaths of natives per 1000 of the living natives, and 30. per thousand of the foreign born; during the 4 years 1871–1874, 34. per thousand of the native, and 33. per thousand of the foreign born population died. This close correspondence of deaths to population proves conclusively that the native and foreign populations as reported by the census *must be* comparatively and approximatively correct.

(*m*). The vote of 1872 as cast (Mitchell-Forman Board) excluded three Parishes, the vote of which, as *counted* (by the Lynch-Bovee-Hawkins Board) was 4,611, viz : 2188 Conservative plus 2433 Republican votes. The figures in Table V have neither been increased by these, nor by any estimated additions for said three excluded Parishes.

(*n*). Note as to Males and Females.—The data in Table I show that there is in New Orleans an excess of 3518 white, and of 7.095 colored females. A similar excess of females will be found in all of the least fluctuating cities of the United States ; and also a greater excess of colored females over colored males, than of white females over white males. A large part of the excess of females in New Orleans is of child bearing age ; and with the increase (over 1860) of this class, the census consistently reports a corresponding increase of children under one year of age ; and in this, our graveyards confirm the census, by testifying to more deaths, and (by unquestionable inference) to more births. In fine, after comparing the census and mortality reports of 1860, with those of 1870, no doubt is left that the census does *correctly* report a large increase, and excess in New Orleans of females, particularly of those of child bearing age, and more particularly still of colored females. Among the causes for these facts, consideration is due the following : the large foreign population is white, and has an excess of males 21 years and over; and cities have a larger number than the country, of occupations for females.

The preceding tables prove, that five data (four of these non-partisan) derived from three different sources, tend strongly to agree with each other ; and that all are at variance with one set of data, derived from one unquestionably partisan source. It should not be forgotten that all of the data are derived exclusively from the officials of the republican party ; and that the term "non-partisan" is used to designate such statistics as were not collected for avowedly partisan purposes. If any of these statistics be unreliable, the responsibility rests on the officials, and not on me, who discharges the office simply of compiler and commentator. As compiler I submit the tables with notes of explanation ; as commentator I will call attention to the important facts proved by the tables.

COMMENTS ON THE 5 TABLES, AS TO POPULATION, MORTALITY, VOTING, REGISTRATION, ETC.

The statistics given (Table I) of the white and colored population in three classes, viz: of totals, of males and females, and of males 21 + years, demonstrate that these three classes are proportionate to each other in perfect accord with the well established laws of population, in stationary or moderately progressive communities. So conformable thereto are these and other data in the census of 1870, that if the mode of taking the census, and the deficiency of culture of those taking it be duly considered,—then this conformity establishes coincidences which argue very strongly in favor of the comparative correctness of said census. It reports that there is in Louisiana a slight excess* of white males 21 and more years old over the colored.

Referring now to the census mortality statistics, it will be found that a good many more whites die than colored; and that this excess of white deaths is especially manifest as to the white males 21 + years. But, as to these mortality statistics of the census, I fully concur with the Superintendent of the Census, who states (Census Vol. III) that though they are characterized by "wholesale omissions," yet, that they are valuable for the comparison of the relative mortality by age, sex, "*race*," etc. Therefore, making due allowance for all defects of omission, it still remains evident, that the census statistics of the dead coincide most emphatically with the statistics of the living, in declaring that there are more white than colored males 21 + years in Louisiana. On further examination of the census, which states that Louisiana had nearly 87,000 colored males 21 + years, it will be found that it reports, as to said colored males, that there are about 77,000 of them who "cannot write." Once more, make liberal allowance for probable errors as to such a statistical item, and consider therewith well known facts as to the education of the colored population, and the approximative numbers given add one more to the list of other coincidences, which constitutes five coincidences so curious,—as to recal a law of scientific evidence, viz: "truth is approximated as the number of coincidences increase." These are a few only of the internal evidences

* The considerations—that there were of the 362,065 total whites, about 60,000 foreign whites, and that the foreign immigrants have always a large excess of males 21 years and over—are quite sufficient to account for the fact, that there were more white than colored males 21 years and over, notwithstanding that the *total* colored slightly exceeds the total white population.

presented by the census of its comparative correctness, yet nothing more is claimed than that there has now been submitted strong presumptive evidence in its favor.

For a sixth coincidence, pass on to a second source, the polls. The census reports that the white and colored populations are about equal,—and, lo! those bitter foes, the Conservatives and Radicals of Louisiana, *both* unite in 1874, in testifying to identically the same fact. Examine well the vote, whether "as cast," or "as counted" by the Returning Board, and see how fully this evidence coincides with the census. For this coincidence from antagonists in favor of the census, the statistician is justified in claiming unusual weight. But this evidence from the votes "as cast," and "as counted" is not all; for, while the census reports that the colored males 21 + years are about 87,000, the Returning Board says they voted in 1874, 72,052 Republican votes, in 1870 only 65,531, and in 1872, the (Lynch, Bovee, Hawkins) Returning Board,—unfettered by any ballots to count, left with their political imagination to riot at will—succeeded in estimating the vote as only 72,890, the highest Republican vote ever cast, or claimed to have been cast in Louisiana. Now, when such partisan authority, under such circumstances, report successively the total votes of 65,531, 72,890, and 72,052, (which last is confirmed by the Supervisors of Election,) it cannot be reasonably doubted that the census report of about 87,000 colored males 21 + years must be a close approximation to the truth; and that even partisan calculations of the vote fail to justify a higher estimate.

Conservatives urge that the census greatly under-estimated the white, and Republicans urge a like claim as to the colored population. Fortunately, the statistician is in the unusual position that he can seize both horns of this dilemma, and, unharmed, turn them aside with the declaration that this man's under and that man's over estimate is of little consequence, compared with the only point essential to be proved, viz: the *relative proportion* of the white and colored populations. I trust due appreciation has been given the fact, that I have already summoned five witnesses, viz: the census, the Supervisors of Election of 1874, he Radical Returning Boards of 1874, 1872, and also of 1870; and that the first three of these witnesses concur in testifying that the relative proportion of the white and colored populations is about equal, while all five of these witnesses concur in stating

that the absolute number of the males 21 + years does not exceed about 87,000.

But, as to this relative proportion, and whether it be correctly represented by the census with the two other witnesses, I have reserved my fourth and best witness—the only one I can guarantee—the only one, who at this very time is so testifying that the evidence can be readily made, at all times, an experimentum crucis. It is to be regretted that this witness limits his testimony to less than a third of the population of Louisiana,—the population of this city. I leave others to estimate what weight should be given to this testimony as to a large part of the population of Louisiana, in reference to the whole; and will now summon our dead to speak, as represented by a Republican Board of Health, reporting for the sextons of the cemeteries of New Orleans.

The census reports that in New Orleans the *total* colored is relatively to the *total* white population as 100 to 269; and the sextons report, that for years past, they died as 100 to 180. No vital statistician has ventured, nor is likely to venture to assert that this relatively greater mortality contradicts the census;—in truth it confirms it for these three reasons, among others, viz: that (1) in this, as in all other cities, the colored death-rate is higher than the white, except during yellow fever epidemics, of which New Orleans has had none since 1867; (2) in this city, this relative disproportion is largely increased by the much greater prevalence of small-pox among the colored, and also (3) by the excessively high death-rate of colored children. Having stated these indisputable facts, as to the *total* population, and the *total* deaths of the white and colored relatively to each other; similar facts, of far greater significance will now be examined as to the population and deaths of the colored and white *males* 21 + *yrs.*

The census reports that there are in N. O., of such males 100 colored to every 271 whites, and the sextons report that they died as 100 *to every* 288, from October, 1874, to January, 1875, the very four months during which they were registered, did vote, and had their votes counted. This surprising coincidence, and unanswerable fact must not be permitted to be glossed over, nor to be swallowed up in a mass of statistical details. It is the most decisive fact yet presented, its value as testimony surpasses the combined evidence of all the other witnesses. This confirmation of the comparative correctness of the census as to the relative

proportion in New Orleans of the white and colored males 21 + yrs. cannot be overthrown. I, who alone know the mode and cause by which the proportion of the white, and colored dead males 21 + yrs., was established, have been so profoundly impressed, that no other figures or testimony can eradicate my conviction, that whatever may be the errors of the census, it does, beyond doubt, represent with wonderfully approximative correctness *the relative proportion* of the white and colored males 21 + years now residing in this city. It should be remembered that there are now four Republican witnesses, who coincide in their testimony as to the relative proportion of the white and colored males 21 + years, and six Republican witnesses who coincide in their testimony as to the absolute number of the colored males,—all being in general accord with one of these witnesses, the census, a witness who has been appealed to, and abused in turn by each political party.

Before abandoning the subject of the *relative* proportion of the white and colored males 21 + years, the inquiry must be answered, whether said colored males have increased in number since 1870. So the Republicans claim, and so I have been assured by the highest authority on this subject in that party. To the mind of the philosophic student of science, restless with a fact until its cause be determined, this alleged fact is most perplexing. For all admit, that Louisiana is sick unto death, and all see learned doctors of statecraft hastening from distant States to hold consultation after consultation at the bedside of the agonized patient. Our maternal pelican gorges an insatiable hunger with food snatched from the very beaks of her starving brood,—our Justice, tearing her holy bandage from off her own eyes, glares unabashed at Rail Road stocks, Superior Court emoluments, and other injudicial compensations,—intimidation, spoliation, and brutal massacres are, it is charged, visited on the lowly and ignorant, but kindly African,—and yet in spite of all this—he persists, so says the same class of witnesses, in flocking here from the Potomac to the Rio Grande, to conceal himself, with hushed voice, in sheltering swamps, from the barbarous banditti of Louisiana! The protective instinct of migratory birds prompts more wisely than this, and the most intensely partisan of all my official witnesses flatly contradict this inconsistent fabrication. For, the Registration reports a slight decrease of these males in 1874, when compared with 1870, and 1872; and

the Returning Board of 1874 "*counted*" the vote, a little less than it was "counted" in 1872. This testimony cannot be impugned by Republicans, except by urging intimidation as the true explanation. Before accepting this explanation, let honest judgment halt, until Republican official statistics have had their say, as to this charge of intimidation.

All the witnesses have now been heard, except the seventh and last set,—the State Registrars of Voters. These are the only witnesses, who have been incessantly appealed to and credited by republicans from the lowest to the highest in the land. On the evidence of solely these witnesses is founded the extravagant claim, that a "fair and peaceable" election in Louisiana would give the republican party "20,000 majority." This, notwithstanding the fact, that these witnesses give the lie direct to all the other six republican witnesses;—this, although they, as will now be proved, scorn the census, and scoff at the graveyard.

The census, confirmed by the other witnesses, reports that Louisiana has about 87,000 colored males 21+yrs. Now, notwithstanding the number of these insane, bed-ridden, unavoidably absent, intimidated, etc., these 87,000 males succeeded in being registered as ninety-odd thousand in 1870, and 1872, and 90,781 in 1874. (See Table II). These remarkable feats of political multiplication cannot be equalled, outside of the reconstructed States. But more still, the census and all the other witnesses, concur in reporting that the white males 21+yrs. certainly equal in number the colored,—yet in every one of these registration years far fewer white, than colored males, were registered. During the very year of the census, it will be found that while "86,913" colored males registered more than 90,000, yet "87,066" white males registered only abont 55,000 voters; and still, it is confidently charged that the colored have, and that the whites have not been intimidated. It is true that the numbers given, as registered in 1870, are estimates of the white and colored, from the only fact reported, viz., the number who signed their names, and who X marked. But Tables II. and III. furnish abundant data for determining the relationship of the latter to the former class, and fully justify the estimates given.

However, a special Table, No IV., has been compiled, in order to get rid of all estimates, and at the same time, to make use of that invaluable confederate of the census, our graveyards;—the facts as to 11 Wards of New Orleans must be stated, inasmuch

as the statistics for these, and only these wards are fortunately complete. (See Tabie III). In 1868, the white and colored registered voters, also the signers and X markers, are both given,—while in 1870 only the latter. In 1868, the difference between the whites, and signers on the one hand, and between the colored and X markers on the other, was only about two per cent.,—and the X markers of 1870 are enumerated in almost identical numbers with the X markers, and the colored, registered in 1868. The Tables (II. and III.) furnish other facts in proof, that the X markers always approximate closely to the number of colored voters registered; and, with slight allowances, represent the number of colored males who were registered.

But these are comparatively insignificant facts as to the registration of these 11 Wards. The astounding facts are that *only* 9,973 colored males 21 + years, registered of colored voters, 15,154 in 1868, an equal number in 1870, 17,469 in 1872, and 14,838 in 1874!! During these four reconstruction-registrations, the whites were never once registered, even as two to one; while in three of these years, (1868, 1870, 1874) more than two colored voters were registered to every three whites. Yet, the census reported in 1870, and our graveyards reported then I doubt not, as certainly they do now report *weekly*, that the colored males 21 + years, both live and die here as about one to every three white males. I must pause to deplore the misfortune that this productive registration secret of converting every one negro male into two able-bodied voters has not yet been applied to the abandoned farms of Louisiana!

During the registration of 1874, as also of its predecessors, a great hue and cry of partisan accusation went forth against the fraudulent registration of foreign white males, not entitled, as was asserted, to become naturalized citizens. Submitting this charge to statistical examination, (see Table IV), the first curious fact detected was, that there were registered in 1874, 5000 more *native born colored voters* than the census reports is the sum total of said males. In proof that this enormous excess in 1874 over the census of 1870 was not due to increase of said population, it will be found that a similar excess occurred both in 1868 and in 1870.

In detecting this gross discrepancy between the census and the registration of native born colored, there was at the same time, detected the only fact yet discovered which apparently

favored the white population. For it was found, that while the native born colored registered in 1874 exceeded said population of males 21 + years by 5000; the native born whites registered did, at the same time, exceed their corresponding population by 1300. Both figures indicate (the one enormously, the other in about one-fourth degree) either fraud or discrepancy with the census. Incredulous, as to the official encouragement of frauds in favor of the whites, but credulous as to the comparative correctness of the census, I appealed to this old friend to explain this embarrassment. Its only reply is, that four years have passed since 1870, and that in that year the native born whites had about 3685 youths, from 17 to 21 years old; while the native colored had only about 1300 of such youths. Now, after making due allowance for the number of white males dead since 1870, it is believed that in the 3685 white youths of 1870, there is margin to explain the discrepancy of 1300, between the number of white males in 1870, and the number of such whites registered in 1874. But, it is a fact of general notoriety, that a considerable number of native born whites understand how to vote, and probably how to register, just as "early and often" as any other race; and I concede from this source any such addition to the 3685 youths as extra-statistical proofs may call for. In connection with such a concession, the fact is not to be forgotten that, in all of the statistics, this is the only advantage discovered in favor apparently of the white race; and that this advantage of some 1300 native whites is far more than compensated for, by a corresponding advantage of some 5000 in favor of the native colored voters registered.

Abandoning this point, in order to seek cause for the hue and cry against the foreign born, it will be found that the total number of these registered in 1874 was "13,368;" while the census reports that there were in 1870, 23,106 foreign born males 21 + years residing in New Orleans! These numbers force at once this curious problem,—Say that 13,000 colored males 21 + years did register 18,000 voters, then ought 23,000 foreign born males 21 + years to register 13,000 voters? The latter part of this problem *might not* be true, though lacking the monstrous absurdity of the former part. An honest solution requires proofs, that there are 23,000 foreign born males 21 + years, and that these have been in the United States five or more years. Is our total foreign born population as large as reported by the census? Our

graveyards for seven years, 1868–1874, answer (as Table IV. shows) *yes*, with reiterated annual emphasis.

Has this foreign born population been in the United States five years or more? In New Orleans, as throughout the Union, the foreign born males 21 + years are necessarily in very large proportion to the total foreign population. Further, New Orleans, if compared with other cities, has in consequence of its uninviting condition attracted, since the war, a smaller proportion of this population, (as the census shows) than have any of the large and prosperous cities of the United States. As an inevitable result of this, New Orleans possesses now a much larger proportion, than other cities, of foreign males entitled, by length of residence, to become naturalized citizens. But should these general facts fail to carry conviction, — then the doubter is earnestly solicited to explain satisfactorily the following statistical data, viz:—New Orleans in 1860, with its four districts only had 168,675 total population, and of these 64,621 were foreign born; while New Orleans, increased in 1874 by three additional districts, had a total population of 197,913, and of these only 51,472 were foreign born. If this 10 years' decrease, by more than 13,000 of the total foreign population, does not prove conclusively, that this population has not been increased by recent immigrants, but is long resident in New Orleans, then I am at total loss to explain its significance; and therefore assert that the census, the New Orleans graveyards, and well known general facts unite to prove,—that the census reports with comparative correctness the number of foreign born males 21 + years,—and that these, for the most part, have certainly been long residents of this city.*

The fact that 13,000 colored males registered, with silent approbation, 18,000 voters; while a great hue and cry were made because 23,000 foreign born white males registered only about 13,000 voters, is statistically but a practical illustration of a well known phenomenon of animal physiology. For, statistics justify the belief, that our registration cuttle-fish squirted his inky filth over his foreign foe,—in order that he might escape unmolested with his "school" of black minnows!

Some other of the dissentient statements, and inexplicable inconsistencies of the registration deserve mention. Notwithstand-

* In the Country Parishes the foreign born population was in 1860, 16,354; and in 1870, only 10,355. Thus the total foreign population has during the last decimal decreased in Louisiana by 6000 out of, and by over 13,000 in New Orleans.

ing that, the Republican colored voters in this city were diminished, on the one hand, by many deserters, yet reinforced, on the other hand,—more largely here, than elsewhere in this State,—by a horde of white officials with their dependants, yet the "18,145" colored voters registered succeeded in having their vote "*counted*" by the Returning Board, as *only* 14,000 in 1874; and its peculiarly unfettered predecessor of 1872 did not count them a whit better. This testimony of these two famous (?) Returning Boards tends strongly to prove that the census and graveyards tell the truth. But, if these last two witnesses are discredited, and it be claimed that these Returning Boards, and the Registration alone tell the truth,—then it follows inevitably, that there must have been greater intimidation of the colored voters in 1874, here in New Orleans, than any where else in this State; and that there is no cause to seek further than this city for proofs of that intimidation. Again, it will be found as to the registration of 1874, in this city, that if, on the one hand, the colored males 21 + years had been registered in the same proportion to the number of said males, as were the whites, then the colored should have been registered 10,300 instead of 18,000; while on the other hand, if the white males had been registered in the same proportion to their number as were the colored, then there should have been 49,000 whites registered instead of 28,000! Finally, if the 18,000 colored voters were registered proportionately to their total population, in the same ratio as the whites were registered, then the total colored population of New Orleans should be really more than 93,000, instead of the "53,837" reported by the census!! This statistical conclusion is a reductio ad absurdum, as the annual mortality statistics for years past, as well as now, fortunately prove abundantly.

I have attempted, in every imagined mode, to solve the great political conundrum,—how did only "13,341" colored males 21 + years, succeed in registering "18,145" voters? A little personal experience in 1870 induced me to investigate what number of negro boys might have loaned a helping hand to their 13,341 elders; and I found that all of these boys, even from 15 to 21 years old, would have swelled the number to only 15,422, thus leaving ample *repeating* margin for enterprising "George Washingtons." In this connection, I recalled the reported boast of a famous Radical chieftain,—that he "held 20,000 votes in the hollow of his hand;" and find, that statistics justify the suggest-

ion, that he spoke metaphorically, and in plain language meant, that he had the registration certificates of 20,000 colored, and X mark voters, in the bottom of his carpet-bag.

Before leaving this subject of registration, especial attention is called to the statistics of 1868. This "military registration" is claimed to have been eminently fair and impartial; and is cited as a criterion by which to judge the correctness of its successors. The simple figures do not need my explanatory aid, for, unadorned they speak with sufficient eloquence, in very solemnly warning every American against military interference. Like the registration of 1874, and all the others, it presents unmistakable evidence of political crime so great, as to stifle, and so silence the voice of *true* Republicanism with indignation. These registrations, when studied dispassionately, present such precious specimens of statistical monstrosities, as to entitle them to that permanent preservation I destine for them in the annals of science.

In addition to the intimidation—facts already stated incidentally, statistics have other facts bearing more directly on the same subject. Did intimidation occur at the registration, or the polls,—or at both?

Louisiana had in 1870,—"86,913" colored males 21 + years, and registered in 1874, "90,781" colored voters;—and the registrations of 1868, 1870, 1872 prove plainly that the registrars did not, in these years, manage to make the 86,913 much more productive than in 1874. While the 86,913 colored males yielded 90,781, the 87,066 white males yielded only 76,823 registered voters. If either race were intimidated from registering, these Republican official figures indicate with sufficient clearness, which race it was.

Were the colored voters intimidated at the polls? Let us examine, what some of the *unintimidated* states are in the habit of voting in time of great political excitement. As the census of 1870 must be used, let us in order to approximate to it take for comparison with it, the exciting presidential contest of 1872; except as to the State of Maine, for which will be used the gubernatorial election of 1872, since this drew out a much heavier vote than did the presidential contest, and the largest vote that State had (I believe) ever cast to that date. [For these statistics of the vote cast see the "Tribune Almanac, 1873"].

The results are found to be that in fortunate Massachusetts,—

where, in consequence of restricted suffrage, Louisiana's "86,913" colored males, having "90,781" registered voters, or rather the "96,213" X markers of 1874, would have (if I am correctly informed by two ex-U. S. Senators) a very slim chance indeed of casting any vote at all,—I say the results are, that—

Massachusetts	cast	100	votes	to	every	162	males	21 +	years.
Illinois	"	"	"	"	"	127	"	"	"
Maine	"	"	"	"	"	120	"	"	"
New York	"	"	"	"	"	118	"	"	"

It would be extremely difficult, if not impossible, to find any unintimidated States whatever, surpassing Maine and New York. How will it be supposed the "*intimidated*" Louisiana of 1874 compares with these unintimidated States of 1872? Even the Returning Board has *so* "*counted*" as to show that "intimidated" Louisiana cast 100 votes to every 121 males 21 years and over; while, the vote "as actually *cast*," (so say the Republican Supervisors of Election,) shows 100 votes to every 119 males 21 years and over,—and 100 Republican votes to every 121 colored males 21 years and over. These statistics fail totally to find any margin for general intimidation,—on the contrary, the vote is so large as to justify suspicion of fraud. If such suspicion be sustained by other proofs, the statistician would seek a solution, by demanding what party controlled the polls, and had the power to perpetrate the fraud?

In conclusion, if, excluding the manifestly fraudulent registration, *all the other* statistical facts deserve credence.—then it is evident that in 1874, as in preceding years, the result of every "fair and peacable election" depended entirely on whether more whites voted with the colored, or vice versa. And taking a statistical view of the question, whether the Conservatives or Republicans did fairly carry Louisiana in 1874, it is necessary to have proofs only, as to which one of the two races was most numerously aided by the other.

The unpartisan educated common-sense of Louisiana has fully concluded, that which statistics, with other proofs, justify,—that the frauds of the registration have been repeated by the Returning Board. This conclusion is sunk in conviction too deep to be dug out with sabres. And, as an American, dating back to a huguenot's escape from La Rochelle, I am not yet prepared to believe, that this ineradicable conviction is destined to be supplanted by the permanent establishment of the political princi-

ple,—fatal to republicanism,—*that the bayonet ought to rule the ballot.*

One last word,—when distant communities are tempted to cast a stone at the *good* citizens of Louisiana,—let them first strive to appreciate our true condition, and, then, to imitate the divine charity of Christ's prayer,—"lead us not into temptation, but DELIVER US FROM EVIL."

ARTICLE VI. *Alcohol—its Action and its Use in Disease. A Clinical Lecture delivered in the Charity Hospital, December 4th*, 1874. By F. HAWTHORN, M.D., Professor of Obstetrics, etc., Medical Department, University of Louisiana.

Gentlemen:—I asserted broadly day before yesterday, while talking over a case of pneumonia in the amphitheatre, the apparently startling proposition that alcohol is not a stimulant, as always supposed, but a sedative. There was present on the occasion a brother chip from Chicago, and as at the moment the eyes of the class turned towards him I inferred that in some way he indicated either surprise or incredulity. Moreover, I have since been informed that in a lecture subsequent to mine one of your professors spoke of alcohol as assisting digestion by its *stimulant* action. and as by these things doubt seems to be thrown upon my statement I propose to give you now some of the reasons for the faith that is in me.

First, allow me to say that I do not accept the notion that any medicinal substance exercises a different effect in proportion to the quantity taken—that is to say that in a small dose it is a stimulant, and in a large dose something else. Whatever *systemic* effect it has at all is the same in kind throughout, whether it be taken in large or in small quantity. A substance may be pungent, however, and therefore, as an irritant, act locally and within limits as a stimulant; but whenever given to such extent as to produce its systemic effect, that effect will always be of one and the same *kind*. Thus alcohol through contact with the stomach may favor digestion, but when taken in such quantity as to produce its systemic effect it always interferes with this process.

Let us consider now its general action—is it a stimulant?

1. Stimulation implies a heightening of function or an in-

crease of force. The important motor influence in the economy is heat. Reduce this below a natural standard and none of the functions are properly discharged; increase it within certain limits and functional activity is proportionally increased. Now according to physiologists alcohol lowers the temperature of the body—therefore alcohol can not be a stimulant.

2. The functional attribute of one portion of the nervous system is personified as "sensibility." The very earliest perceivable effect of alcohol is a feeling of tingling and numbness in the more sensitive parts of the body, which, as the influence deepens, extends to absolute insensibility. This lowering of function in the sensory system is certainly not the effect of stimulation.

3. Again, one of the first evidences that one is under the influence of alcohol is impairment of muscular control—a little thickness of speech, unsteadiness of gait, etc., and then comes muscular paralysis more or less complete. Since stimulation involves increase of function, these certainly are not the effects of a stimulant.

4. If the heart be entirely removed from the body before death it continues for a period still to pulsate. The heart generates its own motor power by means of certain ganglia or nerve centres situated in its substance. The rapidity of its action is regulated by the pneumogastric nerves, much in the same way as the brakes regulate the speed of a railway train. If the pneumogastric nerves be gently irritated, as by pinching, their inhibitory influence (in accordance with the universal law that if any organ or tissue be moderately excited its functional activity is heightened) is increased and the heart beats more slowly. Cut the pneumogastrics so that their influence over the heart is entirely done away with and the pulse vastly increases in frequency. Alcohol increases the frequency of the pulse—therefore it withdraws in part the influence of the pneumogastric nerves; in other words diminishes their functional activity, that is, depresses them. The increased frequency of pulse under alcohol, then, is not due to its stimulant effect but to its sedative action rather.

5. The gross function of the cerebrum is the production of mind, but mind has many elements. The very highest outcome of this organ (the cerebrum) are the moral faculties or qualities, and these are found in the greatest degree of refinement among those classes whose intellectual development is greatest. What-

ever tends to depress man affects these qualities first and approximates him to the inferior orders. It is indisputable that alcohol blunts the moral sensibilities even before its influence in other regards is manifest. The loss of a sense of propriety, as shown in a disregard for persons and places, a neglect of one's self or loss of self respect, and finally outrage on the rights of others, as in theft or murder, are amongst our commonest experiences of its influence. These things are not evidences of an elevation but of a lowering of the function of the cerebrum.

So we may conclude fairly, I think, that alcohol is not a stimulant but a sedative. Still there is no question that alcohol is serviceable, especially in the treatment of those diseases in which the tendency to death is by exhaustion. To what is its favorable influence attributable? I answer without hesitation—*to its sedative action.* Listen to the argument.

In typhoid fever, for illustration, the patient is in danger not immediately from the poison which produces the disease, but from its effects—the wasting or destruction of the tissues under the influence of that poison. Now suppose you should stimulate a patient suffering from typhoid fever, what would be the consequence? Let us see. Stimulation heightens function or increases force. Increased vitality necessitates increased functional activity throughout the body. But force can be obtained only at the expense of matter. Ignite an ounce of gunpowder, and the explosion involves the destruction of the powder. So if you stimulate the system—if you do anything by which an increase of force is secured, you must inevitably cause an increased destruction of tissue. You see at once, then, that a *stimulant* is the last thing you desire in typhoid fever; on the contrary, you wish something which will retard waste and thereby protect against exhaustion. Alcohol is a sedative to function—it is a sedative, therefore, to the nutritive changes by which function is engendered.

To explain how it acts, it is not indispensable to show what becomes of alcohol in the system, as some physiologists have attempted to do. We give a grain of strychnia with convulsions as the result, but we need not show exactly what chemical change has occurred with the strychnia before we can admit how it has affected the spinal cord. So alcohol taken may be finally disposed of in one way or another, but while in the system it affects

the nutritive movements in such way as to lower general functional activity.

But what is the manner of its acting?

The last step and essential part of nutrition is assimilation. This is preceded by two distinct and opposite processes—disintegration or destruction and appropriation. The notion accepted by many in regard to this matter is altogether too mechanical, viz: 1, that by the blood oxygen is brought into contact with the tissues, and that without further ado they combine with it and are destroyed; 2, that the blood then furnishes material for repair, and that without further ado repair takes place. Were this all true you can easily conceive what would be constantly happening—we should either go off like popping crackers, or grow more rapidly than Jack's bean-stalk. Fortunately, however, the whole process is under regulation—supply is proportioned to demand, and everything goes on systematically and smoothly. The affinity of tissues for aliment and their combination with oxygen are instigated and regulated, as there is good reason for believing, by nerve influence.

People who take but little exercise require but little food because there is very limited destruction of tissue, while waste takes place much more slowly during sleep than during the waking period. Thus sleep is important in order that repair may get the advantage over decay. Animals which "hybernate" or go into a state of torpor, live for long periods without eating. A man may subsist on alcohol, opium, and such "stimulants." When under their influence he has very little appetite; indeed, requires very little food because of the retarded waste of his tissues. We have just seen a young woman actually live three weeks in this ward on hypodermic injections of morphia. These substances—alcohol and opium—act alike. They are both nerve agents and their influence on the nervous system is of a blunting, lowering character. They bring the system into a hybernating state, and by thus retarding disintegration they protect against exhaustion. *But they do not stimulate.* Indeed if they should stimulate they would be exceedingly hurtful in exhaustive diseases.

Besides, being sedatives they are always strongly contraindicated in acute depression, as shock, or the cold stage of intermittent fever. So you see it is not a mere matter of opinion but

of practical importance, that we entertain a correct idea of their influence.

ARTICLE VII. *A remarkable Case of Opium Poisoning.* By F. HAWTHORN, M.D., Professor of Obstetrics, etc., Medical Department, University of Louisiana.

Mrs. S., the wife of a physician residing in Mississippi, was operated on for the cure of ———, by Professor T. G. Richardson, of the University of Louisiana, on the 7th of January, at 12 o'clock. Two hours before the operation she took twenty-five (25) drops of "Battley's sedative." At 2 o'clock she took the same quantity and at 5 o'clock in the afternoon fifteen drops more. On Friday morning she took one grain of opium in pill, at 12 o'clock fifteen drops of Battley's sedative, and at 8 o'clock p. m. one grain of opium in pill. She was nursing a child—a well-developed, healthy boy just seven weeks old—all this while and observed nothing wrong until during the day, Friday. All day long he was restless, wakeful, easily startled, and fretful except when imperfectly sleeping. Up to 12 o'clock Friday night, he was very troublesome, but now he nursed, and soon afterwards dropped off into a sound sleep. Sleeping through the rest of the night, no particular attention was given to him until about six o'clock Saturday morning, when he aroused and nursed a very little—scarcely any. About half-past eight o'clock his breathing attracted some attention, but it was supposed he had taken cold. He continued sound asleep during the day, refusing altogether to nurse, but nothing particularly was thought of this until 2 o'clock in the afternoon. His breathing now appeared to be bad—diminished somewhat in frequency, shallow and jerking. At half-past eight in the evening his father became alarmed and within the next hour or two several things were done, but without avail. About ten o'clock Saturday night I saw him, at the request of the doctor (his father), and found him profoundly poisoned by opium. His pupils were very much contracted; his breathing very shallow and jerky (like hic-cup), not much diminished in frequency but very irregular. It was for some time impossible to arouse him and it was with difficulty he could be induced to swallow. Fluids put into his mouth in great part ran out again and his skin was pale and perspiring. Some coffee was given by

the mouth and about an ounce of ordinary hotel coffee injected into the rectum. I took him near the window, put up the sash, shook and annoyed him in one way or another so as to awake him somewhat and left about eleven o'clock. He had improved considerably by this time, though, as his father informed me, he became worse subsequently, and about 12 o'clock, on attempting to cry his respiration was arrested for a considerable time and he was thought to be dead. He recovered and gradually grew better, however, and by half-past two o'clock Saturday morning was regarded as out of danger. The points of interest in this case are—1, opium *poisoning* through the agency of the mother's milk; 2, the length of time (about twenty-six hours) the serious effects of the drug continued.

ARTICLE VIII. *A Study on Croup and Diphtheria.* By J. C. FAGET, M. D.

(See New Orleans Medical and Surgical Journal, January, 1875.)

After close study of the subject, and after due consideration of Dr. Cohen's work, so rich and complete that it may be regarded as a condensed expression of our actual general experience in "Croup in its Relations to Tracheotomy," I believe that there is all reason to maintain the conclusions in our preceding article.

Assuredly, the three operations performed *in extremis*, in November last, and all followed by death, are not given as an encouragment to tracheotomy; but they are practical proofs of my conviction that there is scarcely any case, however desperate it may appear, in which the operation must be refused, because "the prognosis of tracheotomy in croup is an impossibility—children operated in the best conditions have died, whilst others, operated under the most unfavorable circumstances, have recovered." And no proof to that proposition could be more conclusive than the case of *that child*, who presented all the signs of general diphtheritic poisoning, and who operated on, with the assistance of Drs. Borde, d'Aquin, and de St. Rome, when asphyxia was more than imminent, recovered, against all hope, after a long convalescence, during which he was paralyzed, and had great weakening of sight, with strabismus.

We therefore readily subscribe to the following by Dr. Cohen (p. 28): "Although tracheotomy, when postponed to the last

minute, can lead to ultimate recovery only in rare instances, *it should be performed*, under certain circumstances, even *in extremis*, or *even after apparent death;* for it has happened in the experience of several operators, that fleeting life has been recalled by the institution of artificial respiration * * Dr. Drysdale (verbal communication) has operated four times with only one recovery; but the case which recovered was, to all appearances, the worst of all, for it was *apparently dead*, having actually ceased breathing, was pulseless, and required the institution of artificial respiration."

We must admit, however, that the cases in which the operation has been long delayed, and especially when resorted to *in extremis*, are very seldom successful. And, is it not probable that the sad results of the first 15 or 20 years practice of tracheotomy in Paris were due partly at least to its being performed *too late?* A few cases, only from the most celebrated surgeons of that time, from 1820 to 1840, are almost discouraging.

The following results were submitted to discussion before the Academy in 1839: Roux, 4 operations, Blandin 5, Velpeau 6, Amussat 6, Baudeloque 15—in all 36 operations, all terminating by death! In a subsequent discussion, a few years afterwards, Malgaigne introduced the list of operations by two new professors in the School of Paris, Nélaton and Gosselin—for Nélaton (previous to 1848), 23 operations and 23 deaths; for Gosselin, 23 operations and also 23 deaths! On the same general list, Monod, jr., appears for 40 operations and 40 deaths! What a bold tenacity!

Truly, it would have been well to despair forever of tracheotomy in croup, if other surgeons in those times had not been more fortunate! Happily, Bretonneau is noted on the first list for four recoveries in 18 operations, Trousseau, his pupil, for 20 in 80 cases, and Professor Gerdy for 4 in 6.

Since 1840, or 1845, that is to say, during the last 30 years, and no doubt through the impulsion of Trousseau's teachings principally, the statistics of tracheotomy have peculiarly improved. Instead of one-fourth, *more than one half* of the operations terminated by *recovery*, when the cases were operated upon *in proper conditions.* Here are figures, from page 26 of Dr. Cohen's work, that speak very high: Gay, 7 successes in 13 operations, Calvet 13 in 23, Richet and Petel, each 5 in 9, Bourdillat 10 in 16, Von Köpl 11 in 17, Cruickshank 8 in 11, Scholhammer 6

in 7. Even in Paris, Trousseau, who from 1851 to 1854 had practised tracheotomy 24 times, had succeeded 14 times, and in the year of 1854 alone, 7 times in 9 cases.

Then, there is reason to study the circumstances which have caused such remarkable differences in the results of tracheotomy during the first 25 years, compared with the last 25. The principal circumstance is that of the *time* or *moment selected for the operation.* During the first period the operation was delayed *as long as possible*, whilst, in the second period the operation was aimed at, and executed, *as early as possible.*

Bretonneau, since his first cases, had felt the advantage of operating soon. In the report of his first happy case, he says: "The success of tracheotomy depends on the wide and free passage of air * * and afterwards: I can hardly find another success in tracheotomy, *practised in the last period of croup*, and I do not wonder at it. The circumstances in which this operation is resorted to must have allowed but rarely of happy results." (Page 327 Traité de la Diphtherite, 1826.)

We must not, therefore, be surprised if we find the following remark, even in the table of contents of the "Clinique de Trousseau, 1861:" "One condition favorable to the success is *to operate as early as possible.*"

But we must also guard against going to an excess of precipitation, and propose an operation *too soon.* In fact, what would be more humiliating to a family physician than to propose an *operation as being necessary*, and, it being refused to him, to hear afterwards that the child has recovered, *without any operation*, by the sole efforts of nature, or, worse than that, in the hands of a charlatan, armed or not with globules!

Be that as it may, the following remark of Prof. Hueter (p. 33) is appropriate and just: "A few tracheotomies, which were not absolutely necessary for the preservation of life, are not so culpable as neglect of the operation and its postponement to the latest stage." In other words, it is best to operate *too soon than too late.* Perfection would be to select the very time; but practical rules, to arrive at it, are hard to establish: "the harsh and stridulous sounds with which the air passes to and fro through the contracted glottis * * are variable and indeterminate" (p. 32). And even the most abnormal sounds detected by auscultation of the thorax are deceiving—various ronchi, sibilance, etc., might lead to the suspicion that the bronchi have been in-

vaded by *diphtheric exsudation*, whilst there is nothing of the sort. After the operation, if performed, respiration becomes quiet, and the abnormal sounds most frequently cease as by enchantment. In many cases, followed by death, necropsy has shown that the false membranes had not reached beyond the larynx, when many bronchial sounds had been heard during life. Besides, if auscultation would reveal *pneumonia*, it would be, according to Guersant, jr., an additional reason to resort to an operation, because "its resolution would be facilitated by the increased freedom of respiration" (Cohen, p. 31).

The auscultation of the heart is more important than that of the lungs—an extreme frequency in the beatings of the heart is an alarm bell; hence the necessity of exploring the pulse at such moments.

But the greatest practical importance resides in the study of the muscles of respiration, and particularly of the *diaphragm*—it is quite useful to find out to what extent that great *respiratory muscle* works to excess and gets exhausted.

Prof. Hueter has insisted on the prognostic value of *muscular exhaustion*, and has made known the signs by which it can be almost measured in the diaphragm, by the increase of its efforts: "An energetic sinking in of the anterior wall of the thorax, especially of the *lower end* of the sternum (i. e., *epigastrium*), occurs at each inspiration. A remarkable sinking in at the *jugulum sterni* occurs also, and it sets in somewhat *later*, as a rule, than the depression at the epigastrium; so it indicates a more advanced state of dyspnœa. The well known abnormal action of the nostrils is an additional symptom of the difficulty of the respiratory act. The extreme labor of the respiratory muscles always indicates that but little air reaches the respiratory surfaces. Observation of the entire respiratory act affords a much safer estimate of the impediment of breathing."

"The coloring of the blood, by the retained carbonic acid gas, is best examined in the color of the lips. So, for my part," says Hueter, "I consider that the hour for tracheotomy is come as soon as the *blue color* appears in the lips. At this period I propose the operation, and, if it is declined, death by suffocation follows."

These precepts of Hueter are worth a great deal; their practical value will strike all those who have acquired any experi-

ence; hence the necessity to impress with them the minds of young practitioners.

In a memoir, under the title of "Ulcerations de la trachée artère, produites *par le Séjour de la Canule* après la trachéotomie," and read before the Academy in 1859, by Dr. Roger, a physician to the "Hôpital des Enfants Malades," we find this passage at the first pages: "Tracheotomy succeeds so much the more surely that it is practised soon enough to prevent asphyxia. It is unquestionable that the surgeon's success will be greater when he will operate so soon as asphyxia commences, before the period of complete asphyxia, that period of extreme danger which terminates in death 9 times out of 10."

As the title of the memoir shows, its main object was to call the attention of the profession to the great danger accompanying the use of ordinary *metallic canulæ*—(the *ulcerations of the trachea*)—and the means of avoiding such risk. Here is its conclusion at page 202 (in Archives, July, 1859):

"The treatment, essentially prophylactic, shall consist in avoiding compression of the tracheal mucous membrane, by the use of a small canula, and opposing frictions against the internal wall of the air passage by adopting a *movable canula*, the body of which shall move with the trachea itself. Luer's movable canula has seemed to fulfill the indication most completely. Out of 6 patients on whom it was used at 'Hôpital des Enfants' 4 were cured, and one is on the way to recovery."

Ten years later (1869), a former interne in the same hospital, Dr. Sanné, published a remarkable "Study on Croup *after Tracheotomy*."

The "*after-treatment*," no doubt, is of great importance, but we must by no means neglect the "treatment *before* the operation." Indeed both imply the necessity of studying, first of all, the nature of the disease, or the meaning of "Croup and Diphtheria," and the value of *local treatment*, at least in the beginning.

Considérations Générales.—If I have well understood the American and English classic authors, and among them Aitken and Flint, their classification is "*true* and *false* croup;" *false croup* being an acute laryngeal affection with *croupy cough*, of nervous, inflammatory or catarrhal nature, and *true croup* being *laryngitis with exsudation*, or the formation of *fibrinous* membrane in the larynx.

For them, croup has nothing to do with diphtheria, which

is a *general poisoning.* It is true that diphtheria may be accompanied with a *special exsudation* in the larynx, a *kind* of false membrane, but which is not a *fibrinous exsudation*, as in true croup: "It is made up chiefly of *cellular elements*, and *does not consist of fibrin* or *lymph.* The cellular elements are regarded as *modified epithelial cells.*" (Flint, p. 1003.) In true croup there would be no general poisoning.

The ideas of the French school *of this day* are quite different; they tend to an inextricable confusion, and to the most unacceptable theory—*the spontaneous generation* of the disease. Such at least was my impression after reading the book of Dr. Jaccoud, who is one of the representatives of this *advanced* school.

According to this author, "the microscope shows that the false membranes contain granulated and fibrillated fibrin, and amorphous granulations agitated by the brownian movement. In this fundamental gang are entangled normal and granular leucocytes, fat globules, epithelial elements, regular or deformed red blood corpuscles, at times vibrio (*bacterium vibrio*) and vegetable productions in the shape of *spores* and *myeelium.*" (Pathol. Int. VI., p. 760.) There is enough variety, surely, to gratify the taste of every one, in that passage.

As to the *diagnosis*, the author believes that the *laryngoscope* is *neeessary* to establish it. "One thing to be regretted, however, is that this instrument is not applicable to young children. But the examination becomes possible in the cases of children of a certain age, to whom the dread of danger will give an unshaken resolution. Certain precautions must be resorted to, inasmuch as the patient will derive no benefit from the risk which the physician will run? For instance, simple prudence will suggest the closing of the mouth and nose by means of a handkerchief, and the protection of the eyes by eye-glasses." Such are quite minute precautions against a disease of *spontaneous* origin, and which *the organism ereates.*

The French school, referring to croup and diphtheria, which appears to me as the best and most truthful, is that of Bretonneau, as was continued by Trousseau. The "Traité de la diphthérite, 1826," is a masterpiece work; and if it is true that the "*febres periodieas pernieiosas* must be studied in TORTI'S "*Therapeutiee specialis*," that vaccine must be studied in Jenner, auscultation in Laennec, chloroform, as a specific in child-bed pains,

in Simpson," etc., I must confess that it is in Bretonneau yet that I like to study croup and diphtheria.

It may be remembered that it was about 1809, that Napoleon I., seeing one child of the imperial family, whom he much loved, dying with croup, offered a prize for the best work that would be written on this disease. Two memoirs got the prize—one by Jurine, of Geneva, and one by Albers, of Bremen. Both deserved credit, but neither threw great light on the question. Nine years later, in 1818, a strange disease began to spread in the town of Tours and its surroundings. It was called *angina maligna*, and was thought to be truly *gangrenous sore throat.* Bretonneau, who was then the chief physician to the hospital, partook of the general opinion. But he belonged to Laennec school, and was a great partizan to *pathological anatomy.* As a correct and minute observer, with his broad and generalizing intellect, he began to study the prevailing epidemic. What was not his astonishment, from his first autopsies, carried on with all necessary care, to find out that *gangrene* of the throat did not exist, and that under a thick coating of grayish and fetid *false membranes*, which had been mistaken for putrified sloughs, the living tissues were found in all their integrity!

The physicians of Tours, carried off by deceitful appearances, did not believe him. Some even tried to ridicule him, but he amply retaliated. He gathered materials for a series of memoirs which he went and read himself before the Academy of Paris, and which carried him to celebrity: "1° Mémoire sur la phlegmasie diphthéritique, ou inflammation pelliculaire de la bouche, du pharynx et des voies aériennes.—2° Mémoire sur la diphthérite, ou inflammation pelliculaire du tissu muqueux." These works could tell the teacher who was to start Velpeau and Trousseau. By way of anatomy, clinic and history, Bretonneau showed the identity between the "stomacace" or scorbutic gangrene of the mouth, the angina gangrénosa, and croup—all three being but one affection—the *diphtheritic affection.*

This diphtheritic affection which, according to the ideas of the time, he called *a phlegmasia*, was in his opinion specific, or special. The title of his book of 1826 shows it clearly: "Des Inflammations spéciales du tissu muqueux, et en particulier de la diphthérite ou inflammation pelliculaire, connue sons le nom de *croup*, d'angine maligne, d'angine gangréneuse," etc., etc. The main *characteristic* of diphtheria was, according to him, the formation

over the mucous membranes of a *pellicle* (Διφθερα), without any analogous product in other inflammations of the mucous membranes. This opinion was emitted and skilfully developed after long and minute researches.

Rising afterwards to a most beautiful generalization, the great observer remarked that in the same way that there was a series of various specific affections, the manifestations of which take place on the skin (the external tegument), in the same way also, there ought to be a series of various specific affections, the manifestations of which would take place on the mucous membranes (the internal tegument). Diphtheria, he thought, was one of the most remarkable of this last kind, and he announced another special affection localized at the termination of the small intestine, with engorgement of the mesenteric ganglia, which he called *dothienenterie*, supposing it was *pustular*—Δοθιην.

But already in 1813, a physician of "Hotel Dieu," of Paris, Dr. Petit, had published his "Traité de la fièvre entero-mésenterique," which is to Typhoid fever what the "Traité de la Diphthérite" is to Croup, stomacace and gangrenous sore throat, "*a masterpiece*," with this difference, that whilst Bretonneau shows the identity between these three affections, Petit describes most accurately one of the most distinct species of the Fever now called Typhoid fever, and which was then known as "Fievre putride," that famous Fever, which came so near becoming, under the authority of Chomel and Louis, the "Continued Fever" par excellence, uniting together almost all the continued fevers.

Let us return to the "Traité de la diphthérite." Three times, during my practice of 30 years in New Orleans (1848–1850, about 1860, and 1874–1875), I had occasion to prove the accuracy of Bretonneau's descriptions, and the propriety and magnitude of his views. Those are my reasons for valuing his book so much, and wishing that it should be more appreciated or less forgotten. Such works, if they do get old in some respects, always retain a real and indestructible value.

In 1848 I was called to treat a case in which, unfortunately, I remained too long under the false impression that it was a slight attack of common tonsilitis. The patient had swelling of the sub-maxillary glands. I had learned at the clinic of Gendrin the full diagnostic and pronostic importance of such swelling in sore throats, and yet I persisted in consoling the mother who was instinctively alarmed. In a few days, however, I was undeceived

by the foul breath and the grayish putrefaction covering the pharynx, and the opinion of a more experienced confrere who pronounced the case to be gangrenous sore throat. For several weeks afterwards I saw similar cases in the suburbs of the city, all terminating fatally, with symptoms of croup, which I thought was a mere complication. In fact, it was true croup, merely.

In 1860 I was familiar with the "Traité de la diphthérite" when I had to treat quite severe affections of the mouth at the Orphans' Asylum. For several years this poor house, owing to the almost exclusive use of salt meats there, had been visited by cases of real scurvy, running in several instances to necrosis of the maxillary bones. About the same time, I had occasion to see also a few cases of true gangrene of the mouth, such as has been described by Guersant, sr., with perforation of the cheek. However, a farm being attached to the Asylum, and its sanitary conditions having improved, scurvy gradually disappeared and also the serious affections of the mouth. Besides, the Sisters having acquired great skill in the dressings, I had only to indicate the medicines and let them apply them. One day, however, the infirmary Sister, whilst attending to a child, was struck with the peculiar aspect of the back of his mouth, and sent for me. The uvula, all grayish, seemed extremely elongated and much enlarged, dipping into the pharynx or bending over the tongue; the soft palate was also of the same color. I clipped this uvula, but found that I had only removed some diphtheritic exsudation. I clipped again and again, until I saw the extreme end of the uvula enclosed in that conical false membrane, as in a sheath, which seemed as if it had dropped from the palate and formed like a stalactite. It was a case of stomacace (or pretended scorbutic gangrene of the gums), a true diphtheritic affection, having extended from the gums to the palate, and which probably would have extended to the pharynx, if not checked by the application of perchloride of iron.

Several cases of pharyngeal diphtheritis followed the first one, in which local treatment invariably proved successful, with the only exception of one child whose larynx became affected, and who died with *true croup*. He was too young to admit of an operation.

During this series of 1860, all the cases of *true croup* that I attended in private practice had *commenced in the pharynx*. The only exception was the child of whom I spoke at page 537 of the

January number of the *N. O. Medical and Surgical Journal* of 1875, who recovered after tracheotomy, and in whom the disease had commenced in the larynx. In my last series (1874–1875) there seems also to be a case in which the diphtheritic exsudation commenced in the larynx; it was Dr. Gaudet's patient (loc. cit., p. 538).

Having read a great deal, and also acquired some personal experience, on *croup and diphtheria*, I may be allowed before closing this article to expose my *theory* on the disease, inasmuch as our practice is only the application of our theories.

In my opinion there is no croup, false or true; unfortunately this word was introduced into science and it should be forgotten. The various acute laryngeal affections, nervous and spasmodic, catarrhal and inflammatory, constituting *false croup*, are distinct and real affections of the larynx, and must be studied separately. But there is also a *specific affection*, the anatomical character of which is, as demonstrated by Bretonneau, the accidental production of a pellicle over the mucous membranes generally, but especially over the mucous membrane of the air passage and at times on the denuded skin. This pellicle (Διφθερα, pellis, peau) being a peculiar membraniform production, similar in some respects to the false membranes formed in serous membranes. This affection was denominated *diphthéritique* or *diphthérique* by its discoverer; it was isolated and separated from other diseases with which it was confounded, and it is but justice that it shall retain its first and quite scientific name. Bretonneau's Diphtherite comprises the "true croup," or laryngitis with exsudation, and the "diphtheria" of the American and English authors.

I know very well that attempts have been made, with the assistance of the microscope, to establish differences between the exsudation of the mucous membrane in true croup and in diphtheria, the exsudation being *fibrinous* in the former and essentially composed of *cellular elements* in the latter. But the data furnished by the micrographs on this subject are quite confused and contradictory, and we should indeed wait until they agree together. Otherwise, it is not surprising that the minute particle of the morbid product examined under the microscope contains a little of everything, as long as it partly proceeds from the secretions of the affected organism, and partly from the development in situ of the morbific germ itself. Notwithstanding all,

they agree in recognizing, in every case, the existence of *living organisms*, animal as well as vegetable.

Analogy and induction in such studies may assist in prejudging sometimes with accuracy, so that the information furnished by the magnifying instrument has almost always been preceded by equal suggestions which were mere work of the mind. I was a resident student at the "Hôpital St. Louis," in Paris, in 1843, when Gruby came there to make microscopic demonstrations on porrigo-favosa, in the wards of Mr. Devergie, now the venerable President of the Academy of Medicine. Well, I may affirm that previous to Gruby's lectures, and previous to seeing a piece of oak bark on which this ingenious micrograph had inoculated a small capsule removed from the head of a patient, which capsule had grown, formed sporules so as to cover a part of the trunk of the tree with porrigo-favosa, as if it had been moss (my inaugural Thesis, 1844); previous to all this, after reading Alibert (des dermatoses), and after watching the development of the capsules of the porrigo-favosa on the head of children, I had thought that it was a vegetable. In the same way, now and for a long time, after reading Bretonneau, and after examining attentively and minutely diphtheria at its onset, on the tonsils particularly, I remained convinced of the *living* nature of the characteristic pellicle of diphtheria.

We may admit that there are for diphtheria, as well as for all true specific diseases, such as cholera, yellow fever, and others, germs or seeds prëexisting in the universe, morbific germs or principles, preceding their manifestation in man. Their manifestations in the world, and on man, is no new creation or spontaneous generation; we can only say that, until then, their germs or seeds had not come in conflict with human organism. At least such are my views.

As to diphtheria, its germ, as in cholera, yellow fever, etc., lives by itself independently of any organism whatever, and at a given time, in peculiar localities, this germ is spread into the air. What a great contrast exists in the mode of conveyance of the germs in cholera and yellow fever! It becomes more easy to follow the transmission of diphtheria. In general, there is no poisoning at the first onset. The germs, whatever they may be in their infinite minuteness, are first *inhaled;* and in the great majority of cases deposit themselves in the pharynx, or most frequently take root in the tonsils, and there appear as *transpa-*

rent whitish spots, with some redness of the mucous membrane, this spot becoming afterwards the diphtheric false membrane, for the description of which I will refer the reader to Bretonneau (p. 40 and following). Soon afterwards the swelling of the sub-maxillary glands is noticed, showing that absorption has taken place. The swelling of these glands seem as it were to act as a barrier, and retard constitutional infection. The pharyngeal pellicle has a great tendency to spread; it is at first fluid, and seems to flow, says Brettonneau, towards the inclining parts, in the shape of long striæ, like bands. The larynx is soon invaded, whilst the œsophagus almost always remains free. In the chapter devoted to the specific characters, Bretonneau, p. 49, finishes this picture drawn from nature:

"At the outset of the disease, circumscribed red spots are noticed, on which coagulated, semi-transparent mucus is deposited. This first thin, supple and porous coating may also be raised by portions of unchanged mucus, so as to form vesicles. Often in a few hours these red spots gradually extend, by continuity or contact, like a liquid spreading over a level surface, or running through a ditch and forming striæ," etc.—Bretonneau, p. 49.

During the epidemics at Tours the main danger resulted from the invasion of the larynx, and consequently death was most frequently caused by mechanical obstruction of the air passages and asphyxia. Hence the necessity of tracheotomy.

In other epidemics, the lymphatic sub-maxillary glands did not seem to arrest the disease, and general poisoning took place. Such general poisoning and its sequelæ, even during convalescence, must have been very rare in the epidemics of Tours, because it seems quite improbable that so remarkable phenomena as diphtheritic paralysis, either limited or generalized, would have escaped the observation of Bretonneau. Now the perusal of his book will scarcely admit the suspicion of traces of paralysis in a few cases.

The morbific principle of diphtheria will therefore show some analogy with cantharides in its effects. With cantharides there is at first the local effect, first rubifacient and then vesicating, and afterwards general poisoning whenever the active principle is absorbed. And the same process takes place in diphtheria.

The disease most frequently commences in the pharynx, but it may also begin in the *nasal fossæ*. An example of the latter is well described in a "Mémoire sur les paralysies diphthériques,"

by Dr. Maingault (Archives, vol. 2., p. 679, 1859). The subject is Dr. Bretigniéres, who suffered with the most serious phenomena of paralysis. A few soldiers in the garrison of Tours in 1818 offered most remarkable cases commencing *in the mouth*. "Only once the false membrane appeared in the trachea, without any concretions either on the tonsils or any part of the pharynx. Once in 9 cases the membraniform concretion extended to the last bronchial ramnifications; and in the third of the cases it went beyond the large divisions of the bronchi; in the remainder of the cases not one extending beyond the trachea" (p. 33, loc. cit.).

RECAPITULATION AND PRACTICAL INFERENCES.

1. There is but one specific diphtheritic affection, true croup and diphtheria being the same disease.

2. Epidemics are quite changeable, and always contagious.

3. The first individual manifestations are, as a rule, local—most frequently in the pharynx, on the tonsils.

4. Local treatment at the outset is of urgent necessity, and all difficulty or repugnance must be overcome, in order to destroy the disease *loco dolenti*.

5. When general poisoning has taken place, the treatment can only apply to the symptoms, as there is no known specific antidote to the disease.

6. Tracheotomy is only an ultimate resource, and yet of great importance.

7. Traeheotomy in extremis is a sad duty, but must be performed.

8. In order to raise tracheotomy in public opinion, it is proper to compare its results between two periods: from 1820 to 1845 death is the rule; from 1845 to 1870, the rule is the cure of more than half the cases as a result.

9. The difference in the results of the operation in these two periods is explained by the *late* operation in the first period, and its performance *as early as possible* in the second, with a close after-treatment.

10. The practical means of knowing the time when to operate is to measure the amount of commencing asphyxia, and above all, to appreciate the extent of the wasting of the vital forces and the difficulty of breathing from the exhaustion of the respiratory muscles, and particularly the diaphragm.

11. It is necessary to make tracheotomy easy and sure, in order to bring it into general use.

12. In the treatment of this disease, more than in any other, the physician must sacrifice everything to his patient's interest.

ARTICLE IX. *Extracts from an Essay upon the Anatomy, Physiology, and Pathology of the Thyroid Gland.* By Mr. Paul Berger. Translated by JNO. J. CASTELLANOS, M.D.

The removal of the thyroid gland, first performed by Desault, had lately fallen into such disrepute as to have drawn upon itself severest condemnation from the Surgical Society, on two distinct occasions ten years apart from one another.

Several successful attempts, however, in England, America, Switzerland and Germany, and also the favorable issue of a case of his own, induced Professor Michel, of Nancy, to seek and discuss the causes that had led to the abandonment of this operation.

In an essay, the most complete ever written upon the subject, he is led to conclusions differing widely from those hitherto generally admitted, upon the consideration of which we will now enter.

Two causes have prevailed to cast disfavor upon the operation for the removal of the thyroid gland: these are, on one hand, the very large proportion of fatal results, and on the other, the reflection that the affection it is intended to remove does not, at least immediately, in the majority of cases, compromise life.

Truly, statistics do not offer a very flattering record in France, where we are best enabled to appreciate their value. Up to the present time eighteen operations have been reported in this country, and of the patients thus submitted, 10 have died and 8 have been cured.

But, as Mr. Michel judiciously remarks, if in the selection of these observations due restriction be had to such as furnish satisfactory accounts of operative proceedures of the causes of death, etc., this proportion might be lessened to one-third.

Less gloomy, however, are the German and Swiss statistics. Bilroth claims success in 12 out of 20 cases of operation, the alternative being death in the 8 remaining cases. Mayor Junior reports one case cured, while Bruns, in Germany, claims also 2,

but with an equal number of fatal results. Lüche cured 8 cases out of 9, Dick 1 case, Walther 2, Emmest 3, Middeldorpf 1, and in 4 cases Schute met with an equal share of success and failure. The sum total of these results, most of which are consigned in Briére's thesis, exhibits an average mortality of 12 out of 46 cases, or more than one-third, since 1845, whereas in 29 cases of the operation performed in Switzerland and Germany from 1785 to 1845, 11 deaths ensued, which is a still larger proportion. American and English statistics appear very favorable to the operation; yet as there seems to be no grounds to believe that the operators have published all their failures, their reports fail to acquire any considerable value. In 1807 Charles Harris, of New York, successfully removed a goitre extending from the sternum to the chin. Gooch witnessed the operation in 2 cases, 1 of which proved fatal. In 1872 Holmes lost a case from which he had removed a large bronchocele, while, on the other hand, in 1871, Professor Green, of Portland, Maine, reported 3 successful cases of his own, and 1 in the practice of the late Professor Blackman. Quite recently, Patrick Heron Watson published 5 other cases lately treated by him, and crowned with success. If from these we abstract Parsons' successful case, in which the removal was rather that of an osseous tumor of the gland than that of its parenchymatous structure, we can yet sum together 13 cases, 2 of which were fatal. In fine, statistics combined as drawn from these three sources, yield an average proportion of one-third upon 106 observations, the exact particulars of which we have been unable to gather.

From inquiries into the causes of death in these unfortunate cases, we find among others that are imperfectly if at all accounted for, the most important to be hemorrhage, either primary or secondary. The latter, however, occurs quite seldom, although in a case reported by Clein it supervened five days after the operation, and, ligature of the primitive carotid proving unavailing in checking it, the patient sank. In the case operated upon by Holmes, the patient died from sudden hemorrhage the day following the operation. Hemorrhage occurring during the operation, even when it proves not immediately fatal, is always a serious accident. In an observation from Gooch, every attempt to arrest it with the ligature failed. Digital pressure upon the divided vessel, and steadily maintained during eight days, alone saved the patient. So also did Paul Sick, and

Green, have to contend with profuse bleeding upon severing the attachments of the tumor, even when they had resorted to preventive measures previous to the operation. Twice also did Patrick H. Watson have to overcome this fearful complication. But results have not all been similarly favorable. In Gooch's observation the patient sank from hemorrhage during the operation, which was not completed. Bruns had to resort to as many as 24 ligatures in order to check it; his patient, however, sank into collapse, while gangrene invaded the wound and hastened death.

A case of Dupuytren's died from traumatic shock, one of Bilroth's from pyæmia, and two of Chassaignac's from deep-seated inflammation. Some of these seemed to have died from asphyxia, which was readily accounted for in two autopsies, the recurrent nerve having been found in these included in the ligature of the inferior thyroid vessels.

Section and ligation of this nerve are, in fact, some of the many lesions incidental to this operation. When either does not prove fatal, it may lead either to complete and lasting, or to partial and temporary asphyxia.

Professor Michel believes, in certain cases, the external and not the inferior laryngeal to be the nerve ligatured or divided.

An observation by P. H. Watson is worthy of interest. Shortly after the operation, a pulsating tumor, the size of a walnut, presenting a thrill and a "brnit," appeared upon a level with the right superior thyroid artery, and spontaneously disappeared after three weeks.

Messrs. Chassaignac and Sédillot considered partial amputation of the gland, a means of avoiding these complications:— "Partial removal of the goître," says the former of these eminent operators, "as well as successive ablation seems to me to enjoy great advantages, both as regards hemorrhage as well as depth of divided surfaces." Mr. Sédillot observes that, "whereas experienced surgeons should refrain from removing true goître; they should, however, attempt the removal of tumors connected with the thyroid gland, or false bronchocele, especially when these are solid and pedunculated." This rather quaint distinction, according to Mr. Michel, is intended to disguise the author's tendencies towards partial, while it excludes complete, extirpation of the gland itself. But facts do not justify this preference, for if we eliminate the observations in which the latter mode of

the operation has been adopted, from the former, and restrict ourselves solely to such as have been minutely and thoroughly reported, we will still have a proportion of three favorable results to one of death, quite a favorable ratio, for so limited a number of operations.

Besides, in a certain number of patients, a cure was effected in less than one month, and without any accident.

Mr. Michel reports in 5 instances of partial removal, 1 death, and 4 recoveries.

A young girl, thus operated by Mr. Sédillot, recovered after frightful venous hemorrhage.

A young man died from diffused and gangrenous phlegmonous inflammation of the neck, having resulted from a similar operation.

His conclusion, which we cannot but indorse, is that partial removal of the thyroid gland is as bloody an operation as its total removal; and that, whereas the two operative procedures involve an equally long period of time for subsequent cure, the former entails the same amount of danger upon the patient; nay more, may give rise to more serious accidents than the latter.

From the above considerations, we must then infer that the removal of the thyroid gland is a very serious operation, which, when successfully performed, is effectual and radical in its results; but which proves fatal once in every three attempts.

Is it then to be deliberately attempted, or is it preferable to relinquish its execution to such as might be led to it, "through rashness or ignorance?

Answering the above query, we should be guided by the general rules that are applicable to the indications of all surgical operations. Whenever life is threatened by the affection, and less perilous means have proved either ineffectual or impracticable, it is the surgeon's bounden duty to propose and perform it. Ganglionic tumors of the neck are daily removed, sometimes under circumstances less favorable to recovery, more perilous to life, and in fact less urgent in their requirements. It may be objected, however, that the lympho-sarcomatous and lymphadenomatous tumors of the neck are never removed, except under positive conditions, viz: their independence from the mediastinum, and their strict limitation to the superficial or deep-seated regions of the neck; and that, unlike these, suffocating goitre is deeply seated, or retro-sternal. However weighty the objection, it is impossible for us to anticipate here the special indications

or counter indications of every case. Whenever, by physical examination, the surgeon will find the tumor deeply projecting into the mediastinum, prudence must counsel him to refrain from an operation, which, if attempted, he might be compelled to leave undone. Still, even under these circumstances, in which the inferior limits of the tumor are to be reached with so much difficulty, is there not a marked difference between an irregularly defined and adherent ganglionic tumor and goitre in which the real bonds of union and adhesion are to be found only on a level with the neck? Yet, guilty would that surgeon be who would draw his indication from the expressed desire of the patient to do away, at any price, with the tumor as an unbearable deformity. Wherefore Roux, who had been similarly misguided, fully deserved the severe condemnation of Velpeau. Bilroth rendered himself likewise amenable to such a reproof, he having removed eight median goîtres of small bulk, and interfering little, if at all, with respiration. True, the operation, when performed under these circumstances, is comparatively easier and less bloody; nevertheless, it is still attended with too much jeopardy of life to be performed as one of mere "complaisance." On the other hand, one of its most successful performers, Dr. Greene, of Portland, addresses some words of warning to young operators, which coming from so authoritative a source, acquires increased value: "I cannot refrain from one word of warning to my younger brethren, whose ambition may make their fingers tingle, lest they should, in the light of these successful cases be too easily tempted to interfere with these growths. It is, and always will be, exceedingly rare that any such interference is warrantable, *never* for relief or deformity, or discomfort, merely, only to save life; and if it is, beyond all question, determined in any case that such an operation gives the only chance for snatching a fellow being from an untimely grave, be it remembered that accurate anatomical knowledge, and a perfect self-control under the most trying ordeals through which a surgeon can pass, are indispensable to its best performance."

We are not warranted by any precedent to state whether the operation is advisable in cancer of the thyroid gland, although we may predicate that the rapid invasion and course, so characteristic of the affection, might lead us to doubt of the opportunity of the operation in these cases.

Although surgeons are unanimous upon the operative proced-

ure, we must, nevertheless, divide it into two modifications. The first, as recommended by Greene, is the ready exposure of the tumor by a vertical incision. Due caution must be had not to cut it at first, nor its fascia propria. The latter is then incised upon a grooved probe. The tumor is then rapidly enucleated with the fingers and handle of the scalpel, heedlessly of the hemorrhage, how urgent so ever it may appear. The pedicle is then readily reached, transfixed with a needle armed with a double thread, and is thus sub-divided into as many parts as there are important vessels to be divided. These are then to be separately ligated, after which the tumor is excised.

The procedures recommended by Messrs. Michel and Bilroth only differ from the preceding by auxiliary incisions made upon the skin. Michel ligates the vessels previous to their division, in the following manner: before proceeding to enucleation, the thyroid vessels, when found, are ligated, in order to obviate the danger that might arise in case of premature detachment of the gland from rupture of its uniting bonds. The vessels are to be sought for in the following order, viz: the superior thyroid arteries are to be exposed and tied, then the inferior thyroid and the middle, (if it be present); these vessels are to be divided between two ligatures, and as late in the course of the operation as possible; and, finally, all adhesions of the gland to the trachea and larynx are to be severed. In no case should the thyroid vessels be tied as they are being cut, and Michel errs in his assertion that this procedure has been resorted to by Warren Greene.

When only partial removal is contemplated, excessive traction upon the pedicle leads sometimes to rupture of the important vessels which cross through it. According to Michel, this is one of the accidents which renders this operation equally dangerous to that of total excision; and, according to his suggestion, it would be advisable, in such cases, to enclose the pedicle and its vessels in a clamp.

We will end by an explanatory account of the application which Mr. Chassaignac has made of the "*ecraseur*" to this operation. The integuments and muscles anterior to the thyroid gland having been made secure and raised, are subsequently encircled by the chain and a vertical section effected. The gland is then exposed, and easily enucleated with the fingers. The broad pedicle, with its four thyroid arteries, is again encircled

by a chain, and crushed through without any serious hemorrhage, the rate of crushing being that of one ratchet alone every half minute. The same author, elsewhere, remarks upon this subject: "The operation must be very slowly conducted, and may be prolonged to one day, and even two days, perhaps. Through excessive haste, I have experienced disastrous results in my hospital practice, which might have led me to refrain from similar operations, had I not been led back to confide, with renewed conviction, in a method which so frequently yields fortunate as well as unexpected results."

EXCERPTA.

On the alleged Successful Treatment of Typhoid Fever by "Cold."
By Frederick T. Roberts, M.D., B.SC., M.R.C.P.

Among the remedial measures which are attracting attention at the present time, some of the most important are those which have come into prominence in connection with the treatment of febrile diseases, and which have for their object the application of cold to the external surface of the body. Of course they do not involve any newly-discovered principle, for they are merely revived methods of a plan of treatment which is more particularly associated with the name of Currie, who laid down definite rules as to how and when it should be carried out. These measures have been during recent years much more extensively and generally employed on the continent, and especially in Germany, than in this country; but any one who is acquainted with current medical literature must be familiar with the fact that they have not been ignored by the profession here, but have been duly recognized as affording valuable aid in the management of fevers under certain circumstances, and that their efficacy has been tested by several accomplished observers, such as Wilson Fox, H. Weber, Ringer, Thompson, Greenhow, Clifford Allbutt, and others.

The object of the present paper is to offer a few remarks with reference to the alleged advantages of the use of "cold" in typhoid fever. But before considering the matter in connection with this particular complaint, I wish to make some observations of a general character. First, most of the methods by which the application of cold is effected require much care and attention in carrying them out; they must be conducted under the personal supervision of a competent medical attendant, and their effects need to be thoroughly watched. They have been lately alluded to as "simple remedial measures," but this they are not in any

sense, for most of them involve no small difficulty in their execution, especially in private practice, while as regards their effects on the system, they by no means come under the category of simple, harmless measures, such as may be adopted in a routine, off-hand fashion. Secondly, the more evident effects which have been noted from the employment of these applications in febrile cases are, the reduction of the temperature of the body; diminution in the frequency of the pulse; the checking of tissue change, as evidenced by the excretions and by wasting being less rapid; the relief of nervous symptoms; and the modification of the eruption in certain of the exanthemata. The eruption may be encouraged to come out, as in scarlatina or measles; or, it is stated, it may be diminished in amount and improved in quality, as in small-pox. It is in the treatment of cases attended with very high temperature—cases of hyperpyrexia—that the beneficial results of applying cold to the surface, by means of baths, etc., have been most signally obtained; and here it may be said without any exaggeration to have saved the lives of patients who must inevitably have died had not this method of treatment been adopted.

Several German physicians have spoken highly of the treatment of ordinary cases of various fevers, and among them of typhoid fever, to which disease I now desire to call attention more particularly. In a paper on typhoid fever in the recent volume of the St. Bartholomew's Hospital Reports, Dr. Gee considers the use of external cold in its treatment, but apparently rather from a hypothetical point of view than from any actual experience of its advantages. He implies that, if employed at an early period, during the primary fever, it might control the local intestinal lesion, by limiting the number of follicles involved; while in a more advanced stage it might be useful in reducing the secondary fever which accompanies the morbid changes in their progress.

The conclusions of those observers who have practically tested this method of treatment deserve our thoughtful attention, and it is important to ascertain what they affirm respecting it. None of those who are reliable pretend that they can actually *cure* typhoid fever, either in the sense of checking the disease at the outset, or even of shortening its course. To talk of *curing* or *arresting* this complaint shows a want of knowledge as to its nature. At the best, we can only guide it in its progress, and endeavor to bring the patient successfully through the dangers which he has to encounter. This remark applies not only to the treatment now under consideration, but to any other which claims an influence of this kind over typhoid. Some believe that the convalescence is more rapid after the treatment by cold, but others deny this. So also is there a difference of opinion as to whether it affects the more prominent symptoms, such as diarrhœa, tympanitis, intestinal hemorrhage, etc.; some asserting that these are beneficially influenced, others that they are

not. All seem to agree that it does not increase the tendency to lung-inflammations, or aggravate these if they already exist. The most important statement made is, that this method of treatment has greatly reduced the mortality from typhoid fever, particularly when the cases have been treated from an early period. Certainly the returns made show a great difference in this respect, when the past is compared with the present, and seem to indicate a highly satisfactory result. At the same time, before any certain conclusion can be arrived at, a large number of cases ought to be taken into consideration; for it often happens that a series come under treatment which are of a mild type, and the mortality may present a very low percentage, whatever treatment may have been adopted; while some epidemics are very fatal. Judging from personal experience, and combining hospital with private practice, I believe that the rate of mortality has not been higher in the cases which have come under my own notice than it is represented in the German returns. The mortality in former times was decidedly high, and it would be well to know what modes of treatment were then adopted, as they were not always of a harmless character in days gone by, while the diet and hygienic conditions—most important elements in the treatment of typhoid—were not attended to then as they are now.

Those who adopt this plan of treatment are anything but agreed as to the details of carrying it out. The principal methods employed are frequent cold or tepid sponging of the skin, wet-packing, cold baths, affusion, and tepid baths gradually cooled. Some pursue the treatment vigorously in all cases; others only when the temperature reaches a certain height. There are also differences as regards the duration of each application, the frequency of their repetition, and the length of time during which they are continued.

I venture to state briefly the conclusions which I have arrived at with respect to the treatment now under consideration.

1. It is highly desirable that the members of our profession should be more generally impressed than they are at present, with the usefulness of the various modes of applying cold to the surface of the body in febrile cases, under certain circumstances; and that they should be prepared without hesitation to carry one or other of them out efficiently whenever this plan of treatment is indicated. This applies to typhoid in common with other fevers.

2. On the other hand, to adopt a routine hydropathic treatment of any fever seems to me most objectionable, and this applies especially to the more severe methods which are advocated. As already remarked, they are not easily carried out in general practice; they are certainly not required in a large proportion of cases; most of them are anything but pleasant to the patients, and they may prove very trying and exhausting, especially if frequently repeated, as they usually need to be if the treat-

ment is efficiently fulfilled; while it must be remembered that they are not harmless measures, but may have a powerful influence for evil as well as for good. With regard to typhoid, many cases do not come under observation until it is too late to attempt to check the primary fever, even supposing that the intestinal lesion could be thus limited. For these and other reasons I do not see that, at present at least, a hydropathic treatment of typhoid fever in general practice has any claim to our support. If it is thought worthy of trial, it ought first to be fairly tested in *bona-fide* cases of this disease, and under the strictest and most competent supervision. With regard to sponging of the skin, I believe that this is often very useful, and ought to be employed far more frequently than it is at present, in typhoid as well as in other fevers. With proper care it does no harm, while it often gives much relief, and is beneficial in other respects.

3. The cases in which the more severe methods of applying cold are indicated are those in which the temperature is already very high and remains so, or shows a tendency to rise rapidly, especially if at the same time there are signs of much nervous disturbance. Unquestionably this plan of treatment is not resorted to under these circumstances nearly so frequently as it ought to be. It is difficult to lay down any exact rule as to what temperature indicates the necessity for adopting it, but if it reaches to 106° F. and shows no tendency to fall, or, still more, if it continues to rise, this treatment deserves due consideration. Necessarily much will depend on the actual condition of the patient, and every case must be thoroughly considered in all its features. The best method seems to me decidedly that of placing the patient in a tepid bath, and gradually cooling this. Affusion over the head is useful if there are marked nervous symptoms. Of course it is imperative that this treatment should be always conducted under the strictest supervision, and its effects carefully watched.—*The Practitioner.*

On the Treatment of the Diarrhœa of Typhoid Fever. (Part of a Clinical Lecture.) By George Johnson, M.D.,F.R. S.

The diarrhœa of typhoid fever, as it is one of the most frequent symptoms of the disease, so is it one of the most troublesome, and one which often causes the greatest anxiety. It is a fact generally admitted that in the great majority of cases the severity and danger of typhoid fever are in direct proportion to the intensity and duration of the diarrhœa. Delirium and other serious cerebral symptoms, pulmonary engorgement, and renal congestion with albuminuria, are comparatively infrequent complications. The treatment of diarrhœa, then, forms a very important part of the management of the disease. During the many years

of my connection with this hospital I have had the opportunity of seeing the diarrhœa of typhoid fever treated in very different ways and with very different results, and I propose now to give you, in a few sentences, the results of my experience with reference to this important practical subject.

For a number of years the practice strongly advocated by Dr. Todd was generally adopted throughout the hospital. This consisted in persevering attempts to arrest the diarrhœa by repeated doses of opiates and other powerful astringents. It was then a common practice to give an enema containing from 10 to 15 or 20 drops of laudanum after each liquid stool. The result of this treatment, in a large proportion of cases, was that the diarrhœa continued in spite of the repressive treatment, and meanwhile the intestines were distended with gas, and the abdomen became tumid and tympanitic. Then the patients were tortured by the application of turpentine stupes to remove the tympanitis. The results were altogether most unsatisfactory. Nor is it difficult to explain the failure of this opiate treatment. Without entering upon the consideration of disputed pathological theories, it can scarcely be doubted that one effect of opium must be to render the intestines torpid and to lessen their expulsive efforts, and as a result of this their putrid contents are retained until they decompose and give off noxious gases by which the bowel is distended and irritated, and so the diarrhœa is perpetuated and increased. It is pretty certain that the healing of the ulcers must be impeded by the continual contact of the fœtid morbid secretions, and that the distension of the bowel must cause pain and increase the risk of fatal perforation or rupture.

Now for a number of years we have entirely changed our treatment, and I have gradually arrived at the conclusion that in the treatment of typhoid fever careful nursing and feeding are of primary importance, while, as a rule, no medicines of any kind are required, and when not required they are often worse than useless. The result of this change of treatment has been that diarrhœa is a less frequent symptom than formerly, and when it does occur it is far more tractable, while tympanitic distension of the abdomen is a rare event. The mischievous opiate enemata and the torturing turpentine stupes have disappeared together. I believe that one of the main reasons why we have less diarrhœa than formerly is, that we carefully abstain from the employment of irritating drugs of all kinds. As a rule, a fever patient has the "yellow mixture," which is simply colored water; and except an occasional dose of chloral to procure sleep, and a tonic during convalescence, we give no active medicines of any kind. We feed these patients mainly with milk, with the addition of beef-tea and two raw eggs in the twenty-four hours, and we give wine or brandy in quantities varying according to the urgency of the symptoms of exhaustion, especially in the advanced stages of the disease; but in many of the milder cases, and especially

in the case of children, we find that no alcoholic stimulants are required from the beginning to the end of the fever, and when not required they are of course best withheld. I have said that we give no irritating drugs of any kind. For a time I adopted the practice which has been strongly recommended, of giving repeated doses of diluted mineral acids. I have long since abandoned this practice, for I am sure that it was injurious, and it was injurious in a very obvious and intelligible way: it irritated the ulcerated mucous membrane of the intestines, it caused pain and griping, and I believe that it often increased the diarrhœa. I have no doubt that the comparative infrequency of severe and obstinate diarrhœa amongst my enteric fever patients during the last few years is partly attributable to the discontinuance of this mineral acid treatment. The extreme sensitiveness of the intestinal mucous membrane during the progress of typhoid fever is obvious and indisputable. It is admitted on all hands that the greatest care is required in returning to solid food during convalescence; a want of caution in this respect has often been followed by a return of pain and diarrhœa, an increase of temperature, and not seldom by a decided relapse. If, then, a slice of bread or a morsel of fish can excite such local and general disturbance even after the subsidence of the fever, how improbable is it that repeated doses of an irritating mineral acid can be given without injury during the height of the fever, when the ulceration of the intestines is actively progressing.

One more hint I wish to give you with regard to the diarrhœa of typhoid fever, which is, that in all probability it is often increased by the patient's inability to digest the beef-tea and eggs which are sometimes too abundantly given. When you have reason to suspect that this may be the case, I advise you for a few days to keep the patient entirely upon milk, which contains all the elements required for the nutrition of the tissues in a form most easy of digestion. I have had a large experience of the effect of an exclusively milk diet in various forms of disease. In many cases of Bright's Disease it is very efficacious, but one of the inconveniencies in some of these cases is its tendency to cause troublesome constipation. In many cases of chronic diarrhœa and dysentery, milk diet will effect a cure without the aid of medicines of any kind. There is now in Twining ward a girl, aged 14, who for four months has been suffering from dysenteric diarrhœa, the stools containing much blood and mucus. She was put upon a diet of milk alone, without medicine: within a fortnight the diarrhœa entirely ceased, and she is now convalescent. For the reason, then, that milk has this antilaxative and even constipating effect in various morbid states, it is, when given alone, one of the best antidotes for the diarrhœa of typhoid fever.

That our treatment of fever cases is not unsuccessful is shown by the results. I find on reference to my case-books, that during the past year, from November 1, 1873, to October 31, 1874, I

have had under my care in the hospital twenty-nine cases of fever; fifteen typhoid, and fourteen typhus. Some of the cases have been very severe, but all have been discharged well; not one death has occurred. This very satisfactory result I attribute mainly to the admirable nursing which our patients receive, and to our abstinence from mischievous medication. To only one of these patients was opium given, and that was for the relief of an irritable condition of bowel which remained after a very severe attack of typhoid. A few doses of opium soon put a stop to this, and the patient made a good recovery.—*The Practitioner.*

Therapeutic and Physiological Action of Ipecacuanha and of its Alkaloids.

Dr. Polichronie has made, in M. Vulpian's laboratory, a series of very interesting researches in order to ascertain the mode of action of ipecac and emetine. M. Polichronie's conclusions are as follows:

1. Emetine is the true active principle of ipecacuanha; all the physiological, therapeutic, and toxic properties possessed by this plant are due to the presence of its alkaloid.

2. In dysentery, as in diarrhœa, ipecac administered by injection possesses as active properties as when it is given by the mouth, according to the Brazilian method.

3. Ipecac by injection is one of the best forms of treatment which can be employed in cholera infantum; it may be supported for a long time without enfeebling young subjects.

4. In tuberculous diarrhœa, whatever may be the period, injections of ipecac give good results.

5. This medication may also be employed with advantage to combat the profuse sweats of phthisis.

6. Emetine is a highly toxic substance; it may give rise to death in animals in two ways—sometimes by prostration of the nervous system, sometimes, when given in smaller doses, by the intense enteritis which it provokes.

7. Two hypotheses are possible to explain the favorable action of ipecac in diarrhœa: *a*, a vasculo-constrictive action, which diminishes the abundance of the secretions; *b*, a substitutive action, which results from the inflammation of the mucous membrane.

8. The vaso-constrictive action does not take place, as experiments made upon the nerve of the sub-maxillary gland and upon the arterial tension show.

9. The production of inflammation of the gastro-intestinal mucous membrane, the duration of which is prolonged after vomiting has been brought about, seems, on the contrary, to plead in favor of the second view.

10. Emetine causes vomiting at the moment when it is elimi-

nated by the gastric mucous membrane, while vomiting is retarded, and even more frequently does not take place after section of the par vagum. It is quite different with apomorphia and tartar emetic. These two substances bring about vomiting as quickly when these nerves are cut as when they are intact.

11. Finally, physiological and chemical researches upon the medicine argue in favor of elimination of emetine by the gastro-intestinal mucous membrane.

12. Emetine has no vomitive direct action upon the central nervous system, as is proved by direct injections of this substance into the cerebral arteries.

13. Therefore, it may be admitted that, in diarrhœa, emetine acts by substituting for the pathological inflammation a fresh one tending to spontaneous cure, its effects in these conditions being in every way comparable to that of purgatives or nitrate of silver.

14. In sweats we can no longer admit a vaso-motor action, and we are obliged to believe either that being eliminated by the sudoriferous glands it tends to restrain their secretion, or, better, that it acts by the revulsion which it causes to the digestive tube (Thése de Paris, 1874).—*Bulletin Général de Thérap.*, January, 1875.

The Relations in Temperature between the Mother and Fœtus.

Dr. Fehling has made a series of observations with the object of testing Cohnstein's suggestion of determining by the thermometer the life or death of the fœtus in utero. This was based on Bærensprung's observations, which showed that the fœtus is itself capable of generating heat and communicating it to the uterus, thus raising the temperature of that organ above that of the vagina. Consequently, when the fœtus dies the temperature of the uterus must sink. His observations seem to confirm Cohnstein's views, so that he considers that an equal temperature in the uterus and vagina may be regarded as quite sure proof of the death of the fœtus. He then discusses the explanation offered by Pfannkuch of the chill so often observed in women who have just been delivered, which is the following, viz., that the fœtus being a source of heat to the mother, the latter experiences a chill when it is removed at birth. Now it often amounts to only a slight shivering, and at most does not occur in more than from 30 to 40 per cent. of confinements. Thus Pfannkuch's explanation would imply a lowering of temperature in the interior parts of the body, the fact being that in the febrile child, as distinguished from one which is purely nervous, there is a rise and commonly a rapid rise in the temperature of the internal organs over that of the periphery. Among the author's observations, it happened twice that there was a chill after the birth of a putrid fœtus; and this was in neither

case due to septic influences, as the woman recovered without fever. He agrees rather with Schrœder's explanation of the chill in question, viz., that it is due to a difference between the temperature of the skin and that of the interior of the body. The internal temperature rises in consequence of the continued active contraction of the womb, which is now empty, and in contracting meets with no resistance. During the period of expulsion, on the contrary, owing to the associated voluntary efforts involved, the lungs and skin are in great activity, so that the surface is warm and moist. In the repose which succeeds the act of birth the loss of surface temperature is considerable, and a chilly feeling is quite natural; and its intensity will depend largely on the sensitiveness of the woman and the state of her skin.—*Medical Record.*

Poetry and Physic.

One of our contemporaries, discussing this subject some time ago, attempted a quotation from *Terrible Traetoration*, in which he did the poem such injustice that we feel in conscience bound to come to the rescue of our early favorite. The following is the stanza which our learned contemporary attempted to give:

> "Come on, begin the grand attack
> With aloes, squills, and ipecac;
> And then with glisterpipe and squirtgun
> There will be monstrous deal of hurt done."

The fictitious name of the author of this amusing poem was "Christopher Caustic, M.D., LL.D., A.S.S." His real name was Thomas Green Fessenden, an agricultural editor of New England, and a political writer of great point and force at the period when the contest between the Federalists and Democrats was fiercest. His assault upon Thomas Jefferson and Democracy provoked a reply from Irving and Paulding, editors of "Salmagundi," in which they descended to call him "*Dr. Costive*, author of a poem with notes, or rather notes with a poem." There is much to entertain medical readers in his *Terrible Tractoration.*—*American Praetitioner.*

Spinal Meningitis Treated by Chloral.

The *Arehives de Generales de Medecine*, for 1874, contains the record of a case of spinal meningitis, probably of a rheumatismal nature, which was cured by the administration of chloral. The patient was a man aged 37, a currier, working almost constantly in water, but never having been troubled with rheumatism. He had been attacked three weeks before he came into

the hospital with fever, accompanied with cephalalgia, anorexia, then with cramps in the calves of the legs and thighs, with pain in the posterior region of the neck, followed by permanent contraction of that region. When he was admitted into the hospital, on November 29th, 1873, rigidity and contraction of the muscles of the posterior cervical region, pain at the level of the vertebral spinal apophyses, and a little want of sensation in the arms were present, as well as a contraction of the muscles of the thigh and leg, which imparted forced extension to the lower limbs, with complete anæsthesia extending from four inches below the femoro-tibial articulation to the ends of the toes. The muscles of the lower limbs were agitated by fibrillary contractions. The only cerebral symptoms were the diffused cephalalgia, which had been present for three weeks, and some sensations of confusion. There was no functional trouble of the rectum or of the bladder. The initiatory fever had disappeared. The pulse was 76, and the temperature was at 38° 8' Cent. (101° 84' Fahr.). All these symptoms disappeared towards March 1, after various remissions, and by March 20 the patient was completely cured. The contractions, the painful cramps, the anæsthesia with spontaneous pains, the absence of cerebral troubles, the immunity of the bladder and the rectum, the absence of all paralytic or paresic manifestation, and the comparatively rapid cure, all made the diagnosis of spinal rheumatism admissible. The treatment employed certainly produced the best effects. Opium successively associated with sulphate of quinine, with tartar emetic, and with belladonna, baths, dry and wet cupping, etc., only gave a very small amount of ease. Chloral only, administered for several days in progressively increasing doses, from three to nine grammes (45 to 135 grains) *per diem*, in divided doses, was immediately followed by a very remarkable improvement, especially in relation to the tetanic symptoms. It is necessary in these cases to employ somewhat large doses, since it is demonstrated that narcotics or sedatives are only efficacious in convulsive affections of the spinal system, either clonic or tonic, when they almost approach the poisonous dose. In this case symptoms of intoxication only appeared late, and consisted merely of sleepy hebetude, a species of mild intoxication without excitement, hallucinations, dreams, without any change in the contractility of the pupil or any visual troubles.—*London Medical Record.*

Preparation of Koumys.

Five quarts of fresh milk, ¼ lb. grape sugar, and fresh beer yeast of the size of a hazel nut, are mixed, heated upon a slow fire to 25° R. (88° F.), removed from the fire for a short time, then again heated as before, at once filled into champagne bottles to within an inch of the neck, and these well corked. The bottles should be agitated every fifteen minutes during the next forty-

eight hours. If well prepared, Koumys must effervesce like soda water.—*American Journal of Pharmacy.*

Toothache Drops.

The *Dental Cosmos* for November, 1874, publishes the following formulas:

1. ℞.—*Chloroform*,
   Sydenham's laudanum, *aa* ʒii
   Tinct. benzoin, - - ℥i

2. ℞.—*Creasot*,
   Chloroform, - - *aa* ʒii
   Sydenham's laudanum - ʒiv
   Tinct. benzoin, - - ℥i

3. ℞.—*Oil of Peppermint*,
   Rhigalene.
   Chloroform, *aa* ʒiii
   Camphor, - ʒii

4. ℞.—*Chloral*,
   Camphor - *aa* ʒi
   Morphia, - gr. ii
   Oil of pepper't, ʒii

—*American Journal of Pharmacy.*

Diphtheria.

The Board of Health of New York has published (*Medical Record*, January 9, 1874), for popular information, the following precautions which should be observed wherever diphtheria prevails:

Precautions—(*a*) *The Dwelling or Apartment.*—Cleanliness in and around the dwelling, and pure air in living and sleeping rooms, are of the utmost importance where any contagious disease is prevailing, as cleanliness tends both to prevent and mitigate it. Every kind and source of filth around and in the house should be thoroughly removed; cellars and foul areas should be cleaned and disinfected; drains should be put in perfect repair; dirty walls and ceilings should be lime-washed, and every occupied room should be thoroughly ventilated. Apartments which have been occupied by persons sick with diphtheria should be cleansed with disinfectants; ceilings lime-washed, and woodwork painted; the carpets, bed-clothing, upholstered furniture, etc., exposed many days to fresh air and the sunlight (all articles which may be boiled or subjected to high degrees of heat should be thus disinfected); such rooms should be exposed to currents of fresh air for at least one week before reoccupation.

(*b*) *When Diphtheria is Prevailing.*—No child should be allowed to kiss strange children nor those suffering from sore throat (the disgusting custom of compelling children to kiss every visitor is a well-contrived method of propagating other grave diseases than diphtheria); nor should it sleep with nor be confined to rooms occupied by, or use articles, as toys, taken in the mouth,

handkerchiefs, etc., belonging to children having sore throat, croup, or catarrh. If the weather is cold, the child should be warmly clad with flannels.

(*c*) *When Diphtheria is in the House or in the Family.*—The well children should be scrupulously kept apart from the sick in dry, well-aired rooms, and every possible source of infection through the air, by personal contact with the sick, and by articles used about them or in their rooms, should be rigidly guarded. Every attack of sore throat, cough, and catarrh should be at once attended to; the feeble should have invigorating food and treatment.

(*d*) *Sick Children.*—The sick should be rigidly isolated in well-aired (the air being entirely changed at least hourly), sunlighted rooms, the outflow of air being, as far as possible, through the external windows by depressing the upper and elevating the lower sash, or a chimney heated by a fire in an open fireplace; all discharges from the mouth and nose should be received into vessels containing disinfectants, as solutions of carbolic acid, or sulphate of zinc; or upon cloths which are immediately burned; or if not burned, thoroughly boiled, or placed under a disinfecting fluid.—*Medical News and Library.*

On the Treatment of Suspended Animation in New-Born Children.
By Charles E. Buckingham, M. D.

With some obstetricians, the condition of the new-born child, compared with that of the mother, is of secondary consequence. I confess it is so in my estimation. This is a matter which depends upon the religious views of different individuals, and of course is not to be here discussed. Both the mother and the child require attention, and you can oftentimes give directions for the benefit of the child while you are making the required pressure over the uterus which has just expelled it.

Sometimes the child cries lustily as soon as it is expelled. Sometimes it gasps feebly, with long intervals between its respirations, which may of themselves become more frequent and stronger, or less frequent and more feeble. It may come into the world blue and flabby, and without a visible sign of life. If there be beating of the umbilical cord, however, there will almost certainly be a gasp, and that gasp may be repeated, or if not repeated unaided, your assistance may restore the child to life. Even if there be no pulsation to be seen or to be felt, you may in some cases hear it by putting your ear over the heart. You need not trouble yourselves about a ligature upon the cord; make the child breathe. And for this end it is not worth while to spend time in trying the Marshall Hall method; you have a chest to deal with which has never been expanded, and a pair of lungs which have never been inflated. Send for a couple of pails

of water, one cold and the other rather warmer than it would be comfortable to take an entire bath in. A child who has never breathed, if rapidly dipped in these alternately a few times, will often cry audibly. But you must not wait for the pails of water before trying other measures to make the child breathe; if you do, it will be just so much neglect. With a dry rag over your little finger, thoroughly wipe the mucus from the fauces; that operation alone will make some children cry. Take the child up in a dry towel, or a pocket-handkerchief if you have one at hand, or in anything which will keep it from slipping from your grasp; hold it with the scapulæ in the palm of your left hand, the finger and thumb embracing the occiput, which should be firmly pressed backwards; the finger and thumb of the right hand should close its nostrils. Apply your mouth to that of the child and try to inflate its lungs; you need not fear that you will blow too hard; indeed, unless you place a moderately dry cloth between the child's mouth and your own, you will find it difficult to inflate at all. But why press the head forcibly backwards? Because in so doing you close the passage of the œsophagus; and should you neglect that precaution, you would find the stomach inflated instead of the lungs, and a new obstacle thus put in the way of the child's breathing, by your own carelessness.

You should inflate the lungs ten or fifteen times in a minute; and the process should be continued as long as there is the slightest possibility of life. The occasional alternate dipping will help your efforts. In some cases, a rapid and more forcible pulsation of the heart is felt by you upon your very first insufflation, and this, as a rule, will be repeated and increased in strength with every succeeding attempt, until as you take your lips away you will each time see the child gasp, open its eyes, heave its chest, and at last cry. The color, which has been leaden and dull, becomes of a positive red. The points upon which you placed your fingers, before the operation, became white, and remained so long enough for you to count twenty or more; but now the color returns more and more rapidly, and you will find, as the child's respirations become independent of your aid, that the color returns almost immediately on the removal of the pressure.

Be sure that all chance of life is gone before you stop your exertions; I have known an infant, who was laid aside in a sheet as dead by one of our profession, to live to adult age. So long as the breathless child is cool, if pulsation exists even to a slight degree, life is still possible. Excess of heat to such a child will diminish its chances for life. Why, then, you may ask, do I dip it in hot water, as well as in cold, to make it breathe? Simply as a stimulant to its skin. It is not to be left in the hot water an instant; it is dipped in hot water for the same reason that I would spank it, or slap it with a wet towel, the object being to irritate its nervous system and make it cry.

If you will now simply wrap the resuscitated infant in a

blanket, and leave him without washing or dressing or food for a few hours, he will be better off than if you weary him with further attention.—*Boston Medical and Surgical Journal.*

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*On the Relations between Diphtheria and Scarlet fever.** By Arthur Ransome, M.A., M.D.Cantab.

The following case shows the close connection between these two diseases.

On February 23d last year, a boy at school had scarlet fever. He was isolated until March 25th, when his brother saw him and took the complaint. Other cases occurred at the school on the 29th, 30th, and 31st. On April 2d, one of the masters had a rigor and sore-throat; he left at once, and was seen by me next day. On the third day, diphtheritic patches covered the tonsils; but there was no rash. Slight albuminuria accompanied the pyrexia; both disappeared by the seventh day. On April 9th, six days after he arrived at home, his mother was attacked, membranous exudation appearing in both tonsils; but there was no albuminuria, and no rash.

The origin of these cases must surely be traced to the epidemic of scarlet fever at the school; for there were no cases of diphtheria either in the neighborhood of the school or near their home. The close relationship of these diseases has often been pointed out. In the *Report of the Medical Officer to the Privy Council* for 1859, Dr. Burdon Sanderson gives cases of diphtheria and scarlet fever coexisting; there also I find three cases of diphtheria following three of scarlet fever, and the former spreading to a neighbouring cottage. These diseases have much in common. They are alike in their zymotic or epidemic character; both are characterised by high pyrexia; their chief point of attack is the throat and the glands of the neck; and a rash is found in both diseases. Their sequelæ also are similar: suppuration in the lymphatic glands, ulceration of the ears, arthritic affections with or without cardiac complication, kidney-disease and dropsy, general or localised paralysis of the nervous system. I have seen each and all of these consequences after both diseases.

Some physicians have been led by these points of resemblance to regard them as one disease, the same poison producing different manifestations; but, in spite of their similarity, I venture to think the differences are too important and too numerous to permit so simple a solution of the problem. In the first place, diphtheria is seen to arise directly from scarlet fever. I have never known the converse action. There are other etiological differences. Though both are contagious, diphtheria is less so, and

*Abstract of a paper read before the Epidemiological Society of London, January 13th, 1875.

more frequently occurs in single cases. Where several cases occur together, some general cause is probably concerned. Scarlet fever is more frequently conveyed by cloths or other fomites, by attendants on the sick, and it may be conveyed by means of milk. I am not aware that diphtheria has ever spread by any of these means. The two diseases differ in their geographical distribution. Diphtheria is common in India, where scarlet fever is unknown. Colonies and all new settlements seem peculiarly liable to diphtheria; it has appeared in the Australian colonies and in South America, attacking the otherwise healthy town of Buenos Ayres. In Lima, it was noticed that the black races withstand its contagion. It selects high and well drained districts, when low-lying lands in the vicinity escape. Dr. Blake, in the *Transactions of the Medical Society of California*, mentions a most fatal epidemic at an elevation of 4,000 feet. Many places in England subject to diphtheria are either high or well drained. Scarlet fever also haunts certain places; but these scarlet fever fields are not at high levels, nor are they those in which diphtheria is especially prevalent. Diphtheria is often connected with defects in the house drainage, not always due to imperfection in the house connection with a general sewage-system, but to leakage from private cesspools; so that a question of spontaneous origin may arise in this disease, which is hardly admissible with respect to scarlet fever. Season has a different influence on the two diseases; scarlet fever being most prevalent in the autumn, low barometric pressure and greater humidity in the air favoring its diffusion; while diphtheria may arise at any season, and in my own experience, its most virulent epidemics have occurred in the summer months.

Another difference is found in the fact that scarlet fever is not often associated with other diseases, while diphtheritic affections are not uncommon in the course of other pyrexial disorders. It is recorded in the Report of the Diphtheria Sub-committee of the Epidemiological Society, that, in the epidemic of 1861, this disease occurred fifty-seven times alone, thirty-four times in association with scarlatina, nine times with small-pox, seven with measles, six with fever, and three times each with ordinary sore-throat, croup, and catarrh. But in the nature and symptoms of the two diseases are points of unlikeness too great to allow us to rank them as varieties of the same species. Thus, the rash of diphtheria is often absent, is very variable as to the time of its appearance; it occurs seldom at the outset of the malady, and I have seen it as late as the third week of its course. The period at which albuminuria sets in is also different, often appearing in severe cases of diphtheria within the first two or three days, whilst in scarlatina it seldom sets in until degeneration of the kidney is commencing. There is, indeed, an entire absence of definiteness about the duration of the symptoms of diphtheria, which of itself marks it off from the regular sequence of events in scarlet fever. I have known it last for six weeks, and even

two months, without any evident affection of the cervical glands, but with constant formation of exudation on the fauces or on other mucous surfaces.

The mode in which diphtheria localises itself on the mucous membranes, or on the broken surfaces of the skin, would probably be sufficient to separate it from any other disease. I do not know that scarlet fever ever attacks the larynx; but I have seen the diphtheritic membrane appear in different cases upon wounds of the skin, upon blistered surfaces, and upon nearly all the mucous membranes of the body—the lips, cheeks, nose, ears, pharynx, larynx, trachea, and bronchi, and on the anus and vulva; and different epidemics have differed strangely in this respect; in one, the throat would always be the point selected for attack; and in another, the other mucous membranes. It is somewhat remarkable in the latter case that although the disease was mild and there were no deaths, the subsequent complications were much more frequent and troublesome.

The last point of difference that I shall mention is the fact that, while scarlet fever seldom reappears again in the same individual, diphtheria seems by its first attack to confer no immunity from subsequent seizures; on the contrary, the susceptibility of the throat seems to be rather increased.

Upon taking a survey of all these relations existing between scarlet fever and diphtheria, it is evident that they are distinct diseases, and yet there is some very close and definite connection between them. Wherein the bond consists, it is not easy to point out. From the manner in which the diphtheritic poison seeks out the highest points of the best drained localities, it has appeared to me most probable that the virus of this disease must be of a more rarefied and subtle nature than the germs that produce scarlet fever.

It is possible that the nature of some other ferments may throw some light upon this subject. I would specially call attention to the discovery by M. Berthelot (*Comptes Rendus*, vol. l, p. 980) of an unorganised glucosic ferment in an infusion of yeast resembling disatase; also to the fact that one fermentation by organised beings frequently prepares the way for another. It may be suggested, that in one of these ways the singular relations existing between scarlatina and diphtheria might be accounted for. I do not venture to affirm that there is a strict analogy between these fermentative actions and the zymosis of scarlatina and diphtheria; but, as there exist in nature ferments requiring the presence of living germs, and others acting independently of them, so there are disease ferments with organic germs reproducing themselves, and others which have no definite term of life, and which are not self-reproductive.

It is probable that, in most cases of fermentation, the living being connected with each kind of this action is of a specific nature, and peculiar to the medium or to the food upon which it lives. In some instances, a ferment either during or at the end

of its operation will produce the conditions favorable to the growth of another kind of germ, as seen in the crowd of vibrios appearing at the end of a mycodermic fermentation; so the pyrexial state may produce the condition most congenial to the diphtheritic ferment, whether that may prove to be an organised germ or an unorganised catalytic ferment. As my excuse for offering these suggestions, I will quote, in conclusion, the words of the great English philosopher Robert Boyle. "He that thoroughly understands the nature of ferments and fermentation shall probably be much better able than he that ignores them to give a fair account of several diseases (as well fevers as others) which will perhaps be never thoroughly understood without an. insight into the doctrine of fermentation."—*Works*, ed. 1744, vol i, p. 476.

Hydrate of Chloral as a Solvent.

Mr. Robert M. Faithorne writes to the *American Journal of Pharmacy:*

A solution consisting of nine parts of hydrate of chloral and three of water, I find capable of dissolving the following substances, to the extent named:

One grain of morphia is dissolved by a portion of the liquid containing twelve grains of the hydrate, one grain of veratria by a portion containing five grains, and one grain of atropia by a portion containing twenty grains.

These active principles should be in powder, mixed with the solvent in test-tubes, and heated by means of a water-bath, with occasional agitation.

The solutions thus made are in a convenient form of employment, either alone or when mixed with oils, ointment, or with glycerin. Camphor, too, is freely dissolved by them, and in some cases can be added to them with advantage.

Glycerin I find to be a convenient agent for forming solutions with the chloral and the above named substances, and the following will be found, when properly combined, to produce permanent and elegant preparations, viz:

Chloral Glycerite of Morphia—℞. Morphia (powd.), grs. v; chloral hydrate, ʒj; glycerin, fl. ℥ss. M. Sec. art.

Chloral Glycerite of Veratria—℞. Veratria, grs. v; chloral hydrate, ʒj; glycerin, fl. ℥ss. M. Sec. art.

Ointment of Chloral and Veratria—Corresponding in strength to the Ung. Veratriæ, U. S. P. ℞. Veratria, grs. x; chloral hydrate, ʒj; water 6 drops; lard ointment, ℥ss. M. Sec. art.

Chloral Glycerite of Morphia and Camphor—℞. Morphia, grs. 5; chloral, camphor, aa ʒj; glycerin, fl. ℥ss. M.

Lotion of Chloral and Iodine—℞. Iodine, grs. 20; iodide of potassium, grs. vi; glycerin, fl. ℥j; chloral hydrate, ʒij. M. Sec. art.

Chloral can also be combined with collodion, in which it dissolves after the addition of a few drops of alcohol.—*Southern Medical Record.*

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*The Therapeutic Value of Iodide of Potassium.**

As I have for several years paid considerable attention to the action of iodide of potassium, I venture to offer the following remarks as supplementary to Mr. Spurgin's article in the *Journal* of September 5th, 1874. This medicine has been accredited with many modes of action: thus, in struma as an alterative, in asthma as a sedative, and in diphtheria as an antidote. To all these titles it may have a claim which different observers may think fairly borne out; but certainly the one distinct and indisputable action of iodide of potassium which I have noticed, is that of *stimulating the mucous membranes;* thereby influencing their action and promoting their secretions. Thus, as the results of its use, there are pain and sense of fulness across the eyes; increased secretion from the nares, mouth, fauces, and bronchi; leucorrhœa and menorrhagia are greatly aggravated; and in persons very susceptible of its influence, diarrhœa is induced, not so much of a cathartic as of a dysenteric kind; that is, rather an increase of mucus with tenesmus than of serum with catharsis.

In a person suffering from an attack of chronic winter cough, the first symptoms are great difficulty in breathing, amounting to a sense of suffocation; hard, dry, racking cough, which the patient says he cannot subdue; while he expresses a belief that relief would be obtained if something could be brought up. The suffocation complained of has been attributed to a swollen state of the air-passages, obstructing the respiration; but there is a fair probability that the dry congested condition of the membrane is unfavorable to the interchange of gases requisite for blood aeration, and the situation of the patient such that, however he may fill his lungs, his sufferings remain unrelieved. Whatever the actual state of matters at this point, certain it is, that as soon as expectoration sets in, the breathing is improved; and, although the disease has by no means gone, the patient is so far better. Many hours of severe suffering may be obviated by taking advantage of the power of iodide of potassium to restore and promote the secretion of the bronchial membranes, thereby greatly relieving the congested blood-vessels, producing comparative tranquility of breathing, and getting the patient over the first stage of the disease much sooner than he otherwise would. This, however, is possibly not its only value. For, here again, however opinions may differ as to the cause of the emphysema, which from an early period exists in these cases, no one can have witnessed the severe and straining cough at the onset

* Extracted from a paper read before the Glasgow Northwestern Medical Society in the winter of 1872.

of the attack, without feeling that it is at least possible for either dilatation of the air-cells or rupture of the tissue of the lung to take place—complications much less likely to occur, so far as the cough is concerned, when the sputum has been rendered easier of expectoration, and the irritability of the congested membranes removed by free secretion. It is further to be remarked that the action of the iodide of potassium changes the purulent character of the sputa in chronic bronchitis to a much healthier appearance. From this view of its operation, it follows as a matter of course, that when free secretion of mucus has set in the medicine should be used with caution or altogether abandoned; and, therefore, when in the treatment of bronchitis—capillary or chronic—moist *râles* are fairly established, the further management of the case should be on the principle of preventing a too abundant secretion, at the same time employing such means as may assist expectoration and maintain the strength.

In asthma, iodide of potassium is recognised as a valuable medicine. Here the explanation of its action generally given, is that of a sedative relieving bronchial spasm; evidence of the presence of spasm being found in the wheezing and whistling sounds heard in auscultation. Either of these sounds, however, fairly suggests the question, how far a fit of asthma is dependent on, or, at all events, greatly aggravated by, an abnormally dry condition of the mucous membranes, acting as in the diseases already mentioned, which is relieved by the iodide restoring the secretion.

In diphtheria, iodide of potassium is looked upon by many practitioners as the best remedy we possess. Here its alterative and sedative actions are laid aside, and we have it doing duty as an antidote to the diphtheritic poison; although, so far as can be seen, it exercises no new influence. In this disease, while there is free secretion from the nares, the breathing and cough-sounds are usually not very alarming, nor is respiration greatly impeded. It is not till the nares become dry—and doubtless the pharyngeal, laryngeal, and tracheal secretions diminished—that the formation of false membrane proceeds with fatal rapidity; hence, it does not seem too much to assume, so long as an iodide can keep up these secretions in such profusion as to prevent them from remaining on the parts sufficiently long to undergo membranous change, so long will its action be beneficial. The idea of an antidote might be more satisfactory; but it cannot be substantiated; nor does this view of its action afford any indication as to what extent the medicine should be given; whereas, by paying attention to the degree of influence exerted upon the mucous secretions, the dose and frequency of administration may fairly be ascertained; if not, indeed, the knowledge acquired as to whether or not it is doing any good.

Without at present entering into a consideration of the influence of iodide of potassium on digestion and assimilation—the

real sources of its so-called alterative power—I may state as my conviction, that in all the various manifestations of struma, etc., where this medicine is of service it acts, so far as the iodide is concerned, in stimulating the mucous membrane of the stomach and duodenum—possibly, by sympathetic action, the liver and pancreas also—to increased secretion, whilst its alkaline base tends to promote the digestion of fat and starch.

For the dose no absolute rule can be laid down, because, in few respects, indeed, do constitutions and temperaments differ more than in the relative irritability of the mucous membranes, and, consequently, the power of iodine to influence their action. Persons of the bilious temperament usually resist its power to a wonderful degree, whilst in those of the lymphatic, sanguineous, and above all, the nervous, a few doses of two grains each will often suffice to cause coryza, ptyalism, pharyngeal irritation, and cough. In such diseases as diphtheria, the object should be to produce its influence as rapidly as possible, whilst in others, as struma, small doses long continued are preferable.—JAMES LAWRIE, M.D., Glasgow.—*The British Medical Journal.*

Medical Notes.

In cases of dropsy of the joints, especially that of the knee, Dr. Bergeret finds the continued application of bags of hot sand to answer better than any other kind of treatment. When the acute stage is passed, and whatever may be the cause of the dropsy, he wraps the joint in a thick layer of cotton-wool, and applies to this a sack containing two or three litres of fine and very hot sand. The dropsy disappears in a few days. The sand must be very hot, and the heat may be kept up by means of covering with a blanket. The sand must not be two thick in the bag, so that it may extend easily on the knee, and overhang the hydrarthrosis in every direction.

Dr. Paul F. Munde, in a letter to the *New York Medical Record*, calls attention to Hoenig's method of removing the ovum in cases of abortion. Hoenig recommends to express the ovum, either entire or in part, if the fœtus be already removed, by means of bi-manual compression, two fingers of one hand being introduced into the vagina, and passed as far up as possible into the fornix vaginæ, the other hand grasping the uterus through the abdominal parietes, thus firmly compressing the organ between the fingers of both hands, and slowly and surely expelling its contents. If the uterus is anteverted or anteflexed, as usual during the earlier months of pregnancy, the two fingers should be passed into the anterior cul-de-sac, or the corpus uteri may be firmly pressed against the symphysis pubis by the external hand alone (the bladder having been emptied). If the uterus is retroflexed, the two internal fingers go behind the cervix. Of course, it is essen-

tial that the cervix be sufficiently dilated before this method is attempted.

Dr. Samuel W. Francis, Fellow of the New York Academy of Medicine, has recently published pamphlet No. 3 of his medical series. It contains short articles on the term "syphiline" to indicate the syphilitic poison, on a glass glove or boot for the treatment of sprains or burns, on "Self-Opening Coffins and an Alarm Telegraph," etc. The longest paper is entitled Man and Nature; from it we extract this paragraph: "It is my firm conviction that the mosquito was created for the purpose of driving man from malarial districts; for I do not believe that in nature any region where chill and fever prevail can be free from this little animal. Now if man will not go after warning is given in humming accents, then the mosquito injects hypodermically a little liquid which answers two purposes—firstly, to render the blood thin enough to be drawn up through its tube; and secondly, in order to inject that which possesses the principles of *quinine!*"—*Boston Medical and Surgical Journal.*

Metastasis of Parotitis to the Brain.

To the Editor of the *New York Medical Record:*—Sir: In the *Record* for January 9 there is a case, reported by Dr. Kemp, of parotitis, terminating fatally by metastasis to the brain. He observes that it is the only instance reported of which he is cognizant, with the exception of one occurring in the experience of Dr. Dickson, of Charleston.

To add to the literature of this uncommon complication I would mention two cases, which came under my observation while a student of medicine in Washington, D. C. They occurred in the practice of Dr. W. P. Johnston, a leading practitioner of that city. One of the cases was a fellow medical student, aged twenty. The other was his brother, two years younger. The eldest was taken ill first, and in a few days metastasis to the brain occurred, which terminated fatally in four days. About a week later the brother was attacked and succumbed from the same complication.

I am not aware that these cases have ever been placed on record, but they have been related by me at several of the medical societies, while a resident of New York.

E. Lee Jones, M.D.

Oakland, California, January 18, 1873.

Other cases of this disease will be found mentioned in an article by Dr. Radcliffe, published in the *Philadelphia Medical Times* for August 15, 1874, an extract of which appeared in the *Medical Record* for 1874, p. 514.—Ed.—*Medical Record.*

Hypodermic Injections of Atropia in Certain Cases of Sunstroke. By J. R. Barnett, M.D., of Neenah, Wisconsin.

It is believed that while no new principle of treatment is discovered, in the clinical histories of the two cases of sunstroke recorded below, a new remedy has been suggested, which promises, from its availability and the promptness and certainty of its action, to supply a want which most practitioners have, at one time or another, felt, viz., a cerebral and cardiac stimulant which can be administered regardless of gastric irritability—so often present in sunstroke to a degree rendering medication by the stomach impracticable—and which can compass the desired result more speedily if not more surely than the usual alcoholic stimuli.

It can only be necessary, in this connection, to advert to the fact, that, while in a certain proportion of cases of sunstroke the pathological element of apoplectic compression is the leading phenomenon, indicating prompt and active depletion, by far the larger proportion of cases display other and sometimes widely different morbid phenomena, indicating a more or less stimulating treatment. In these cases the pathological condition is that of prostration, denoted by a weak, rapid, and more or less irregular pulse, and a difficult respiration, either accelerated or retarded, and frequently superinduced by the cachexiæ following previous exhausting diseases. In these the tendency is to death by asthenia or rapid syncope; and it has been the experience of many to know, as far as falls within the reach of human knowledge, that this impending fate has been averted only by the prompt and adequate use of alcoholic stimuli. It has also been the deplorable experience of many, that in not a few cases these remedies have been unavailable because of the patient's intolerant stomach, or unavailable because of indolent gastric and intestinal absorption, or of perfect non-absorption.

There is a large class of cases in which symptoms of prostration perhaps predominate, but in which apoplectic phenomena appear sufficiently marked to create somewhat of doubt in the mind of the physician as to which line of treatment is called for—stimulation or depletion. The exhaustion, indicated by the rapid and irregular but somewhat hard pulse, calls for stimulation and support, but the accumulated masses of blood within the cerebral vessels, which have extinguished sensation and overwhelmed innervation, call as imperatively for some means of a prompt unloading. Of course, such cases are to be treated upon their own special indications, and it may be said that when these do not warrant stimulation, in the general sense, they will hardly suggest it in an expectant treatment. There is, however, a consideration which I believe will permit the use of atropia when other stimuli are contraindicated, or permissible only in small doses most carefully watched; it is the well-known influence

of atropia over the vaso-motor nervous centres. Occupying a position at the head of that class of agents which contract the calibre of the blood-vessels, and at the same time a cardiac stimulant of undoubted energy, it is rationally indicated in those cases characterized by congestive compression conjoined with cardiac prostration, and practical trial confirms its appropriateness. I believe it will herein be found to be most peculiarly acceptable as an effective weapon from our medical armamentarium, covering a doubtful ground which else must often give us sore perplexity, and exalting at once the overwhelmed sensory, motor, and vaso-motor nervous functions as can no other remedial, however rationally indicated.

Again, in those occasional cases characterized by an abnormally slow, weak, and irregular pulse, slow and difficult respiration, and usually marked contraction of the pupil, denoting basilar oppression, the powers of atropia become serviceable, either unaided or reinforced by other appropriate stimuli, and for the added reason that while, in these cases, there may be a sufficient degree of gastric tolerance to permit medication by the stomach, stomach absorption is usually so impeded that this medium of medication is obviously not to be trusted alone, where the eminent danger is from rapid syncope.

The powers and advantages of atropia in insolation may be thus summarized—

1st. It is the most general excitant of the vital nervous centres available.

2d. It is available hypodermically irrespective of gastric or enteric conditions.

3d. Like other stimuli, it is indicated in by far the greater proportion of cases of sunstroke; but, further, it is permissible in many cases not amenable to other excitants.

I report the following two cases as a limited illustration, mindful of their small value from the standpoint of clinical experience alone, but trusting to the future investigations of the profession in this direction to develop what I believe to be a valuable resource in the therapeusis of insolation.

Case I.—August Naegel, German, aged 20, a farmer, while riding home from Neenah on the afternoon of July 25th, noted as the hottest day of the year up to that time, had proceeded but two miles, when he became so overpowered by the intense heat that he had to be taken from the wagon and carried to a farm house; close by, for assistance. I found him half an hour afterward in the following condition: semi-comatose; pulse about fifty, extremely weak and irregular; breathing slow and stertorous, inspiration, even at times when patient was roused to half consciousness, snoring and labored, expiration short and puffing; head hot; pupils intensely contracted.

His attendants said that he had at first complained of great epigastric pain, accompanying intense headache. He had been given a quantity of mustard to produce emesis, together with

two or three ounces of whiskey as a restorative. In spite of this last remedy the pulse was at the low ebb spoken off, although as to whether there had been any improvement in this symptom from the stimulant I could not ascertain. The patient was somewhat plethoric, but the indication obviously was to arouse the heart until the brain could be unloaded and restored to its natural functions. The whiskey already given had accomplished so little in this direction that I did not feel encouraged to try it further; accordingly I injected hypodermically 1-50th of a grain of atropia with 1-10th grain of the sulphate of morphia, assiduously plied cold affusions to the head, and awaited the result. Within five minutes the pulse numbered sixty-eight, and was full and soft, and at the end of fifteen minutes the pupil had dilated to rather more than its natural diameter. The change in the character of the breathing was equally marked. The stertor subsided, except at long intervals. The patient got up, talked with tolerable coherence, and asked to go home. Most of the foregoing symptoms, though less in degree, returned about half an hour afterwards, apparently the result of injudicious exercise and excitement, but rest, and a few drops of the tincture of digitalis, rallied the patient, and at the end of an hour from the time I arrived I considered it safe to leave him for the night. He made a full recovery without any subsequent alarming symptoms, resuming active farm work a few days afterward.

Remarks.—The promptness of this relief from a condition that could only be regarded as critical, surpassed anything of the kind that I had previously witnessed, and it did not fail to impress the bystanders with the abundant resources of medicine. The case undoubtedly might have recovered under a slower stimulation, but this admission in no wise weakens the contrast between the effectiveness of the two. Alcoholic stimulation had failed to accomplish in half an hour what the atropia wrought in ten minutes. The possibility of rapid recovery from these conditions, irrespective of treatment, is remembered; but to suppose the recorded effects in the present instance—all symptoms of cerebral oppression disappearing, *pari passu* with the progressive change in the iris—to be a mere coincidence, is to struggle for a remote conclusion when an easier one is right at hand.

The part played by the fractional dose of morphia was, as anticipated, simply that of a cardiac stimulant. It did not seem to modify the influence of the atropia upon the vaso-motor nervous centres in the least.

Case II.—Simon Nelson, a Dane, aet. 40, laboring in a lumber yard, was prostrated by the excessive heat of Aug. 12th. But little could be learned of first symptoms of attack, other than that he complained of faintness and nausea. After a severe attack of vomiting he started alone for home, half a mile away. When within a few rods of his house he fell down insensible, where he was immediately found by the neighbors and carried into the shade. Visiting a patient near by at the time, I was

summoned, and reached the spot within a few minutes. I found him in profound coma. His clothing was covered by ejecta vomited just before. Breathing was stertorous and very labored; head and skin intensely hot; pupils contracted to a mere point; pulse 156, small and somewhat cordy. There was complete anæsthesia, no reflex movements following pinching of the skin or eyelids, or even rubbing of eyeball. Conjunctiva injected, but whether an effect of the sunstroke or not I could not judge. I resorted to cold affusions to the head and sponging of the body immediately. The pulse rapidly weakened, and in a few minutes I attempted to give whiskey. This proved to be impracticable, as suffocation seemed imminent upon placing any fluid in his mouth. Concluding to give atropia, but not having it with me, I returned for it to my office, being absent ten minutes. Meantime every symptom grew worse. At 3 o'clock I injected subcutaneously a solution containing 1-40th grain of the muriate of atropia. Five minutes afterwards the pulse had fallen to 144. At 3.10 it was 136. Ten minutes afterward it had risen to 180, but remained of about the same force. Breathing was much less labored; anæsthesia still complete; pupil commencing to dilate. Heat of head and surface had been much lessened by the cold applications, which had been kept up continuously. I may as well remark that these applications were continued until 5 o'clock. For half an hour the pulse did not materially change. Breathing still improved. At 4 the pulse notably weakened, and following that hour until 4.40 it was at times so rapid and irregular that it could not be accurately counted. During this time efforts to administer whiskey failed as before. Breathing became again extremely difficult, being obstructed by the tongue falling back over the larynx. Patient had to be held on the side to permit adequate respiration. Another vomiting fit occurring nearly suffocated him, suspending breathing until the countenance became cyanotic. This ominous hue persisting, and death being imminent, I again injected atropia gr. 1-60. The effect upon the pulse was magical. Within ten minutes it fell from the condition I have spoken of to 152, with more force than it had hitherto exhibited. Other symptoms of marked improvement accompanied. Slight motions of the hands and feet were observed; the dorsal decubitus permitted proper respiration, and the patient occasionally moaned, as if in pain. The eyes, however, remained fixed and insensible, the pupil dilated slightly above its normal size. At 4.55 there appeared a slight weakening of the pulse, the rate remaining the same. At 5 o'clock I injected hypodermically ʒss of whiskey. Found the anæsthesia had been so far removed that patient tried to escape the needle of the syringe, and the irritation of the whiskey drew from him frequent moans, as of pain. Pulse recovered under this and fell to 144. At 5.10 I injected same amount of whiskey, the pulse falling to 132; at 5.20 another similar quantity; at 5.30 still another, pulse 120. The introduction of the needle always gave rise to great uneasiness, moaning,

and an apparently voluntary effort to avoid it. The same followed brisk frictions of the hands and feet with whiskey, instituted to restore warmth and capillary circulation. Patient was now conveyed to his house and placed in bed, and as there was a steady improvement in all his symptoms, no further hypodermic injections were administered until 6 o'clock, when ʒss of whiskey was again given. At 7 the pulse was 120, tolerably full; breathing regular and comparatively easy, and temperature normal to the touch. Patient was still unconscious, but, as I was obliged to go into the country, I did not hesitate to leave him for a few hours. Returning at 9.30 I found the pulse still at 120, but strong and becoming hard. Heat of head and skin rather excessive, consequently the cold applications were resumed. The bromide of potassium was also ordered to be given in five grain doses every two hours There was no notable change in the cerebral symptoms, although I fancied an effort on the part of the patient to follow my movements as I was occupied about his bed. At 10 o'clock I left him, instructing the nurses to call me should any worse symptoms arise, but feeling that the chances for and against him preponderated on the side of recovery. I learned early the next morning, while on my way to visit him, of his death at 5 o'clock, following a severe vomiting fit and hemorrhage from the stomach. The amount of blood lost must have been excessive, as it was said to have saturated his clothing and run across the floor.

Remarks.—A predisposing cause of this attack, and one which undoubtedly determined largely its phenomena, was the cachectic condition which the patient was in, the consequence of an eight weeks' run of typhoid fever, from which he had scarcely convalesced.

In estimating the part played by the atropia in the treatment of this case, I trust will be borne in mind the extreme difficulty, nay, the impossibility of conveying, in a clinical history, a just conception of those manifold changes in symptoms resulting from either the remedial means employed, or from the varying progress of the attack. They were such as have to be witnessed to be fully appreciated. The rapid, weak, and rapidly weakening pulse; the labored and choking respiration; the cyanotic countenance; the almost obliterated pupil, and the total anæsthesia—all betokened an overwhelming collapse of sensory, motor, and vaso-motor innervation, under which it momentarily seemed that the patient must succumb. The temporary relief from this truly desperate condition following the atropia injection was so prompt as to claim the undeniable relation of effect to cause. The effects of the second injection were still more marked, appearing wonderful to all who witnessed them. To me they were so striking that I can but deplore my poverty of language to adequately portray them. The question will naturally be asked, why, after observing the prompt benefit following the first injection, was not another resorted to before the patient's pulse was permitted

to sink to the low ebb described? Frankly, without the guide of sufficient previous experience, I was afraid to repeat what I could not fully persuade myself might not be as potent for harm in its secondary effects, as it had been active for good in its primary. Again, it may be asked, why distrust the atropia, after the second unmistakable exhibition of its powers and resort to whiskey? Every cautious physician will understand and respect my reluctance to use so potent an instrument when a safer one might be made to answer in its place. Had the whiskey failed to sustain well the effect of the atropia, I should have used the latter, in minute quantities, as long and as frequently as necessary. I believe now that had I at first injected, in place of the single larger quantity, 1-70th to 1-60th of a grain every twenty minutes, for a time, I could have sustained the improved cardiac impulse throughout.

Respecting the final fatal termination, it may be said to have been an accident essentially independent of the characteristic morbid phenomena of the affection, an added lesion which could not have been anticipated nor prevented. A return of the vomiting while the patient's condition was otherwise promising, undoubtedly led to the laceration of some gastric vessel, the hemorrhage proving almost immediately fatal. Such an accident could no more detract from the results of the previous treatment than could an accidental shooting or a stroke of lightning. But for this complication a recovery might have been reasonably expected.—*Amer. Journ. Med. Sciences.*

Quinia as an Oxytocic. By Wm. R. D. Blackwood, M.D., of Philadelphia.

During the last five years a considerable amount of controversy has existed in our various journals, with reference to the action of salts of *quinia* upon the uterus during pregnancy. The weight of opinion appears to lean toward the side of those who deny that any impression is apparent, so far as any tendency to abortive action is concerned. During my residence in a part of the country where intermittent fever abounded, I had occasion to note the effect of the sulphate of quinia on three ladies whom I attended during the time of their pregnancy, each of whom also suffered from ordinary intermittent fever.

The first case was that of a lady who was then pregnant with her fourth child. She had never been affected with any uterine trouble before coming under my care, and her previous confinements were normal. Upon being consulted by her when she was attacked by an intermittent of quotidian type, I placed her upon the usual treatment adopted by me in similar cases, viz., five grains of mass hydrarg. at bedtime; followed next morning by a saline laxative. Sulphate of quinia was then given in doses

of three grains every four hours, but no decided improvement being apparent on the third day, I increased the amount of quinia to five grains at each dose, and after twenty grains had been taken during the fourth day, decided signs of labor presented themselves. She was in her eighth month at the time, and I at once took measures towards repressing the uterine contractions, but nevertheless continued the quinia in smaller doses. In spite of all my efforts, labor set in fully, and in seven hours after the premonitory symptoms appeared, I delivered her of a boy weighing four pounds. She had no trouble during her lying in, but the child lived only a week. Her intermittent returned in two weeks after her delivery, and was easily cured.

The second case was a lady in the same neighborhood, in the seventh month of her pregnancy. She also contracted quotidian intermittent fever, and was treated in a similar manner to the first case reported. Three grains every four hours failed to cut short her chills, but in four days after commencing the quinia she was thrown into labor, like the first case, and I delivered her of a still-born child, after a labor of thirteen hours. She had previously given birth to four children, the first a boy, the second twins, both girls, and the fourth a girl also. The still-born infant was a boy, fairly developed. After her confinement I had no difficulty in controlling her fever. Her previous pregnancies presented nothing peculiar, nor had she ever suffered from any form of uterine complaint.

In the third instance, the lady presented a history similar to the two first; perfect health until the accession of the intermittent fever (also quotidian). The chills were not controlled to any decided degree, but labor set in after *thirty* grains of quinia sulphate were taken. The means taken to check the advance of her labor were futile, and in ten hours she was delivered of an infant, certainly of not more than a seven months' pregnancy, which agreed with her computation. As each of these ladies had histories showing unusually good health before contracting the intermittent fever, and as each of them had evidenced at no previous time any tendency to abort, it is fair to presume that either the fever under which they labored, or the means employed to check that fever, had some influence in inducing their premature labors. Each of them unhesitatingly ascribed their early labor to the quinia given for their relief. The small amount of blue mass (five grains), and the equally harmless Rochelle salt taken on the succeeding morning, produced only a gentle laxative impression, and in none of the cases did any unpleasant or undesirable symptom appear until the system was more or less influenced by the quinia which had been administered. Neither could I ascribe to the intermittent itself the blame, as the sequel will show.

Without entering into a lengthened history of each case, it will be sufficient for my purpose to state that within the succeeding two years I attended the same ladies for intermittent

fever again, and in each case pregnancy existed at the time. Having seen one other case in the practice of a professional friend in the interval, which resembled, in many respects, those referred to above (premature labor following the use of quinia), I determined to employ other measures in dealing with the complication which had so greatly troubled me before. I brought each of the cases fully under the influence of arsenic, by the employment of Fowler's solution, and with satisfactory result. Although a relapse occurred in two of them, the same means again proved effectual, and but little trouble was experienced in maintaining a state of perfect health in each case. The labors were normal, but in order to test the effect which quinia might have upon cases during the parturient act, I administered to each patient, when in the expulsive stage, five grains of sulphate of quinia. The effect was, in each instance, decided and prompt, as much so in the particular patients under consideration, as ergot ever has been in my hands in any other case. The contraction of the uterus was firmer when it took place, and the intervals between the pains were notably shortened. One of the ladies, the wife of an officer connected with the troops with whom I was then serving, came under my hands a third time in her confinement. Having left the post at which she contracted the intermittent fever, she was not again troubled with that disease. However, I experimented in her case, with a view to satisfy myself as to the effect which quinia seemed to have upon her, and when the expulsive stage of labor had fully set in, I gave her, at first, thirty drops of a reliable fluid extract of ergot, repeating the dose in fifteen minutes. Her pains were certainly intensified by the drug, and after waiting a short time, probably half an hour, I gave her five grains of sulphate of quinia dissolved in water by the aid of 10 drops of aromatic sulphuric acid. The effect of the medicine was plainly apparent to both a medical friend and myself, and we fully agreed in attributing a much greater improvement in the character of the pains to the quinia than we did to the ergot. The labor was tedious, and having repeated the dose of quinia, we found no reason to doubt as to which agent was of greater energy.

I habitually take with me, to the lying in chamber, a small vial of Squibb's fluid extract of ergot, together with my forceps, and I never regret the trouble of carrying them along, whether they prove to be necessary or not, for often time is lost by the distance which has to be traversed in order to get your instruments, or those of a friend, or to wake up a sleepy druggist, and after all to get an unreliable article of ergot, when you need the best. Frequently, of late, I have also had with these a small vial of the solution of quinia, for the purpose of further experiment as to its power over the uterine contraction. Although in many instances it is difficult to determine the positive action of this agent, I am nevertheless led to ascribe to it, in other instances, an undoubted good result. So far as my own observa-

tion is concerned, the number of cases is doubtless greatly too small to be of much use, yet in the moderate way in which I have given it, no harm can result from its administration, and sometimes it proved decidedly beneficial. As was stated at the beginning of this paper, the weight of opinion is adverse to the claim of quinia in promoting delivery, but might not further investigation be advantageous in throwing more light upon the question as to whether quinia is or is not an oxytocic?—*Medical and Surgical Reporter.*

The Management of Delirium. By J. Milner Fothergill, M.D.

There are few more complex problems put before the practitioner than how to conduct the treatment of a case of delirium. Not rarely the problem is brought somewhat abruptly before his notice. Whether so suddenly presented to him, or its advent has been foreseen for some days, it is always accompanied by points of difficulty. In this respect, each case differs somewhat from every other case, and there are peculiarities in each and every one In order, then, to meet such cases with a fair attention to their needs, the first thing requisite is a pretty clear comprehension of delirium as a whole. The next thing is to supplement this general knowledge by attention to the exigencies of each individual case. The latter, it is obvious, is a matter which must rest on the knowledge and good sense of the practitioner, and his allies, the nurse and the friends of the patient. Little can be done here. The first, however, is a matter which may well engage our attention.

Delirium manifests itself under various circumstances. It is not by any means a matter of indifference what these circumstances are, as giving indications for treatment. Delirium may be induced by sthenic conditions, as when it is causally associated with meningitis. It may mark exhaustion of the cerebral cells, together with general adynamy, as in delirium tremens. It is obvious that the treatment of delirium under these different circumstances must be varied according to the indications. Then, again, delirium is very common in the pyretic affections of childhood, not only in the more advanced stages, but in the commencement of the ailment. The younger the child, and the more emotional its temperament, the more readily are the evidences of disturbed cerebral action evoked. There is, however, as much difference betwixt the delirium of the commencement of a febrile attack in a child and that of advanced typhoid conditions in the adult, as there is betwixt the delirium of meningitis and delirium tremens. When delirium comes on in the latter stages of continued fever, it is usually accompanied by other evidences of an impure condition of the blood, as subsultus tendinum and muscular prostration. When it shows

itself in the exanthemata, there is not usually such a waste-laden condition of blood as to occasion those marked evidences of its effect upon the cerebral centres, and the delirium is probably associated with the effects of the fever poison.

We may now proceed to consider the changes which are the physical side of the question. In delirium, there is a modification of the functional activity of the cerebral cells. In meningitis there is an active and violent condition of delirium, together with strong muscular movements. There would seem to be an exalted condition of the cells of the grey matter to such an extent as to disturb the equilibrium ordinarily existing, and so to institute an escape from the control of the will. In delirium tremens, the condition is more commonly that of exhaustion of the cerebral cells from the sustained alcoholic stimulation to which they have been subjected. There is that excitability and irritability which are the precursors of exhaustion in nerve matter. In the case of children, a slight rise of temperature is commonly sufficient to institute an irregular action in the cerebral cells, which becomes manifest in incoherence and rambling. The delirium of the typhoid condition, and especially when associated with specific fever, is the result of excited cerebral action, leading to exhaustion, combined with an impure state of the blood. In the acute delirium of the exanthemata, there would appear to be a certain amount of cerebral hyperæmia, together with the effect of the poison upon the cells of the grey matter.

In all these cases, and doubtless equally in the other less common conditions which furnish delirium, there is an irregular or exalted action of certain cerebral centres, together with a diminution of the controlling forces ordinarily in action. That balance and harmony which normally exists in all, to a greater or less extent, is disturbed by certain physical conditions, and then irregular cerebral action results. In children, where there is much more mobility than is normal in adults, not only is delirium readily induced, but irregular muscular movements, or convulsions, are common, indicating disturbance in the motor centres. In adults, too, there is a wide difference in their respective proneness to delirium.

The emotional mobility and ready excitability of one person, indicating an unstable equilibrium, contrast with the well-controlled cerebral action in others where reason is rarely and but with difficulty unseated. Much slighter causes of disturbance will excite delirium in the first-named; great and sustained disturbing action is necessary to induce it in the latter. When, however, the brain is kept for some time at a high temperature, and fed by impure blood, its excitability is much increased, and its equilibrium is very apt to be overthrown.

These last are the conditions under which we find delirium usually induced; and it is with delirium so associated that the present consideration chiefly deals.

For the proper comprehension of the measures required for the

management of delirious patients, some review of delirium from its psychological side is clearly indicated. There has been little scientific attention paid to delirium, and there has little attempt been made to comprehend and unravel the workings of the mind in this state. It is common enough for medical men to have delirium when seized with fever, but unfortunately a very large proportion die. Very few care to analyze their sensations or their remembrances of a delirious past, and consequently there is but little in our literature which tells us of the attitude of delirium from the patients' point of view. It is chiefly from the stand-point of the careful clinical observer that we can look at delirium. Experience soon tells us that delirium is not what unprofessional people imagine—namely, an upheaval of hidden thoughts—of thoughts associated with fears and dreads, which then manifest themselves openly, escaping from the weakened control of the ruling centres which ordinarily restrain them from outwardly indicating themselves. All that is carefully hidden does not ordinarily escape in delirious raving; though the more such matter obtrudes itself in the consciousness in health, the more likely it is to reveal itself when the will is laid in abeyance by disease. The evil conscience may keep the prospect of delirium and its revelations before the possessor of a guilty memory; and it is no great evil if it does so. But the material of raving thought is chiefly the objects upon which the mind ordinarily dwells. The bricklayer's mind wanders amidst bricks; the medical man commonly wishes to visit his patients; the soldier's thoughts turn to the grim matters belonging to his avocation. But maidens do not mutter their lovers' names nor men their *liaisons* in preference to anything else. Delirium is but a modification of our ordinary thought; frequently it is but little more than incoherent thought. At other times the imaginative centres may be active, and instead of a day-dream, the fancies are expressed in words. More frequently, however, delirium is immediately excited by disturbed or imperfect impressions coming from the periphery, or from some idea occasioned by what is seen or heard. The unstable brain is easily perturbed; and if the eye sees but imperfectly, or does not recognize readily the object seen, then a flood of erroneous ideas is inaugurated. If the eye falls upon unwonted objects or an unfamiliar aspect, then a direction is at once given to the wandering thought.

Nothing is more common in a delirious patient then a fixed determination to escape from his bed and from the apartment which he occupies. Again and again have fever patients to be strapped to their bed, in order to prevent them from injuring themselves in attempts to get away. No amount of watchfulness, which is feasible, can be given to their uninterrmitting aim of escape. But why do they want to get away? Why does the delirious patient in private practice give so much trouble, and require incessant watching? Because, like the insane patient, he wishes to get home!

When the brain is wandering, and reason no longer rules, the prevailing desire is to get away to familiar scenes and wonted surroundings. The mind craves for its usual associations of ideas as to locality and neighbors. Consequently, when the delirious patient wakens up in the fever ward, there is nothing on which his eye may rest with which it is familiar, or which helps the wandering brain to collect its erratic thoughts. The ruling idea is to get home; and a very natural idea it is. Every sick person craves ardently te be at home amidst relatives and friends; and in delirium the craving commonly takes the direction of an attempt to get away home by immediate escape from the room occupied at the time. It is no matter for surprise that, under such circumstances, the delirious patient will make a desperate resistance to the attempts to restrain him. Not only his sensations, but his ideas, that it is but proper that he should be at home when ill, will urge him to the utmost resistance, and not often to resort to active measures in order to overcome an opposition which seems to him as unreasonable and unjust as it is uncalled for. The feeling prompting the patient is a very proper one; and he is conscious enough that what he feels to be right is upon his side. Consequently, his struggles only cease with exhaustion, and a wailing cry indicates that the consciousness of powerlessness is as painful as the bitterness of death. When that sound falls upon the ears of near relatives, it adds no little to the painfulness of their position, and heightens a scene already tragic! There is nothing wrong, improper, or perverse, about this persistence in the desire to escape from a strange place, and from those who so cruelly restrain a natural wish. Can it be any matter for surprise that, under such circumstances, a patient will evince a keen dislike as well as distrust of those whom, from his point of view, he has so little reason to love? Neither can it occasion surprise if the desire to leave his room and to get away is unwearying and persistent. The natural desire is sustained by a consciousness that there is nothing but what is proper and creditable in the wish to get away. The room is a hateful prison, and the attendants are heartless jailors, who restrain the sick man from getting away to his home, where he feels it but right that he should be.

In exactly the same way do erroneous impressions arise in a delirious patient's mind even when lying in a wonted chamber, if the aspect of that chamber be altered until the unsteady eye and wandering brain no longer recognize it on awaking. So familiar must all experienced practitioners be with this, that it is somewhat surprising more allusion is not made in our textbooks to the necessity for keeping a fever patient in his own room, and also of retaining as far as possible the aspect of that room unchanged. A personal experience of delirium and its sensations, together with an experience of a near relative in delirium, compel me to write very distinctly about the desir-

ableness of retaining the wonted associations of a sick room, when its inmate is delirious. To the painful experiences of the craving to get home, experienced during delirium, are added the no less painful memories of watching a relative praying to be permitted to return home, and of feeling the powerlessness to accede to the request or to dispel the erroneous impression. After such experience, a strong wish exists to impress upon the minds of others the misery that may ensue from altering the arrangements to meet the necessities of a sick room; to demonstrate what the drawbacks are to such changes; that in spite of the obvious advantages of the change it is undesirable. The impression made upon the mind of the patient that he is imprisoned by hostile attendants is not only exquisitely painful, but it exercises an evil effect upon the progress of the case. A feeling of suspicion and dislike is built up which interferes much with the general understanding which should always exist betwixt patients and their attendants.

It is obvious, then, that if opposition must be offered to a patient's wishes, it should be so conducted as to create no unnecessary impression of restraint, and that if the desires can be combatted by cajolement, it is infinitely better than overt opposition. If the mental attitude of the delirious patient were but better understood, the attendants would be much encouraged to persevere in their attempts to restrain the patient by arguments, and appeals to what is left of reason; and their arguments would be all the more effective if they were directed to the erroneous impressions of the patient, and were calculated to correct the aberrations. If, instead of holding the idea of wild, chaotic, objectiveless, mental wandering, the attendants but thoroughly realized the patient's objects and wishes, how tolerant they would become—how patient in attempting to correct the patient's delusions, and in restraining them if necessary. It would substitute an intelligent tolerance for a feeling of simple necessity for opposition to the patient's wishes. Amidst the advances of modern medicine a better comprehension of the nature of delirium is very desirable.

Copland, in speaking of the treatment of the delirium of continued fever, states that Hildenbrand and Naumann advised that attempts should be made "to rouse the patient's moral sentiments and affections, and to disperse his fugitive and chaotic ideas by recalling former associations and objects of affection or of ambition;" and in one case he found such attempt successful. In other words, it is desirable to dispel the erroneous impressions, not so much by combating them as by trying to substitute other thoughts and other ideas in the place of the diseased ones, and to occupy the foreground of the consciousness by normal associations of ideas. This is more desirable in delirious patients nursed at home than among patients in a hospital, where coercion is simply necessitated; as a sufficient staff to attend to every patient in such place would be impracticable on account

of its cost. In the case of patients at home, however, matters are different, and such plan should always be aimed at. There is something very staggering to the delirious patient in the conception that his near relatives and friends, those, indeed, upon whom he previously thought he could most rely, are prepared to offer the most uncompromising resistance to those wishes, which appear to himself well founded and perfectly proper. There is no difficulty in seeing how very painful such impression must be to a mind wandering, and yet conscious of its bodily feebleness, and how desirable it is that such impressions be avoided.

Having said so much for the psychical treatment of delirium, the question of its physical treatment can be entertained. In order to make this lucid, it may be as well to eliminate two of the various forms of delirium from our inquiry by dismissing them briefly. As to the delirium which marks the onset of febrile conditions in children, it is a mere symptomatic affair, scarcely calling for treatment. Delirium tremens is a complex affair, and to be treated by the indications of each case. If the pulse be full, bounding, and incompressible, a good dose of opium with antimony is indicated in order to procure rest. But if the pulse be small, compressible, and very quick, when sleep is kept off by that irritability which is so associated with commencing exhaustion in nerve structures, full doses of opium are dangerous. The sedative effect of opium upon the nerve centres under these circumstances is such that death has not rarely followed the administration of a medicinal dose of opium or morphia. It is possible that death here results from the effect of the opium upon the ganglia of the heart. Mr. Jones, of Jersey, gave tincture of digitalis in half-ounce doses for such delirium with good effect. Probably in such cases the combination of digitalis with bromide of potassium and some of the vegetable narcotics is indicated.

In the consideration of the delirium associated with sustained pyretic conditions from a therapeutic point of view, it is of the utmost importance to carefully weigh the different factors of the case, and to be guided accordingly. If there be considerable vascular excitement and heat of head, then opiates are inadmissible. Chloral hydrate in fifteen-grain doses, repeated at hourly intervals till a drachm be given, is a much more suitable agent, as it exercises a very decided effect upon the vascular system as well as upon the nerve centres; or bromide of potassium may be exhibited with advantage. One point must be remembered, and it is this: in the typhoid state, when the tongue is covered with a dark fur of dead epithelium cells, a similar layer of effete cells lines the intestinal canal, and medicines given by the mouth are of little avail, because they are not absorbed. Subcutaneous injections of chloral hydrate of a strength of not more than ten grains to the ounce—in order not to cause severe local disturbance, as stronger solutions are prone to do—will often prove of decided service. The application of cold to the head by

means of an indiarubber water pillow, and if necessary a bladderful of cold water placed on the top of the head, is a very useful measure. There is no doubt but that the temperature has much to do with the oncome of delirium, and the intelligence is rarely if ever unaffected when high temperatures are reached. Draughts of cold fluids are also serviceable. The cold pack will restore the intelligence readily, even in severe delirium, when it lowers the temperature markedly; and the rise of the temperature to its previous height is accompanied by a return of the delirium. The lowering of the temperature is an efficient means of affecting the abnormal action of the cerebral centres.

Such are the measures to be resorted to in delirium associated with high temperatures.

At other times a form of delirium is met with after the acute pyretic condition is over. It seems rather a form of cerebral exhaustion than a form of active delirium. Dr. Stokes enumerates three conditions which call for the use of opium in fever: (1) where there is persistent watchfulness; (2) where an inflammatory condition of the brain has existed and been subdued, but delirium or other nervous symptoms still remain; (3) where an excited state of the sensorium exists without heat of scalp or remarkable throbbing of the arteries of the head. To these Copland adds the condition of there being free discharges or unrestrained evacuations.

The use of opium is clearly indicated in all these conditions except the first—when it appears along with much vascular excitement. Here it would be much better to combine the opium with chloral, or to use chloral instead. At the time when Stokes wrote these directions, chloral was undreamt of, and so could not be alluded to.

Well advised and judicious therapeutic measures will greatly aid the moral treatment of delirium.—*The Practitioner.*

Trichina Spiralis and Trichiniasis. Read before Detroit Academy of Medicine by Judson Bradley, M.D., Detroit.

Of all known parasites which infest the human subject, the trichina spiralis is believed to be most fatal to human life. These parasites have without doubt abounded from earliest times, and have been the cause of much suffering and many deaths, where learned diagnosticians have failed to discover the cause, or to understand the nature of the disease. But so far as I have examined their history, I am led to conclude that Tiedeman was the first to discover this peculiar worm, as early as the year 1822, but he did not consider it worth a name. In 1832 Mr. Hilton, of Guy's Hospital, again called attention to this same species of parasite found in the human voluntary muscle. In 1835 Mr. Wormald, Demonstrator of Anatomy at St. Bartholomew's Hos-

pital, sent a portion of human muscle of peculiar appearance to Professor Owen. This portion of muscle presented to the unassisted eye a mouldy appearance, but a section of the muscle under a microscope showed each "mould spot" to be a shuttle-shaped cyst, having in the broadest part a thread-like worm, coiled in two or two and a half spiral coils. From this characteristic spiral coil Prof. Owen derived a name for the new worm, and called it trichina spiralis.

It appears that Mr. Paget, while yet an undergraduate, had read a paper on this same entosoon, a week prior to Prof. Owen's announcement, but so far as recorded he did not suggest a name.

From 1835, when the subject of this paper was dignified by a name, until 1860, trichinæ were often noticed in dissecting room subjects, but were believed to be harmless.

In 1860 Prof. Zenker, of Dresden, had his attention called to a young hospital patient, who died after many weeks of peculiar illness. Her illness was "characterized by debility, fever, sleeplessness, abdominal tenderness, great pain in the muscles and œdema of the lower extremities." Microscopical examination of mucus from the intestines, and of sections of voluntary muscles, showed numerous trichinæ, and by persistent inquiry the professor was able to trace the beginning of the illness to a time when the girl's diet had consisted largely of ham and sausage, portions of which yet remaining were submitted to the microscope, and were found to contain trichinæ in abundance. Prof. Zenker also learned that other servants in the same family had been sick about the same time, and also the butcher who slaughtered the animals, he having eaten of the raw flesh. Hence it was concluded that, although a few trichinæ may do little or no harm, yet a great many of them may be a cause of death, and numerous experiments with flesh containing trichinæ (this young girl's among the rest) proved the conclusion to be correct.

The sickness produced by trichinæ is called "trichina disease," "trichiniasis" or "trichinosis;" more commonly "trichiniasis."

During the years that have elapsed since Prof. Zenker proved the connection of the trichina with a certain form of disease, other and highly corroborative evidence has not been wanting. "Wunderlich has reported four cases among the butchers of one establishment, who were taken ill after eating raw pork." At Planen, in Germany, one case died out of thirty attacked. At Colbe, out of thirty-eight attacked seven persons died. At Heltstadt, in 1863, after a single hotel dinner, one hundred persons out of a sum total of one hundred and three people at dinner were attacked with trichiniasis, and twenty-eight died. All known appliances and treatment of the medical art proved of no avail in these cases. At Hedersleben, in 1865, trichiniasis caused forty deaths in three hundred cases. These cases were traced to trichinous pork, which was eaten underdone.

Fortunately our own country has not suffered from such exten-

sive epidemics of trichiniasis as has Germany, yet since 1864 cases have not been wanting to show how much mischief certain parasites of microscopic size are capable of creating. Dr. Joseph Schnetter, of New York, reports that in 1864 he attended several members of a single family who were taken sick soon after having eaten of raw smoked ham. Dr. Schnetter examined the ham, as did also Prof. Dalton, and found it literally filled with trichinæ, estimated by Prof. Dalton to number as many as 85,000 to the cubic inch of the lean or muscular portions. Soon after Dr. Schnetter, Dr. Voss, also of New York, reports a case which he suspected to be the trichina disease, and to settle the diagnosis cut down upon and removed a portion of the deltoid muscle, which under the microscope showed numerous trichinæ and confirmed his suspicions.

The next cases were reported in the *Buffalo Medical and Surgical Journal*, 1864, by Drs. Krombein and Dingler, German physicians then practicing in Erie County, N. Y. Here then were three distinct sets of cases reported in the United States in a single year.

In 1866, Marion, Iowa, furnished nine cases of trichiniasis in a family of ten persons. The nine ate of raw ham, the tenth only ate of that which had been cooked. Five of these cases died, and in a portion of muscles removed from one of the bodies Prof. Dalton found in a section one-twelfth of an inch square and one-fiftieth of an inch in thickness, 29 trichinæ, or about 20,800 in the cubic inch—a number sufficient, one would suppose, to create the most unendurable irritation and agony.

Since trichiniasis has been admitted as a special and peculiar disease, it has happened not unfrequently that whole communities in cities, towns and hamlets, have been thrown into a violent ferment by the advent of one or more cases of the dreaded disease. At these times active imaginations draw vivid pictures of horrid flesh worms; pork is banished and forbidden the table, and a general nervousness seizes both young and old for the time being, until, in the rapid whirl of events, a new excitement succeeds the old, and trichinæ are remembered only as things of the past.

Since 1835, when Prof. Owen described the trichina spiralis, various observers have carefully studied its habits, and have arrived at a very good knowledge of its natural history. From the statements of the various observers, it appears that the trichina as seen coiled in the muscle is sexually immature; that after ingestion of food rare done that contains individual parasites, the digestive fluids act upon the cells that surround them and set them free. The parasites are able to resist the action of the digestive fluids on themselves, and soon pass from the stomach into the small intestines, where they thrive so well in their new home that in two days' time they become sexually mature. Unlike the tæniæ, the trichinæ are bisexual, and their mode of introduction is viviparous. According to one author, each female

will, in six days after arriving at maturity, produce as many as five hundred thread-like larvæ, which immediately begin their wanderings. (I will mention here, parenthetically, that each young trichina measures about one-thirtieth of an inch in length, while the mature male measures one-eighteenth, and the mature female trichina one-eighth of an inch in length.)

The larvæ pierce the intestines and peritoneum, and do not rest in their movements until they find a congenial place for themselves among the primitive fibres of the more superficial muscles. Why they choose the superficial muscles is not well explained. Here they coil themselves; the connective tissue accommodates itself to the interloper, and makes room for him by giving him a cell, where he remains dormant, or is set free by the process of digestion.

Assuming Prof. Dalton's estimate, and that half the immature trichinæ are set free and reach maturity in any given case, if a man eat but a single meal of raw or underdone meat which contains them, he has within himself a capacity for the reproduction in eight days' time of more than 20,000,000 active parasites for every cubic inch of the meat ingested; and if, as some suppose, the adult worms reproduce more than once, the capacity for infection would be past all ordinary comprehension or belief.

It might here with propriety be asked how long the larvæ of trichinæ may exist in the dormant state before losing their vitality? Such a question cannot be definitely answered, but Virchow states that in one case he found them alive eight years, and in another thirteen and a half years after the infection of the subject.

The pathognomic symptoms produced by these parasites of such microscopic size are not so well marked as to be unmistakable. According to Prof. Hartshorn, of Philadelphia (I quote), "the disease produced by the trichinæ has two distinct stages: 1st, That of the presence of the worm in the alimentary canal and their multiplication there; 2d, That of their migration to and location in the muscles. Of the first period, malaise, vomiting and diarrhœa are the leading symptoms. Of the second, fever resembling typhoid, severe pains, with stiffness of the muscles and prostration. As the muscles of the larynx are often attacked, hoarseness is a common symptom. The complication of pneumonia is not infrequent. The first stage above mentioned lasts about a week or less; the second may terminate fatally within six days, but usually has a duration of from two to four or five weeks."—*Essentials Prin. and Prac. of Med.*

Hitherto treatment has been almost or altogether nugatory. It is true that a majority of the patients recover, but the recovery seems to be owing to a good fortune that but few of the immature parasites have been ingested, and to a vigorous constitution that is able to endure a great degree of irritation and not succumb to its effects. The percentage of deaths in 480

cases was 17.08, while those that have recovered have convalesced very slowly.

Of the two cases of trichiniasis that were brought prominently to notice in this city during the last winter, one—the husband—died, while the wife recovered. The woman stated that she ate but little of the meat that caused the mischief in the man.

These patients when I saw them were at Harper Hospital, very much emaciated, having the appearance of tuberculous patients more markedly than typhoid fever patients usually have; abdomen flat; an intense burning sensation at the epigastrium, and consequent thirst. Sleepless nights and restless days made life a burden. An intermittent diarrhœa, still more troublesome than the other symptoms, coupled with almost complete anorexia, soon carried off the man, as before mentioned.

In reply to a question, Dr. Felch (at that time one of the house physicians at Harper's) stated that he should have put down the cases as "chronic diarrhœa," if he had not been assured by the physician who sent the patients to the hospital that they were infected with trichinæ.

After the man died, at the suggestion of Prof. L. Connor, large doses of tinct. chloride of iron and sulphate of quinine were given to the woman at stated intervals, whereupon she almost immediately began to mend, and ultimately recovered her former health, after an illness of nearly five months.

If I were to suggest a line of treatment for trichiniasis, I should, if the patient is seen early and the diagnosis sure, first evacuate the bowels most thoroughly, to remove as many as possible of the mature worms from the alimentary canal. Then I would, by all means, during the second stage support the patient on easily assimilated food, and I would give as much of the tinct. chloride of iron and sulphate of quinine as the patient could bear, until the symptoms showed that the patient was out of danger and convalescent.

My faith in capsules of naphtha or turpentine, in gamboge or chloroform, or any of the usual parasiticides, would not induce me to employ them to kill or render innocuous the trichinæ, except so far as they act upon the bowels as cathartics, and carry out of them any of the worms that happen to fasten there and are in the act of reproduction.

In conclusion, I would urge that prevention is better and far easier than cure in the disease that I have had under consideration. Therefore, eat only thoroughly cooked pork, whether it has been smoked or not, and you will avoid all complications of trichinæ.—*Detroit Review of Medicine.*

Treatment of Ammoniacal Cystitis by Benzoic Acid.

MM. Gosselin and A. Robin have shown in a previous memoir

that human urine, when ammoniacal, is toxic in a high degree, and that it plays an important part in the production of those accidents which occasionally supervene upon operations in the genito-urinary passages.

From this idea to that of prevention is but a step, and it is this which MM. Gosselin and Robin endeavor to elucidate in the *Archives de Médecine.* Their conclusions are as follows:

As the ammoniacal condition enters to a certain extent into the production of those accidents which supervene upon operations on the urinary passages, it is very desirable that it should be controlled, prevented, or diminished. Benzoic acid, the gums containing it, and probably also other vegetable products (salicine, cinnamic acid, etc.), conduce to this result. The hippuric acid thus created acts in various ways: *a*, in forming hippurate of ammonium, less toxic than the carbonate of ammonium; *b*, in retarding the decomposition of the urine, and consequently the production of carbonate of ammonium; *c*, in preventing the formation of insoluble phosphatic deposits, which are one cause of cystitis and may become the starting-point of vesical calculi.

The administration of benzoic acid is to be recommended in the case of patients suffering from ammoniacal purulent cystitis, and particularly in the case of those who have submitted to operations on the genito-urinary passages.—*L'Abeille Méd.*, November 23, 1874.

The Transportation and Inoculation of the Virus of Malignant Pustule and of Other Affections by Flies.

The idea that malignant pustule is the result of the bite of a fly is very wide spread, not only in the community at large, but even in the minds of a great number of distinguished physicians and savants, although no proof perfectly demonstrated has as yet been given of the fact. One of these latter, M. Davaine, has even advanced the opinion that flies are the *only agents* in the propagation of malignant pustule among cattle. He has been opposed by a very serious objection, to-wit, that *charbon* shows itself among animals in all seasons, even during the most rigorous days of winter, when it is impossible to ascertain either in the stables or out of doors the existence of a single fly. This objection, however, does not destroy the hypothesis of the possible transportation and inoculation of the virus of malignant pustule during the hot season by flies. In support of this hypothesis, MM. Raimbert and Davaine have brought forward, each on his own side, the results of experiment, by means of which they believe to have shown, the former, that those flies which do not bite and which feed upon cadavera are those which communicate malignant pustule by depositing virulent liquids on the skin, which, although intact, absorbs them; the latter, that these same flies are unable to transmit malignant pustule except

on wounds, but that on the contrary the biting flies are, *probably*, the ordinary agents of the transmission of charbon. The experiments of these two savants, in spite of the difference of their conclusions, are identically the same: both made use exclusively of the blue meat fly (musca vomitoria of Linnæus) which does not bite, causing it to drink the blood from a case of malignant pustule placed in a saucer, and immediately afterward making inoculations, which were followed by success upon cobayes and rabbits, with the blood contained in the abdomen of these insects or with their probosces, their feet or wings. These experiments, in spite of the contrary opinion of their authors, prove only one thing: that the blue fly may serve as a receptacle for the blood of malignant pustule, as many other objects may; in regard to its being an agent of transmission of the disease *without the aid of a lancet* the experiments in question prove nothing at all. The knowledge of the habits of this fly shows, on the contrary, that it cannot play this part ascribed to it; *it never frequents wounded, sick or healthy living animals*; it obeys in this the general law which all flies of cadavera follow, the counterpart of the law followed by all biting flies, which live upon fresh living blood and never touch dead bodies nor even sick animals, at least those that are seriously sick (we can even show that the larvæ of wounds are not of the same species as those of cadavera).

Observation has recently proved to us that some flies belonging to this latter category, those which suck the blood of living animals by means of a penetrating proboscis, attack, equally, animals gravely diseased and animals in good health, and can, in consequence, actually become the agents of transmission of virulent diseases.

In the camp of Gravelle, where all the artillery of Vincennes, man and horse, sojourned for six weeks in the months of August and September of that year, we surprised some *Stomaxes* regaling themselves upon the pathological fluids of a gangrenous erysipelas (of which a horse was the subject upon his leg in consequence of a severe kick) with as much avidity as those feeding on the warm blood of the neighboring healthy horses. We obtained voluminous erethemata by inoculating horses in good health with the probosces of the flies thus engaged, probosces which contained, as we ascertained by microscopic examination, a fluid swarming with the bacteria of putrid fermentation and entirely similar to that which flowed from the diseased limb. The direct inoculation of the erysipelatous fluid itself produced the same result. The counter experiments by puncture with a clean bistoury were followed by no effect.

We have made the same observation with a little fly scarcely 3 millimetres long, which belongs to the genus *semulia* and to the family *Tipulares florales*. This fly known under the name of *simulie tachetée*, perfectly innocuos under ordinary circumstances, as we have proved over and over again, which bites after the manner of gnats, and is sometimes present in such numbers as

to form a dense cloud around the cattle, was accused in 1863 of being the cause, *by its own proper poison*, of a murderous epizootic which raged in the canton de Condrieux, near Lyons; the veterinary professor, M. Tisserant, appointed to make an inquiry into the causes of the disease and who gave vent to this idea, only saw one side of the truth; the fly indeed aided in the development of the epizootic, but it was only in the character of carrier of the virus, which was the essential cause; in fact the disease was nothing more than malignant pustule, as is proved by the description of the autopsies of the animals that succumbed, and we are astonished that its nature was easily overlooked.

It is certainly in the same manner that the famous fly of Abyssynia acts, the Tsetse (genus Glossina, altogether akin to our Stomoxis and our Hematobia) which slew forty-three of Livingston's oxen by a few bites, which bites were utterly harmless to the children of the notorious traveller, to the sucking calves, to the goats, asses, and to the wild animals, and were fatal only to the horses, oxen, sheep and dogs!

Concluding from the preceding experiments and observations we regard the fact at the present day as settled that certain flies, which suck blood and are possessed of a rigid penetrating proboscis, such as the *stomoxes*, the *simulæ*, the *glossinæ*, may be, under certain circumstances, the agents of transmission of certain virulent diseases, among others malignant pustule.—*Clinic* (translated from *La France Médicale*, Dec. 16, 1874.

A Remedy for Toothache.

The following recipe I have prescribed for toothache, and have never found it fail. Should you deem it worthy reproduction in the *Reporter*, please insert it.

℞. Chloral hydrat., - ℥j;
Aqua pura, - - f℥ss;
Misce, et adde,
Tinct. aconit. rad (*Flem.*) ℳxv;
Chloroform pura,
Ether sulph., - - aa ℳxx;
Sp. vini rect., - - f℥ss —M.

Ft. mistura.

Signa. Rub the gums with a little of the mixture; put a few drops upon cotton, and insert in any cavity of the tooth. Also, take the following dose, on sugar: for an adult, fifteen to thirty drops; for a child, two to ten drops, according to age. The above doses can be repeated quite frequently, if necessary.

The remedy suggested by my late college friend, Dr. James Sawyer, may also prove very beneficial, if the dose is lessened, and repeated at shorter intervals of time. Anything to relieve the pain of toothache is very frequently tried; for you are aware that there never was yet the philosopher that could endure the

toothache patiently, however at his ease he talked like the gods.—Dr. C. C. Cranmer, in *Medical and Surgical Reporter.*

In Asthma.

℞. Ext. belladonnæ,
Ext. opii, aa $3\frac{3}{4}$ grs.—M.

Ft. pil. no. x.

Sig.—One in the evening, to be repeated if necessary during the night.—*Philadelphia Med. Times.*

Syrupus Asari Canadensis Comp (*Compound Syrup of Canada Snake-Root*).

Take of—Canada snake-root one troy ounce;
Alcohol, three fluid ounces;
Water, six fluid ounces.

Mix, and digest for twelve hours, strain, pack the root into a percolator, pour the tincture upon it, return the first portions that percolate to the instrument till it passes clear; pass sufficient water through the percolator to make the tincture measure nine fluid ounces; add to the tincture one avoirdupois pound of sugar, and dissolve without heat. To this add

Powdered cochineal, ten grains;
Carbonate of potassium, twenty grains;
Wine of ipecac, four fluid drachms.

Mix thoroughly; strain.

In reply to numerous letters of inquiry respecting the compound syrup of asarum mentioned by Dr. S. S. Bond in his article on the treatment of "Ascaris Oxyuris," page 818, *Phila. Med. Times*, September 26, 1874, I send the formula for publication, disclaiming any intention to have it appear as original with me. This preparation has been in use in this city for twelve or fifteen years as a remedy in whooping-cough, and the above formula is copied from page 21 of "Non-Officinal Formulæ in Local Use, compiled and published by the Joint Committee of the Medical and Pharmaceutical Associations of the District of Columbia, Washington, 1867."

The discovery of the anthelmintic properties of this syrup was quite accidental. About three years ago the daughter of Mr. F., then four years old, was attacked with vaginitis and pruritus, which was found to be due to migration of ascarides to the vagina. He prescribed the usual remedies, bitter injections, etc., with considerable relief, but the symptoms would return in about a week after treatment was discontinued. The child had suffered more or less for over a year, when Professor Johnson Eliot, M.D. was called on to prescribe for whooping-cough, and ordered the comp. syrup asari in teaspoonful doses three or four

times daily. A few days afterwards the child passed seat-worms in large quantities, either alone or agglutinated together into balls as large as a hazle-nut. The father consulted me as to the probable cause of their sudden destruction. Remembering that I had heard anthelmintic properties ascribed to the European asarum, I was led to suppose that our plant might possess similar properties, and commenced a series of experiments on such persons as applied in my store "for something for pin-worms," with such flattering results that I requested my medical friends to experiment also. Drs. Bond and Duncan seem to be the only ones who have experimented to any extent, and both with excellent success. Dr. Bond's article you have published; I will give Dr. Duncan's experience in a few words. He had but one patient, a little girl who suffered from vaginitis due to migration. He used the syr. asari comp. internally, and a decoction of asarum as injections to both vagina and rectum, and in two days all trouble ceased. This was six months ago; there has been no return of symptoms since.

My own method of administering the medicine differs somewhat from the others. I mix equal parts of comp. syr. asarum and fluid ext. senna, and order a desertspoonful for a child, or a tablespoonful for an adult, to be taken four times daily, and at bed-time a suppository consisting of one or two drops of oleoresin of asarum* with cacao butter q s. Three or four days' treatment usually effects a cure. I furnish the above facts that the value of the medicine may be more fully tested by the profession in general.—(Dr. J. Schaffert, Pharmacist, Washington City, D. C., in *Philadelphia Medical Times.*)

Toothache Drops.

Dr. Q. C. Smith praises the following most highly (*London Medical Record*): Take of carbolic acid, saturated solution, chloral hydrate, saturated solution, paregoric, fluid extract of aconite, of each an ounce; of oil of peppermint half an ounce; saturate the pledget of cotton or a piece of sponge, and tightly pack in the cavity.—*Philadelphia Medical Times.*

PROCEEDINGS OF THE NEW ORLEANS MEDICAL AND SURGICAL ASSOCIATION.

Some observations on the Induction of Labor, particularly in Connection with Puerperal Convulsions. By Joseph Holt, M.D.

Gentlemen:—I do not propose to enter into a detailed consider-

*The oleoresin asari is prepared by exhausting powdered asarum with sulphuric ether, and evaporating spontaneously.

ation of the induction of labor, nor to announce to you any fact with which, taken singly, you are not already acquainted; nor is it my purpose to discuss the question of puerperal convulsion, as to its ætiology and pathology. My object is this: first, having directed your attention to the method approved by my judgment strengthened by some clinical observation, as the one offering the surest, the speediest, the least painful, and, by far, the safest means whereby parturition may be induced, and pregnancy terminated, I will then submit to you a definite plan, which it is proposed shall serve for our guidance when we are suddenly confronted by at least one of the most perplexing and intricate problems we shall have thrust upon us, demanding, too, an immediate solution, during the whole course of our professional lives. Gentlemen, I declare emphatically, believing that my assertion will be endorsed by our surgeons present, as well as by our general practitioners, that within the whole range of surgical, indeed, of professional experience, a case of puerperal convulsions, in the frightfulness of its aspect, in the clamorous uproar occasioned by the terror it arouses in the anxious lookers-on, in all that is amazing in horror, is utterly without a parallel. Even when prepared for the emergency, the physician himself shrinks for the moment appalled before such a hideous spectacle, though it be enacted in the person of a poverty-stricken, obscure negress. Picture one's self, then, unprepared, without a prearranged plan, ushered into the presence of a delicate lady, a near relative, or an influential constituent, and she in a puerperal convulsion, complicated by the presence of the ovum in utero, with no sign of approaching labor; and having thus pictured one's self, you will look upon the most helpless and pitiable object ever presented in the person of a physician!

First, to avoid confusion or future explanation, let us agree concerning the signification of the terms used in the title of this paper. The term, "induction of labor," is generic in its scope, and means the deliberate cutting short of pregnancy by provoking parturition, without reference to the time when accomplished. This general term is subdivided into two lesser, having a special relation to time, induced abortion, and induced premature labor.

By common consent, writers have agreed upon the period of viability as the time, before which parturition is called abortion, and after which it is called premature labor. This is, for the most part, arbitrary, and is practically objectionable. In the first place, there is no such time in existence when we can say—"here is the limit of non-viability. Before this the chances are, the child born cannot survive; after this, the chances are it can survive!" Many circumstances render this impossible. Our reckoning of time is exceedingly liable to be incorrect. The very best obstetrician may make a miscount of fifteen days, or a month. The evidence furnished is purely commemorative and circumstantial, and our calculation one of probability and conjecture. Besides, authors are by no means agreed as to *this time*

of earliest viability; some placing it at six and a half months, which is, excepting a few singular cases, far too early; others, as Cazeaux, at seven months complete, or the beginning of the eighth. Taking the mean of the several times stated by our best authorities, we shall find about the middle of the seventh month to be the earliest period at which we may reasonably hope for a child born, to survive. But, as an actual fact, so far from there being a *line* in time between abortion and premature delivery, there is a broad period of six weeks, or even two months, in which a child being born, its survival is extremely doubtful. Finally, gentlemen, and of more importance than all the rest, this question of the precise time of viability is of much less practical use than one would suppose, considering the amount of literature on the subject. For myself, I do not regard it of any value whatever, since in practice the question of inducing labor is either one in which there is no option as to the time, because we are driven to act immediately, as in dangerous hemorrhage, puerperal convulsions, and the like, or we are free to choose our own time, as in certain degrees of pelvic narrowing, making it advisable to anticipate labor at term.

In any case not immediately pressing, it is the duty of the practitioner to approach as near full term as the nature of the difficulty will allow. He makes no reference whatever to this period of viability, but holds the child in utero, if we may so express it, as long as possible, yet to accomplish the good he expects to derive from induced labor.

Parturition occurring in the earlier months of gestation possesses many features peculiar to itself; and in addition, the class of dangers attending it, the plan to be pursued and appliances used in its induction or general management, are all so different from those of labor in the later months, as to afford a clinical distinction between induced abortion and induced premature labor.

There is a period of time, from about the first to the middle of the sixth month, when parturition loses nearly all the characteristics of miscarriage, and assumes the type of labor at term. Before this period the method of inducing labor belongs to the subject of abortion, and therefore, in speaking this evening of the induction of premature labor, we shall include all labors induced after the fifth month.

And now, let us agree as to what we mean by puerperal convulsions.

It matters not what variety of opinion may here exist as to the essential nature of puerperal eclampsia; the convulsions themselves are unmistakable, and their tendency to the speedy destruction of the mother, and therefore of the child, if *in utero*, whether viable or not viable, is a fact of which we may rest absolutely assured. Upon this single point, we may pile up in evidence the accumulated experience of all the authors who have

ever written upon, or physicians who have ever had to do with, puerperal convulsions.

When we find associated with pregnancy, and not having previously existed, a condition of hydræmia, manifesting itself in a disposition to general anasarca, with that waxy appearance of the skin peculiar to Bright's disease; when there is evidence of acute renal hyperæmia, or even of acute nephritis, furnished by albumen, tube-casts, and blood disks, one or all found in the urine, and this secretion diminished; when, in such a patient we find nervous symptoms indicating a profound disturbance of the sensorium and sympathetic system, such as intense piercing headache, unusual drowsiness, flashes of light before the eyes, a dark spot with a halo suddenly appearing on any point or small object looked at attentively, nausea, and cardiac disturbance, a general numbness, excessive nervousness, with a sense of some impending calamity—this woman is on the verge of puerperal convulsion.

We do not expect to find all of these forewarnings associated in every case, but there is usually to be found—if not always—a sufficient number to apprise us of danger, if only looked for and rightly interpreted.

And finally, gentlemen, when a woman in this condition is seized with general convulsions, ushered in often by a peculiar outcry, accompanied with a total loss of consciousness, the convulsions continuing a minute, or at most a few minutes—the time is really short, though it seems interminable—and succeeded by the quiet of coma, the fit closely resembling epilepsy, this woman has had a puerperal convulsion. It was not a fit of epilepsy, nor a stroke of apoplexy, nor hysteria, nor anything other than true puerperal eclampsia associated with and dependent upon pregnancy.

To have seen one is equivalent to having seen many, inasmuch as one is the image of all others, varying only in intensity. Nor is it likely that, having seen one we, shall err in diagnosis seeing another; such obtuseness in a physician can hardly be imagined. A correct diagnosis is literally seared into us.

Of one thing we may feel perfectly assured, if we see one puerperal convulsion, the woman not having been delivered, we have but to wait a very little while when we shall see another, and still another, and so on unto death, unless we bestir ourselves and relieve her.

The signification of terms having now been agreed upon—only for the sake of the argument, if you differ with me—let us consider the induction of premature labor by that method which offers itself as the simplest, the easiest, speediest, least painful, and, above all, as the safest.

In this little box is our entire equipment. I will name the instruments in the order in which we are likely to use them. For the sake of some present, who may not be familiar with the

manner of applying them, I shall enter more into detail on this point than would otherwise be necessary.

Let us bear in mind that in bringing on parturition, we endeavor to make it imitate as closely as possible a natural labor, in its advent, and in the progress of its several stages.

First I show an elastic bougie of medium size. We remember that a natural labor is ushered in by obscure uterine contractions, occurring at long intervals, feeble at first, but increasing in frequency and in strength. Coincident with these initiatory pains, and influenced by them, the cervix and genital parts undergo certain preparatory changes, facilitating the later stages of labor.

When we wish to induce labor, and can appoint our own time, we pass this bougie into the uterus, making it insinuate itself between the membranes and the womb, and passing up gently a distance of six or eight inches. By using a bougie which is quite flexible, it insinuates itself by the readiest course, and in case of contact with the placenta, glides around its base of attachment. And, above all, with such an instrument we are least liable to rupture the membranes—a thing to be avoided. The effect of this instrument, so placed, is to excite gently uterine contraction, and to initiate the true first stage of labor. Dr. Barnes advises this to be done overnight, and the next day at an appointed time and at one sitting to complete delivery.

This instrument is of no use, and had better be forgotten, when we are called upon to act in the face of some imminent danger, when every moment of time is precious to us. We can get along admirably without it.

All the processes of labor tend to the fulfilment of two acts, the consummation of which is parturition: first, opening wide the uterine door, then driving through it the ovum. In order that these shall not be acts of violence a certain physiological preparation, marked by the lubrication of the mucous surfaces, with softening and relaxation of tissues, must coexist with dilatation and expulsion. (Gentlemen, this is school-day territory, but we are bound to travel over it.) With the warm douche we induce this physiological preparation. The manner of using it is perfectly simple. We take a Davidson, or a fountain, or any other syringe wherewith we can direct a stream of water, as warm as we can use it with the assurance that it is not hot enough to injure; we will say as warm as can be borne by the hand comfortably.

I insist upon the water being quite warm; it is the effect of the heat we desire, and not of the water.

The stream need not be thrown with much force, and is always to be directed upon the inner surface of the cervix, the nozzle having been passed through the os externum. We shall rarely meet with difficulty in effecting its entrance after the sixth month of gestation. If we should meet with a very narrow os, direct the stream upon it for a few minutes, and then dilate

with the finger. The point of the syringe having entered, the stream must not be continued upon one, but directed to first one and then another, part of the cervical periphery. This should be continued five, ten, or fifteen minutes, as indicated by circumstances. According to Professor Elliot, the using of the stream inside the cervix, or merely directing it into the upper portion of the vagina, makes the difference between the success or failure of the warm douche.

We must be exceedingly careful that the water forced in shall flow out freely, and shall not subject the womb to any degree of hydraulic pressure. It is in the use of water for its hydraulic power on the one hand, and of water as a simple carrier of heat on the other, which makes the difference between Cohen's and Kiwisch's methods of inducing labor.

The first cannot be condemned in terms sufficient. The second has been condemned with some show of fact, arising from the misuse of the warm douche. To attempt to induce labor by this alone, which is Kiwisch's method, is not only not the best way of accomplishing it, but is altogether uncertain, excessively tedious, and might readily give rise to the congestion of the lower segment of the uterus, and metritis, spoken of by Dr. Barnes, when continued two, three, or ten days.

But that a stream of warm water, directed for a few minutes upon an irritable and rigid os, or used in connection with Barnes' dilators, for the simple purpose of inviting and hastening dilatability, the same to be repeated a time or two, if necessary; that such a procedure is fraught with danger, or that there is any risk whatever attending it, I know it is not true.

After a proper application of the warm douche, it is remarkable to observe how soon an os which has shown no sign of softening becomes patulous, and the mucous surface of the vagina lubricated.

These effects grow out of the afflux of blood to the cervix and vagina, occasioned by the warm water. Having used the warm douche five or ten minutes, we will nearly always be able to use the smallest size Barnes' dilator; if not, however, dilate with the finger until we can.

In applying the dilator, we can generally manage it better by catching the little pocket near its upper end upon the point of the forefinger, covering well with lard, and passing it up and on into the os. Or, having caught the pocket on the end of a small rod, we pass the fingers of the one hand up to the os to guide and manipulate the dilator, which we pass up by means of the rod.

We must endeavor to make the dilator enter well the cervix, so that its constriction shall be quite beyond the external os.

Withdrawing now the rod, and holding the bag steadily in situ, an assistant attaches the syringe, and with our free hand we throw in water until the bag has either attained its maximum of expansion, or has put the parts upon a moderate degree of

stretch. We are to give time for the tissues to yield before the dilator, and must therefore bring its dilating force gradually to bear.

Having run dilator No. 1 up to its limit of expansion, we withdraw it and use the warm douche.

We now apply No. 2, which is best done with the hand, and expand it in stages, gradually, as we did the first.

When the os has opened to the limit of No. 2, we may rupture the membranes if we have a special reason for doing so, as in puerperal convulsions, wherein evacuation of the waters, in addition to hurrying on the labor, exercises a mitigating influence over, and sometimes even checks, the paroxysms.

Unless dilatation is effected with unusual facility, we use another warm douche, and apply dilator No 3, and proceed as heretofore. No. 3 having finished its work, the way is sufficiently open for us to conclude the delivery, or leave it to the efforts of the woman, as we think proper; in either case, our actions should be in accordance with the principles of obstetrics.

In all this business we should make haste slowly, act quickly, but not rashly. The time occupied in dilating will be from one to five hours.

I wish now to direct your especial attention to a measure of the highest practical importance in the induction of premature labor in puerperal convulsions. I refer to the anæsthetic preparation of our patient. Chloroform heads the list, but only in the outset. When called to a case of puerperal convulsions, our first object is to quiet the patient, and with chloroform we effect this sooner than with anything else. Its use should be limited to the emergency of the moment. There are very serious objections to keeping a patient under chloroform during the time of inducing and completing labor. Its effects are evanescent; it requires, therefore, frequent or even constant administration. It cannot be given for such a length of time without danger. It is apt to give rise to serious annoyance and hindrance, as from vomiting, failure of the pulse, delirious tossing, the peculiar wilting of all the powers, marked by a death-like pallor, feeble pulse and breathing; so that we are in danger of being as much alarmed at the effect of our remedy on the one side as of the disease on the other, and in this plight struggle through between Scylla and Charibdis. The plan I wish to present is this: administer chloroform during the time which must elapse until we can bring the patient under the influence of remedies more stable in their anæsthetic effect, and freer of the objections mentioned. The induction of premature labor in puerperal convulsions should not be commenced until we have first prepared our patient by causing this permanent anæsthesia. It is easily and quickly done. The agents are chloral, and some form of opium, Battley's sedative, we will say. A hypodermic injection of morphia, or of morphia with atropine, may accomplish the same end. If the patient is in coma and unable to swallow, give by enema

℥i of chloral in ℥iv of water; and as soon as she is able to take it, give by the mouth twenty grains of chloral with fifteen drops of Battley's sedative in syrup, and wait.

If, at the end of an hour, there is still restlessness, repeat one or both potions, according to the effect of the first, and keep on repeating them until we induce perfect quiet, endeavoring to avoid profound narcosis.

By this course, reflex irritability is so blunted that we may proceed in our work of inducing labor at our leisure, and without occasion for alarm. Momentary restlessness or threatened convulsion may be controlled by a few inspirations of chloroform. By this plan our work is done leisurely, painlessly, and we avoid the danger and unpleasantness likely to complicate the administration of chloroform long continued.

And now, gentlemen, in conclusion, let me state what I conceive to be the only rational plan of managing a case of threatened or of actual puerperal convulsions during gestation. Suppose one of us called to a lady in the seventh month of gestation, and he finds her the subject of recent anasarca; the urine shows albumen, one-half or two-thirds its bulk, with or without the tube casts and blood discs; she is suffering from some form of neuralgia, or drowsiness, nausea, or other symptoms indicating serious nervous disturbance. He should at once put in action the general course of therapeutics prescribed in such cases; cut cups over the loins, mercurial and saline purgation, vapor baths, etc., whatever he will. Having given therapeutics a fair trial of a week, or of a month, suppose the patient has not responded favorably, but on the contrary, has gone from bad to worse; new symptoms, indicating increasing disturbance of the sensorium, have commenced to show themselves; flashes of light, the dark spot with its halo, momentary loss of consciousness, etc., all pointing to an approaching convulsion. What is he to do? He has failed to arrest the progress of the disorder. To wait is simply to invite the calamities of which his patient stands in instant jeopardy—œdema of the lungs, serous effusion upon the brain, eclampsia, one or all. Shall he quietly abide the time until these overwhelm her? Shall he pause to consider the viability or non-viability of a fœtus whose doom is already sealed?

There is but one hope, one last chance to such a patient, and that is in the speedy removal of the prime cause of all the trouble, the ovum.

The duty of the physician is squarely before him—to act at once, and snatch the mother from impending destruction by delivery, whether the child live or die, since the death of the mother means the death of the child.

By all means call a consultation, and try to have it with one competent to advise; but, above all, with one who is above the reproach of meanness, and who will abide consequences with us, come weal or come woe.

Unfortunately, gentlemen, to be called in during the prodro-

mata of puerperal convulsions is the rare exception. In the majority of instances, we are called only when a fit has frightened the people into rushing for all the doctors in the neighborhood. Sometimes, however, they are cool enough to wait for twenty convulsions to occur before calling a physician, as I have known.

Being called to a case of puerperal eclampsia already developed, our course should be absolute. Demand a consultation, if practicable, and insist upon the immediate induction of labor; or, if persistently refused, abandon the case, and let them employ some one who is more easily controlled, and whose judgment may be hampered by the fear of offending or of assuming responsibility.

Women who are stricken with eclampsia during gestation for the most part die. Why do they die? Because they are neglected, either through ignorance and consequent lack of apprehension preventing the invoking of medical aid during the prodromata (the only time when therapeutic measures can be of use—the time when, these having failed and the symptoms having become more threatening, labor should be induced); or they die because a physician having been called, he fails to render promptly the assistance demanded, and his course, on the contrary, is one of hesitation, vacillation, and that uncertainty which amounts to inaction.

The bed side of these unfortunates is too often the scene of a grand medical consultation, or pow-wow, marked by a vast deal of learned medical talk and rare variety of opinion, the conclusion of which is, the "Letting I dare not, wait upon I will," until death has set his seal, and dissolution is, to a discerning eye, only a question of a few hours.

How often, too, when the vital moment has been frittered away in discussion, and the rapping is heard upon the door, there is a sudden unanimous resolve to deliver, which is accomplished in desperate and indecent haste and confusion. The final comment of these advisers, these conservators of life and of health, is embodied in that stereotyped *douceur*—"there is one consolation; all was done that medical assistance could possibly have offered!" And with this paltry assurance, a stricken family is left to console itself. The criminality is not so much in the expression as in our believing it.

The question might be asked in objection, if we expect to put an end to eclampsia by premature delivery, what shall we say of those cases of continued convulsion despite of delivery. In the first place, eclampsia is by no means the only danger we have to anticipate or contend with—there are other threatenings oftentimes as deadly. It is the established result of experience, that convulsions cease with delivery in a large majority of instances. The same experience has established the parallel fact, that puerperal convulsions, having once occurred, do not cease short of delivery.

The exceptions to this must be exceedingly rare, and not to be hoped for.

The cases of convulsion continued beyond delivery are to be found in women who are the subjects of extreme deterioration of the blood, and of pathological changes so profound, in whom all the functions have become so altered, that even after having removed the prime cause of trouble, their condition is so depraved as to render return to health difficult, tedious, and sometimes impossible.

These cases are simply conclusive arguments in favor of that early delivery which anticipates eclampsia, œdema of the lungs, dangerous cardiac disturbance, irreparable injury of the kidneys, etc. Procrastination, gentlemen, sounds the death knell of many an obstetric patient.

Treatment of Acute Rheumatism by Propylamin. By Edward L. McGehee, MD..

For years the medical profession has sought for an antidotal specific for rheumatism. Ever and anon the glad "eureka" of some seeker has proclaimed a discovery of the long sought desideratum. But none of these pronounced specifics has been able to stand the test of wider experience and more careful experiment. I am not prepared to say that in propylamin we have a specific for this formidable disease, but the favorable results with which I have met justify me in saying, that in my hands it has been far more efficacious than any known remedy in the treatment of acute rheumatism.

This comparatively new remedy is found in different vegetable and animal substances—in the ergot of rye, narcotina, cod-liver oil, herring pickle, and also in saline combination in various flowers. It was discovered by Dr. T. Winkler. For commerce it is obtained from two sources. From ergot by distilling the watery extract with potassa.

But it is with less expense and therefore generally derived from the herring pickle, by mixing the herring pickle obtained from the dealers with sufficient potassa to render the liquid strongly alkaline, then introducing into a retort and heating. The vapors are condensed in a receiver containing distilled water, acidulated with muriatic acid and duly refrigerated, thus directly forming the muriate, which may be obtained crystalized by subsequent concentration.

In consequence of the volatility of propylamin it is better kept in a state of the muriate, and this may be either used by the physician, or extemporaneously converted into propylamin, should it be wanted, by adding an equivalent of solution of potassa. Propylamin is a colorless liquid of a fishy characteristic

odor. It is soluble in water and alcohol; has a strong alkaline reaction. It consists of carbon, hydrogen and nitrogen, formula C_6H_9N which is represented by one equivalent of propylene C_6H_6, and one of ammonia NH_3.

In 1859, Dr. Awenarius, of St. Petersburg, Russia, called the attention of the medical profession to the use of propylamin in rheumatism, for which he considered it such a specific that a case of doubtful rheumatism might be diagnosticated by the successful use of this remedy in the course of a few days. In two years he treated two hundred and fifty cases of rheumatism with it successfully. It is said that some of the cases were chronic, some acute, many metastatic, with pericardial, pleuritic and meningeal complications; and hemiplegic and paraplegic cases were not wanting, but all recovered.

Before the administration of this alkaloid, care should be taken that it be freed from all impurities and freshly prepared, for, if kept long, unless hermetically sealed, it undergoes a change, absorbs oxygen and becomes inert. It is best given in the watery solution. Two drops of the fluid extract, or three grains of the muriate, in a little camphor or peppermint water, every two hours in acute cases.

Experiments are wanting to explain the precise physiologieal properties of this drug, and how far and in what manner it may prove deleterious in over-doses. In addition to the evidence of its positive efficacy, it has the negative merit of doing no harm when properly administered, if it fail to do good. It would be superfluous for me to attempt to speculate upon the modus operandi of this efficient drug in this disease. The therapeutists cannot be expected to go before the pathologists. The pathology of rheumatism is veiled in mystery, for, as Dr. Flint says, "in the existing state of our knowledge, we are alike ignorant of the special perversions in which consists the essential pathology; and the special agencies upon which these perversions depend." Yet the empirical conclusion to which the result in the following cases has brought me, justifies me in joining Dr. Awenarius in his laudation of propylamin in acute rheumatism.

Case I. Mr. R. Gibson, an intelligent gentleman of this city, aged 35 years, has been the victim of repeated and excruciating attacks of acute rheumatism since the day after the battle of Shiloh, in the spring of 1862. He was there much exposed to rain and cold. Nearly every joint in his body, also his testicles, have in their turn been the seat of pain. Numerous physicians, both North and South, have tried their pet treatments on him, which embraced nearly all the best remedies known to the materia medica. The duration of an attack would vary under different treatments. The speediest relief was derived from the heroic prescription of Dr. J. O. Stone, of New York city, which is—quiniæ sulphatis grs. xx, tinct. opii gtt.xl. Taken at once.

Fever always accompanied and a chill generally preceded the

attacks. For a day or two prior to the chill the urine was loaded with the urates.

He has had two attacks within the last two months, which I have treated with two-drop doses of propylamin, repeated every two or three hours with the happiest results. For in the first attack within six hours, and in the second within twelve hours, of the commencement of treatment he was freed from pain and fever, and the urates cleared up. The next day he was able to follow his usual occupation. Strange to say, with him propylamin seemed to act as a purgative.

Case II. Wm. H. Carter, a gentleman favorably known in this city, has for two years been the subject of attacks of acute rheumatism, generally affecting his hips and knees. Many remedies have been tried by him, but the unparalleled and speedy abatement of all disagreeable symptoms under the use of propylamin, causes him to go armed with a prescription for it, which he has filled and takes two drop doses every three hours, commencing the moment he observes the invasion of this malady, with the never-failing result of aborting it and restoring comfort within twelve hours.

Case III. An old colored man, who had not been entirely freed from rheumatic pains for thirteen months, and was therefore not able to work, the third day after taking three drop doses of propylamin every three hours, was able to do field work.

I could mention several other cases in which the same results followed this treatment, but as there was nothing unusual or interesting, no complications in any of them, I deem it useless. There were no cardiac implications in any of these cases.

It is fair that I should mention the one and only case of rheumatism with which I have met, that propylamin failed to cure. This was one of *chronic* rheumatism of three years duration. The patient was of malarial cachexia, ænemic, with distortion of limbs and partial paraplegia.

Dr. John M. Cullen, of this city, adds his testimony to the beneficial effects of propylamin in acute rheumatism. He has never failed to effect a cure within seventy-two hours, by giving two drop doses every three hours. His personal experience also adds evidence to its efficacy. When he is attacked by rheumatism, he quickly finds relief in the use of propylamin, but he takes it in combination with iodide of potash. So the advocates of the alkaline treatment surely will not attribute the cure to propylamin.

I am aware that some members of the Medical and Surgical Association of New Orleans have tried this medicine and have been disappointed. I would suggest that possibly their lack of success was due to exposure to the air of some of the preparations they used—its action was thereby rendered less vigorous.

Whether the administration of propylamin and its uniform results in acute rheumatism bear to each other the relation of

cause and effect, or are merely coincidental, time and further investigation will decide.

Treatment of Ulceration of the Stomach. By Alfred W. Perry, M.D.

Ulceration of the stomach is to my mind a far more common and important disease than would appear by the records of mortuary statistics and of medical practice in this city. Thus I find by examining the statistics of New Orleans from 1869 to 1874, there were recorded—

Deaths by ulcer of stomach				1869	0
"	"	"	"	1870	2
"	"	"	"	1871	3
"	"	"	"	1872	2
"	"	"	"	1873	2

This would make the disease very rare in such a large population as we have. The obscurity of symptoms of this disease, and there being no truly pathognomonic symptom, is, I am satisfied, the cause that the disease is frequently overlooked. About two years ago, while investigating an early death of yellow fever, I found that the man had suffered several months from "dyspepsia," and he died vomiting blood in large clots: this was called the black vomit of yellow fever. The symptoms of ulcer of the stomach are, pain in the epigastrium, and in the back opposite the stomach, increased after meals, and especially after taking hard solid food, particularly meat; there is tenderness on pressure in the epigastrium constantly present, and this is circumscribed to a small spot not larger than two inches in diameter. There is also vomiting of food, mucus or blood. These symptoms last many weeks or months with exacerbations and remissions.

In the treatment of this disease, a modification of that usually recommended, suggested itself to me and has been used with good results. The natural tendency of all benign ulcerations is toward a spontaneous cure, and the utmost that medical art can accomplish is to remove as far as possible sources of irritation, to neutralize those which can not be removed, or to diminish the irritability of the affected part.

Ulcerations of the stomach are susceptible to two modes of irritation, viz., mechanical and chemical. The mechanical irritations are caused by the churning motions of the stomach during digestion, by its alternate distension and relaxation, and by the contact of solid hard food.

The chemical sources of irritation are—the contact of normal gastric juice; the contact of food in that state of fermentation which results from a deficient quantity or depraved quality of the gastric juice, and the irritation of various drinks chiefly alcoholic; the physiological act of secretion of gastric juice is

also a cause of irritation. The mechanical irritation is obviously avoided by giving only liquid food, and in small quantities frequently, so as to prevent distension.

The chemical irritation of the gastric juice can be almost entirely removed by the use of food which does not require stomach digestion. The act of digestion is principally to bring solid substances to a state of perfect solution. Milk is invariably recommended as a food by writers on this subject (Budd, Chambers, and Brinton). Milk is certainly fluid when it enters the stomach, but is immediately coagulated to solid pieces of casein, which require about two hours digestion in the stomach for perfect solution. Well boiled starch, or arrowroot, or beef essence, require no stomach digestion, they are absorbed and altered in the small intestines. The beef essence made by cold maceration of meat in water acidulated with hydrochloric acid, is more nutritious than the ordinary meat essence, is not precipitated on entering the stomach, and requires no stomach action for its digestion. With a farinaceous diet and weak action of the stomach, there may be lactic, butyric or acetic formation in the stomach: this can be prevented by using pepsin and hydrochloric acid, or artificial gastric juice, which has a powerful antiseptic action. This modification of the usual treatment, by giving only boiled starchy foods and meat essence, I have used with success in a few cases which I have had of this disease.

Orloff W., 23 years old, a druggist, has been sick six weeks with diarrhea and vomiting immediately after meals; has taken much medicine without relief. Entered the hospital March 27; then had slight fever, diarrhea, and vomited everything he ate. Ordered hydrocyanic acid and morphia every two hours and milk diet.

March 28.—Vomiting not so frequent, but has still great tenderness over a space 2½ inches in diameter in the epigastrium. Solid food gives him violent pain.

March 29.—Ordered him to take only a tablespoonful of arrowroot at a time, frequently repeated, and pills of nitrate silver.

April 3.—Patient much improved; vomited only once in last 24 hours.

April 4.—No vomiting; arrowroot continued.

April 6.—Patient discharged at his request, feeling perfectly well.

Ann C., 15 years old, was taken sick two months since with pain in head and back lasting two weeks; loss of appetite, and occasional vomiting for the last two weeks; she has been vomiting her food with a little blood soon after each meal. She commenced menstruating three months ago, and has done so since with regularity; had a menstruation one week ago. Came into the hospital June 4th; is very pale and thin, but cheerful; has great tenderness on pressure in a space 2 inches in diameter over the epigastrium, and at a point in the back opposite.

Vomited to-day here four several small clots of blood. Ordered hydrocyanic mixture, and beef essence and milk as diet.

June 6.—Has vomited three times food and blood since the 4th. Ordered to continue hydrocyanic mixture, and only a tablespoonful of beef essence every hour, and to eat ice.

June 7.—Vomited nothing yesterday; tenderness much diminished.

June 9.—Improving; no more vomiting; no tenderness.

" 10.—Ate a little chicken.

" 13.—Ate a little mutton.

" 14.—Discharged, well, at her request; saw the girl ten days afterwards, and she continued well.

In most diseased states of particular organs attended with irritation, an effort is made to give the organ rest, whether it be a fractured arm, an inflamed lung, or a rapidly acting diseased heart: rest in these cases means cessation of motion. The activity of a gland is its particular function, and interferes to some extent, when diseased, with the efforts of nature to repair the diseased condition. In diseases of the kidney, by diaphoretics and hydragogue cathartics we try to induce the skin and intestines to take up a portion of the function of the kidneys, and give them rest to that extent.

This is accomplished in diseases of the stomach by giving it nothing to do except to act as a receptacle for food, which in a few minutes (if not digested by the stomach) is hurried out into the intestines. The stomach seems to have a kind of intelligent action in this way. I have noticed that after eating apples unpeeled, the skins eructated alone, although the stomach was filled with food, and an ordinary contraction would have rejected just what happened to be immediately under the opening of the æsophagus; the same thing happens with me with other indigestible parts of food.

My object in specially mentioning this is to show that food not digestible by the stomach tends to pass out of it quickly, and has therefore less mechanical irritation than food of the same physical qualities which is digestible.

NOTICES OF NEW BOOKS

Therapeutics and Materia Medica. A Systematic Treatise on the Action and Uses of Medicinal Agents, including their Description and History. By Alfred Stillé, M.D., Professor of the Theory and Practice of Medicine, and of Clinical Medicine, in the University of Pa., etc. Fourth edition, thoroughly revised and enlarged. In two volumes 8vo., pp. 968 and 976. Philadelphia: Henry C. Lea; 1874.

This work has been for some time out of print, and the present edition was anxiously awaited by the medical public. The three

editions which previously appeared since 1860, have given the work an established reputation, and the estimate of its merits is generally and deservedly so high, that no words of commendation are needed from us. It is indeed a cyclopœdia of information, gathered from the most various sources, and the references to authorities are so indicated that the investigation of subjects can be pursued to any desired extent.

The chief intent of the present notice is not to add to nor detract from the general estimate of the work, but to point out what we consider some faults in the arrangement of the topics treated. With this view the author's classification of remedies is herewith presented:

I. Medicines which allay local irritation.	—Lenitives.	
II. Medicines which repress local action.	—Astringents.	
III. Medicines that irritate the part to which they are applied,	Irritants.	
IV. Medicines which promote nutrition.	—Tonics.	
V. Medicines which stimulate the whole economy.	General Stimulants.	
VI. Medicines which stimulate the cerebro-spinal system.	Cerebro-spinal Stimulants,	Narcotics. / Anti-spasmodics.
VII. Medicines which especially stimulate the spinal nervous system.	Spinants. (Tetanica.)	
VIII. Agents which depresss the whole economy.	General Sedatives.	
IX. Medicines which depress the vascular system.	Arterial Sedatives.	
X. Medicines which depress the nervous system.	Nervous Sedatives.	
XI. Mediciues which produce a discharge from particular organs.	Evacuants	Epispastics. / Errhines. / Sialagogues. / Emetics. / Cathartics. / Expectorants. / Diaphoretics. / Diuretics. / Emmenagogues. / Anthelmintics.
XII. Medicines which modify the nutrition of the body without producing any antecedent phenomena.	Alteratives.	

To every writer on this branch of medicine must be conceded the right to make his own classification, and accordingly all display in this respect more or less originality. The one before us certainly has the merit of brevity in a high degree, but we incline to the opinion that this quality has been rather overstrained in the classes of stimulants and sedatives, and to the exclusion of

some divisions, such as ecbolics, antacids, anæsthetics and disinfectants (including antizymotics). In the notice of another work on Therapeutics we expressed disapproval of such a class as *antispasmodics*, inasmuch as this effect is generally an indirect one, and may be produced by a large number of agents not arranged under this head.

But it is less with the classification as it stands that we find fault, than the arrangement of remedies according to this classification. We quite agree with the following view, quoted from the preface to the present edition: "In the first edition of the work he contended against the mischievous error of seeking to deduce the therapeutic uses of medicines from their physiological action. Continued study, observation and reflection, have tended to strengthen his convictions upon this subject, and to confirm him in the faith that clinical experience is the only true and safe test of the virtues of medicines." After this expression we might naturally expect that the author would arrange medicines in the class indicated by their most conspicuous and important therapeutic action; but in many instances this is not the case. For instance, in the class of irritants we find both the mineral and vegetable acids, the alkalies and their carbonates, chlorine and most of its compounds, and even the sodium and potassium sulphites, potassium permanganate and sodium borate. Rarely are any of the above used as irritants, with the single exception of nitric acid, while most would be susceptible of such action only by the grossest abuse. Indeed, we think it would puzzle the author's ingenuity to make an irritant of prepared chalk otherwise than mechanically, as might be done with any other substance. With far greater propriety creasote and carbolic acid could be included here, but the author chooses to place them among the General Stimulants, along with alcohol and carbonate of ammonia. In this class of stimulants are also arranged phosphorus and the hypophosphites, though the permanency of their action should assign them to the class of tonics. Sweet spirit of nitre is classed among the Cerebro-spinal Stimulants, and digitalis among the Arterial Sedatives, though it is allowed that the latter increases the force of the heart's action.

Another noteworthy feature of the work is the fact that very few remedies are discussed at all in more than one class; but under one head the various actions and applications of the agent are all included. This is contrary to the general usage of syste-

matic works, and we think not conducive to the plan proposed by the author himself—to arrange medicines according to their therapeutic uses.

Having pointed out these faults of method, our adverse criticism is ended, and very much might be said in praise of the work, which displays immense research, great experience and sound judgment. Its strong feature is particularly the therapeutic action of remedies, to the neglect of their natural history, their chemical relations, and their pharmaceutical manipulation. It is plainly intended for the use of practitioners rather than students of medicine, for reference rather than systematic study. A copious index of materia medica and another of therapeutics facilitate consultation of the work.

The mechanical execution of these volumes is in the usual unexceptionable style which characterizes the house from which issue a large proportion of our best medical books. S. S. H.

How do the Spermatozoa Enter the Uterus? By Joseph R. Beck, M.D. Reprinted from the "American Journal of Obstetrics" for Nov., 1874. Pamphlet, pp. 39.

This paper was read before the American Medical Association in 1874, and was compiled from some previous articles by the same author and others by Dr. Wernich of Berlin. Dr. Beck's contribution quotes largely from the principal gynæcological writers of the day, and a large portion is taken *verbatim* from Dr. Wernich's article.

The most valuable feature in Dr. Beck's paper is his account of an observation, which was confirmed by a second opportunity in another instance a few days after. The case is remarkable enough to justify quotation at length:

In making my visit at the residence of the patient, next day, for the purpose of adjusting the supporter, I made a second examination by the touch, and upon introducing my finger between the pubic arch and the anterior lip of the prolapsed cervix, I was requested by the patient to be very careful in my manipulation of the parts, since she was very prone, by reason of her nervous temperament and passionate nature, to have the sexual orgasm induced by a slight contact of the finger, a fact which I believe had been manifested in my office examination of the previous day, and which she afterwards admitted had been the case. Indeed, she stated further, that this had more than

once before occurred to her while making digital investigations of herself. Here, then, was an opportunity presented me to make a second observation, under vastly more favorable circumstances than had accompanied the first; an opportunity never before, in so far as I knew, afforded any one, and a chance for clearing up the hitherto regarded unknown and unknowable, which in my opinion was not to be lost under any consideration. Carefully, therefore, separating the labia with my left hand, so that the os uteri was brought clearly into view in the sunlight, I now swept my right forefinger quickly three or four times across the space between the cervix and the pubic arch, when almost immediately the orgasm occurred, and the following is what was presented to my view:

The os and cervix uteri had been about as firm as usual, moderately hard, and, generally speaking, in a natural and normal condition, with the external os closed to such an extent as to admit the uterine probe with some difficulty; but instantly that the height of the excitement was at hand, the os opened itself to the extent of fully an inch, as nearly as my eye could judge, made five or six successive gasps, as it were, drawing the external os into the cervix each time powerfully, and, it seemed to me, with a regular rhythmical action, at the same time losing its former density and hardness, and becoming quite soft to the touch. All these phenomena occurred within the space of twelve seconds of time certainly, and in an instant all was as before. At the near approach of the orgastic excitement the os and cervix became intensely congested, assuming almost a livid purple color, but upon the cessation of the action, as related, the os suddenly closed, the cervix again hardened itself, the intense congestion was dissipated, the organs concerned resolved themselves into their normal condition, and their relations to each other became again as before the advent of the excitement.

The remaining steps of the entrance of the spermatozoa into the uterus may be indicated by the following passages, quoted from Wernich:

We may now with the greatest propriety inquire for what purpose, then, has the lower segment of the uterus been endowed with erectility? I reply, in the first place, for the purpose of ejaculation. The fact that at the height of the orgasm there is ejaculated on the part of the female, and from the uterus, a fluid, has been known for a long time, and numerous observers have from time to time called attention thereto. This fluid is generally mucous in its character, small in quantity, of an alkaline reaction, and of a high temperature. I say generally, for the reason that there are exceptional cases, where, when the orgasm occurs in individuals who possess certain very voluptuous temperaments, this fluid is said to pour forth from the os uteri in quite considerable quantities. This

fluid, as we all know, consists of the secretions of the cervical glands.

* * * * * *

The act of coition, in order to be perfectly satisfactory to both parties, is characterized by a contemporaneous erection of the organs immediately or rather principally concerned therein, and also by an ejaculation of a fluid material from both sides at the same instant.

* * * * * *

If the strongly erected cervix uteri now suddenly relapses into the condition of relaxation, there is manifested a larger opening at the external os than had existed in its erected state, or even in its normal condition.

The second process which manifests itself immediately after the sudden relaxation, and one which is to be necessarily inferred from both the physical and physiological relations of the parts, is aspiration. This is the means by which the mucous mass, including the spermatozoa, as above referred to, is drawn through the cervical canal into the uterus, and this latter organ, in all probability, aids in this movement, inasmuch as its congested and swollen walls also relapse into their normal condition immediately after the relaxation mentioned. This aspiration, or sucking-up movement, by the os and cervix uteri, by which the semen, mixed with the fluid which is ejaculated from the cervical canal, is brought into intimate mechanical contact with the cavity of the fundus of the uterus, is no new idea. It has been frequently mentioned in works on physiology and gynæcology, and it has also been observed in animals, for example in the rabbit, by means of vivisections. It is further well known to many women, who have been guided by former experiences, that certain sensations which peculiarly accompany the process of aspiration, taken in connection with the almost absolute absence of mucus from the vagina after cohabitation, constitute to them some of the surest signs of conception. This aspiration movement can also be frequently distinguished by the male during coitus, and in a word, the aspiration movement is a well-known fact; but it is only intelligible when viewed in connection with the rigid erection and the consecutive rapid relaxation of the lower segment of the uterus, as already described.

Having thus given an epitome of Dr. Beck's views, it is well to take further testimony on the subject. In his work on Human Physiology, Dr. Carpenter observes: "It has been supposed by some, that the os uteri dilates, by a kind of reflex action, to receive the semen; but of this there is no evidence. The introduction of a small quantity of the fluid just within the vagina, appears to be all that is absolutely necessary for conception; for there are many cases on record, in which pregnancy has occurred

in spite of the closure of the entrance to the vagina by a strong membrane, in which but a very small aperture existed."

In Leishman's System of Midwifery, p. 96, Am. ed., we find: "There are perhaps few physicians of experience who have not met with cases where women, believing themselves *safe*, have permitted a certain amount of sexual contact without penetration, and have thus become pregnant. All, in fact, that seems essential is contact of the animal fluid with the pudendum, which is further proved by observations of pregnancy coincident with perfect hymen."

In Taylor's Medical Jurisprudence, 7th Am. ed., p. 719, we read: "Conception, it is well known, does not depend on the consciousness or volition of a female. If the state of the uterine organs be in a condition favorable to impregnation, this may take place as readily as if the intercourse were voluntary; even penetration to the vagina is not absolutely necessary for impregnation."

It is quite likely that abundant evidence of a similar nature could be found in standard works, and the subject may here be left to the reader without comment. S. S. H.

Archives of Dermatology; a Quarterly Journal of Skin and Venereal Diseases. By L. Duncan Bulkley, A.M., M.D.

We have had occasion once before to make mention of this journal, the first number of which was published on the 1st of October, and now having received the January number, we desire to call more particular attention to its design and to the excellent qualities which it has thus far developed. The articles which have been contributed to its pages are written with ability and good judgment, and the selections are of an equally apposite character. There are, strictly speaking, no editorials, the editor wisely preferring communications of a strictly professional nature, and as these are sufficiently numerous and interesting to fill the journal, there is no room for extraneous or even collateral matters.

We wish the *Archives* the greatest success, and in order to assist in its distribution take great pleasure in assuring our friends that it is well worthy of their support.

It is published by G. P. Putnam & Sons, and in point of print-

ing and paper is surpassed by no professional periodical in the country.

A Series of American Clinical Lectures; edited by E. C. Seguin, M.D. Vol. I., No. I. On Diseases of the Hip-Joint. By Lewis A. Sayre, M.D., Prof. of Orthopedic Surgery, and Clinican Surgery in Bellevue Hospital Medical College, New York. 8vo. pamphlet, pp. 24. G. P. Putnam's Sons: 1875.

We are informed by the publishers that this is the first "of a series of *Clinical Lectures* by representative American Medical Teachers," intended "to express not only the personal views of the lecturers upon the subjects treated of, but also the *latest pathological and therapeutical opinions connected with these topics*, and will therefore be trustworthy guides to practice." (The italics are our own).

We commend the undertaking with all our heart, and shall look forward to its farther progress with a lively interest in its success; but we very seriously question whether this first number comes fully up to the standard fixed by the editor and publishers? We cast no imputation upon Professor Sayre's standing as a surgeon, nor upon his well known skill in the management of cases of "hip-disease;" but we must be permitted at least to enter our *caveat* against the attempt to set up his crude pathology as representing the "latest pathological and therapeutical opinions connected with this topic." It is necessary only to quote the single paragraph which he devotes to this branch of the subject, in order to justify our position, italicizing those passages which we think particularly objectionable, and let the reader judge for himself.

It is not our intention, nor have we the space, to enter into a discussion of these several points, but there is one question which we should like to ask the author. "If tearing the ligamentum teres from its attachment induces necrosis of the head of the femur, as you here distinctly assert, why is it that such results do not invariably follow upon ordinary dislocations of the head of the femur so common in adults? In these cases there is undoubtedly rupture of the round ligament, as may be easily demonstrated upon the *cadaver*, but how many cases of necrosis are recorded as a consequence?

Again, will the author explain what he means by extravasa-

tion of the blood in the "articular lamella?" And again, are the non-vascular tissues *never* the primary seats of disease?

We sincerely hope that the author will take our questioning in good heart, and re-write the objectionable paragraph, so that it may in fact represent "the latest pathological opinions" of American surgeons. He is competent to the task and owes its performance to the position which he occupies as a teacher, and his popularity as a practitioner.

The pathological changes that these causes produce are either *Synovitis; from a rapid and excessive change in temperature;* or violent wrenches or strains, *tearing from its attachments the ligamentum teres*, thereby inducing necrosis of the head of the femur from rupture of the blood-vessel which supplies it; or concussions, falls, jumps, blows, will produce an extravasation of blood in the *articular lamella*, which sets up an inflammation, the cartilages soon die on account of their low vitality, become eroded and necrotic, interstitial absorption of the bones takes place, due to the constant pressure from muscular contraction, and, finally, exfoliation. But, no matter what the cause, or which of the tissues originally involved, they all, sooner or later, become commingled and included in the general destruction. *I don't believe the disease ever commences in the cartilage, as it is entirely without blood-vessels and nerves*, and lives simply by imbibition.

Eating for Strength. A book comprising the Science of Eating; Recipes for Wholesome Cookery; Recipes for Wholesome Drinks; Answers to Ever-recurring Questions. By M. L. Holbrook, M.D., Editor of the "Herald of Health," "Parturition Without Pain," etc., aided by numerous competent assistants. New York: Wood & Holbrook, 13 and 15 Laight street; 1875.

"Remembering how I used to lay about me in my youthful days."—*Extract from Thackeray's Letters.*

The above words we have written, in letters of gold, before our eyes, that they may be a sort of *frenum* to our spirits whenever we take the critical flail in hand. Thus, it may be we find ourselves sincerely desirous to recommend Eating for Strength, for the sake of the good it contains. Unfortunately, however, the author does not state for whom his work is designed, seeming to forget that what is instructive and pertinent to one class, may be inappropriate to another, whilst the three divisions—into Science of Eating, Recipes, and, Answers to Ever-recurring Questions—differ so widely as to demand each its own audience.

The catechism at the end of the book, propounding and answering its own questions, discusses with rare versatility both the dilletante question of "Humiliation in Eating and Drinking," and the more practical one of Making Yeast Powder and Keeping Ice. The Recipes are many of them excellent, though perhaps too varied for one bent on a strict course of dietetics.

Finally, or firstly, for we are reading the book backwards, we doubt if any beginner would glean, from the first division of the work, more "science" than would suffice to remind one of the "Little Learning" couplet once so believed, now so despised. Nay, it almost seems as if this were, for the mass, at least, the Little Learning's day. Every man his own doctor, lawyer, and preacher, with a "devil take the hindmost" in the mad race for learning! No need for professors now, when each man, in this nineteenth century, is born with a patent right to do his own professing!

Pulmonary Tuberculosis; its Pathology, Nature, Symptoms, Diagnosis, Prognosis, Causes, Hygiene, and Medical Treatment. By Addison P. Dutcher, M.D., late Professor of the Principles and Practice of Medicine in the Cleveland Charity Hospital Medical College, Ohio. Philadelphia: J. B. Lippincott & Co.; 1875.

We are indebted to R. C. Eyrich, 130 Canal street, New Orleans, for this book, an octavo volume of some 380 pages, published in exceedingly faultless style.

It is evidently the production of a very earnest and zealous physician, who seeks to benefit his profession by displaying the means by which he benefitted his patients. While the general practitioner will find in this volume enough of value richly to reward his perusal, it cannot be admitted that it reaches the standard of scientific accuracy requisite to class it with late essays upon the same subject, in point either of learning or careful elaboration of his subject.

Physician's Office Case-Record and Prescription Blank Book; from 187– *to* 187–. Third edition. Cincinnati Case Record Company; 1875.

We have in a former issue given this book a favorable notice.

We now repeat our warm commendations in respect to its convenience and perfect adaptation to the practitioner's wants. These commendations are at this time based upon several months daily use. The book may be had of James Gresham, who is agent for the company.

The Histology and Histochemistry of Man. A Treatise on the Elements of Composition and Structure of the Human Body. By Heinrich Frey, Professor of Medicine in Zurich. Translated from the fourth German edition by Arthur E. J. Barker. Surgeon to the City of Dublin Hospital; Demonstrator of Anatomy, Royal College of Surgeons, Ireland; Visiting Surgeon, Convalescent Home, Stillorgan. Revised by the author, with six hundred and eight engravings on wood. New York: D. Appleton & Co., 549 and 551 Broadway; 1875.

This book has reached us at the latest moment prior to the issue of our present number. It is, no doubt, the very best, as well as the latest manual upon its subject in medical literature. It forms an octavo volume of 683 pages, excellently well printed both as respects its type and also as it respects the execution of its numerous illustrations.

Compendium of Children's Diseases. A Hand-Book for Practitioners and Students. By Dr. Johann Steiner, Professor of the Diseases of Children in the University of Prague, and Physician to the Francis Joseph Hospital for Sick Children. Translated from the second German edition by Lawson Tait, F.R.C.S., Surgeon to the Birmingham Hospital for Women, Consulting Surgeon to the West Bromwich Hospital, Lecturer on Physiology at the Midland Institute. New York: D. Appleton & Co., 549 and 551 Broadway; 1875.

We have given this work a very careful examination, and do not hesitate to pronounce it the best hand-book upon children's diseases to be found in the English language. The author exhibits unusual skill in seizing upon those salient points in pathology and treatment which are the matters of practical concern both for the student and physician.

The mechanical execution of the work is in the usual style of the publishers.

EDITORIAL.

Annual Meeting.

The American Medical Association will hold its next annual meeting at Louisville, Kentucky, on the first Tuesday in May. Louisville is a central point, easy of access by numerous railroads, and famous among travellers for the good cheer and agreeable surroundings of its fine hotels. It is a city which possesses more than ordinary attractions for visitors who cultivate taste for pure æsthetic enjoyments, no less than for those who, in anticipation of visits there, sharpen their appetites for what the primitive parsons would call its table comforts. It is a city renowned for a high toned but gushing hospitality. We hope that the profession in the South will be able to attend in large numbers, and we can assure them that noble specimens of our brotherhood will greet them with most cordial welcomes.

In urging our professional brethren of the South to attend the approaching Convention of the National Medical Association, we comprehend very fully the many difficulties in their way. But at this time it is a matter of very great importance that we should place the Association above the reach of reproaches and disputations, in respect to its standing as a national medical congress, by sending from every section of the country delegates who, with our full consent, shall appear as representative men of the profession.

Vital Statistics and Politics.

Our patrons may consider that some explanation is due for the appearance of an article in the present number of the JOURNAL, in which direct reference is made to the political parties dividing the country at the present juncture. While we do not hesitate to admit that the phraseology of Prof. Chaillé's article is a proper subject for criticism, when appearing in a scientific periodical, we earnestly contend that its subject matter is eminently proper to appear. We shall never exclude from our page

articles from scientific and truth-serving contributors, intended to correct either popular or party errors in regard to vital statistics. Every physician at this time recognizes the great and growing value of the study of vital statistics in determining some of the most important truths of medicine, and of social hygiene and politics. Since the days of Quetelet, every cultivated medical practitioner works out his calculus of probabilities in reference to those events which chiefly concern his profession, from data obtained from this source. If only correct data be afforded, his problems are wrought out as naturally, and almost as correctly, as the merchant's balance upon his ledger.

We have therefore a right, and properly exercise that right, when we devote scientific researches to the prevention of any misinterpretation, or perversion of vital statistics, for the purpose of upholding fraud, or to promote party ends. To relinquish such a right, would be to permit the profanation of truths whose value and fixedness are not well understood outside of our profession, and whose proper defence and interpretation rest with ourselves. The accuracy and reliability of Prof. Chaillé's paper are altogether above successful attack, and while explanation is offered for the modes of expression employed by him, we congratulate ourselves upon being able to afford our readers its strong array of facts. The explanation to be offered is simply the statement, that the author did not write the article for publication in a medical journal, and after he was assured that it contained facts valuable to be preserved as a future reference and guide by both the medical and political statistician, it was too late to alter its phraseology without emasculating its vigor.

Postponed.

"We are informed that the meeting of the Association of Medical Officers of the Confederate States Army and Navy, which adjourned from Atlanta to reconvene in Richmond in July next, has been postponed to the 19th of October following. The Medical Association of Virginia holds its annual session on the succeeding day, the 20th of October.—*Atlanta Medical and Surgical Journal.*

METEOROLOGICAL REPORT FOR NEW ORLEANS.

Table I---January.

Day of Month.	Temperature.			Mean Barometer Daily.	Relative Humidity—Daily.	Rain fall—inches.
	Maximum.	Minimum.	Range.			
1	73	58	15	30.249	92	.38
2	62.5	56	6.5	30.224	78	.11
3	54	50	4	30.145	84	.91
4	53	45	8	30.200	83	.01
5	41	34	7	30.312	76	.37
6	55	36	19	30.219	87	.58
7	55.5	44.5	11	30.175	82	.01
8	56	46	10	30.230	82	.00
9	43	39.5	3.5	30.381	55	.22
10	32	29	3	30.345	72	.35
11	51	32	19	30.213	86	1.28
12	66	42	24	30.040	90	.14
13	66	63	3	———	80	.00
14	50	47	3	30.369	69	.00
15	53	36	17	30.345	57	.00
16	66.5	45.5	21	30.269	86	1.48
17	64	52	12	30.291	88	.00
18	64	53.5	10.5	30.210	88	.06
19	51	47.5	3.5	30.251	81	.02
20	57	44	13	30.235	82	.00
21	74	50	24	30.193	83	.00
22	66	62.5	3.5	30.113	88	.53
23	69	53.5	15.5	30.029	89	.98
24	68.5	59	9.5	30.082	77	.00
25	58	47	11	30.228	58	.00
26	71	51	20	30.206	73	.00
27	76	58	18	30.099	84	.00
28	77.5	61	16.5	29.865	82	.00
29	65.5	59	6.5	29.885	70	.00
30	55	45	10	30.071	62	.00
31	55	46	9	30.160	39	.00
						Total.
Mean..	59.64	48.14	11.50	30.188	77.51	7.43

Table II---February.

Day of Month	Temperature. Maximum.	Minimum.	Range.	Mean Barometer Daily.	Relative Humidity—Daily.	Rain fall—inches
1	60	35.5	24.5	30.329	55	.00
2	71	47.5	23.5	30.028	84	1.50
3	63	50	13	30.189	45	.00
4	40	37.5	2.5	30.535	30	.00
5	47	31	16	30.604	37	.00
6	55	34	21	30.476	41	.00
7	56	42	14	30.334	46	.00
8	56.5	36	20.5	30.305	55	.00
9	59	44	15	30.287	71	.98
10	70.5	48.5	22	30.025	83	2.24
11	59	54.5	4.5	30.231	60	.00
12	66	46	20	30.298	69	.00
13	74	54.5	19.5	30.170	80	.00
14	77.5	63	14.5	30.112	82	.00
15	55.5	50.5	5	30.249	75	.18
16	50	43	7	30.253	78	
17	50.5	45	5.5	30.115	76	2.37
18	56.5	45 5	11	30.056	70	.70
19	56	49	7	29.684	88	4.71
20	56	47.5	8.5	29.835	66	.00
21	61.5	43	18.5	30.136	65	.00
22	69	47	22	30 117	84	.56
23	79.5	57	22.5	29.924	82	.00
24	80	71.5	8.5	29.915	80	.00
25	66	60	6	30.130	48	.00
26	67	48	19	30.170	66	.00
27	73.5	56	17.5	30.135	66	.00
28	73.5	57	16.5	30.044	76	.00
Mean..	62.46	48.00	14.46	30.166	69.93	Total. 13.24

Mortality in New Orleans from January 3d, 1875, to February 28th, 1875, inclusive.

Week Ending	Pneumonia.	Small-Pox.	Diphtheria.	Consumption.	Total Mortality.
Jan. 3	10	3	2	11	113
Jan. 10	16	10	0	23	131
Jan. 17	14	10	0	22	150
Jan. 24	18	5	1	19	137
Jan. 31	10	6	0	12	110
Feb. 7	12	12	3	23	135
Feb. 14	6	9	2	27	116
Feb. 21	10	6	0	16	89
Feb. 28	12	17	1	18	114
Totals	108	78	9	171	1095

THE

NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

MAY, 1875.

ORIGINAL COMMUNICATIONS.

ARTICLE I. *Bedside Notes on the Pulse, Temperature, and Urine, in cases of Yellow Fever observed at Pensacola Navy Yard, in the Fall of* 1874. By SAMUEL LOGAN, M.D.

The thermometry of yellow fever has already attracted the careful attention of southern physicians. Prof. Joseph Jones, Dr. J. C. Faget, of this city, and others, have contributed valuable data towards its elucidation. Having been called to the assistance of the medical department of the Navy in the epidemic at the Navy Yard at Pensacola, last fall, the opportunity was afforded me of contributing my quota of clinical facts to this department of the semeiology of the disease, and I avail myself of this occasion to place those facts on record. This paper, therefore, is intended mainly for that purpose. But since great importance had been justly attributed, in the differential diagnosis of yellow fever, to the mutual *relations* of—or perhaps we had better say, want of any relations between—the *pulse* and temperature, I was careful, in making the bedside observations, to take note of both at the same sitting, counting the pulse while waiting for the mercury to reach its highest point; and since some have been inclined to attach a diagnostic as well as prognostic importance to the presence or absence of albumen in the urine, I have in most of the cases taken note also of this clinical phenomenon. For examination of the urine I am indebted to the

hospital steward, Mr. W. J. Smith, one fully qualified to make such examinations. I must also acknowledge the very valuable assistance rendered me by Dr. Wm. Martin, Acting Assistant Surgeon of the Navy, who was my diligent co-laborer throughout the epidemic, having been ordered to the navy yard at the same time that my services were engaged.

It will be observed that the notes are confined almost exclusively to the points indicated in the heading of this paper. Little or no mention is made of the treatment of each case, because, in the first place, I adopted no pet scheme of therapeutics, and therefore have no hobby to parade; and, in the second place, I thought it best to give a general outline of the treatment in these preliminary remarks, so as to avoid unnecessary repetition. The general plan adopted was simply to treat the symptoms, support the patient, and watch the nursing most carefully.

In the conduct of a case of yellow fever, it has long been my belief that as much good is accomplished in a negative as in a positive way, by preventing the friends and the nurses from killing the patient—as in the administration of medicines.

If the case was seen early, a mild saline, or mercurial, or castor oil purge, was given. A common prescription would be a seidlitz powder every two hours till the bowels acted two or three times. If the attack came on just after a meal, a mild emetic was ordered, and usually a quantity of warm water was sufficient, after which the purgative was administered. The conventional mustard foot-bath was pretty nearly always ordered; and the patient was encouraged to drink warm teas, usually orange leaf tea, but not deluged, or parboiled, with it. If he craved cold drinks or ice, he was allowed either or both in moderation. As soon as the bowels were emptied by the cathartic, nutrition was carefully begun and systematically conducted. I found rice gruel to agree well with most of the cases during the febrile stages; it was ordered by so many tablespoonsful at intervals of every two or three hours, varying the dose to suit the special case. Beef and chicken tea were used, especially later in the case. Stimulants were used later, as the stage of prostration came on; and great care was observed, as in regard to the nourishment, to adapt the dose and the intervals to the particular case. Such is a general outline of the treatment pursued. But of course, if any special organ was threatened, special additional treatment was resorted to, to meet the complication, such as

rubefacients to the surface, frictions, anodynes—very carefully given—sometimes saline diuretics, etc. Absolute recumbency was insisted on throughout the attack, especially in the third stage.

In regard to the history of the epidemic, the manner of its introduction, its fatality, etc., I am not now prepared to report, further than to give the following extract from a letter from my friend, Dr. Robert B. Hargis, of the city of Pensacola, dated October 23d, 1874. Dr. Hargis, as is well known, is an able and close observer. His views of the inception, progress, and nature of the epidemic, are worthy of the utmost confidence:

"The yellow fever was introduced into Pensacola last summer from the bark Elmira Coombs, at the Quarantine Station opposite the city, by four dissolute men who had been discharged from that vessel. They reached the city on the 20th of August, at night, and took up their abode at Kelly's sailor's boarding-house, on Main street, near the water. All of them fell sick about the same time—23d of August—and removed to the Marine Hospital, where two died with black vomit on the 29th of August.

"The first case that occurred among the citizens was a Mrs. C., who was an inmate of a disreputable house contiguous to Kelly's boarding-house. She was taken sick on the 25th of August, and died on the 28th with black vomit. The houses in this locality are wretched dens of iniquity, wherein the 'offscourings' of the city denizens and degraded wretches from abroad congregate at night, to 'lay their unholy offerings at the shrines of pagan deities.' Kelly's house then was the centre of infection—the *focus*, whence the disease radiated and fell like sparks in different parts of the city, lighting up the separate *foci* not otherwise kindled by emanations through the air. The atmosphere did not become generally infected, but served as a medium for the transmission of the morbific agent, from a *focus*, to a distance not exceeding 80 feet. Thus, in a very few instances, it migrated from one house to another. Except persons in those houses reached by the infection through the medium of the air, every case that fell under my care or observation received the infection by immediate contact with another laboring under the disease. For example, Mr. J. visited a friend sick of the malady, in an isolated place; contracted the infection; but before the disease became fully developed, went into the country several miles distant from the city; about the fourth day of his sickness was visited by a woman from a remote part of the country—twenty miles distant; her stay was not longer than an hour; she returned home, and died a week afterwards with black vomit.

"The type of the yellow fever that prevailed here this season was extremely malignant and peculiar—peculiar in this regard—

though known to be protean in its character—i. e., the pyrexial stage in a very great majority of the cases continued five days with very short diurnal remissions, which, though slight, were distinct, thermometer falling 1° to 1.5°.

"The duration of the disease was from seven to nine days. In fatal cases death took place generally about the sixth day; some died as early as the third or fourth day, while others survived the ninth—even a later period; one reached the thirteenth day and then succumbed. The malady concentrated its malign influences on the great nervous centres, brain, and spinal cord, as the symptoms, etc., indicated, and necropsies revealed. The statements I have made with regard to the communicability of the destroyer, its peculiarities and *post mortem* appearances, ought to correspond with your observations at the navy yard, as the disease undoubtedly emanated from the same source. The two marines who "lighted up" the first focus at the navy yard. did, as I have ascertained from persons of unquestionable credibility, visit the houses in which the disease made its first appearance in Pensacola, which was on or about the 25th or 26th of August.

"P.S.—I should have mentioned in the proper place that hemorrhages occurred in many cases, and scarcely half that died had black vomit. An inscrutable desire to get out of bed manifested itself in nearly all cases about the third or fourth day. A "wild delirium" came on about that period in some cases; the head was hot, eyes fiery red; constant jactitation, and finally death by complete exhaustion. Some, after all this, would recover their senses and retain them till death closed their eyes. The temperature was in all the cases taken in the mouth.

"Yours, etc., R. B. H."

Case I.—F., white, aged about 40, a strong, hearty man; sick four days before my arrival on September 18th, 1874.

September 18th, 5 a. m. Head warm; eyes suffused; tongue swollen, and covered with a whitish paste but red at edges; gums spongy; skin moist but warm. Pulse 96; temperature 104° F.

September 19th, 8½ p. m. Had a tolerable night, but now anxious, restless and despondent. This patient had already lost his wife and child, and had nursed them himself. Head and skin generally cooler to the touch; eyes and skin jaundiced; tongue and gums same as yesterday; urine freely charged with albumen, but still tolerably abundant. Pulse small and 88; temperature 103.8° F. ℞—Brandy f℥ss every two hours in chicken tea.

12 m. General symptoms much the same. Pulse 88; temperature 103.5°.

6 p. m. Same. Pulse 92; temperature 104.2°.

September 20th, 6½ a. m. Had but little sleep; low delirium;

restless; can be roused to consciousness, but soon mutters again. Pulse small, compressible, and 88; temperature 103.5°. ℞—Continue supporting treatment.

7 p. m. Very prostrate; nurses have succeeded in getting him to take but little nourishment or stimulants, although no nausea or vomiting has occurred; totally unconscious; head hot. Pulse imperceptible; temperature 103.5°.

Died during the night.

Case II.—Capt. C. L. F., white, aged 40; plethoric; sick five days. Has been kept very warmly blanketed, in room with doors and windows all closed, deprived of all cool drinks, and deluged with hot teas, the nurse having persistenty disobeyed the physician in attendance—actuated by the inherited prejudices still too common among the ignorant.

September 18th, 10 a. m. Body covered with eczema tropica from excessive sweating, as red as a boiled crab; restless; eyes injected; head and skin hot, but saturated with perspiration; anxious delirium. Pulse 120 and bounding; temperature 104°.

6 p. m. Worse; wildly delirious; nurse has still persisted in keeping him under three blankets, though the day is a warm one; head very hot. Pulse 140; temperature 107.8° ℞—Ice to head, iced drinks.

8½ p. m. Seemed for a little while more calm; but convulsions came on at 8, and death almost immediately followed.

Case III.—Capt. A. A. S., aged 50, white, rather thin but tolerably healthy, and of temperate habits.

September 18th, 2 a. m. Taken about 12½ o'clock in the night with headache and fever. Head warm; eyes slightly suffused; cephalalgia; rachialgia; skin hot but rather moist. Pulse 98; temperature 104.5°. ℞—Mustard foot-bath, and a seidlitz powder every two hours till bowels moved tolerably freely.

8 a. m. General symptoms much the same; bowels moved freely. Pulse 90; temperature 103.1°.

12 m. General symptoms the same. Pulse 104; temperature 103.1°.

This sudden increase of the frequency of the pulse was in all probability due to a little excitement from hearing accidentally of the death of one of his comrades.

7 p. m. Rests quietly and complains but little. Pulse 84; temperature 104°.

September 19th, 7½ a. m. Had a good night; tongue clean and soft; skin moist. Pulse 80; temperature 101°.

12 m. Calm and quiet, but some headache still; eyes more suffused, and head and skin warm though moist. Pulse 72; temperature 102.7°.

5½ p. m. Same. Pulse 72; temperature 103°. Has taken a small cup of rice gruel at intervals of two hours.

September 20th, 7½ a. m. Slept well; calm and quiet; head rather warm, but body cool and skin moist; eyes a little suffused. Pulse 70; temperature 100.5°.

12 m. Same. Pulse 74: temperature 102°.

5 p. m. Same. Pulse 76; temperature 102.3°.

September 21st, 7 a. m. Had a good night; head cooler; skin cool and moist; tongue slightly furred, red on edges and pointed; eyes injected and sclerotic slightly yellow. Pulse 74; temperature 101.5°.

12 m. Same. Pulse 76; temperature 101.4°.

5 p. m. Feels much the same, but skin warmer. Pulse 76; temperature 103.5°. This afternoon he accidentally became aware of the death of two comrades who were sick when he was himself attacked, and whose deaths had been concealed from him. Query—was the sudden increase of temperature due to this fact?

September 22d, 7 a. m. Head and skin generally cool; slept well; feels better. Pulse 72; temperature 99.5°.

12 m. Same. Pulse 74; temperature 100.4°.

3 p. m. Head a little hot, but feels no discomfort. Pulse 74; temperature 102. Has had no operation since first day of attack. ℞—One seidlitz powder.

7 p. m. Had two stools; head cooler. Pulse 80, a little weak but otherwise normal; temperature 101°. ℞—Brandy ℥ss every three hours.

September 23d, 6 a. m. Calm and quiet; head and body cool; tongue whitish; conjunctiva a little more jaundiced. Pulse 78; temperature 99.6°.

1 p. m. Same. Pulse 74; temperature 99°.

7 p. m. Same. Pulse 78; temperature 99.5°.

September 24th, 7 a. m. Rested well; in pretty much the same general condition; feels better. Pulse 72; temperature 99°.

11½ a. m. Same. Pulse 72; temperature 98.5°.

5 p. m. Same. Pulse 74; temperature 98°.

September 25th, 6½ a. m. Slept well; general condition same. Pulse 64, and rather weak; temperature 98.2°.

12 m. Doing very well. Pulse 64; temperature 98°.

7 p. m. Same, but tongue getting clean and less red on the edges. Pulse 64 and stronger; temperature 98.5°.

September 26th, 7 a. m. Slept well, and general condition good. Pulse 64; temperature 98°.

September 27th. Convalescent. Pulse 66; temperature 98.5°.
" 28th. " " 64; " 98°.
" 29th. " " 74; " 98.5°.

The urine was tested each day, and no albumen was ever detected in it.

Case IV.—September 19th, 1874—7 a. m. Richard B., black, aged 23; robust laborer; here 19 months from one of the northern States. Taken sick at 6 a. m. on same premises as last case; cephalalgia and "choking" feeling; head hot—skin of body also, but not dry. Pulse 110; temperature 102.5°.

12 m. Same. Pulse 78; temperature 103°. Cephalalgia less severe; skin feels cooler.

6 p. m. Pulse 84; temperature 103.2°.

September 20th, 7 a. m. Quiet and calm, head and skin cool and moist; no pain. Pulse 72; temperature 100.5°.

12 m. Same. Pulse 72; temperature 100.8°.

7 p. m. " " 80: " 101.5°.

September 21st, 6½ a. m. Good night, and feels better. Pulse 68; temperature 99.7°.

12 m. Same. Pulse 66; temperature 99°.

7½ p. m. " " 66; " 99.2.

September 22d, 6½ a. m. Feels stronger; pronounced convalescent. Pulse 60; temperature 98.6°.

12 m. Same. Pulse 62; temperature 98.5°.

7½ p. m. " " 64; " 98.6°.

The urine was examined every morning, after the first day, and no albumen was found in it.

Case V.—September 18th, 1874. Louisa B., black, aged about 20, is a consumptive—wife of last patient. Third day of sickness. 7 a. m. Skin hot, but not very dry; restless; cephalalgia; eyes much injected; has been heavily blanketed and sweated, and the room has been kept well closed up. Pulse 108; temperature 104.5°.

12 m. Much the same. Pulse 104; temperature 104.6°.

6 p. m. Quite restless; tongue dry and pointed, otherwise the same; bowels move freely. Pulse 120; temperature 105°.

September 19th, 7 a. m. Bad night; restless still; distressed expression. Was prevented from recording pulse and temperature.

12 m. Much the same; nervous and very thirsty; menses appeared at about 10, and the loss is considerably more than natural. Pulse 100; temperature 104.8°.

5½ p. m. Same. Pulse 112; temperature 104.4°.

September 20th, 7 a. m. Much the same; slept some little; menses still profuse. Pulse 92; temperature 102.5°.

12 m. Same. Pulse 116; temperature 103.8°.

Urine passed this morning was examined and found highly albuminous.

7 p. m. Quite weak; skin cool; anxious and distressed; passed but very little dark urine during the day. Pulse 104; temperature 102°.

September 21st, 6½ a. m. Very restless; gags frequently, and vomits a very little fluid with greenish black specks in it. Pulse 96; temperature 99.5°. Sclerotic coats quite yellow.

12 m. More quiet; skin cool. Urine collected this morning loaded with albumen, and very dark and scanty. Pulse 84; temperature 100°.

5 p. m. Same. Pulse 96; temperature 102°.

About two hours after this visit she was taken with violent convulsions, and died almost immediately.

Case VI.—Henry S., German, aged 45; robust; in South five years; gardener.

Sept. 18th, 12 m. Fifth day of illness. Has been excessively purged from the first; the purges were frequently repeated; has been also fearfully blanketed; eyes injected, gums spongy, and tongue dry and red; head hot; skin hot but moist; very restless; constant nausea and sighing. Pulse 86; temperature 103.1°.

8 p. m. Has been very unmanageable; contrary to express orders, the nurse, at his solicitation, sat him up several times to pass his fæces and urine, and also allowed his relatives to change his clothing and bed sheets; general condition same as at last visit. Pulse 88; temperature 103°.

9 p. m. Called to him, and found him ejecting quantities of black vomit. Ice to the epigastrium seemed to check it for a time; but he died at 12 p. m. Post mortem, 9 hours after death:

body deeply jaundiced; not permitted to open skull; heart presents nothing peculiar; lungs ditto; stomach contains about six ounces of black vomit; mucous membrane highly congested; liver and spleen very much congested—former fatty; kidneys highly congested; no time for more minute examination.

Case VII.—Mary S., wife of above, German, aged 28; here six months; moderately robust.

September 18th, 12 m. Cephalalgia and rachialgia; been much exhausted waiting on husband; head hot; skin of body warm but moist; eyes slightly suffused. Pulse 100; temperature 102°.

8 p. m. Much the same. Bowels opened by some purgative pills she had taken yesterday. Pulse 92; temperature 101.5°.

September 19th, 9 a. m. Slept tolerably well; general symptoms much the same, but pulse 104; temperature 103°.

5 p. m. Feels much the same, but skin hot though moist; had one more stool. Pulse 110; temperature 105°.

September 20th. Had a good night; complains of no pain; eyes suffused; head and skin of body moist, and only a little hot; tongue moist, whitish in middle and red on tip and edges. Pulse 100; temperature 103.8°.

12 m. Much the same. Pulse 104; temperature 104.2°.

7 p. m. Feels much the same; quiet and tractable. Pulse 96; temperature 101.4°.

No albumen in urine.

September 21st, 6 a. m. Head and body cool and moist; had a good night. Pulse 98; temperature 100.5°.

12 m. Same. Pulse 84; temperature 99.3°.

4½ p. m. " " 88; " 99.5.

September 22d, 6 a. m. Rested well. General condition as when last seen; passed a small quantity of slime and blood per anum, with some griping pain. Pulse 72; temperature 98.5°.

12 m. Same. Two more stools similar to that in the night. Pulse 72; temperature 98.5°.

5½ p. m. Same, but two more dysenteric stools. Pulse 82; temperature 99°.

September 23d, 6 a. m. In excellent condition, though she has had still another stool of same character as last. Pulse 80; temperature 99°.

1 p. m. Bowels again moved twice, stools of same nature. Pulse 82; temperature 99.4°.

6 p. m. Same. Pulse 80; temperature 99.2°.

September 24th, 7 a. m. Slept well; only one bloody stool; Pulse 88; temperature 100.5°.

12 m. Same. Bowels quiet. Pulse 92; temperature 100.5°.

7 p. m. " " " " 88; " 100.5.

September 25th, 6½ a. m. Same. Pulse 92; temperature 99.5.

7 p. m. " " 90; " 99.

September 26th, 8 a. m. Feels better. " 72; " 99.

8 p. m. Same. " 86; " 99.

September 27th, 10 a. m. Better. " 76; " 98.5.

" 28th, 7 a. m. Convalescent. " 74; " 98.5.

Case VIII.—Lieut. L., of Engineers' Department, white, aged 32; very plethoric, and a free drinker; native of Baltimore, Maryland.

September 23d, 7 a. m. Felt badly since 10 o'clock last night; violent cephalalgia and rachialgia; head and skin hot; eyes burning and suffused; has vomited once—bile and water. Pulse 112; temperature 102.8°.

1 p. m. Same. Pulse 112; temperature 103.5°.

6½ p. m. " " 114; " 104.2.

10 p. m. " " 128; " 104.

Bowels have acted freely from a cathartic given at first visit.

September 24th, 7½ a. m. Had a tolerably good night; head less hot; pain much better. Pulse 100; temperature 101.8°.

12 m. Same. Pulse 96; temperature 102°.

7 p. m. Some headache and backache; skin moist but warm; eyes injected. Pulse 80; temperature 103°.

September 25th, 8 a. m. Had some nasal hæmorrhage in night, and some nausea which still exists after taking anything on the stomach; skin and head cool and moist; tongue furred and red on edges, but gums tolerably firm; eyes red; feels a little restless. Pulse 82; temperature 101°.

12 m. Same. Pulse 96; temperature 101.5°.

7½ p. m. Same, except that now the gums are spongy. Pulse 88; temperature 101°.

September 26th, 7 a. m. Calm and quiet; symptoms same. Pulse 80; temperature 99.8°.

12 m. Feels better; less nausea. Pulse 68, temperature 98.5°.

6 p. m. Same. Pulse 80; temperature 99.7°.

September 27th, 7 a. m. Improving. Pulse 76; temperature 99°.

12 m. Same. " 64; " 98.5°.

6 p. m, " " 72; " 98.5.

Sept. 28th, 8 a. m. Slept well. Pulse 60; temperature 98.5°.
12 m. "Feels well." " 64; " 98.6.
7 p. m. " " " 68; " 98.2.

September 29th, 7 a. m. Convalescent, though still a slight feeling of nausea if he eats the least too much. Pulse 72; temperature 98.5°.

12 m. Same. Pulse 74; temperature 98.5°.
7 m. " " 76; " 98.5.

Note.—The urine of this patient was examined every morning, and no albumen was ever found.

Case IX.—W. H. S., Lieut. of Marines, aged 26, white; native of New York; rather spare; been very much broken down with nursing and fatigue, a death having occurred in his quarters, and his wife being now sick; exceedingly despondent and alarmed.

September 24th, 10 a. m. Taken sick last night at his present quarters, six or eight miles from the navy yard; high fever; eyes suffused; head hot; skin same but moist; very restless, and suffers very much from head and back. Pulse 120; temperature 104.4°.

6 p. m. (Too far for a mid-day visit.) Same. Three stools from a saline I had ordered. Pulse 110; temperature 104°.

September 25th, 7½ a. m. "Slept well;" symptoms same. Pulse 88; temperature 104.4°.

6½ p. m. Been quite restless; bowels inclined to run off; three operations; eyes very red; skin moist and hot; very despondent. Pulse 90; temperature 104°. Urine tested and no albumen found in it.

September 26th, 10 a. m. Restless night, but head and skin cooler; eyes red; gums spongy. Pulse 84; temperature 102°.

5¼ p. m. Pain in pit of stomach; respiration distressed—36 to the minute; nervous and despondent. Pulse 84; temperature 101°. Albumen in urine in moderate degree.

September 27th, 7½ a. m. A very restless night—almost constant hiccough; five or six black stools in the night and early morn; examined the last and pronounce it hæmorrhage. Respiration 42; pulse 92; temperature 101. Passed no urine. (The patient died at about 2 o'clock.)

Case X.—D. W. C. McC., Sergeant of Marines, white, aged 30; native of New York; in South 6 years: robust and healthy looking.

September 24th, 11 a. m. Taken sick about 8 or 9 o'clock this morning; cephalalgia and rachialgia violent; eyes suffused and head hot; skin hot and moist; restless but not alarmed. Pulse 110; temperature 104°.

2½ p. m. Much the same. Pulse 108; temperature 104.5°.

7 p. m. Same. " 108; " 103.5.

September 25th, 8 a. m. Slept tolerably well; symptoms same. Pulse 106; temperature 104°.

12 m. Same. Pulse 104; temperature 104.5°.

5 p. m. " " 92; " 103.

September 26th, 9½ a. m. Slept well; feels better; skin cooler; head still too hot. Pulse 80; temperature 101.5°

4½ p. m. Gums spongy; eyes red; a little restless. Pulse 66; temperature 101.5°.

September 27th, 8 a. m. Better. Pulse 68; temperature 101.5°.

1½ p. m. Same. " 60; " 101.5.

7 p. m. " " 70; " 101.5.

September 28th, 11 a. m. Better; eyes less red; head and skin cool. Pulse 64; temperature 100.5°.

5½ p. m. Same. Pulse 62; temperature 100°.

September 29th, 12 m. Improving. Pulse 56; temperature 99.5°.

September 30th, 8 a. m. Improving. Pulse 60; temperature 99°.

October 1st, 8½ a. m. Convalescent. Pulse 56; temperature 99°.

October 2d, 8½ a. m. Pulse 68; temperature 99°.

Note.—Daily examinations of the urine showed it free from albumen from first to last.

Case XI.—Dr. J. E. M., white, aged 26; native of Pennsylvania; rather spare and quite tall.

September 27th, 7½ p. m. Taken sick at 12 m.; cephalalgia; rachialgia; head hot; eyes watery; surface of body hot but moist. Pulse 108; temperature 103.5°.

September 28th, 9 a. m. Same. Pulse 96; temperature 102.5°.

12 m. " " 84; " 102.5.

6 p. m. Has had three stools from a saline given early in the morning; symptoms same. Pulse 80; temperature 102°.

September 29th, 6 a. m. Same. Pulse 70; temperature 102°.

12 m. " " 72; " 102.

6 p. m. Same. Pulse 76; temperature 102°.

September 30th, 6 a. m. Head still a little too hot; other symptoms same. Pulse 60; temperature 100°.

11½ a. m. Same. Pulse 56; temperature 100°.

7 p. m. " " 56; " 101.

October 1st, 6 a. m. Better. Pulse 48; temperature 99°.

12 m. " " 48; " 99.

5½ p. m. " " 48; " 99.5.

October 2d, 6 a. m. Improving. " 52; " 98.5.

12 m. Convalescent. " 54; " 98.5.

6 p. m. " 52; " 98.5.

No albumen was ever found in the urine in this case.

Case XII.—Com. M. B. W., white, aged 55; native of ———; Feeble and dyspeptic; quite thin.

September 28th, 8½ p. m. Head and backache most of the day yesterday, increasing towards night; feeling of nausea all night; vomited a seidlitz powder taken about 8 o'clock last evening, and at intervals all night; eyes injected. Pulse 100; temperature 98.5°.

12 m. Same, except skin hotter and moist, and head hot. Pulse bounding and 104; temperature 100°.

7½ p. m. Same. Pulse 104; temporature 101.5°.

September 29th, 6 a. m. Had a tolerable night; general symptoms same. Pulse 96; temperature 102°.

1 p. m. Same. " 96; " 101.5.

6 " " " " 96; " 101.5.

September 30th, 6 a. m. Bad night, from bowels griping and moving frequently—a slimy light colored fluid only passing. Pulse less bounding and 84; temperature 100.5°.

11 a. m. Three more actions. Pulse 80; temperature 101°.

5 p. m. Same. Pulse 88; temperature 101°.

October 1st, 6 a. m. Skin and head cooler; tongue of a *dark* red, almost purplish hue; takes but little nourishment; wanders slightly at times, but slept at intervals. Pulse 92; *temperature* 97.4°.

9 a. m. Worse; can keep nothing on stomach—subsultus tendinum; manner very agitated, even when attention fully awake; more low delirium, but mind still intact if aroused. Pulse 86; temperature could not be taken without agitating him too much, He continued to fail, emitted black vomit at intervals all day, and died at midnight.

Case XIII.—Lieut. Commander K., white, aged 31; native of Maine; very delicate appearance, and suffering from phthisis; habits temperate.

September 28th, 12 m. Feeling sick since last night; now violent headache and backache; eyes watery; head hot; skin hot, and sometimes dry and again moist. Pulse 126; temperature 102.5°.

6 p. m. Much the same. Pulse 100; temperature 102.1°.

September 29th, 6½ a. m. Had a good night, but tongue dry and furred in centre; eyes injected; head and skin hot yet moist; bowels been well opened. Pulse 100; temperature 103°.

12 m. Same. " 94; " 103.6.

7 p. m. " " 104; " 105.

September 30th, 6 a. m. Slept a little, but was very restless at times and anxious; head and skin cooler. Pulse 96; temperature 103.5°.

12 m. Same. Pulse 88; temperature 103.8°.

5½ p. m. " " 80; " 104.2.

Albumen in slight quantity in urine passed to-day.

October 1st, 6 a. m. Rested tolerably well; took rice gruel three or four times; head and skin cooler to touch; eyes red; tongue covered with a very white fur; manner nervous and anxious. Pulse 72; temperature 102°.

12 m. Has had a little irritability of stomach, vomited once, otherwise the same. Pulse 76; temperature 102.5°.

5 p. m. Feels better. " 76; " 103.2.

October 2d, 6 a. m. Much the same. Pulse 76; temperature 101°.

12 m. Much the same. Conjunctivæ yellow; urine very dark, of yellowish hue, and full of albumen. Pulse 80; temperature 102.2°.

4 p. m. Same. Skin jaundiced. Pulse 76; temperature 102.4°.

October 3d, 7 a. m. Has been very restless and despondent, but more quiet since daylight; jaundice deepened. Pulse 84; temperature 102.2°.

12 m. Same Pulse 84; temperature 102.6°.

7 p. m. Anxious and restless; has considerable subsultus tendinum of muscles of extremities. Pulse 100; temperature 103°. Urine loaded with albumen.

October 4th, 6 a. m. Had but little sleep; had a slight nasal hæmorrhage; subsultus more severe; dozes and wakes up in

great alarm; wanders in mind when attention not specially engaged, but takes nourishment regularly about every two hours and retains it. Pulse weak and 100; temperature 102.2°.

12 m. Same. Jaundice excessive. Pulse 100; temperature 102.1°.

6 p. m. Weaker and more delirious; subsultus extends to muscles of face and jaws; very restless; nose bleeds at intervals; urine free, but very dark yellow and loaded with albumen. Pulse 110, and so much subsultus and restlessness that it is impracticable to take the temperature, especially as the attempt to do so annoys him very much.

October 5th, 6 a. m. Very restless and confused; tongue dry and dark red; sordes on lips and gums. Pulse 110 and very feeble. No urine since 6 o'clock yesterday afternoon.

2 p. m. Worse; black vomit and black stools in abundance; violent jactitation. No observations of pulse or temperature could be taken without exciting him still more.

He died at 7½ a. m., October 6th—9th day of sickness.

Case XIV.—Lieut. E. F. McE., white, aged 27; native of Philadelphia; quite robust, and till recently has drank pretty freely.

September 28th, 10 a. m. Woke up this morning with great pain in back and head; head hot; eyes suffused and injected; skin hot but moist. Pulse 120; temperature 102.1°.

6 p. m. Much the same. Pulse 120; temperature 102.1°.

September 29th, 6½ a. m. Head and skin cooler; tongue whitish; had some sleep, and feels better. Pulse 96; temperature 100.5°.

12 m. Feels better. Pulse 80; temperature 102 2°.

6 p. m. Same. " 82; " 103.5.

September 30th, 6 a. m. Slept well; takes his rice gruel and retains it comfortably—otherwise about the same. Pulse 84; temperature 100°.

11½ a. m. Same. Pulse 76; temperature 100°.

5 p. m. " " 84; " 100.5.

October 1st, 6½ a. m. Rested well; tongue whitish; head and skin cool; eyes less red; takes and keeps nourishment. Pulse 64; temperature 98.5°.

12 m. Same. Pulse 64; temperature 99°.

5 p. m. " 60; " 99.

October 2d, 6 a. m. Improving. Pulse 72; temperature 98.5°.

12 m. Feels well. Pulse 72; temperature 98.5°.

6 p. m. Same. " 60; " 99.

October 3d, 7½ a. m. Slept well; convalescent. Pulse 64; temperature 98.5°.

In this case the urine was examined each day from the first, and no albumen was found at any time.

Case XV.—McM., Paymaster, aged 31; native of ——, (New York?) medium in size and flesh.

October 2d, 11 a. m. Taken sick last night; head hot; eyes watery and injected; pain in limbs and back; bowels opened to-day by a saline. Pulse 120; temperature 102.5°.

6 p. m. Same. " 104; " 103.5.

October 3d, 7 a. m. Slept tolerably; no special change. Pulse 92; temperature 104°.

12 m. Same. Pulse 96; temperature 104.3°.

7 p. m. " " 96; " 104.3.

October 4th, 6½ a. m. Very restless night; eyes still redder; head and skin still hot—latter only at times moist; tongue dry and furred. Pulse 88; temperature 104.2°.

12 m. Same. Pulse 96; temperature 104.1°.

6 p. m. " " 96; " 104 1.

Urine examined to-day for first time, but no albumen found.

October 5th, 6 a. m. Had a bad, restless, and comparatively sleepless night; very irritable; head not so hot; tongue red all over and rather dry. Pulse 80; temperature 103.4°.

1½ p. m. Same. " 80; " 103.2.

6 p. m. Same. Has vomited a little simple fluid. Pulse 80; temperature 102 1°.

Albumen is found in to-day's urine.

October 6th, 6½ a. m. Cooler and calmer, but has frequent bilious passages, and still vomits at times contents of stomach simply. Pulse 80; temperature 102°.

11½ a. m. More quiet, but has had no bilious actions; vomited water once. Pulse 80; temperature 101.4°.

5 p. m. No more actions; is rather restless again. Pulse 84. temperature 103°.

Urine full of albumen.

October 7th, 7 a. m. Had better night; skin cooler and moist; no more actions of same kind. Pulse 84; temperature 101°.

12 m. Feels better. " 84; " 101.5.

7 p. m. Same. " 96; " 101.5.

Urine still heavily charged with albumen.

Oct. 8th, 6½ a. m. Slept well; tongue clean; no longer restless, and "feels as if getting well." Pulse 84; temperature 102.2°.

12 m. Still better. Pulse 84; temperature 100.2°.

7 p. m. Same. " 100; " 98.6.

A little albumen still in urine.

As I left the navy yard the next day I took no further notes in this case, but learned by letter from Acting Assistant Surgeon Martin, left in charge, that his convalescence progressed favorably towards a permanent recovery.

Case XVI.—M. E. M , gardener, white, aged 19; native of New Orleans, of German parentage, but never had yellow fever! Robust.

October 5th, 12 m. Cephalalgia, and rachialgia all the morning; head hot; eyes suffused; skin hot and rather dry. Pulse 100; temperature 103°.

6 p. m. Same. Pulse 90; temperature 103.6°.

October 6th, 8 a. m. Feels better; head and skin cooler, several free actions from the medicine (seidlitz powders); slept tolerably. Pulse 84; temperature 102°.

12 m. Same. " 86; " 102.

October 7th, 7 a. m. Slept tolerably; one more action. Pulse 100; temperature 104.7°.

6 p. m. (Could not pay him a mid-day visit.) Much the same as at last visit. Pulse 110; temperature 103.5°.

October 8th, 8 a. m. Slept well; feels much better. Pulse 84; temperature 101°.

12 m. Better. Pulse 86; temperature 101°.

7 p. m. " " 78; " 100.

The urine in this case was not examined. Dr Martin took charge of it from this date, and reported a fair and steady convalescence.

The following case was found in the hospital convalescing when Dr. Martin and myself arrived, and the notes are copied from the hospital books, and the observations were taken by Surgeon J. B. Ackley, who at first diagnosed it as "febris remittens." His own death followed not long afterwards from yellow fever:

Case XVII.—A. C , private of marines; native of England; age 22; admitted from U. S. Marine Barracks September 6th, 1874. Eyes suffused. Pulse 96; temperature 101.6°. R—

Hot bath at once, and quiniæ sulph. ℈i half hour after. Pain in head and limbs very severe.

September 7th, 1874. Continues in about same condition; skin acting freely; pain in head and extremities less severe; eyes yet suffused; ordered warm drinks to promote diaphoresis; kidneys acting freely. ℞—Potass. bicarb. grs. xxx, spts. nit. dul. ʒi, buchu ext. fl'd ʒi, every two hours.

September 8th, 1874. Has still considerable fever; eyes yet suffused; pain in head continues. Temperature 102°. Bowels open; urine free.

September 9th, 1874. Very much improved; suffusion of eyes nearly gone; pain in head and limbs much less. Pulse 88; temperature 99.6°. Urine scanty and traces of albumen; ordered potass. bicarb. ℈ss every four hours; warm drinks continued.

September 10th, 1874. Doing very well; fever abating; traces of albumen in urine. ℞—Potass. bicarb. ℈ss every four hours. Temperature 99.6°. Warm drinks continued.

September 11th, 1874. Doing very well; fever abated; urine free; bowels constipated. ℞—Seidlitz powder, at once. Temperature 98.5°. Continue potass. bicarb. and warm drinks.

September 12th to 21st. Convalescent; light and nourishing diet.

September 22d. Discharged to duty.

The notes of this case are also derived from the books of the hospital, and were written by the accomplished Assistant Surgeon, Dr. G. B. Todd, who also soon fell a victim to the disease, being desperately ill in an adjoining village when we arrived.

Case XVIII.—John B. Ackley, Surgeon U. S. N.; native of New Jersey; age 39 years.

Sept. 7th, 1874, 5. p. m. Patient was violently attacked at 4 p. m. this day, with severe pain in the head, especially in the orbits, back and limbs; had a severe chill, followed by a severe fever. Pulse 120; temperature 103.6°. Put him in warm mustard bath (110°) ℞—Hyd'g. chlor. mit. grs. xij at once; wrapped him warmly in bed; produced profuse perspiration; pain in head and back soon relieved.

September 8th, 1874. Perspiration continued all day; has still considerable fever. Pulse 100; temperature 102°. Bowels have been freely opened; urine in full quantity, loaded with albumen.

℞—Potass. bicarb. ʒss, olei terebinthinæ gtt. xx. M. et. S at once. Has only slight pain in head and limbs to-day.

September 9th, 1874—a. m. Somewhat improved; continues in a gentle perspiration; bowels opened; urine plentiful, but a trace of albumen. Temperature 101.6°; pulse 88.

P. M. Complains of great prostration ℞—Quiniæ sulph grs. ij every 6 hours; continue warm drinks and watch closely.

September 10th, 1874—a. m. Passed a very restless night; slept very little; no evacuations from the bowels, which this morning gave him pain. ℞—Seidlitz powder no. j, which produced copious and very offensive stools. Perspired very little during the night; kidneys acting slowly; urine albuminous and acid. At 5 30 a. m. temperature 101.2°. After the action of the seidlitz powder temperature 100°, which gradually increased to 104° Ordered warm drinks, extremities to be bathed in hot water, and ℞—digitalis ext. fl'd ♏ iij, æther spts. nit. ʒss every two hours. Temperature at 12 m. 104°.

4 p. m. Has had a very high temperature since 1 p. m.—105°. Have given him the digitalis and æther regularly, and though he has voided urine frequently, it has been loaded with albumen. Complains of considerable uneasiness in lower portion of abdomen—ordered hot bottles in each iliac region.

September 11th, 1874—a. m. Passed a quiet night, sleeping at times; voided about 20 ounces of urine; kept in a moderate perspiration all night; took the digitalis regularly every 2 hours. Temperature at 8 a. m. 105.2°. ℞—Wine whey, as the heart's action is very feeble. Pulse 99. Discontinued the digitalis.

12 m. Became delirious; refused to take anything; skin very hot and moist. Temperature 104.6°. Urine very scant.

1 p. m. Temperature 106.2.°. Muttering delirium; urine entirely suppressed; no evacuation from bowels since last evening Pulse 128, very weak and easily compressed. Congestion of the capillaries was noticed at 2 p. m., and he sank rapidly until 3.45, when he died.

The notes of this case are also copied from the hospital books, having occurred before our arrival, and having been treated by Dr. Todd.

Case XIX.—Charles E. Peret, private of marines; native of United States; age 23. Admitted from U. S. Marine Barracks, September 9th, 1874. When admitted, temperature 105°; pulse

120; skin hot and dry; tongue furred and tremulous. ℞—Mustard foot-bath (110); hyd'g chlor. mit. grs. xv, followed in 3 hours by olei ricini ℥jss; covered patient warmly in bed, but produced only slight diaphoresis; very restless.

September 10th, 1874—a. m. Had a very restless night; severe pain in both iliac regions. Pulse 130; temperature 105°. skin hot and dry; although all possible means were tried to produce perspiration, had but meagre success; tongue dry and tremulous; had free evacuations from bowels; kidneys moderately active; had his feet placed in warm water—hot bricks to extremities.

September 11th, 1874. Skin hot and dry, despite all I can do; petechial spots appearing all over the body. Pulse 130; temperature 105.2°. Very restless and anxious at times; some delirium; urine suppressed; considerable nausea. Applied turpentine stupes over kidneys. ℞—Creosote gtt ij, mucil. acaciæ ℥ij M. et S. Teaspoonful every 2 hours. Applied emplas. cantharides over epigastrium, and administered potass. bicarb. ʒss every 2 hours.

September 12th, 1874. No improvement; not quite so much nausea; urine voided once in 24 hours—about ʒij, loaded with albumen; blister drew very well—was filled with yellow serum; dressed it with simple cerate; not one favorable symptom. Continue the potass. bicarb. Distinct increase in number and size of discolored patches. Continue creosote mixture.

September 13th, 1874. Much worse; case evidently a fatal one. Temperature at 9 a. m. 106°. Skin persistently hot and dry; discolored patches increased in size, some an inch in diameter; urine entirely suppressed; no evacuations from the bowels; delirious; at 10 a. m. ejected from the stomach about one quart of a dark grumous fluid, filled with flakes of epithelium and blood, evidently "black vomit," which was repeated at invals until 6.50 p. m., when he died.

Case XX.—William Fluke, sergeant of marines; native of New Jersey; age 25; admitted from U. S. Marine Barracks, September 19th, 1874.

1.30 a. m. Head hot; skin hot and dry; eyes and face suffused; intense cephalalgia and rachialgia. Pulse 120; temperature 105°. ℞—Seidlitz powder every hour until purged; cold applications to head; hot foot-bath; warm teas in moderation.

6 a. m. Very restless; other symptoms same. Pulse 140; temperature 105°.

12 m. About the same. Pulse 140; temperature 105°. ℞—Tinct. aconiti rad. gtt. jss every hour.

7 p. m. Bowels moved freely- other symptoms same. Pulse 124; temperature 105°. Increased aconite to gtt. ij every hour.

September 20th, 7 a. m. Tongue and gums remarkably red; eyes very much suffused; skin rather moist and cooler. Pulse 92; temperature 101.8°. ℞—Chicken tea or gruel every 2 hours. Stop aconite.

12 m. General symptoms same. Pulse 94; temperature 103.5°.

7 p. m. " " " 104; " 105.2.

Urine free of albumen—sp. gr. 1023.

September 21st, 7 a. m. Head hot; tongue, gums and eyes very red. Pulse 88; temperature 103.8°.

12 m. Much the same. Pulse 100; temperature 103.8°.

6 p. m. " " " 88; " 103.5.

Urine contains no albumen—sp. gr. 1021.

September 22d, 7 a. m. Somewhat improved. Pulse 104; temperature 102°.

12 m. Same general condition. Pulse 104; temperature 102°.

6 p. m. " " " " 88; " 102.1.

Urine heavily loaded with albumen—sp. gr. 1019.

September 23d, 6 a. m. Symptoms slightly improved. Pulse 84; temperature 101.5°.

12 m. About the same. Pulse 84; temperature 101.5°.

6 p. m. " " " 84; " 102.5.

Urine albuminous—sp. gr. 1015.

September 24th, 7 a. m. Comfortable night; calm and quiet. Pulse 84; temperature 102°.

12 m. Calm and quiet. Pulse 92; temperature 101.5°.

6 p. m. " " " 80; " 103.2.

Still has spongy gums and very red tongue. Urine heavily loaded with albumen—sp. gr. 1020.

September 25th, 6 a. m. Somewhat of a restless night; tongue still red and pointed; gums still red and spongy. Pulse 96; temperature 102°.

12 m. General symptoms about the same. Pulse 96; temperature 102°.

6 p. m. Head and skin not quite so warm; conjunctivæ yellow; all symptoms slightly improved. Pulse 88; temperature 102.5°.

Urine heavily loaded with albumen—sp. gr. 1016.

Sept. 26th, 6 a. m. Improving. Pulse 92; temperature 101°.

12 m. Improving. Pulse 72; temperature 100.5.
6 p. m. " " 72; " 100.
Urine loaded with albumen—sp. gr. 1017.
Sept. 27th, 6 a. m. Improving. Pulse 64; temperature 98.5°.
12 m. " " 60; " 98.5.
6 p. m. " " 72; " 98.5.
Urine loaded with albumen—sp. gr. 1017.
Sept. 28th, 6 a. m. Improving. Pulse 64; temperature 98.5°.
12 m. " " 60; " 98.5.
6 p. m. " " 72; " 98.5.
Urine slightly albuminous—sp. gr. 1017.
September 29th to October 2d. Convalescent.
October 3d. Discharged to duty.

Case XXI—Philip Kellar, cabin steward; native of Germany; age 23½ years; admitted from U. S. Str. Manhattan, at 2.30 p. m. September 20th, 1874.

2½ p. m. Skin hot; eyes injected; headache and backache; great nervous excitement. Pulse 128; temperature 103.2°.
7 p. m. much the same. " 124; " 103.4.
Urine free of albumen—sp. gr. 1021.

September 21st, 8. a. m. Had a good night; slight pain in back and head. Pulse 88; temperature 102°.
12 m. Much the same. Pulse 92; temperature 101.5°.
6 p. m. " " " 92; " 103.

September 22d, 6 a. m. Not so well; restless. Pulse 80; temperature 102°.
12 m. Feels about the same. Pulse 84: temperature 103°.
6 p. m. " " " " " 94; " 104.
Urine quite albuminous—sp. gr. 1027.

September 23d, 6 a. m. Skin and head warm but moist. Pulse 82; temperature 103.2°.
12 m. Feels about the same. Pulse 112; temperature 104°.
6 p. m. " " " " 88; " 105.
Urine albuminous—sp. gr. 1027.

September 24th, 6 a. m. Had a good night; head still hot; skin moderately cool. Pulse 84; temperature 103°.
12 m. Much the same. Pulse 80; temperature 103°.
6 p. m. " " " 88; " 104.
Urine free of albumen—sp. gr. 1021.

September 25th, 6 a. m. Had a good night; tongue furred; skin cool. Pulse 68; temperature 99°.

12 m. Much the same. Pulse 72; temperature 100°.
6 p. m. " " " 68; " 102.5.
Urine free of albumen—sp. gr. 1024.
Sept. 26th, 7½ a. m. Improving. Pulse 72; temperature 101°.
12 m. " " 60; " 99.
6 p. m. " " 64; " 99.5.
Urine free of albumen—sp. gr. 1015.
Sept. 27th, 6 a. m. Improving. Pulse 60; temperature 98.5°.
12 m. " " 74; " 98.5.
6 p. m. " " 60; " 98.8.
Urine free of albumen—sp. gr. 1018.

September 28th to October 3d. Urine at times slightly albuminous.

October 4th to 10th. Convalescent.

October 11th. Discharged to duty.

Case XXII.—C. W., seaman; native of Germany; age 29; admitted from U. S. Str. "Rose," at 9 a. m. September 20th, 1874. Has been a walking case for the last four days. Pains in back, head and limbs; constipation. Pulse 80; temperature 100°. ℞—Olei ricini ℥jss S. at once; mustard foot-bath; warm teas.

5½ p. m. Much the same. Pulse 80; temperature 100°. Is also troubled with valvular disease of heart.

September 21st, 6 a. m. Improving; skin moist and warm. Pulse 52; temperature 99°.

7 p. m. About same. Pulse 52; temperature 99°.

Urine free of albumen—sp. gr. 1018.

Sept. 22d, 6 a. m. Improving. Pulse 52; temperature 98.5°.
7 p. m. " " 48; " 98.5.

September 23d to October 2d. Convalescent; light and nourishing diet.

Urine loaded with albumen—sp. gr. 1019—until October 1st.

October 3d. Discharged to duty.

Case XXII.—A. J. (colored), laborer; native of New York; age about 25; admitted from U. S. S. Ajax, at 11½ p. m. September 21st, 1874. Skin hot and dry; very restless; delirious; eyes red and watery. Pulse 128; temperature 102°.

September 22d, 7 a. m. Bowels moved; still delirious; skin cool and moist. Pulse 68 and very irregular; temperature 98.5°.

12 m. Much the same. Pulse 92; temperature 100.5°.

6 p. m. Skin hot again, but moist; tongue furred. Pulse 88; temperature 103°.

Urine full of albumen—sp. gr. 1025.

September 23d, 6 a. m. Still delirious; head hot; skin inclined to be dry. Pulse 72; temperature 104°.

12 m. Much the same. Pulse 84; temperature 105.4°.

6 p. m. " " " 88; " 105.

Urine full of albumen—sp. gr. 1025.

September 24th, 6 a. m. Had some sleep; still delirious; head hot; skin warm. Pulse 64; temperature 102.5.

This patient informed us that he could take no sudden exercise without palpitation of heart and shortness of breath. Auscultation gives no positive evidence of organic disease.

12 m. About the same. Pulse 80; temperature 104°.

6 p. m. About the same; ejected about 4 ounces of black vomit. Pulse 96; temperature 102.5°.

Urine full of albumen—sp. gr. 1025.

September 25th, 6 a. m. Restless night; delirious; nasal hemorrhage; tongue and gums red. Pulse 60; temperature 99°.

12 m. About the same. At 8½ a. m. ejected about half pint of black vomit. Pulse 68; temperature 99.8°.

6½ p. m. Somewhat improved; skin cool and pleasant; tongue and gums improved; stomach quiet. Pulse 68; temperature 101.5°.

Urine slightly albuminous—sp. gr. 1005.

September 26th, 6 a. m. Irritable stomach. Pulse 68; temperature 99°.

12 m. Ejected about half pint of black vomit; delirious; delirium gradually becoming wilder; great jactitation; almost impossible to keep him in bed. No observation on temperature possible, owing to restlessness.

6 p. m. Same condition; passing black vomit; no observation on pulse or temperature.

Urine free of albumen—sp. gr. 1009.

September 27th, 6 a. m. Still wildly delirious; same condition all day.

September 28th. Remained in same condition until he died at 4½ p. m.

Case XXIV.—F. M., private of marines; native of Germany; age 30 years; admitted from U. S. Marine Barracks, at 2.10 p. m. September 21st, 1874. Pain in back and calves of legs; intense cephalalgia. Pulse 108; temperature 105°.

6 p. m. About same. Pulse 108; temperature 104.5°.

September 22d, 6 a. m. Restless and nauseated; pains somewhat relieved. Pulse 100; temperature 104.5°.

12 m. Same symptoms. Pulse 101; temperature 104°.

6 p. m. " " " 96; " 103.8.

Urine very heavily loaded with albumen—sp. gr. 1013

September 23d. Extremely restless; great mental excitement; head hot. Pulse 112; no observation on temperature.

12 m. Wild and delirious; at 11.45 a. m. ejected about one pint of black vomit; owing to extreme restlessness was unable to take the temperature.

Urine heavily loaded with albumen—sp. gr. 1013.

Died at 2.10 p. m.

The following cases, as far as No. 28, are from the case book of Dr. Martin, who has kindly furnished them to me. We attended the patients together, and the observations were made with equal care as in the others.

Case XXV.—M. McG., white; male; aged 32; plethoric—weighing 200 pounds.

September 23d, 10 a. m. Has been sick five days; was considered recovering till this morning; has had no physician; now slightly delirious; head and skin hot but moist; tongue furred and red on edges. Pulse 92; temperature 103.5°. ℞—Seidlitz powder every hour till purges; orange leaf tea, etc.

4½ p. m. Eyes red; slightly jaundiced; restless; otherwise much the same; had two actions. Pulse 92; temperature 103.5°.

September 24th, 10 a. m. Restless till midnight, and then had three or four hours sleep; symptoms much the same as last night. Pulse 92; temperature 103.5°.

7 p. m. Somewhat more quiet. Pulse 88; temperature 103°.

September 25th, 7 a. m. Much the same general symptoms; restless; had a rather wakeful night. Pulse 88; temperature 103°.

7 p. m. Head not so hot; skin cooler; less restless. Pulse 80; temperature 102°.

September 26th, 7½ a. m. Good night; head and skin cooler; conjunctivæ yellow; jaundice more apparent, though not excessive; gums slightly spongy. Pulse 80; temperature 101.5°.

5 p. m. Tongue furred; skin cool and moist; other symptoms same. Pulse 72; temperature 102°.

September 27th, 9 a. m. Better; had a good night. Pulse 80; temperature 101°.

6 p. m. Improving. Pulse 80; temporature 100°.

September 28th, 8 a. m. Still improving. Pulse 84; temperature 100°.

5 p. m. Improving. Pulse 80; temperature 99°.

Sept. 29th, 7 a. m. Improving. Pulse 80; temperature 99°.

6 p. m. " " 72; " 100.

Sept. 30th, 7 a. m. " " 72; " 99.

October 1st, 9 a. m. " " 80; " 100.

October 2d, 9 a. m. " " 80; " 100.

October 3d, 9 a. m. " " 80; " 99.5°.

Ceased to visit patient, who, however, continued steadily to convalesce, and recovered.

Case XXVI.—E. M., female; white; aged 13; native of Alabama.

September 23d, 6 p. m. Taken sick at daylight this morning, with violent pains in back, head and stomach. Has taken calomel gr. vi, followed in eight hours by dose of oil, which has acted three times. Head and skin hot, but wet with a profuse perspiration; eyes suffused; tongue furred. Pulse 120; temperature 104.5°.

September 24th, 8 a. m. Head and skin hot and moist; tongue furred; stomach irritable; slept tolerably well. Pulse 120; temperature 104°.

6 p. m. Same. Pulse 128; temperature 104°.

September 25th, 8 a. m. Restless night; one free action from bowels; tongue furred and dry. Pulse 124; temperature 102°.

6 p. m. Skin cool and moist; stomach still quite irritable; tongue still furred, and now red on edges. Pulse 104; temperature 101°.

September 26th, 8 a. m. Had better night, but stomach still irritable—vomits if the least over crowded. Pulse 108; temperature 99.5°.

6 p. m. Very irritable stomach; otherwise improving. Pulse 112; temperature 100.5°.

September 27th, 8 a. m. Better; tongue cleaner; stomach less irritable. Pulse 84; temperature 99.5.

September 28th Continued to improve.

September 29th. Convalescent.

Case XXVII.—C. T., white; female; widow; aged 50; native of Alabama.

September 24th, 10 a. m. Taken last night with violent pains

in back and head; skin hot and dry; eyes suffused: tongue furred and dry. Pulse 104; temperature 104°.

6 p. m. About the same. Pulse 88; temperature 104°.

September 25th, 8 a. m Bowels moved freely by the seidlitz powders; skin now moist; tongue soft, but still furred. Pulse 80; temperature 102.4°.

6 p. m. Eyes injected; otherwise much the same. Pulse 88; temperature 104.5°.

September 26th, 8 a. m. Much the same as last night; tongue inclined to get dry again. Pulse 80; temperature 103°.

6 p. m. Bilious vomiting during the morning; head hot, but rest of surface cool and moist. Pulse 80; temperature 103°.

September 27th, 8 a. m. Slept tolerably well and feels better; head still rather warm—rest of surface cool; tongue soft but somewhat furred. Pulse 72; temperature 101°.

6 p. m. Improving. Pulse 72; temperature 101.3°.

September 28th, 8 a. m. Slept well; head cool, and skin pleasant. Pulse 72; temperature 101°.

6 p. m. Better. Pulse 88; temperature 101.1°.

Sept. 29th, 8 a. m. Better. Pulse 100; temperature 100°.

" 30th, " " " 68; " 99

October 1st. Convalescent.

Case XXVIII.—E. T., white; female; aged 18; native of the village of Warrenton (just outside the navy yard). Here in 1867 (when the yellow fever was here), but had no fever that summer.

September 28th, 5 p. m. Taken sick this morning; cephalalgia and rachialgia; head hot; eyes watery; skin hot but moist. Pulse 124; temperature 102°.

September 29th, 8½ a. m. Tolerable night, with some sleep; much the same general symptoms. Pulse 104; temperature 101°.

5½ p. m. Same, except that tongue is now furred and red on edges. Pulse 104; temperature 102°.

September 30th, 8½ a. m. Much the same. Pulse 96; temperature 100.5°.

5 p. m. Seems a little excited. Pulse 100; temperature 102.5°.

October 1st, 10 a. m. Good night; feels better; tongue cleaner and symptoms generally indicate improvement. Pulse 88; temperature 100.5°.

October 2d. Still improving. Pulse 76; temperature 99.7°.

From this date the convalescence progressed steadily towards recovery.

The following case occurred after I left, and was attended by Dr. Martin, to whom I am indebted for the notes.

Case XXIX.—W. A., white; male; aged 21; machinist; native of Florida; employed on U. S. Steamer "Rose."

October 16th, 10 p. m. Had a chill about 3 p. m. yesterday; pain in head and back; head warm; eyes suffused; tongue furred and dry; skin hot. Pulse 116; temperature 103.5°. R—Seidlitz powder every hour till purged; mustard foot-bath; warm orange leaf tea, etc.

October 17th, 8 a. m. Restless night. Much the same. Pulse 84; temperature 100.5°.

6 p. m. Same. Pulse 84; temperature 102.5°.

October 18th, 8 a. m. Eyes less watery; tongue soft and moist; pains less. Pulse 72; temperature 103°.

6 p. m. Slowly improving. Pulse 72; temperature 102.5°.

October 19th, 8 a. m. Good night; pains gone; other symptoms better. Pulse 64; temperature 100°.

6 p. m. Better. Pulse 52; temperature 99.6°.

October 20th, 8 a. m. Better. Pulse 56; temperature 98.5°.
6 p. m. " " 52; " 98.5.
Oct. 21st, 8 a. m. Convalescent. " 48; " 98.5.

Case XXX.—L. D.; white; male; aged 25; native of France.

September 18th, 7 a. m. Fourth day of sickness; salivated; eyes yellowish and suffused; head rather hot; skin warm and moist. Pulse 80; temperature 100.2°. R—Chicken broth or beef tea ℥j every two hour.

12 m. Same. Pulse 90; temperature 102.2°.
7 p. m. " " 96; " 101.
September 19th, 7 a. m. Same. Pulse 82; temperature 100°.
12 m. Same. " 80; " 100.
6 p. m, " " 80; " 100.

Urine examined; no albumen—sp. gr. 1013.

Sept. 20th, 7 a. m. Much the same. Pulse 72; temperature 99.5.

12 m. Head a little hot; somewhat excited; tendency to nausea. Pulse 104; temperature 104°. R—Seidlitz powder every hour until purged.

7 p. m. Had two motions; somewhat relieved. Pulse 76; temperature 100.2°.

No albumen in urine—sp. gr. 1024.

September 21st, 8 a. m. Skin and head warm; was rather excited, on account of a screen falling on him about 7 a. m. Pulse 64; temperature 99.4°.

12 m. Pretty much as above. Pulse 72; temperature 100 5°.

6 p. m. Continued the same. " 104; " 103.8.

No albumen in urine—sp gr. 1022

September 22d, 7 a. m. Slowly improving. Pulse 64; temperature 99.4°.

12 m. Slowly improving. Pulse 72; temperature 100.5°.

7 p. m. Skin and head hot again. Pulse 104; temperature 103.8°.

Salivation still annoys him. No albumen in urine—sp. gr. 1022.

September 23d, 6 a. m. Much the same. Pulse 118; temperature 103.4°.

12 m. Much the same. Pulse 120; temperature 103.4°.

6 p. m. " " " " 88; " 102.

No albumen in urine—sp. gr. 1022.

September 24th, 6 a. m. Slowly improving. Pulse 64; temperature 99.5°.

12 m. Slowly improving. Pulse 72; temperature 100°.

7 p. m. " " " 62; " 100.

Urine contains no albumen—sp. gr. 1022,

September 25th. Convalescent. Light and nourishing diet.

October 26th. Discharged to duty.

The above case would seem to illustrate the disturbing influence of salivation during the convalescence. It agrees with other experience I have had elsewhere.

Case XXXI—T. K., private of marines; white; aged 29; native of United States.

September 23d, 1 a. m. Head and skin hot and moist; pulse rapid; cephalalgia and rachialgia; thirst. R—Seidlitz powder every hour until purged; hot foot-baths; warm teas. Pulse 112; temperature 100°.

September 24th, 6 a. m. Exceedingly restless; suffering from pains in head, stomach and back; skin hot and moist; eyes suffused. Pulse 110; temperature 101.9°. Had four motions from bowels during night.

12 m. Much the same. Pulse 100; temperature 103.°

6 p. m. " " " " 80; " 104.5.

September 25th, 8 a. m. Slept at intervals during night;

other symptoms about the same. Pulse 72; temperature 100.2°.

12 m. Much the same. Pulse 80; temperature 101 5°.

6 p. m. Head cooler; skin cooler and quite moist; eyes injected. Pulse 72; temperature 101.8°.

Sept. 26th, 6½ a. m. Better. Pulse 60; temperature 99.7°.

12 m. No change. Pulse 64; temperature 99°.

6 p. m. " " " 64; " 100.

Sept. 27th, 6 a. m. Improving Pulse 60; temperature 99°.

12 m. " " 56; " 99.5.

6 p. m. " " 68; " 99.2.

Sept. 28th, 6 a m. "

12 m "

6 p. m. "

Urine slightly albuminous all through the attack after the second day—sp. gr. 1024.

September 29th. Convalescent. Light and nourishing diet.

Case XXXII.—F. R.; white male; aged 20; German.

October 1st, 6 p. m. Had a chill about 3 p. m., and at 5 p. m; admitted with violent pains in back and head; skin now hot and wet; eyes injected; constipation. Pulse 144; temperature 105.5°. R—Seidlitz powder every hour until purged; hot mustard-foot-baths; orange leaf tea; rice gruel.

9 p. m. Seidlitz powder not having the desired effect, gave him olei ricini ℥jss.

6½ a. m. No motion; feels better; head and skin less hot. Pulse 104; temperature 102°. R—Enema of soapsuds and salt water.

12 m. Large motion. Pulse 112; temperature 103°

6 p. m. About same. " 108; " 104.

October 3d, 6 a. m. About same. Pulse 96; temperature 102.5°.

12 m. About same. Pulse 96; temperature 102 5°.

6 p. m. " " " 96; " 102.4.

October 4th, 6 a. m. Head and skin cool and moist; had a good night; tongue furred; eyes less injected. Pulse 88; temperature 101.2°.

12 m. About same. Pulse 92; temperature 101.°.5

6 p. m. " " " 92; " 101.

Oct. 5th, 6 a. m. Improving. Pulse 84; temperature 100°.

12 m. " " 68; " 98.8.

6 p. m. " " 76; " 99 5.

October 6th, 6 a. m. " " 64; " 99.

12 m. " " 64; " 99.

6 p. m. Improving. Pulse 60; temperature 99.
October 7th, 6 a. m. " " 60; " 99.5.
12 m. " " 60; " 98.5.
6 p. m. " " 60; " 98.5.

October 8th. Convalescent. Light and nourishing diet.

Case XXXIII—A. C., 1st class fireman; white; age 33; native of Scotland.

October 2d. Admitted at 3 p. m. with violent pain in back and head; skin hot and dry, tongue furred and dry Pulse 106; temperature 105.6°. R—Seidlitz powder ever two hours until purged; hot mustard foot-bath; warm teas.

6 p. m. Bowels moved. Pulse 106; temperature 105°.

October 3d, 6 a. m. Symptoms same. Pulse 80; temperature 102.5°.

12 m. Symptoms same. Pulse 92; temperature 103.2°.
6 p. m. " " " 92; " 102.2.

October 4th, 6 a. m. Restless night; head and skin cool and moist; rapid respiration; eyes yellow; tongue furred and dry; urine suppressed. Pulse 88; temperature 102°.

12 m. Distressed respiration; wild delirium; growing cold. Pulse 96; temperature 101.5°. At 1.10 died in a comatose state.

Case XXXIV.—W. J. M., machinist; age 36; white; native of Pennsylvania.

October 2d. Admitted at 3 p. m. with violent pains in back, head and lower extremities: distressed respiration. Pulse 100, temperature 104.4°. R—Seidlitz powder every two hours until purged; mustard foot-bath and warm teas.

6 p. m. Very restless; no motions. Pulse and temperature not taken.

October 3d, 6 a m. Head and skin warm and moist; tongue furred and dry; R—Potas. chloratis, sodæ bicarb., aa ʒj, aq.. cinnamon aq. aa ʒij. M. et. Tablespoonful every 3 hours, etc. Pulse 104; temperature 104.2°.

12 m. Same. Pulse 100; temperature 104.5°.
6 p. m " " 104; " 104.8.

October 4th, 6 a. m. Head and skin hot and dry; eyes yellow; tongue furred and dry. Pulse 116; temperature 104.5°.

12 m About 2.30 passed 4 oz. black vomit; other symptoms about same. Pulse 128; temperature 102°.

6. p. m. Owing to delirium, subsultus and jactitation, no temperature could be taken. Pulseless.

October 5th, Died at 3.45 a. m.

Case XXXV.—P. M., 1st class fireman; white; age 25; native of England.

October 3d. Admitted at 10 a. m.; skin hot and dry; tongue furred; violent pain in back, head and limbs. Pulse 104; temperature 103.8°. ℞—Seidlitz powder every hour until purged; mustard foot-bath; warm teas.

6 p. m. About the same. Pulse 104; temperature 104.5°.

October 4th, 6 a. m. Restless night; irritable stomach; tongue furred; eyes injected. ℞—Sinapisms to stomach; ice. Pulse 92; temperature 105°.

12 m. About same. Pulse 96; temperature 104.5°.

6 p. m. Complains of an obstinate diarrhœa, with griping. ℞—Tinct. opium camph. ℥jss; mist. cretæ ℥iss. M. et. S. Tablespoonful every two hours until relieved. Pulse 96; temperature 101.5°.

Oct. 5th, 6 a. m. Did not sleep well, but bowels relieved; distressed respiration; restless; delirious. Pulse 118; temperature 101°. Urine suppressed.

8 and 9 a. m. Ejected about 14 oz. of black vomit; in a comatose state.

Died at 3.30 p. m.

Case XXXVI.—H. L., white, aged 23; native of Germany.

October 6th, 5 p. m. Skin hot and wet; eyes injected; cephalalgia; rachialgia. Pulse 126; temperature 104.2°. ℞—Mustard foot-bath; warm teas.

6 p. m. Same. Pulse 126; temperature 104.2°. ℞—Olei ricini ℥jss, S. at once; mustard foot-bath; warm teas.

12 m. Same. Pulse 118; temperature 103.8°. ℞—Rice gruel or chicken tea ℥j every two hours.

October 7th, 6 a. m. Slept well; had one large motion; skin warm; eyes injected. Pulse 88; temperature 102°

12 m. Same. Pulse full, 96; temperature 103 2°.

6 p. m. Intense cephalalgia and rachialgia. Pulse 100; temperature 104.5°. ℞—Hot mustard foot-bath.

October 8th, 6 p. m. Slept well; feels comfortable. Pulse 92; temperature 104.2°.

12 m. Same. Pulse 92; temperature 104°.

6 p. m. " " 84; " 104.

October 9th, 6 a. m. Slept well; other symptoms same. Pulse 76; temperature 101.6°.

12 m. Same. Pulse 76; temperature 102.2°.

6 p. m. " " 76; " 102.2.

October 10th, 6 a. m. Slept well; other symptoms same. Pulse and temperature not taken.

12 m. Pulse and temperature not taken.

6 p. m. " " " " "

Oct. 11th, 6 a. m. Improving. Pulse 78; temperature 101.1°.

12 m. " " 80; " 101 2.

6 p. m. " " 72; " 102.

Oct. 12th, 6 a. m. Improving; rather spongy gums. Pulse 72; temperature 100°.

October 13th. Convalescent; light and nourishing diet. Continued treatment.

Case XXXVII (attacked the day before I left; notes furnished me by Dr. Martin).—R. McG., native of Ireland.

October 8th. Admitted at 2 p. m.; skin hot and moist; eyes injected and watery; cephalalgi and rachialgia; distressed respiration Pulse 120; temperature 103.2°. ℞—Olei ricini ʒjss at once; hot mustard foot-bath; warm teas and rice gruel.

6 p. m. Had one motion. Pulse 125; temperature 105.5°.

October 9th, 6 a. m. Slept well; feels comfortable. Pulse 112; temperature 104°.

12 m. Same. Pulse 108; temperature 104°.

6 p. m. " " 108; " 104.

October 10th, 6. a. m. Feels much worse; had very restless night; urine suppressed. Pulse 100; temperature 102°. ℞—Soda bicarb., potas. chloratis, potas. nitras., aa ʒjj; aquæ; aq. cinnamon, aa ʒiij. ℥ et. S. ʒss every three hours. Mustard plaster to loins.

12 m. Same. Pulse 96; temperature 101°.

6 p. m. Slightly delirious; hiccough; ejected 4 oz. black vomit; urine suppressed. Pulse 84; temperature 102°.

6 a. m. Ejected about 4 oz. black vomit during night; comatose; hiccough; pulseless; remained in that condition until 11 a. m. and died.

REMARKS.

The 37 cases above reported were not of course all which came under my observation, nor, indeed, all I attended. They comprise all those of which careful observations of the special points mentioned at the head of this article were taken. I do not deem it necessary to call attention to the bearing of these bedside notes on those points—the facts will speak for them-

selves. On the whole, they will be found to agree with the similar observations made by others, and to support the generalizations already made. If there is one peculiarity which, above all others, obtains in the symptomatology of yellow fever as distinguished from any other fever, it is the absence of any uniform or even frequent relationship between frequency of pulse and height of temperature. These observations—as do all others I have in a rather extensive experience made in yellow fever epidemics—markedly illustrate this fact.

As regards the significance of albumen in the urine, we must infer from these observations, that, while in many cases the phenomenon is totally absent, it is almost always if not always present in severe cases, and that, therefore, it should be classed as one of the decidedly serious symptoms. It frequently points to a threatened suppression with all its desperate consequences—or perhaps more properly its fatal concomitants.

Of the 37 cases reported, the urine was examined for albumen in 20 cases, and albumen was found in all but seven. The particulars will be better apprehended by means of the following

TABLE.

No. of Case.	Albumen in Urine.	Days when Found.	Result of Case.
III.....	Absent.	.....	Recovered.
IV.....	"	.....	"
V.....	Abundant.	5th to 6th day.	Died.
VII.....	Absent.	.....	Recovered.
VIII.....	"	.....	"
IX.....	Moderate.	3d day.	Died.
X.....	Absent.	.....	Recovered.
XI.....	"	.....	"
XIII.....	Abundant.	2d to 9th day.	Died.
XIV.....	Absent.	.....	Recovered.
XV.....	Abundant.	3d to 7th day.	"
XVII.....	Traces.	4th and 5th day.	"
XVIII.....	Abundant.	2d to 6th day.	Died.
XIX.....	"	4th day.	"
XX.....	"	4th to 10th day.	Recovered.
XXI.....	"	3d and 4th day.	"
XXII.....	"	5th to 10th day.	"
*XXIII.....	"	2d to 5th day.	Died,
XXIV.....	"	2d day to death.	"
XXX.....	Absent	3d to 7th day.	Recovered.

* Owing to a mistake of the printer, this case, instead of being numbered XXIII in the prededing text, is duplicated XXII.

In conclusion, and as a contribution to the statistics of yellow fever, I may state that in the cases treated exclusively, from the beginning, by Dr. Martin and myself, the mortality was one death to every 4.6 cases. The total number of cases thus treated by us was 42, of which 9 died, making the above mentioned proportion. It must be remembered, however, that of the 42 cases only 6 were under 18 years of age—all of these recovered. I need hardly request the reader to remember the established fact of the vastly greater fatality of the disease in the adult than the child.

ARTICLE II. *Cases Selected from the Charity Hospital Clinics of* T. G. RICHARDSON, M.D., Professor of Surgery in the Medical Department of the University of Louisiana. Reported by Members of the Class.

Dislocation of the Elbow of Nine Weeks' Standing—Partial Reduction.

F. McGourn, a strong laboring man, 33 years old, was admitted into hospital November 27th, 1873. Nine weeks previously, travelling in the interior of Texas, he had been thrown from his horse, and falling upon the palms of his hands, seriously injured the right elbow. He consulted a physician the following day, who pronounced the case one of dislocation, and after much manipulation thought that he had effected reduction. Patient was directed to carry the arm in a sling, and to make slight movements from day to day to prevent the joint from becoming stiff; but at the end of more than four weeks, finding that there was no improvement, he went to Galveston. Here he remained more than three weeks; during which time the physicians in attendance also diagnosed dislocation, and having made one or more fruitless attempts at reduction, advised him to proceed to New Orleans.

Two days after his admission the man was exhibited to the class in the amphitheatre, by Professor Richardson, who, after calling attention to the well marked evidences of dislocation of both bones of the forearm backward, stated that, notwithstanding the improbability of a successful result after the lapse of so long a time since the occurrence of the accident, he deemed it his duty to make a faithful attempt to restore the bones to their natural relations,

and if possible, to prevent a great inconvenience which would otherwise continue through life with but little mitigation.

Chloroform having been administered, forced movements of flexion and extension were made in order to break up the adhesions around the joint. Pulleys were then applied, and powerful extension and counter-extension in a straight direction were steadily continued for nearly half an hour, the limb being at the same time freely moved back and forth to facilitate the dislodgement of the bones, but with no apparent success. The pulleys having been now detached, and the extending bands committed to the hands of assistants, the forearm was forced backward some distance beyond the line of the humerus, and while strong traction was made, the limb was slowly brought forward and flexed around the Professor's knee. After two or three such efforts, it was evident to every one that reduction had been accomplished, although, owing to the changes which had taken place upon the articular surfaces, this was not manifested by the usual snap.

Owing to the unavoidable bruising of the parts in making the manipulations, high inflammation and great swelling followed, notwithstanding the attempts to prevent their occurrence by the persistent use of cold. When these had measurably subsided, it was discovered that the head of the radius had slipped back upon the external condyle of the humerus. A slight attempt was made to replace it, but owing to the tenderness of the parts nothing was effected. A month later and passive motion was begun, but it was many weeks before the patient could bear the necessary handling. During this time the fingers became somewhat drawn, but he refused to allow them to be straightened in consequence of the pain produced in the attempt. After four months the elbow continued quite stiff, and the patient had lost all power of voluntary flexion, but by the persistent use of galvano-electricity by Dr. E. Souchon, who had in the meantime taken charge of the ward, this was considerably bettered, and the contraction of the fingers much lessened. Now, at the end of a year, the man is able to flex the forearm nearly to a right angle with the arm, and to extend his fingers so far as to enable him to grasp an object two inches in diameter. The head of the radius remains, of course, in its unnatural position, but the ulna occupies its proper place.

On the whole, it is doubtful whether this patient has been

much benefitted by the reduction of the ulna. With both bones out of place he would, in the course of time, have obtained pretty fair use of the limb with some little motion at the elbow; and the greater degree of motion he now possesses is scarcely more than a compensation for the existing partial contraction of the flexor tendons of the fingers. This latter condition is not, however, a necessary result of such operations, and should not therefore enter largely into consideration in canvassing the propriety of interference. It was evidently induced by the great stretching and bruising accompanying the reduction, but in many other cases doubtless the same amount of local injury would not occur.

The duration of the dislocation in this case was not excessive. for in some instances complete success has been obtained after the lapse of a somewhat longer period; and surgeons are fully justified in attempting reduction even as late as the third or fourth month. Of the failures after the sixth or eighth week we have no statistical record, but it is not unreasonable to infer from information in our possession, that the successes do not exceed one in five or six; and if the cases attended by fracture of the olecranon, and those followed by re-luxation of the radius, anchylosis, contraction of muscles, suppurative inflammation, etc., be added to the failures, the proportion of the latter will be largely increased.

In striking contrast with the great difficulties attending the case just described, the following was exhibited a few days subsequently.

Recent Dislocation of the Elbow.

A. E., a young laboring woman, fell from a step-ladder upon the palm of the left hand, and found, upon rising from the floor, that she had seriously injured her elbow. She came immediately to the hospital, where Professor Richardson saw her not more than three hours after the accident; and pronounced it backward dislocation of the ulna. The patient having been taken into the amphitheatre, and chloroform administered, the professor's knee was placed in the bend of the elbow after Sir Astley Cooper's method, and when traction, not exceeding the pressure of twenty-five or thirty pounds, was made, the bones suddenly came into position with an audible snap. Such, indeed, was the ease with which reduction was effected, that the class, in view of the

importance which had been given to the case, and the explicit directions to the assistants in regard to extension and counter-extension, could not restrain a small exhibition of merriment.

Dislocation of the Head of the Humerus into the Axilla, of Three Months' Standing, Reduced by Manipulation.

Feb. 22d., R. S., a thin, feeble looking middle-aged woman, was brought into the amphitheatre by Professor Samuel Logan, M.D., who kindly placed her in the hands of Professor Richardson for operation. The case had been already diagnosed as downward dislocation of the head of the humerus. Indeed, owing to the emaciated state of the patient and the flabbiness of the muscles, the characteristic signs were so strikingly manifest that no one could mistake the true nature of the difficulty. The accident had occurred just three months before by a fall upon the shoulder.

The patient was then brought partially under the influence of chloroform, and manipulation employed. The first effort was unsuccessful, but upon repeating the procedure, the head of the humerus went back into the glenoid concavity; but again slipped out before it could be secured in place. Prof. Richardson then placed his knee in the axilla, and the bone was lifted without difficulty into its socket, where it was firmly confined by broad bands of adhesive plaster encircling the chest and binding the elbow to the side, with the forearm flexed obliquely across the sternum in the direction of the opposite shoulder. The plasters were allowed to remain four weeks; at the end of which time they were removed by Prof. Logan, and the arm placed in a sling. It is now two months since the reduction, and Professor Logan reports that the head of the bone is in position, and that there is every prospect that the limb will be soon restored to its natural functions.

Dislocation of the Head of the Humerus into the Axilla, of Three Months' Standing—Manipulation—Accidental Fracture of the Surgical Neck of the Bone—a Useful Limb the Result.

Two days after the appearance of the case just described, another middle-aged woman, with a similar dislocation of precisely the same duration, presented herself for relief. In this case, however, the physical condition of the patient was apparently

far superior to that of the preceding, although it was subsequently ascertained that she was very dissipated. The accident occurred, as in the former case, from a fall.

After pointing out again the characteristic signs of this form of dislocation, Prof. Richardson dwelt for some time upon the dangers of attempts at reduction in cases of long standing. He called especial attention to rupture of the axillary artery as the most serious and the most common of these accidents, and ascribed it to a variety of causes: 1st, to adhesion betwen the blood-vessels and the tendons, immediately surrounding the head of the humerus; 2d, to the degeneration of the coats of the artery; 3d, to the stretching of the artery by extension of the limb; and 4th, to the bruising of the artery between the heel of the operator and the head of the humerus in the ordinary method of reduction. All of these sources of danger to the artery, except the first, are avoided by manipulation. Fracture of the bone being of less frequent occurrence, was mentioned as not likely to occur except in those procedures in which considerable leverage is employed.

The method of reduction by manipulation was then discussed, and its great superiority to all other procedures in old cases minutely defined. The credit of originating this method was ascribed mainly to Sir P. Crampton, but that of systematizing it and establishing in this country, to Professor H. H. Smith, who describes it succinctly as follows:

"Elevate the elbow and arm as high as possible, and flex the forearm at a right angle with the arm, thus relaxing the supra-spinatus muscle. Then, using the forearm as a lever, rotate the head of the humerus upward and forward, so as to relax the infra-spinatus, carrying the rotation as far as possible, or until resisted by the action of the sub-scapularis muscle, keeping the forearm for a few seconds in its position with the palm of the hand looking upward; then bring the elbow promptly but steadily down to the side, carrying the elbow towards the body, and keeping the forearm so that the palm of the hand yet looks to the surgeon. Then quickly but gently rotate the head of the humerus upward and outward by carrying the palm of the hand downward and across the patient's body, and the bone will usually be replaced." *

The patient was then placed under the influence of chloroform,

* Ashurst's Principles and Practice of Surgery.

the elbow gently raised until it formed a right angle with the body and rotation about three-fourths completed, when the bone snapped in two at the surgical neck. Examination showed the fracture to be transverse, and quite close to the head of the bone, and it was therefore determined to establish an artifical joint, as near the site of the natural one as possible. For this purpose, the upper extremity of the lower fragment was pressed close up underneath the acromion, and therefore directly over, if not within the glenoid concavity, and the limb closely confined to the side of the chest with the forearm flexed obliquely across the sternum, by means of a long strip of adhesive plaster more than a foot in width, carried twice around the chest.

The woman presented herself from week to week, and at the end of a month the adhesive plaster was removed and substituted by a sling. It is now seven weeks since the accident, the patient can already move the limb with considerable freedom, can bear to have it raised to a horizontal position without pain, and there is every prospect of its becoming more useful than could possibly have been the case had reduction not been attempted. Indeed, the patient can already place the hand upon the left shoulder and upon the nose, and, by the aid of the opposite hand, can raise the arm to a right angle with the body without pain.

The causes of fracture in this case were undoubtedly first of all unnatural fragility of the bone, and secondly, a rigid and unyielding sub-scapular muscle. The amount of force employed was not greater than is ordinarily required, nor was it suddenly applied, but, on the contrary, the movements were made in the most deliberate and cautious manner. A great number of such accidents by other methods are recorded in works on surgery, but it must rarely happen that the humerus is broken during rotation.

The reasons assigned for putting up the fracture with a view to produce a false joint instead of union between the fragments, was the supposed greater usefulness of the limb which would result from the former. As well as may be judged from the present condition, the wisdom of the measure will be unmistakeably established.

Fractures.—Of the various fractures treated in Professor Richardson's wards during the winter, the only one deserving of special notice was a case of

Transverse Fracture of the Patella followed by Bony Union.

In this case the method pursued was bandaging from the toes to the upper part of the leg, a posterior straight splint, and a figure of 8 bandage around the knee. By this means the upper fragment was easily brought down and maintained in contact with the lower uninterruptedly for six weeks. At the end of this time union was so complete, that it was with difficulty that the line of fracture could be discovered by the most delicate touch, as no provisional callus was developed, and the union was like that by first intention in the soft parts. The functions of the knee joint were speedily restored by passive motion after the removal of the apparatus, and the patient left the hospital a few days afterward perfectly well.

Resection of the Wrist-Joint.

J. H., an emaciated exsanguineous man of not more than 35 years, was admitted for a chronic disease of the left wrist, Oct. 11th. The joint was much swelled and very painful, and presented five fistulous openings in the skin, discharging a more or less fœtid fluid which excoriated the parts around. The disease had begun about a year before from no assignable cause, and had so exhausted the patient by constant pain, that from having been once a strong and hearty man he was now reduced to a mere skeleton, with scarcely strength enough to carry him from the bed to the fire-place: otherwise his general health had been always good.

An examination with the probe disclosed extensive caries, and it was at once determined by Prof. Richardson to attempt to save the limb by resection. Before this could be done, however, it was necessary to build up the patient's strength by tonics and liberal diet, so that it was not until the middle of November that the operation could be performed.

Upon the day appointed the patient was brought into the amphitheatre, chloroform administered, and Esmarch's bandage applied for the purpose of preventing hemorrhage. An H shaped incision was then made upon the posterior aspect of the joint, the extensor tendons held aside, and the interior of the joint exposed. It was then discovered that the caries affected not only the lower extremities of the radius and ulna, but also the carpal bones, the first row of which latter were duly dissected out, and

about an inch of the former removed by the saw. The operation was somewhat protracted, owing to the large amount of exudation and softening; and when finally accomplished, such was the unhealthy appearance of the pedicle which then attached the hand to the forearm that scarcely any one supposed that the member could be saved, and Prof. Richardson himself expressed his regret that he had not amputated the forearm. However, the limb was supported upon a straight anterior splint, the patient put to bed, tepid water dressings applied, and stimulants and nutritious food ordered to be given every six hours.

It has now been four months since the operation, the patient's general health has greatly improved, and the wound nearly healed. Some minute fragments of necrosed bone have been discharged through one or two fistulous orifices which still exist, but there is good reason to believe that the man will ultimately recover a very useful hand.

By the employment of Esmarch's apparatus in this case, not only was the patient saved a great loss of blood which he was in no condition to spare, but the operation was greatly facilitated by the *entire absence* of blood from the wound. After the removal of the tourniquet, there was of course some bleeding, and a few small arterial twigs were tied, but the total amount of blood lost did not exceed four ounces.

Two Cases of Stricture of the Urethra, with Rupture and consequent Extravasation of Urine—Perineal Section—One Case Saved. Reported by N. W. Williams.

Case 1st.—S. A. L., a laboring man, act. 33; unmarried; has been troubled with stricture for several years; was admitted to hospital November 16th, under the care of Dr. G. W. Lewis, Assistant House Surgeon, with retention of urine of two days standing, a painful diffused swelling in the perineum, irritative fever and partial delirium. An attempt was made to introduce a catheter, but this failing, Dr. E. Souchon, the Chief of Clinics, drew off the water by means of an aspirator, when the patient was turned over to Professor Richardson, by Dr. Lewis, for farther treatment.

Extravasation of urine into the perineum had clearly occurred, but owing to the desperate condition of the patient it was doubtful whether any means could save him. However it was decided to give the man the benefit of an operation, and for this purpose

he was carried into the amphitheatre, and in the presence of the class an incision four or five inches long was made along the perineum, and the offensive pus and urine pressed out from the blackened and mortified tissues. The incision was then carried cautiously down to the membranous urethra, which, notwithstanding the want of a bougie or other definite guide, was readily found in consequence of its pouched condition behind the stricture. The stricture itself was then divided, and a No. 10 silver catheter carried along the canal into the bladder, where it was effectually retained by a T bandage. The patient was then put to bed, and stimulants and beef tea directed to be freely given, but all to no avail, as death took place from uremic poisoning and irritative fever on the third day thereafter.

Case 2d.—This case was quite similar to the preceding, only the irritative fever was less marked, and uremic poisoning not clearly manifest. The patient was also from the pay-ward of the hospital, and was courteously placed at the disposal of Professor Richardson by the distinguished House Surgeon, A. W. Smythe, M.D.

The extravasated urine in this case had not only spread throughout the perineum, but formed a large black swelling in the scrotum which was formidable to behold. It was impossible to introduce an instrument through the urethra, which was strictured in the bulbous and membranous divisions, and it became therefore necessary, as in the former case, to operate without a guide. The first incision, involving only the skin and subcutaneous areolar tissue, was quite twelve inches in length, and terminated in front at the most dependent part of the large scrotal tumor. After pressing out of the gangrenous tissues as much as possible of the pus and urine with which they were soaked, the incision was deepened behind, and after considerable searching the urethra was discovered at the site of rupture behind the stricture, freely opened from behind forward, and a No. 10 catheter, after many efforts, lodged in the bladder.

Under the faithful use of stimulants, tonics, and nourishing food, the patient rallied; a large slough of areolar tissue was slowly detached from the scrotum and perineum; and the immense gap closed by contraction and granulation. A fistulous opening communicating with the urethra just behind the scrotum remained for some time; but by the continued use of a large-sized catheter in the urethra, and cauterization of the fis-

tulous track, the latter subsequently closed, and the patient was discharged with directions to introduce his catheter at least once a week for the balance of his life.

Suppuration and Partial Sloughing of the Testicle in Connection with Enlarged Prostate Gland and Chronic Cystitis. Reported by D. C. Warren.

J. B., aet. 70, admitted into hospital December 30th, with inflammation and suppuration of right testicle. Is not aware of having suffered injury in the organ, but has been laboring under incontinence of urine without retention for some weeks past. The testicle was much enlarged and adherent to the scrotum, and upon its interior aspect presented a small gangrenous ulcer extending into the body of the gland. The closest cross-questioning developed no rational cause for this unusual condition. The prostate gland was found upon examination to be enormously enlarged, and the interior of the bladder columnar-reticulated from hypertrophy of the muscular bundles; but these were looked upon only as coincidences, as no connection could be established between them and the diseased testicle. It is true that the acute inflammation of gonorrhœa affecting the prostatic urethra not unfrequently gives rise to acute epididymitis by extension of the morbid action along the deferential tubes, but there is no good reason to believe that acute orchitis of a limited character can be induced by a chronic prostatitis.

After the detachment of the slough the excavation in the testicle filled by granulation, and in the course of three weeks the parts had entirely healed.

Considering the age of the patient, his feeble general health, and the great enlargement of the prostate gland, it was not considered advisable to make any special attempt to correct the incontinence of urine, which, however, gradually improved under the necessary confinement to bed and the use of general tonics.

This patient presented also upon his person twenty-five or thirty fatty tumors, varying in size from that of a marble to that of a hen's egg. They had been in existence many years, gave rise to no inconvenience, and did not appear to be enlarging.

Retention of Urine from Stricture of the Urethra—Bladder Emptied Several Times with Aspirator—Stricture Subsequently Ruptured with Holt's Instrument. Reported by J. B. King.

J. G., aet. 22, a hearty, florid Welshman, was admitted to hos-

pital October 30th, with retention of urine from old stricture. It having been found difficult to introduce the smallest-sized elastic catheter, Prof. Richardson drew off the water by means of an aspirator, and the next morning succeeded in passing a No 6. catheter, the man having in the mean time emptied his bladder without assistance. The patient's statement of his history is as follows: Had gonorrhœa for the second time in 1871, after which stream of water gradually lessened in size, scattered, and often difficult to start. In May, 1874, while in London, had a debauch, slept in a pool of water, and upon awaking several hours afterward, found that he could not pass his water; spent the day and following night in great agony, and was then taken to hospital, where chloroform was administered and a catheter introduced; urine was drawn off once or twice more, when the natural power of the bladder returned, and no farther retention occurred until the day previous to admission into the Charity.

It was proposed to treat this case by systematic dilatation of the stricture, but the patient disappeared after the first introduction of the catheter.

February 15th, patient was readmitted with retention of 24 hours' duration. The urethra was much inflamed in consequence of attempts which had been made to introduce the catheter, and the urine was consequently again drawn off by means of the aspirator. This was repeated twice a day for three successive days, at the end of which time the patient passed his water voluntarily. It was now found that a No. 4 bougie could be passed with some difficulty, and that the stricture, seated for the most part in the spongy portion of the urethra, was much indurated. Professor Richardson determined to operate by divulsion, as the speediest mode of relief. Accordingly, on February 20th, the patient was taken into the amphitheatre and the operation performed in the usual way. A No. 11 catheter was then introduced into the bladder, and directed to be worn for a week. At the end of this time, the instrument having been removed, a tumor about the size of a marble was discovered along the lower wall of the urethra, just in front of the bulbous division. It was quite painful to the touch, but could be materially diminished in size by gradual pressure. It was evidently the result of a very limited extravasation of urine, and upon enquiry, it was ascertained that on the day after the operation the catheter became clogged, and the patient straining very hard forced the urine out alongside of

the instrument, and the following day could detect a small swelling at the point indicated.

The student of the ward was now directed not to allow the man to pass his water, but to draw it off, so as to prevent any further extravasation. This succeeded, and in the course of a week the swelling had entirely disappeared, having in the mean time discharged a small quantity of pus into the urethra.

The patient remained in hospital for some weeks longer, and was finally discharged with directions to continue the introduction of a No. 11 bougie once a week for three months, once a fortnight for the following three months, once every three weeks for six months longer, and afterwards once a month during the remainder of his natural life—directions which he is not at all likely to regard.

Several other strictures of a somewhat similar nature were treated during the winter by Professor Richardson, after the same plan and in most cases with the same happy results; but the one above detailed is of special interest, in consequence of the slight extravasation of urine which occurred.

The following may be taken as a sample of the more common cases:

Stricture of Urethra of Seventeen Years' Standing. Divulsion. Reported by G. A. Wise.

James Brennan, aged 31; occupation a brick mason; a native of New York; has been working in the country, but came to New Orleans to be treated for pneumonia a month ago; was readmitted into the hospital, for stricture, February 20th, and sent to Professor Richardson's ward. He says he first discovered interruption to the stream of water about seventeen years ago; had had gonorrhœa ten months previously, and several times since; has never had syphilis; was treated for chordee, however, and was told by his physician that this was the foundation of the stricture. He would not allow a catheter to be introduced, until he suffered such excruciating pain in making water that he could endure it no longer. The introduction of the catheter always produced great and persistent pain; but has never had urethral fever; on the contrary, has always been a remarkably healthy man; does not know of but one stricture, which is about an inch and a half from the meatus. A short time before his admittance into the hospital he was compelled to rise every hour

to empty his bladder, but could pass only a few drops at a time; can now pass it without much pain, but in a very small stream. His suffering is most intense after a day of hard work, or exposure to cold or damp weather.

The patient was taken into the amphitheatre on the 25th, and a divulsion effected by means of Holt's instrument. This was immediately followed by the introduction of a No. 12 silver catheter, which was confined *in situ* by a T bandage, with directions to wear it for at least a week. At the end of four days, however, the patient withdrew the instrument and left the hospital. He was seen on the streets some weeks afterwards, and declared himself entirely well. In this, however, before long he will probably find himself mistaken, unless he should take the precaution to keep the stricture dilated by the occasional introduction of a good-sized bougie.

Penetrating Wounds of the Abdomen. Reported by J. A. Larcade, Resident Student.

Case 1.—Punctured wound involving the stomach or the duodenum—Protrusion of Omentum—Recovery.

F. R., an active, muscular Irishman, twenty-three years of age, was admitted to hospital November 29th, having been stabbed in the abdomen only two or three days previously. The wound was found to be situated in the upper part of the umbilical region, near the median line, measured about three-fourths of an inch in length, and was occupied by a protrusion of the omentum. The omentum was reduced without great difficulty, the edges of the wound brought together with adhesive plaster, and a grain of sulphate of morphia given by the mouth as a preventive of peritonitis. Shortly after taking the morphia, the patient vomited a large quantity of blood mixed with undigested food. As the blood was coagulated, and was ejected without difficulty, there could be scarcely a doubt that either the stomach itself or the adjacent division of the duodenum had been penetrated. Half a grain of morphia was now given hypodermically, and repeated twelve hours afterward, with the effect of allaying pain and producing sleep. The next morning there was a slight vomiting of blood, and all the symptoms of rapidly-developing peritonitis, namely, acute pain in the neighborhood of the wound, tympanitis, small, hard, wiry pulse, etc. He was now seen by Professor Richardson, who ordered tinct. opii ℥ xxx and tinct.

verat. virid. (Norwood's) ℥ iv, every four hours. The following day showed no abatement in the symptoms of peritoneal inflammation, but on the contrary, the tympanitis was more general, the fever more marked, and an anxious expression upon the patient's countenance. At night there was great restlessness and delirium. The next morning, however, there was a change for the better; the pulse, now reduced to 70 beats per minute, was larger, softer and slower, the skin less dry, the tenderness less acute, the anxiety of countenance gone, and the patient expressed himself as feeling decidedly better. The opium and veratrum had evidently taken effect, and it was ordered therefore that they should be given at intervals of eight hours, in order to maintain the impression already made.

This treatment was continued in force from day to day for a week, by which time the evidences of peritonitis had nearly entirely disappeared, but it was thought prudent not to suspend the medicine at once, but to give it twice a day. Three days thereafter all tympanitis and tenderness having disappeared, and the wound entirely healed, the medicine was abandoned, and a dose of sulphate of magnesia administered to open the bowels, which had been confined for more than ten days. No bad result followed the catharsis, and the patient, after remaining in the ward for a few days longer, left entirely well. It is proper to state that beef tea and other liquids were allowed to be taken so soon as there was an evident check of the inflammatory symptoms.

This case presented a marked illustration of the controlling influence of the two remedial agents employed over acute inflammation of a serous membrane. When, as in this instance, there is a wound involving one of the hollow viscera, it is absolutely necessary for the safety of the patient that a certain amount of inflammation should occur, in order to attach the edges of the wound to the walls of the abdomen, and thus prevent effusion of blood or the contents of the organ throughout the peritoneal cavity. The remedies should not therefore be given in large quantities in advance of inflammation with a view to prevent it wholly from appearing, but only to quiet peristaltic action and allay nervous excitement. When once, however, the morbid action has passed this limit, the salvation of the patient depends upon rapid narcotism with cardiac sedation.

Case 2.—Simple Penetrating Wound of Abdomen—Peritonitis—Recovery.

J. S., a large, muscular man, aet. 32, admitted to hospital December 21st, for abdominal cut, made with pocket-knife twenty four hours previously. The wound was situated in the upper half of the umbilical region, was not more than half an inch in length, did not present protrusion of bowel or omentum, and no escape of fœcal or other matter had been observed. There was positive evidence, however, of rapidly-spreading peritonitis, and the inference therefore was very clear that the cavity of the abdomen had been opened, more especially as no other injury but the wound had been received.

The case was treated precisely as the preceding, and in ten days' time the patient was discharged cured.

Case 3.—Incised Wound of Abdomen—Large Protrusion of Bowel and Omentum—Ileum completely Severed—Incision involving also the Mesentery—Peritonitis—Recovery.

R. W., colored, aet. 26, was brought to hospital February 24th, at 11.30 p. m., in consequence of incised wound of abdomen received in an affray only an hour previously. The cut was nearly four inches in length, situated in the left hypochondriac region, and extended across the margin of the thorax obliquely from above downward and backward, and through it protruded portions of the stomach, ileum, colon and omentum, forming altogether a mass as large as a fœtal head. The case was seen immediately by Dr. A. W. Smythe, the House Surgeon, who upon cleansing the parts found the ileum completely severed, the incision extending an inch or more into the mesentery; and the omentum much torn. There had evidently been a considerable loss of blood, large coagula of which were scooped with the hands out of the peritoneal cavity. This, together with the probability of a recurrence of the hemorrhage if reaction should take place, and the certainty of extensive peritonitis if the patient should survive, rendered the case almost hopeless. Indeed there was scarcely a doubt upon the mind of any one present that death would ensue within a short time from either the shock, the hemorrhage, or the inflammation. Notwithstanding this, Dr. Smythe brought the two ends of the divided bowel together by means of the continual or Glover's suture, and having pushed the pro-

truded parts back into the abdomen without reference to their natural relations, closed the external wound by means of a few points of interrupted suture and strips of adhesive plaster, and ordered ¼ gr. of morphia hypodermically, to be repeated in half an hour if necessary. The second dose was given, and the patient passed a quiet and rather comfortable night.

The next morning (25th) reaction had occurred, pulse 110 and feeble, and a general but not very decided tenderness over the abdomen. During the day he got six hypodermic charges of morphia of ¼ grain each, the object of which was to arrest the rising peritonitis.

February 26th. Pulse 132 and corded; temperature much above the natural standard; tenderness over the entire abdomen and well marked tympanitis. Peritonitis had clearly developed, but nevertheless patient was permitted to take liquid food, while the morphia was repeated sufficiently often to maintain a slight degree of narcotism.

February 27th. Fever increased; pulse 163; tympanitis and tenderness upon percussion more marked, and patient delirious. During the day 2 grs. of morphia were given in ½ gr. doses hypodermically, and in the evening there was an evident abatement of the symptoms.

February 28th. Patient very restless all night, but now clearly better; pulse 132; delirium continues; morphia repeated.

March 1st. Delirium subsiding; patient complains of nausea and chilliness—nevertheless took his daily allowance of beef tea; got hypodermic injections of morphia of ¼ grain each.

March 2d. Took dressing from wound of abdominal wall, and found that union had taken place by first intention. Pulse 128. Morphia given as before.

March 3d. Pulse 123; bowels moved several times of themselves; local symptoms of peritonitis abating.

March 5th. Pulse 118; expression of countenance good; bowels moved of their own accord; morphia continued.

March 12th. The patient exhibiting some thoracic symptoms, Dr. Smythe examined and found an effusion into the left pleuritic cavity, which gradually disappeared, and the patient is now (April 1st) quite well with the exception of a considerable protrusion of the stomach or bowels—it is impossible to determine which—at the site of the wound and just underneath the skin. A more accurate examination of the exact situation of the wound

and of the parts involved, was made after the patient had recovered, which demonstrated conclusively that the knife entered very obliquely between the ribs and passed through the diaphragm into the abdominal cavity, as it is clearly along this track that the protrusion above mentioned now occurs. This will account for the pleuritis and the pleuritic effusion subsequently discovered, the fluid being prevented from passing into the peritoneal cavity most probably by adhesion of the protruded part to the edges of the wound in the diaphragm; for by reduction the hernia cannot be made to return entirely into the abdominal cavity. But what is very remarkable is, that there was no evidence of wound of the lung when the patient was admitted, notwithstanding he was questioned closely to ascertain whether there was any expectoration of blood.

The prominent feature in the case, however, is recovery from a complete division of the bowel, and without any subsequent interruption to the performance of its functions. That there is more or less constriction at the seat of injury cannot for a moment be doubted; but up to the present moment (April 5th), no difficulty referrable to this has occurred.

Case 4th.—Penetrating Wound of Chest—Pleuritis with Effusion—Recovery. Reported by J. A. Larcarde.

M. S, aet. 23, a sailor; admitted into Professor Richardson's ward December 13th, in consequence of a wound received in a brawl. The patient was suffering somewhat from shock, and upon examination a stab wound, about half an inch in length, was found between the 8th and 9th ribs on the left side. Air was passing in and out through the opening, but there was no expectoration of blood or other evidence of injury to the lung. The edges of the wound were brought together with adhesive strips, and ½ gr. sulphate morphia administered hypodermically. The following day there was emphysema of the subcutaneous areolar tissue, and a physical examination revealed the presence of air in the pleuritic cavity. In the course of forty-eight hours symptoms of extensive pleuritis showed themselves, and very soon afterwards the accompanying effusion reached as high as the fourth rib, when the patient assumed the sitting posture.

The treatment consisted mainly in the use of tinct. verat. virid. (Norwood's), and tinct. digitalis, three minims of the former and twenty of the latter being given every four or six hours, accord-

ing to the effect upon the heart. By this means the pulse was rapidly brought down to 70 per minute, and maintained at or about this rate for several days. In addition to these remedies, 15 grains of Dover's powder were given every night, and every second day a simple cathartic. Fluid food was administered regularly from the commencement. At the end of a week the inflammatory symptoms were in a great measure subdued, but the effusion into the pleuritic sac was not entirely absorbed for nearly a month.

Empyema, Opening externally and through the Lung—Rib Trephined.
Reported by N. W. Williams and R. H. Dean.

I. A. S., aet. 45, a farmer, naturally very robust, and without hereditary predisposition to any chronic disease, was attacked with acute pleurisy in the early part of 1872, the inflammation occurring first upon the right, and about ten days afterward, upon the left side. The disease became chronic in the latter situation, and after some time there was a sudden expectoration of pus, which led his family physician to suspect perforation of the lung by ulceration and consequent communication between the lower bronchial tubes and the pleuritic cavity. The discharge of pus in this direction became afterwards quite profuse, and the patient's health having become greatly impaired, an opening was made between the 7th and 8th rib in order to discharge the contents of the pleura in this direction.

Patient was brought upon a litter into Professor Richardson's ward, November 15th, 1874. He was extremely emaciated; his thighs were semi-flexed and inextensible from protracted muscular contraction; the left arm was partially paralyzed, and the flexors of the hand contracted; he was coughing and expectorating purulent matter every few minutes, and his countenance expressed the greatest anxiety and distress. His whole appearance was, indeed, so miserable that while it excited the sympathy of all who saw him, gave little or no promise of relief. Upon examining the left side, a small slit-like fistulous opening was found at the site of the operation referred to in the previous history, and from it an offensive pus, amounting to three or four ounces in twenty-four hours, discharged itself with difficulty in consequence of the approximation of the ribs. The expectoration was of the same nature, but more abundant; and to prove conclusively that it proceeded from the same source,

Professor Richardson injected through the external opening a half pint of tepid starch water, which immediately excited a profuse expectoration, and this, upon being tested with iodine, was found to abound in starch.

In order to empty the pleuritic cavity more effectually and arrest the flow of the pus through the air passages, it was proposed to enlarge the external opening and keep it perfectly patulous, but this it was found impossible to do, in consequence of the partial collapse of the side and the consequent near approximation of the ribs. Professor Richardson therefore determined to take out a section of one of the ribs with a large trephine, an operation which he stated he had performed several times before in such cases with remarkably good results. Accordingly, a few days afterwards the man was carried into the amphitheatre, and in the presence of the class the largest-sized trephine was applied to the 8th rib, immediately below the fistulous opening. After the removal of the disk of bone, false membrane nearly half an inch in thickness was divided with the scalpel, and out gushed nearly a pint of most offensive sero-purulent fluid. Within a few days after the operation the patient began to mend, and in three or four weeks he was in a condition to return home. The expectoration of pus had in the mean time ceased entirely—owing probably to closure of the communication with the lungs—and that which discharged itself through the external opening had not only diminished greatly in quantity, but was now quite healthy in appearance and odor. The external opening manifested no disposition to close, and the patient was directed if this should occur before the purulent discharge entirely ceased, to keep it fully dilated by means of a plug of lint, which could be removed several times a day to allow the accumulated fluid to escape.

Nothing has been heard from the man since he left the hospital, and as he promised to write in case he should not continue to improve, there is good reason to suspect that he has done well. Whether this be true or not, the case is eminently instructive as showing the great value of large openings into the chest in chronic suppurative pleuritis. Not only do such openings permit the free discharge of the secreted fluids, and thus give opportunity to the cavity to close by expansion of the lung and collapse of the chest, but it also enables the surgeon to make use of medicated injections which are oftentimes of the greatest benefit.

Chronic Cystitis of Many Years' Standing Cured by Three Injections of a strong Solution of Nitrate of Silver. Reported by J. A. Larcade, Resident Student.

The history of this case, as gathered from the patient, is not very connected and not altogether satisfactory, but it is in substance as follows:

Martin Solan, aet. 44, by occupation a laborer, was seized several years ago by an attack of sub-acute inflammation of the bladder, with which he suffered more or less severely for several months, when he entered the City Hospital of Cincinnati, and was there treated for a short time by the distinguished Professor Mussey, now dead. Weak injections of nitric acid were repeatedly used, but with only partial success. Left the hospital of his own accord, and commenced treating himself by taking nitric ether, turpentine, etc., which had the effect of allaying pain and other urgent symptoms, and thus enabling him to do light work.

His next appearance in hospital was in 1864, when he was admitted to the Charity Hospital of this city for a hydrocele, the inflammation of the bladder continuing. The hydrocele was operated upon by Professor Chaillé, but subsequently returned. Nothing was done for the cystitis.

He next turned up at San Francisco, where the hydrocele was again emptied and injected with iodine by Professor Toland, who also prescribed medicine to be taken by the mouth for the bladder trouble, but without any marked result. The hydrocele was, however cured. After this he returned to the use of nitric ether, turpentine, etc., with the same effects as before.

February 11th, 1875, he was admitted again to Charity Hospital, and sent to Professor Richardson's ward. After careful examination the case was pronounced one of undoubted chronic cystitis. There was a constant sense of weight and uneasiness behind the pubes, itching at the external meatus, inability to retain the water longer than an hour or two, frequent incontinence, and a large admixture of mucus and pus with the urine. A sound was introduced to ascertain whether calculus existed, but with negative results. The bladder was also injected with tepid water to ascertain its capacity, and found to be so greatly contracted that not more than three or four ounces of fluid could be forced in without great pain. The patient's general health was fairly good.

The case was taken into the amphitheatre, where the subject of chronic cystitis in general, and the great diversity of treatment recommended by surgeons were fully discussed by Professor Richardson. He commented particularly upon the great caution advised by most authors in the use of even a very weak solution of nitrate of silver as an injection. Sir Henry Thompson, one of the most recent and highly esteemed writers upon the subject, says in regard to its employment: "Then you may try nitrate of silver in small quantity—*certainly not more than one grain to four ounces of water to begin with, going up to about half a grain, or one grain at most, to the ounce.*" Other writers upon surgery express substantially the same opinion, and in many instances in nearly precisely the same words. Not a single exception to this statement can be found in any of the treatises upon general surgery or special works upon diseases of the genito-urinary organs. There is the most remarkable unanimity in this respect; and yet, strange as it may appear, no writer condemns the use of this same agent, either solid or in very strong solution, as an application to every other inflamed mucous surface which can be conveniently reached. The conjunctiva is certainly quite as delicate a structure as the lining membrane of the bladder, and what more common than to brush a sixty-grain solution over the former.

Professor Richardson then stated that for several years past he had invariably employed in such cases solutions varying in strength from ten to sixty grains to the ounce of water, and with almost uniformly successful results. He argued that an injection of less than ten grains is utterly worthless, in consequence of its immediate decomposition by the small quantity of urine which it is sure to meet with so soon as it enters the bladder.

He then proceeded to wash out the patient's bladder with tepid water, and followed the discharge of the latter immediately with two ounces of a solution of nitrate of silver, twenty grains to the ounce. This latter was allowed to remain in the organ about a minute, and then to flow out through the catheter, the salt having in the mean time become almost entirely precipitated in the form of chloride of silver. The injection was not succeeded by another of tepid water for washing out the remains of the nitrate of silver. This is not necessary except where much stronger solutions are employed. The patient complained of a

severe burning at the neck of the bladder for a few hours, but the next morning he expressed himself as feeling remarkably well. In fifty-eight hours there was a marked improvement in the character of the urine, and great amelioration of the dysuria which had formerly been very annoying.

After the lapse of eight days, another injection was made containing thirty grains of the salt to the ounce of water. This was followed by a corresponding improvement. Again, in eight more days, the same was repeated, after which every particle of pus and mucus disappeared from the urine together with all other symptoms of inflammation. Owing to the permanently contracted state of the bladder, the patient is unable to hold his water longer than three or four hours, but even this may be improved by daily repeated efforts, with the view to put the bladder slightly upon the stretch and thus gradually increase its capacity.

Elephantiasis Arabum—Barbadoes Leg. Ligation of Femoral Artery.

J. B., a typical African, but born in Louisiana; aet. 25; admitted into hospital January 1st, and sent to Dr. J. J. Castillanos' ward. Left foot began to enlarge three years since, unaccompanied by pain or other evidences of inflammation. Disease gradually extended up the limb, and at the time of admission had nearly reached the knee. The pachydermatous condition of the limb so characteristic of elephantiasis was exceedingly well marked, and in the neighborhood of the instep the hypertrophied skin was disposed in folds with intervening sulci not less than two inches in depth. Owing to the great weight of the member, the man moves about with very great difficulty.

Dr. Castellanos having placed the case at the disposal of Professor Richardson, the patient was taken into the amphitheatre March 1st, and the femoral artery tied in the lower angle of Scarpa's triangle. From this time the limb began to diminish rapidly in size, and now at the end of six weeks is not more than two-thirds its former dimensions, the decrease still continuing. Professor Richardson proposes to tie the external iliac artery at some future day, if the operation already performed should not result in a complete cure.

ARTICLE II. *Theory of the Nerve Centres.* By W. CHAMBERLAYNE JONES, M.D.

To assert that the cerebrum is the organ of knowing or cognition is in accordance with the established opinions of physiologists, since they believe it to be the seat of what are in an undefined manner termed higher mental manifestations.

The sensory ganglia and gray matter of the spinal cord are believed to be the centres which receive impressions from the external world and the body. The theory which I propose assumes the action of these centres to be the same in character as that which has been heretofore restricted to the centres of the spinal cord, and they will be both included under the term excito-motor ganglia, which is intended to include every mass of vesicular nerve matter within the body which is not a part of the cerebrum or cerebellum.

Those masses which are in distant parts of the body have more or less imperfect connection with the brain, and possibly some of them have no connection at all by which they can make their condition known to the cerebrum. The brain is the assemblage of nerve centres for all those purposes of co-operation required by the nature of the organism.

If the cerebrum be admitted to be the organ of cognition—and of this there can be no doubt—animals which are devoid of this organ can have no knowledge of the impressions made upon the excito-motor ganglia, and all of their movements are purely reflex or instinctive in character—and instinct is thereby defined to be the power to perform definite acts without knowledge of either the act or the end. We have the evidence of such acts within our own bodies, where the complicated operations of organic life go on with perfect system and order outside the boundaries of our cognition.

It is believed by physiologists that the invertebrate animals have no cerebrum: if they have not this organ they can have no cognition.

Of cognition there are several kinds, but it is only for the present purpose to mention two of them—perception and consciousness.

Perception is the knowing or cognition of the impressions received by the excito-motor ganglia of the external world.

Consciousness, which is the knowledge of self, is of two kinds:

the knowledge of the impressions of states of the body secured by certain special excito-motor ganglia—to which I restrict the name sensations—and knowledge of states of the immaterial cause which uses the body as its material instrument—which states are impulses, which, when so strongly excited as to produce certain characteristic bodily effects, are termed emotions.

These views render one of two conclusions necessary—either that the activity of invertebrate animals is entirely devoid of consciousness and perception—they acting directly from the impressions of savors, odors, contacts, sights, sounds, and states of size—or else that they possess the rudiments of the cerebrum; and as all animals which are known to have this organ have the cerebellum also, that they possess the rudiments of both of these organs, which I believe to be the most probable conclusion.

It is consistent with the harmony of the system of organic life to believe that the lowest order of creatures endowed with a nerve apparatus have at least an obscure perception world around them, and a faint consciousness of their limited sources of activity.

The cerebellum is supposed by physiologists to be the organ by which movement is conducted: this opinion I believe to be erroneous, and that muscular contraction is regulated by certain gangha among those which I have termed excito-motor.

The mass of nerve matter known as the optic thalami is the centre of the nerves which convey impressions termed common sensations, and that known as the corpora striata is the common centre of the nerves of motion; these masses are intimately united, and I believe they do by their combined and reciprocal action constitute the centre by which muscular contraction, and the movements produced thereby, are co-ordinated or regulated.

Under common sensation is included the impression termed the muscular sense, and it is this impression which, when communicated to the centre of the nerves of motion, regulates movement.

If it be considered as an admitted fact of physiology, that invertebrate animals possess the excito-motor ganglia alone, it is impossible to deny that their movements are as well co-ordinated as those of animals which possess the remaining organs.

Having premised thus much, I will state that I believe the cerebellum to be the organ of the emotions and of their expression.

My opinion of the mode of its activity is as follows: Impressions secured by the excito-motor ganglia, or ideas arising within the cerebrum, are carried by the known means of communication to the cerebellum, by which emotional influences are exerted—which are, however, not known as emotions at this stage of the process. These influences act directly through efferent nerves which proceed from the cerebellum to the spinal cord, to bring forth the expression and produce the other well known effects of these impulses upon the body. The state of the body produced by the action through the efferent nerves is carried back by the efferent nerves proceeding from the spinal cord to the cerebellum to produce the impression, which, when cognized by the cerebellum, is emotion; and the impulse, either before or after being cognized, may proceed to the common centre of motion to cause movement. When the impulse is not sufficiently strong to be known in this manner, it can be known by its influence upon the current of ideas, and less distinctly the current of action.

Impulses are modes of our causality, and are of two kinds—emotions and propensities—which last are made known by impressions upon the excito-motor ganglia, and have doubtless each one of them a special centre of its own; these relate to states of the body, and include hunger, thirst, and the sexual desires. The emotions relate to states of our immaterial nature, and include anger, fear, joy, grief, love, hate, and all states referred to by the common meaning of the terms emotion and passion.

Every emotion has an expression peculiar to itself! How different is the expression of humility and pride—of joy and grief—of love and hate! The expression of emotions brings into action more or less every muscle of the body, and when strongly excited, affects even those of organic life. The expression first occupies our attention in the physiognomy of persons; and there are individuals whose ruling passion sets the mark of an ever-controlling instinct upon their bodies, and these impulses, as a whole, form the principal sources of human character and human activity.

The facts which have been brought to show that the cerebellum co-ordinates movement are equally conclusive in proof of the present theory. The most important of these facts relate to the proportionately increased size of the cerebellum, according to the greater variety of movement of which animals are capable. As capacity for increased variety of movement is of necessity

accompanied with an increased capability of expression, the facts support our opinion as well as the other.

The facts also which have led other observers to believe it to be the organ of the sexual propensity are also in favor of the present theory in so far as they relate to the emotion love, which is seated there.

In man the cerebellum is a large organ, inferior in size only to the cerebrum, exceedingly complex in structure, and is evidently only second in importance to the latter in the part it takes in mental operations.

If the theory which I have advanced regarding the function of this organ be true, there are many modes of investigation by which it will be proven—more especially by the evidences furnished by the records of disease. In cases where its functional activity had been destroyed, and the remainder of the brain left sound, a condition would ensue which I will term emotional idiocy—which is complete imbecility of character, in which no emotional impulse to action exerts itself, and no desires exist excepting those whose exertion satisfy bodily wants. The body of a person in such a state would be devoid of expression, and present the appearance of total apathy.

In cases where the opposite condition or morbid irritability of the organ was present, there would be emotional insanity, or the overwhelming predominence of highly excited passions over the dictates of reason and common sense. The cerebrum having been supposed to be sound in both cases, there would be no deficiency of understanding. In the first example there would be poverty of thought, owing to the absence of most of the ordinary stimulants to cerebral action; in the last the thoughts would be rational, except as distorted by the prejudices and passions.

This is all I have to advance about the nerve centres at present. I may at some future time bring forward certain additional views.

I have for many years been laboring to construct a true analysis of the mind, and the above theory is the result of the application of some of the truths acquired in that line of inquiry to the common facts of anatomical and physiological knowledge. If proven by experimental investigation to be true, I will be happy to have directed physiologists in the right direction in search of the facts. If on the contrary, it should prove to be false, it will in no way affect the truths of the system of meta-

physical ideas which suggested it, and which I hope in a few years more to have connected.

ARTICLE III. *Ipecac in Hemoptysis.* By E. L. LEWIS, M.D., Professor of Materia Medica, Therapeutics, and Clinical Medicine, Medical Department, University of Louisiana.

Amongst the various medicinal agents employed to arrest pulmonary hemorrhage, there is none whose action is more speedy and satisfactory than that of ipecac, when given in doses to cause vomiting. In fact, its hemostatic properties have long been known to the profession, though its mode of action has only been rationally accounted for within recent years. Experimental physiology, to which therapeutics as a science is so largely indebted, has elucidated this subject beyond cavil, and the views of Pecholier, that the capillaries are contracted under its influence, have been corroborated by the researches of the latest experimenters.

With the results of their investigations in view, we can no longer regard the cessation of hemorrhage as due to the mere revulsive action of ipecac, but must consider the vomiting, the increased action of the heart, the diuresis, and the arrest of the hemoptysis, as the result of a primary excitation of the nervous and muscular systems, during which the calibre of the vessels is diminished and arterial pressure consequently increased. Its action in that particular is nearly identical with that of digitalis, for the primary action in both is accompanied by a contraction of the involuntary fibres; whilst the relaxation which succeeds is the result of a paralyzing influence over the nervous and muscular systems. The great advantage it possesses over digitalis is in the rapidity of its action, which almost immediately follows its administration, whilst the latter does not produce its systemic effects under several hours.

It was regarded by Dr. Trousseau as the remedy "par excellence;" in hemoptysis, but his views on its use were based on clinical observation only, and not on its physiological action, which was then but imperfectly understood.

By its prompt action, in cases treated in vain by the ordinary means employed to remove this morbid condition, we relieve at once the anxiety and mental depression which are generally

present, and better still, lessen the liability to permanent injury of the delicate cell-structure of the lungs which so often follows hemoptysis, laying, as it does, the foundation for the future development of tubercles, even when no hereditary tendencies exist. Debility from the continued loss of blood does not contraindicate its use, as the relief afforded more than compensates for the general relaxation which succeeds the vomiting, whilst the removal of the cause soon enables the recuperative powers to restore the loss sustained. There is a natural hesitation, as Dr. Trousseau remarks, in first prescribing an agent which produces such a commotion of the system, but experience soon satisfies one that these fears are groundless, and that its use economizes both the patient's time and strength. Still there are cases where even ipecac fails to effect a cure; but even then the relief obtained is great, and it does not preclude the use of other known remedies.

As an exemplification, I append the following report of its successful use in immediately arresting pulmonary hemorrhage when other measures previously employed failed to produce any effect.

Case.—Edward C., an Irishman of intemperate habits, aged forty-five years, who had suffered on two occasions from delirium tremens, and at different times from epistaxis, applied at my office on the 22d of March, 1875, to be treated for hemoptysis of a few hours' standing. He had taken a cathartic in the morning, which had freely purged him. I found no excitement of the pulse, and nothing abnormal in the condition of the lungs, but a few crepitations below the right clavicle, no doubt produced by effused blood. I directed him to take thirty drops of Battley's ergot every two hours, and enjoined rest and cold drinks. About 12 o'clock the same night he sent for me. I found him sitting in bed, coughing up mouthfuls of blood with which he had half filled a wash basin at his bedside. His anxiety was very great, as well it might have been, and his pulse frequent and jerking. I prescribed sixty grains of ipecac in four powders, directing that he be given one every ten minutes. Even before the vomiting ceased the hemorrhage was arrested, and did not return until the day after, when from over-exertion in walking, and perhaps also from drinking, it again appeared. My patient objecting to another emetic, I prescribed Dobel's mixture, composed of digitalis, ergot, sulphuric acid, and gallic acid, direct-

ing him to take a tablespoonful of the mixture every two hours. This he took for about ten hours without the slightest effect. He was evidently becoming weak and very much alarmed, and I was not free from anxiety at the persistence and profusion of the hemoptysis. The mixture having failed to relieve him, he was now easily induced to try the ipecac again in the same manner as before. The result exceeded my expectations, as the bleeding was immediately arrested; still fearing, however, that it might return, I gave him a mixture containing gallic and sulphuric acids to maintain the contracted state of the capillaries. Since then his health has been good; in fact, he says he feels better than before the attack—probably because he drinks less.

This is only one instance in many where I have seen ipecac render as good service, but not having kept a record of other cases, I can not give a detailed account of them.

ARTICLE IV. *Suicide by Chloral Hydrate and Chloroform.* By J. E. MAYFIELD, M.D.

H. B.; male; native of Prussia; age 25 or 30 years; very small phisique; weight about 100 lbs; delicate and weakly; had constitutional syphilis of several years' standing; somewhat given to inebriation, both as to alcoholics and opiates; has been known to take in usual health two ounces of laudanum within the space of half an hour, a half ounce of chloral at one dose, and large doses of morphine.

On Friday he took during the day (perhaps at one draught) 2 oz. laudanum, without serious effect.

On Saturday following, 2 oz. laudanum and 2 fluid oz. chloroform, causing considerable stupor.

On Sunday, 3 fl. oz. chloroform and 1 dr. morphine.

Monday, 4 fl. oz. chlorform, and on Tuesday 2 fl. oz. chloroform and 1 oz. chloral hydrate. About 12.30 p. m. this day, he sat up in bed and ate a tolerable dinner, apparently well, and in less than a hour afterward, he being alone in his room, was heard to make a groaning or stertorous noise, and was found to be in a very alarming condition—still in bed—almost lifeless, breathing with great difficulty and foaming at the mouth, and seemed as if just recovering from an epileptic fit.

I reached him in a few minutes afterward; found him in deep coma, his breathing somewhat stertorous but almost normal in

frequency; eyes open, pupils slightly contracted and insensible to light; nervous sensibility wholly absent; pulse very weak and irregular, and too frequent to count. In a few minutes he had another spell of suffocation; breathing stopped for a considerable time, but was restored by a kind of artificial respiration produced by my rolling him on his side and pressing upon his chest. He then continued in a comatose condition, with regular respiration, profuse perspiration, and weak, irregular pulse, varying from 100 to 120 per minute, until towards the close of the following day, when there was a very slight general improvement, but he gradually sank, and died about 46 hours after I first saw him as above stated. It is known, almost to a certainty, that he took all of the medicines above named, but it is to be regretted, for the sake of science, that the quantities and frequency of the doses are not more definitely known.

EXCERPTA.

Treatment of Pruritus Vulvæ.

M. Hardy frequently employs the following formula:

℞. Hydrarg. chlor. corros., gr. xv;
Alcohol, ʒiii;
Aq. destillat., ℥iii ʒv.—M.

Tablespoonful in a tumblerful of warm water. Avoid rubbing the parts while applying the lotion.

In that form of pruritus vulvæ so often found accompanying pregnancy, Danyau employs the following formula:

℞. Zinci oxid., ℥i;
Sodii borat., ʒss;
Cerat. simpl., ℥ss;
Ol. amyg. dulc., q. s.;
Morphiæ muriat., gr. iii.—M.—*Medical Times.*

Rapid Child Bearing.

Dr. John Parr, of Buel, reports the following case: On March 7th, 1874, I attended Mrs. D. in labor, and delivered her of a male child, which she continued to nurse up to the birth of a second child (girl), January 17th, 1875. The age of the first child at the birth of the second was ten months and ten days.—*The Medical Record.*

*On the Management of Labor as far as regards the Prevention of Hæmorrhage and Septicæmia.** By I. Harrison, F.R.C.S.

It was well remarked by a speaker in one of the great discussions which took place at a metropolitan society, that it was needful to have a clear and distinct understanding of what they were talking about. The remark holds good in a debate on obstetrics, and fitly applies to our doings in its practice. It is a mistake, I contend, in the practice of obstetrics, to look upon it as a series of regulation-formulæ, to be treated after a set and recognized fashion, rather than as a natural process, modified as it may be, as it must be, and as it is, by varied accidental complications. We must act on general principles, founded on the anatomy and physiology of the parts concerned.

Up to the time of the establishment of the true action of the spinal marrow and of the reflex system by Dr. Marshall Hall, of their application to obstetrics by the late Dr. Tyler Smith, and of the splendid dissections and demonstrations of the uterine supply of nerves by Dr. Robert Lee, many of the phenomena of parturition were inexplicable, or their explanation was contradictory. The observations about to be made are founded on their teachings.

The uterus is a muscle of the involuntary class, of a non-striated character, the fibres of which, in the pains of labor, after remaining in a state of contraction for a brief period, relax; as is invariably the case in non-striated muscle, it being incapable of sustained effort, says Dr. Leishman. It has a peristaltic action of its own. It is connected with all the nerves from the medulla oblongata to the sacrum—cerebral, spinal, and sympathetic. The uterine group includes the nerves of the ovaria, Fallopian tubes, uterus, vagina, and the external parts of generation. Hence its sympathies and synergies are innumerable; they include affections of the head, breasts, stomach, suprarenal capsules, bladder, rectum, os and cervix uteri, and of the external parts; and may be direct or reflex—mainly reflex. "Thus, in parturition, the uterus may be excited in a reflex form by irritation of the mammary incident excitor nerves, the pubic and abdominal branches of the intercostals, the rectal, the gastric division of the pneumogastric, the ovarian nerves, and also by the nerves of the vagina and the os and cervix uteri. These act through the medium of the spinal marrow, the great organ of physical motion." (Dr. Tyler Smith.)

It will not be contended, I take it for granted, that the uterus is an isolated organ, having no nervous connections or supply. If any doubt—and there are some who do—let him look at these beautiful plates of the dissections of Dr. Robert Lee. It will be admitted, also, that the vessels and nerves enlarge *pari passu* with the growth of the uterus during pregnancy.

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* Read before the Reading Pathological Society.

An organ so endowed must have extensive susceptibilities, and be the recipient of a wide range of reflex impressions. Let us take an example or two. A nurse upsets a well furnished clothes-horse on to the fire. The consequent blaze excited an immediate convulsion in a lady in the last pangs of labor. A lady was confined after an easy labor; but the uterus contracted and relaxed alternately for three hours afterwards. The hæmorrhage was alarming. At length violent vomiting occurred; the stomach was unloaded of a basinful of boiled mutton and turnips—the dietetic ingestment of the previous evening, and then contraction firmly took place.

The obstinate vomiting of pregnancy is, I believe, mostly dependent either on disease of the suprarenal capsules or on ulceration of the os and cervix uteri. The reflex action of an irritated nipple, a loaded rectum, a distended bladder, a vaginal examination, a hand on the sacrum, are familiar to all. Ulceration of the os and cervix uteri, the irritation caused by the various mechanical appliances to and in the uterus, the process of periodic and final uterine involution, etc., elicit a multitude of nervous expressions in various parts of the body as yet dimly recognized as regards their true source, and entirely misunderstood as regards any other.

Let us now clinically sketch a case of first labor. It may be presumed that the woman is married, of a proper age, her surroundings favorable, and that all her bodily functions are in good working order. We will grant that she has arrived at the full term of utero-gestation. Pains begin, at first slight, and then more severe. I need not hazard any conjecture on the causes which determine the commencement of pains at the completion of the term of utero-gestation; it is enough in this place to say that so it is appointed. It is, however, important to observe, and is never to be forgotten, that each pain is followed by an interval of rest. In spite of flattering first impressions, experience now dictates the negation that no adverse agencies, direct or indirect, are at work; that there is nothing in mind, body, or estate, operating injuriously—nothing inimical to uninterrupted progress, nothing likely to disturb the even course and finish of the coming trial. Have seduction, desertion, depression, fatalism, etc., any occupation to lower, preoccupy, spoil, divert, harass, the unfettered play of the nervous power, and cripple its undivided exercise? Let no stranger intrude; let the officiousness of friends be curtailed; and, above all, let the practitioner take care that he himself is not the bearer of any pestiferous agency.

All these preliminaries being duly considered and disposed of, let us fancy that in due time the head has been brought to, or nearly to, the perinæum, and there remains. Time is precious; thoughts of pressing engagements intrude themselves; the loss of a night's rest is imminent. The occasion is a tempting one to try something to rouse these tiresome efforts into something like

a decent state of activity and progress. Ergot has an abiding place in most men's minds, and usually turns up uppermost. What indication could be clearer? A few good pains would do all that is needed. Ergot produces pains, and therefore ergot is just the thing required. Ergot of course is given. Probably no action is induced; possibly a violent, long-continuous one; but no further advance is effected. Let it not be forgotten that, for successful efforts, there must be a general consensus of conditions. Who has not observed how the patient, by a wondrous intuition, anticipates the coming struggle? Who so unobservant as not to know when that is approaching by the flow of lubricating fluid? In short, the woman instinctively prepares herself by position, by inspiration, by profound rest, and by silence.

How does the action of ergot accord with these conditions? What is its action? "The contractions and pains caused by ergot," Pereira, "are distinguished from those of natural labor by their continuance. Scarcely an interval can be perceived between them, but a sensation is experienced of one continued forcing effort."

Nature beneficently ordains that the uterus shall fulfil its office by alternate pain and pause. Ergot obliterates the normal rhythm. Nature says a congeries of conditions is essential for the final effort. Ergot violates them all, invites irregular contraction and its consequences, and delivers the patient an easy prey to the immediate and ulterior probabilities of hæmorrhage and of septicæmia. In addition, let it not be forgotten that in primiparæ the parturient passage is unstretched, and not yet to the manner used,

In primiparæ, ergot may be useful, or rather used, in some cases, to save the time of the attendant, all other things being favorable; and, if it act and succeed, all may be well. It is equally evident, however, that if it succeed, the success will be suspicious; and if it do not, it must be calamitous.

It may be asked, not unreasonably, by younger members, if ergot should not be used, what may? Tax voluntary and emotional effort. Draw on the rich fund of direct and reflex action. Initiate contractions by a lump of ice in the stomach; by frictions to the abdomen and spine; a stimulating injection into the rectum for one reason, or one of hot water for another; by a dig at the sacral plexus, or a pull at the perinæum, on the access of pain; by the sudden application of cold, with a bellows if you please, to the sacrum; and by many other means yet to be found out. Try all or some of these, and, after them, the forceps.

We will now suppose that, by a grand effort without ergot, the head is born. There is no need of hurry. There is no necessity to seize it with both hands and drag the body into the world; let it alone. Bearing in mind what has just happened, and how it happened, the right hand must be placed on the bare abdomen of the mother, and gently carried over every available surface of

the uterus, and repeated till contraction comes. It comes soon, and by it the shoulders and body are usually expelled.

The child, being now free to kick and cry, should be brought near to the edge of the bed, and the bedclothes put down between the mother and child, so as to cover the mother and expose the child. The hand should then again be carried to the abdomen, and gentle manipulations exercised, till not only angularities, but nodosities and even rugosities, are obliterated, and the uterus has become round and smooth.

The baby can now be attended to, separated, and delivered over to the nurse. This being done, the uterus must again be similarly attended to. After another short interval, the cord should be wound round the forefinger of the left hand; and at the same time the forefinger of the right hand run up the cord to the os uteri, and traction made. If the placenta be ready to come, it will come by a gentle effort. If the insertion of the cord or an edge of the placenta can be felt, then, by a combined action of traction by one finger and hooking with the other, the expulsion of the placenta is nicely assisted. The time occupied is generally under ten minutes. If the combined effort do not succeed, a little more time must be given, the uterus again stimulated, and the trial renewed.

After the placenta has come away, the uterus must again be tested as to its firmness and rotundity, and this twice again repeated, before the patient can be considered safe. In perfect contraction of the uterus is comprehended the safety of the patient, immediate and prospective. Up to this period it is no part of the attendant's duty to leave the room; much less to adjourn to the drawing-room, to scatter congratulations all around, and, perhaps, to the health of the new-born stranger. Such customs are more honored in the breach than in the observance.

This practice, simple as it is, may be managed in a different way by others. Some have the hardihood to affirm that the uterus should be grasped at the moment of the birth of the child, and the placenta forced down *vi et armis* and out by the same pain that expelled the child, or, at the longest, by the next. The practice seems to be founded on a mistake. What occurs when the hand is placed over the uterus? The slightest touch produces an immediate reflex action and a contraction of the uterus; and it is thus by a series of successions that the crooked uterus is made straight, and the rough places smooth. Thus it is that the placenta is readily brought down. On the contrary, if an untimely contraction be produced by forcible pressure, it follows that a corresponding relaxation must ensue and danger come.

How does forcing down the placenta by one fell swoop agree either with nature's method, or with the teachings of reflex physiology? Admitting that the pain which expels the child separates the placenta, surely it is more in accordance with nature's doings that it should be expelled gradually and by

gentle efiorts. It is unpractical to suppose that the uterus should pass from extreme distention to extreme contraction without a rebound to the intermediate state of vacuity and of peril. Again, I am not prepared to say what changes precisely occur in the relative circulation of the placenta and the uterus after the birth of the child. It is quite certain that the cord pulsates for a short time afterwards, and that the uterus has at least to adapt itself to its altered contents.

If the waiting plan be adopted, the placenta may be retained for an indefinite time; and then instructions are given, even to women, to introduce the hand and remove it. This direction is given with the greatest coolness, as if it were an easy or a safe thing, particularly for a woman, to introduce the arm into the uterus and rifle it of its contents. Such advice can only be given in utter disregard of the peristaltic action of the uterus itself. Ergot, forcible extension of the placenta, introducing the hand to remove it—these three can only be regarded as practices of misleading efficacy, and as *membra disjecta* of the mechanical school. They must be reserved for cases alone of special exemption. In this counterblast, of course, the triumphs of cautious instrumentalism are not included.

Some practitioners, immediately after the birth of the child, think that their next duty is to separate it; and whether the child cries or not, proceed to do so. Under the cover of the bedclothes, perchance the lady should for a moment be exposed to cold, or for some other equally cogent or perhaps unknown reason, the separation is mysteriously accomplished. It is better to avoid such subtegumentary proceedings, involving as they do the expenditure of critical time and a risk of the mutilation of a penis or a finger; and also if it be wished to prevent that bane of infant well-doing, "atelectasis pulmonum."

It may be expected that something should be said about the binder. I have little to say, except in its condemnation, if used for the purpose of restraining uterine hæmorrhage. If applied when the uterus is well contracted, it is useless; if when relaxed, it will be injurious by covering over and concealing the source of danger. Its great use is as a surgical appliance in giving support to the relaxed abdominal parieties, securing their equable adaptation to the abdominal contents, and thus facilitating their contraction and timely involution. On this account, it is neither well nor fitting to pull the patient about, when quiet is so essential, to adapt the nicely made bandage. An easily applied slip is all that is needful for the first twenty-four hours.

My conviction is that the bandage should be continued for at least a month after each confinement, if we are anxious to preserve the normal relation of the axis of the uterus to that of the pelvis, and so prevent delay and danger at subsequent confinements; or if we desire to avoid the unseemly production of a pendulous belly. Let cases be stripped of the accidents, largely avoidable, which we have been considering, and how many

remain to be dealt with? Only those unforeseen and unavoidable cases of peculiar hæmorrhage, dependent on idiosyncrasy, examples of which were so well and so courageously related by Mr. Crisp at our last meeting.

In such cases, it is not to be expected that remedies having a reflex operation should be of all avail; more immediate and direct means must be employed, as the hand to the internal surface and the introduction of various substances into the cavity of the uterus. I say introduction, not injection. When, in the induction of premature labor the injection of water alone has proved fatal, I think we should be cautious how we inject fluids which may prove fatal not only in that way, but by reason of their engendering septicæmic sequelæ. Besides, the styptic fluid needs only to be applied to the surface of the uterus, and should not permeate the sinuses.

It will have been seen that principles rather than practice, except in some of its more prominent bearings, have engaged our attention; that the aim has been to inquire how and by what agencies nature manages the process of parturition, and to insist that she must be acknowledged, not ignored; assisted, not forced; unloosed, not fettered; supplemented, not supplanted—in her operations; and that physiological actions rather than mechanical expedients should be had recourse to and depended on, and to show that the glory of contingencies consists in their prevention; that the eternal law of labor and of rest should be borne in mind in all our doings, and also that the more violent the action and consequent exhaustion, greater will be the probability of prolonged rest and of undue relaxation; hence the occurrence in the greater degree of hæmorrhage, and in the lesser of imperfect contraction and subsequent septicæmia.

It may be objected that such a sketch is visionary and unreal, and that it refers to the occupants of a too happy land, and that in the hurry and bustle of this lower world accidents must needs come, and therefore it is folly to ignore, taboo, and discard the well used helps of obstetric necessities. All I contend for is that those helps should be studied from a different point of view; that the act of parturition should be looked on as a natural process, to be gently assisted by natural means, and not by any foreign to, and entirely subversive of, her teachings, and that general principles are applicable and adaptable to every emergency.

Let it be borne in mind that midwifery has not till very lately, and certainly not now generally, been studied in this way; that labor has been looked on as a mechanical act, to be completed as soon as commenced, and the sooner the better; that the effects of the nervous relations of the uterus are only just beginning to be perceived; and that in this direction, and from this source, our future remedies are to be sought and are to be found.

I am quite aware it may be said that the standard is placed much too high for such a common and everyday occurrence as

that of parturition. Be it so. The object is the alleviation of pain and suffering; to aim higher is impossible; lower would be reprehensible.

My conviction is that, till the science and art of obstetrics are placed on a physiological and not on the prevailing mechanical basis, we shall continue to be the slaves of routine, and our patients the victims of fortuitous complications; and that then, and not till then, will the possibility of hæmorrhage be mainly prevented and the chances of septicæmia reduced to a minimum.—*British Medical Journal.*

On the Treatment of Rheumatism and Gout with Trimethylamine.
By W. H. Spencer, A.M., M.D., Cantab.

Twenty years ago Dr. Awenarius, a Russian physician, first used trimethylamine (under the name *Propylamine*) in the treatment of rheumatism. He recorded 250 successful cases, and looked upon the remedy as little less than specific. Since then, much has been done on the Continent to work out the action of trimethylamine on the physiological and clinical sides. The memoirs of Dujardin Beaumetz, Peltier, Bourdit, Cottard, and Hambdy, especially merit notice, as giving with much detail all that continental workers have as yet made out about the properties of the remedy and its therapeutic effects. Whatever may have been done in this country to test and verify what the continental writers have set forth, and to carry on their work, nothing on the subject has been published. During the past eighteen months I have treated almost all the cases of rheumatism, and some cases of gout that have come in my way at the Bristol Infirmary, with trimethylamine in one or other of its forms. I have records of a large number of cases, some analyses of urine and thermometric observations made during the treatment, and some observations in comparison of this mode of treatment with other modes, made in the same patient and in different cases. This experience I now propose to place before the profession. I propose, first, to give a brief *résumé* of the chemical history and properties of trimethylamine. This, because it is important to know as precisely as may be what it is that we speak of as a new remedy in rheumatism, and because there has been much difficulty and discussion about this question. Next, to record my facts, to relate some of my cases, and to give an analysis of others. And I shall not relate my successful cases only; the record of my experience shall be as faithful as I can make it. Finally, I propose to give some thoughts of my own; these shall be chiefly in the form of commentary on the cases as they come.

First, then, as to the history of trimethylamine, and as to what it is.

Wertheim, in 1850, prepared from narcotine a body having the formula C_3H_9N; he called it *metacetamine*. Anderson, in 1850, prepared from codeine a body having the same formula; he named it *propylamine*. Hoffmann, also in 1850, showed (from his own and Würtz's researches) that by the substitution of three molecules of methyl for the three hydrogen atoms of ammonia a body was formed having the formula C_3H_9N; this body was, according to Hoffmann's nomenclature, *trimethylamine*. Wertheim soon after extracted from herring-brine a body isomeric with his metacetamine; thenceforward he adopted for both bodies the name propylamine. And now, many chemists drew from many different sources, animal and vegetable (cod-liver oil, human urine, putrid calf's-blood, guano, ergotine, chenopodium vulvaria and numerous other plants), a body having the formula C_3H_9N. Always, this body was called propylamine; the substitution body prepared artificially in the laboratory was, as yet, the only trimethylamine. Then the reconciliation took place, and chemists (following up Würtz's and Hoffmann's researches) found that the body got from natural sources was not propylamine at all, but the same in all chemical respects as the artificial compound ammonia called trimethylamine. *Propylamine* is a primary compound ammonia (monamine). *Trimethylamine* is a tertiary compound ammonia (tri-amine). Thus:

$$\left.\begin{matrix}\text{Propyl } C_3H_7\\ H\\ H\end{matrix}\right\} N = C_3H_9N \text{ (Propylamine).}$$

$$\left.\begin{matrix}\text{Methyl } CH_3\\ CH_3\\ CH_3\end{matrix}\right\} N = C_3H_9N \text{ (Trimethylamine).}$$

Essentially the difference is in the radical; *propyl* is one thing, *methyl* is a different thing altogether. It is methyl that exists in the body extracted from narcotine, codeine, herring-brine, and all the natural sources already named; which exists, that is to say, in our new remedy for rheumatism. I do not know that propylamine has been obtained from natural sources at all. I believe it is as yet only an artificial product, got by treatment of the iodide of propyl with an alcoholic solution of ammonia.

But more than that, the artificial propylamine differs from the natural trimethylamine in these respects: it has a different odor, its boiling point is 49° C. instead of 9° C., and in its chemical reactions it gives results quite different to those of trimethylamine. The name propylamine should be discarded without delay for the substance now used medicinally; this is trimethylamine, or at least contains it. But what we use as a remedy is not purely and only that body which the formula C_3H_9N represents. No chemically pure trimethylamine has as yet been used in medicine, not even in the important physiological experiments of M. Hambdy. The substance used in medicine is the product known commercially as propylamine. This substance is obtained chiefly by distillation of herring-brine with

potash or lime, and subsequent more or less repeated rectification. It is a mixture of trimethylamine (C_3H_9N), ammonia, and ammoniacal compounds of unknown composition, with probably other organic compounds, all in solution in water. The amount of ammonia and organic impurity (chiefly animal oil) in the solution depends upon the greater or less completeness with which the process of rectification is carried out. If the brine is simply distilled with lime, and the products condensed in water, the resulting solution will contain much ammonia and impurity. If, on the other hand, the rectification is carried out fully, as in the process presently to be described, there results a tolerably uniform product containing only traces of ammonia and organic impurity. The composition of the commercial solutions, as might be suspected from this, has been found to vary greatly. Nor is this variation in composition due, even chiefly, to the mode of preparation. MM. Girardin and Marchand analyzed specimens of Scotch, Yarmouth and Channel brines (*Journal de Pharmacie et de Chimie*, 1860) during several years, and found, not only that the composition and quality of the different brines varied greatly, but that the same brines varied in different years. M. Petit examined numerous specimens of commercial propylamine, not only French, and found that the amounts of alkaline products in the solutions varied from 2 to 55 centigrammes in the gramme. Since these alkaline products really expressed several ammoniacal compounds as well as trimethylamine, the quantity of this supposed essential ingredient must have been, in some of the specimens, extremely small. Within the past two years, however, the process of manufacture has been much improved. In the solutions now supplied for medicinal use, at least by our best manufacturers, constancy of composition and purity are very fairly attained. The solution I have used was supplied by Messrs. Ferris & Co., of Bristol. They inform me that the solution they sell is prepared solely from herring-brine, and by the following process. The brine is distilled with soda-lime and the products condensed. The alkaline distillate is treated with hydrochloric acid and evaporated. The residue is treated with absolute alcohol, whereby ammonium chloride is separated. The alcohol is driven off, what remains is distilled with caustic lime or potash, and the products are condensed in cold water. The solution in water is usually sold as "propylamine." But if rectification is pursued still further, the products are condensed in hydrochloric acid. This acid solution is evaporated to dryness, the residue again treated with absolute alcohol, and again distilled with lime. This last process is repeated until the product is uniform. The solution as supplied to me contains 20 per cent. of this final product. From careful consideration of this process, I conclude that the solution is a 20 per cent. solution of an alkaline product (expressed as trimethylamine) with traces of ammonia and organic compounds not ammoniacal. The alkaline product contains trimethylamine

and undetermined ammoniacal or other compounds. What may be the absolute proportions of trimethylamine (C_3H_9N) and other compounds in this alkaline product appear to be quite unknown. Constancy as to the alkaline product is certainly attained, but constancy as to the amount of trimethylamine in the product would seem still to depend on the quality of the original brines. With a view to avoid inconstancy and impurity in the commercial product, a salt of trimethylamine has been used in medicine instead of propylamine. This salt is called, according to the barbarous and corrupt nomenclature perpetuated by our pharmacopœias, hydrochlorate of trimethylamine. It is in reality a chloride of trimethylamine, and is prepared either by halting at a particular stage in the process of rectification of the propylamine solution, or synthetically from tetramethylammoninm iodide. In the former case the base is not simply the body represented by the formula C_3H_9N, any more than the trimethylamine of the commercial propylamine is that body. It is in fact, both in the chloride and the commercial solution, C_3H_9N with other compounds existing in herring-brine, all separated as chlorides during the process employed. The chloride is of necessity only an approximately pure salt; it contains a notable quantity of ammoninm chloride, the presence of which is essential to the stability of the salt in the solid form.

Such being the state of the case as regards the chemistry of the product "propylamine" and the chloride of trimethylamine now used in medicine, the question arises what in these may be the efficient cause of their effects. The effects may be due to the trimethylamine (C_3H_9N) or to the other ammoniacal compounds contained in the products, or perhaps to both. The effects of the synthetically-made chloride of trimethylamine could not be taken to settle the point; for a salt like a chloride is, in the body, a very different thing to a solution of the base. These two—the commercial product containing trimethylamine (C_3H_9N) with other ammoniacal compounds, and the chloride of trimethylamine—are clearly not interchangeable as remedies until it can be shown that their effects are precisely the same. The similarity of effects has not yet been shown. Again, the experience of the effects of the alkaline treatment of rheumatism, and I may say also of gout, can hardly go for nothing when seeking in the composition of a substance like "propylamine" a cause for any effects it may produce. I confess on my own part to a strong suspicion that the ammoniacal compounds will be found to take at any rate the larger share in the effects. It is clear that in estimating and comparing the therapeutic effects of the commercial "propylamine" the variability in its composition must be taken into account. Much that has been anomalous in the experience of the remedy may possibly be accounted for if solutions varying greatly in composition have been used. It is a first condition in therapeutic observation that the composition of drugs should be referrable to known standards. In the

particular case, if this condition were fulfilled, a decision upon the absolute and relative values of "propylamine," pure trimethylamine, and the pure chloride could very soon be arrived at. Nor would it appear to be difficult to fulfil the condition. M. Würtz* has proposed a way of preparing trimethylamine artificially; the solution obtained by this process can be titrated, like any ordinary solution of ammonia, to a definite and constant standard. Many points might be soon cleared up if the effects produced by such a solution were compared with the effects produced by the ordinary solution now supplied by pharmacists, and if both these again were compared with the effects produced by a product obtained by distillation of herring-brine with lime or potash and not subjected to subsequent rectification.

Both the solution and the salt have generally been prescribed on the Continent in the form of mixture, the vehicle being aqua menthæ piperitæ, and aromatis being used to disguise the unpleasant taste. The dose employed has varied much; it has ranged from 9 to 30 minims for the solution, and has not exceeded 10 minims for the salt. The chloride has been used in the form of pill (made up with marsh-mallow, honey, and tolu), and the solution in the form of capsules. I have, except in two instances, invariably prescribed both trimethylamine and the chloride in the form of mixture, and my formula is this:

℞. Trimethylamini ℥iv. vel viij℥
Syrupi zingiberis ʒj
Aquæ menthæ piperitæ ad ℥j. Misce.
Talis fiat modus, secundâ quâque horâ sumendus.

And, as supporting the probability of variation in composition, I have at different times found that the same results were obtained with a dose of two minims as with a dose of eight minims. Now, I give always four to eight minims, at first every hour or two hours, increasing the interval as the pains diminish and the case progresses. When all pain is gone I cease to give the remedy, and substitute, in most cases, quinine. Rarely have I to re-exhibit the trimethylamine, but I do so if pains recur.

Next, for my cases. They are sixty in all, and of these I propose to relate only so many as may be of use for illustration of the effects of the remedy, and for commentary on the effects—whatever they may have been.

Case I.—G. C., aged 50, a coachman. A healthy man up to forty years of age, and always in good circumstances; had lived freely and drank a good deal of beer. When forty had an attack of gout, the toe only being affected. The next year had another attack; this time the knees were affected. During the next eight years had four attacks, in the last of which (six months ago) several joints were affected, and he was in bed for six weeks. He always remained weak for some time after the

* Report to the Paris Pharmaceutical Society, March 9, 1873.

attacks. No family history of gout or rheumatism; has a tophus in the auricle of the right ear. The present attack dates back three weeks; whilst washing his carriage he thinks he caught cold. The first joint affected was the metacarpo-phalangeal of the right third finger (he says he had received a blow on this finger just before). Subsequently the left knee and hip and the right elbow and shoulder became involved in succession. Admitted on June 20th, 1873. He has pain in all the joints above named, severe and preventing movement, and nearly all the affected joints are swollen. The local heat is considerable, without redness. There is little general pyrexia; the skin is moist; tongue white; pulse 80; has no appetite. The urine is acid, specific gravity 1·020; no albumen. There is no cardiac affection, old or recent. *To take two minims of trimethylamine every four hours.* 21st: There have been frequent remissions in the severity of the pains; otherwise is much the same. 22d: Has no pain at all when lying still; the pains, he says, ceased suddenly at 10 a. m. (forty hours after the first dose). 23d: Slept well; has a little pain at times in the shoulder and in the finger-joint of the left hand; can raise himself in bed with little discomfort; the swelling is hardly appreciable; he feels much better, and *very hungry.* 26th: Has no pain at all; can move freely, and feels well. *To take the mixture three times a day.* 28th: Is up and able to walk easily. *To take quinine.* July 6th: Some slight pain and swelling have recurred in the finger joint of the left hand. Has been in the garden daily for four days. 9th: The finger got well very soon, and he was discharged to-day well and strong.

[This I take to have been a case of gout. The man himself expressed surprise at the rapidity of his recovery, as compared with all the previous attacks. Two points in the case deserve notice: in less than three days the appetite returned; the man felt hungry and requested more food; the recovery of strength was rapid and marked. These are very frequent effects of the remedy.]

Case II.—J. B., aged 49, a brewer's cellarman. Has been working in wet cellars, frequently standing in the wet. About once a month, for some time past, he has had attacks which compel him to keep in bed for a week. These begin with pains in the wrists, thence extending to other joints, and finally involving the whole body; he loses all power of motion, and even has to be fed. In the intervals he is weak, has frequent pain, and is unable to move freely. Admitted as an out-patient April 8th, 1873. He is now reduced to a very crippled state, and is weak and miserable. The left leg, from the knee downwards, is much swollen, livid, and purpuric. From this date to June 24th was treated with quinine and iron, cod-liver oil and iodide of potassium, with improvement only to the general health. June 24th: Has now pain in both legs and the left shoulder, which is very severe at night. The attacks have occurred as before. He walks with great difficulty. *To take two minims of trimethyla-*

mine three times a day, and pills of iron and quinine. Took the medicine for a week, and came up on July 8th, no better. Then he took the medicine *every four hours* for a week; he felt much better the week after, but during the following week had one of his attacks in a modified form. Came up on July 29th. The mixture was repeated, and in a fortnight, August 12th, he was quite free from pain. A fortnight later, August 26th, he was still free from pain, and much stronger. *He was put upon quinine.* In another fortnight, September 9th, he was still free from pain, and had no attack. Was much improved in health; appetite good. The leg was better, but still somewhat swollen and purpuric. Ceased attendance. I saw this man in September, 1874, twelve months after he ceased attendance. He was then looking well; had been quite well and quite free from anything like rheumatism. The leg was better but still stiff and brawny.

Case III.—A man, aged about 45, had been an in-patient from January 28th to July 7th, 1873, for chronic rheumatism, dating from November, 1872. He had been treated in many various ways, with some relief; but had never been quite free from pain. He came under my care, as an out-patient, July 11th, 1873. At this time he suffered severe pain in the shoulders, hips, knees and ankles; was very weak and decrepit, could not stand upright, and walked with great difficulty. *He took two minims of trimethylamine every four hours, and pills of quinine and camphor.* In a week the pains were much better, but he had had some diarrhœa. In a fortnight more he was quite free from pain, and was then put upon *quinine and cod-liver oil.* A month after he was still free from pain, and was altogether a different man; he was able to walk without a stick, and was back at his work.

[These two cases of chronic rheumatism are interesting from the fact that both patients, before taking trimethylamine, had been subject to prolonged treatment in other ways. The first was under the care of a colleague, and the other under my own care during the whole of this time. Both patients were intelligent and respectable men. From the fact that, being out-patients, the men could attend at their discretion, and were only supplied at each visit with medicine enough to last for a week, little can be said about the effects of the remedy beyond the broad fact that these men were cured of a long-standing ailment, the one within three weeks, and the other within five weeks. It is noticeable, also, in Case II, that two minims of trimethylamine taken three times a day failed to produce any good effect.]

Case IV.—A. S., aged 11. Had been ailing for a few days, when he got very hot, and then stood about without a coat in cold and damp night air. Felt very chilly, and shivered the same night, August 11th, 1873. The next morning he had pains all over, especially severe in the fingers and groins. On the 16th had not much pain except in the groins, where it was worse

than before. On the 18th he was very bad; the groins were very painful, and now also the right ankle was extremely painful and swollen. There was considerable pyrexia; the tongue white and no appetite. The treatment up to this time had been chiefly by warm baths. *To take two minims of trimethylamine every two hours* (began at 12 noon). 19th: The pains remitted during the day, and by the evening were much less severe; the ankle was less swollen. 20th: slept well during the night. On awaking at 6 a. m., the boy found he had no pain anywhere, so he got up, dressed, *put on his boots*, and went about his play as usual. He was very hungry at breakfast, was pretty well all day, and walked about a good deal. *Citrate of quinine and iron* was substituted for the trimethylamine at noon. All trace of the attack passed off in a day or two.

[This case occurred in my own house, and is of interest and value because I was able to watch it closely throughout.]

Case V.—W. M., aged 31. Had been in the infirmary for gonorrhœal rheumatism until August 14th, 1873. Was readmitted on August 28th, with very severe lumbar pain and pain in the left foot; the foot was swollen. *To take four minims of trimethylamine every two hours.* The pain gradually decreased until, on September 5th, he had no pain at all when lying still, and felt better in himself. The pains did not leave him entirely, and on September 10th he was put upon *iodide of potassium.* September 15th: Continues much the same; the back is bad again, and one knee is affected. *To take five minims of trimethylamine every two hours.* September 22d: Is much better; the pain is gone. Was put on *bicarbonate of potash with liquor cinchonæ.* September 27th: He is worse again. The iodide of potassium in ten-grain doses was repeated. October 18th: The iodide has been continued; the man has improved, and is discharged to-day, pretty well.

[I have not found that trimethylamine is able to do any good in such cases as this. Yet it would seem that some temporary relief occurred on the two separate occasions of its exhibition. In another case, a man who, eight months before, had been in the infirmary for syphilis, was admitted under my care for rheumatism. I treated him with trimethylamine on four separate occasions, with some relief to pain each time. A cure was at length effected by iodide of potassium.]

Pills of Treacle and Soap in Congestion of the Liver.

In the case of subacute congestion of the liver and of the biliary canals in relation to or in conjunction with phenomena of gastro-intestinal irritation, Dr. Ferraud advantageously prescribes pills of treacle and of medicinal soap mixed, in proportions varying as there may be need to favor the dissolving and

secretory action which is the effect of the soap, or the effect of the treacle which tempers and regulates the action of the intestine.

They prescribe two to four pills per day, each containing—

Treacle - -	6⅞ grains,
Medicinal soap - -	7½ to 3 grains.

The digestive troubles with congestion of the liver justify this prescription.—*Medical Therapeutics.*

Treatment of Itch.

Dr. Weinterg advises the following preparation:

Liquid storax, Flour of sulphur, White chalk,	4 drachms 8 grains.
Green soap, Animal fat,	8 drachms 16 grains.

This pomade has a greenish yellow color, a consistency and odor rather pleasant. The patient uses it before going to bed, taking care to rub briskly the region where the eruptions are usually found. These frictions are renewed two days in succession. The patient may during the day follow his usual avocations. At the end of three days the patient may take a bath for cleanliness. For children at the breast an equal portion of simple ointment must be added.—*Translated from "Journal de Médecine et de Chirurgie Pratiques."*

De Pietra Santa on the Pathogenesis of Diseases from Morbific Ferments, and their Treatment by Alkaline Hyposulphites and Sulphites.

Dr. De Pietra Santa has published, in the *Mouvement Médical*, p. 91, advance sheets of the chapter on medication by sulphites, from the volume which he is about to publish on the rational treatment of pulmonary phthisis (*Le Traitement Rational de la Phthisie Pulmonaire*). He announces that treatment by the alkaline and earthy sulphites has been and will always be fruitful in good results. The experimental researches of Dr. Polli, of Milan, have been conducted with all desirable science and skill. (See Dr. Timermans' report to the Academy of Turin.) These researches of Dr. Polli on diseases from morbific ferments, and their treatment by alkaline and earthy sulphites and hyposulphites, have given rise to numerous ideas, writings and practical applications of the treatment in Italy, England, and Belgium. Dr. De Pietra Santa has thought it his duty, before making

public the contingent of facts and observations collected by him during a first period of ten years, to thoroughly determine the theoretic principles of medication by sulphites, and to make known with impartiality the results obtained by a large number of distinguished practitioners. Numerous experiments made on animals with all the guarantees of scientific exactness, have demonstrated that the artificial injection of certain morbific substances produces serious and well-marked diseases, presenting the characteristics of catalytic affections. 1. The injection of a certain quantity of pus into the blood produces pyæmia, and the affections characterized by multiple of abscesses. 2. The injection of putrid matters produces septicæmia; that is to say the diseases known under the name of putrid infections, and characterized by the symptoms of gastro-enteric fever. 3. The injection into the blood of materials eliminated by the mucous membranes in certain contagious diseases, as glanders, reproduces the glanderous affection itself. The question then arises whether the morbific ferments in the blood of animals can be neutralized, without altering that liquid so as to be irreconcilable with life; since facts and varied experiments lead to the admission of this neutralization, notwithstanding the doubts of Claude Bernard, who writes that "naturalization of ferments is impossible, because, for that purpose, it would be necessary to change the properties of the blood to such an extent that life would no longer be possible."

By examining the action of sulphurous acid on organic substances, it is easy to become convinced that it is one of the most powerful of the antifermentative agents. It prevents or stops all fermentation of known organic matters, and impedes the putrefactive metamorphoses in the animal tissues and liquids.

Sulphurous acid acts on the fermentative principle, not by decomposing it, like most so-called antiseptic substances, but by simply modifying its molecular aggregation. Its antiseptic action is as powerful as that of arsenious and hydrocyanic acids, without being, like the last, a poisonous substance. The impossibility of administering sulphurous acid either pure or in a gaseous state, or in a watery solution, has led Dr. De Pietra Santa to combine this acid with alkaline or earthy bases, under the form of sulphites of potash, soda, lime, magnesia, etc. These sulphites possess the antifermentative properties of sulphurous acid. Their action is even more constant and more penetrating; and, as they are perfectly tolerated by the living organism, they may be administered internally in medicinal doses, and sometimes in the liquid state. Absorbed by the lacteal vessels and by the veins, the sulphites diffuse themselves throughout the blood-mass, exercising a specific and benign action on the various materials of the blood and on the tissues derived from it, during the whole time indispensable for their complete transformation into sulphates. This transformation, which is produced

by the oxidising action of the phenomena of respiration, takes a somewhat long time.

In fact, whilst the sulphites are found again in the urine a few hours after their administration, it is only at the end of twenty-four hours that they are eliminated in the condition of sulphates. To prolong the presence of the sulphites in the organism, and to retard their conversion into sulphates, when it is desired to protect the blood against the absorption of a morbid ferment, hyposulphites of the same bases must be substituted for sulphites. These hyposulphites require a longer oxidising action to bring them into the condition of sulphates, and they at the same time provide a means of carrying and introducing into the blood very active sulphites in the nascent state. Thus, as a general rule, the sulphites and the hyposulphites which are introduced into the organism pass into the secretions in an always higher degree of oxidation; the hyposulphites as sulphites, the sulphites as sulphates.

The seventy experiments instituted on animals for the purpose of demonstrating the possibility of neutralizing the morbid ferment may be divided into three categories—*A* with pus, *B* with putrefied blood, *C* with glanders. They completely confirmed the principles laid down, whilst furnishing an amount of complementary information, useful in showing with greater certainty the numerous therapeutic applications of this new system of medication. 1. The blood, urine, viscera, and muscles of animals submitted to the daily action of alkaline sulphites, are, after death, less quickly attacked by the phenomena of decomposition and putrefaction. 2. Animals to which a certain quantity of sulphites has been administered resist the morbific action of pus, putrefied blood, and glanders injected into their veins, whilst the same doses of virus produce serious disease and death in animals to which sulphites have not been administered. 3. Animals submitted to the injection of putrid matters (pus or putrefied blood), into whose veins a solution of alkaline sulphite is immediately afterwards injected, resist infection or recover in a few days; whilst other animals, placed in identical conditions, but without the sulphites, succumb to typhoid fever. 4. The same animal which has undergone putrid injection without any serious accidents, because it had been previously submitted to the action of sulphites, becomes seriously disordered or dies, if it receive a fresh injection of the same quantity of putrid matter without having been sulphitised. 5. In animals inoculated with the mucus of glanders, and treated before or after the operation with sulphites, even when the resulting wound is converted into the characteristic sore of glanders, a sensible modification of the sore is noticed, followed by cicatrisation. The same inoculation of glanderous mucus in other animals not subjected to the influence of sulphites, produces, *in situ*, a phlegmon which kills in a few hours, or a general infection with multiple abscesses leading to marasmus and death.

It now becomes a question, to which alkaline salts the preference should be given. The sulphite of magnesia in the solid state is easily administered internally. The sulphite of soda, which effloresces readily on contact with the air, possesses a very marked odor of sulphurous acid; very soluble in water, it is better in fresh solutions for dressings, lotions, or external fomentations As the sulphites of potash and ammonia have a very disagreeable flavor, and change very easily, they should be reserved for the external treatment of certain sores of a malignant nature.

The hyposulphite of soda may in many cases be substituted for sulphite of magnesia. It is very soluble, and its flavor is very tolerable; its most logical indication is as a prophylactic. As it contains more sulphur, it furnishes, under the influence of the action of the respiratory organs, sulphite of soda in the nascent state in the heart itself.

The hyposulphite of lime, which has not a disagreeable taste and keeps well, is specially indicated when it is desirable to protect the organism against the effects of purulent absorption, or to favor calcareous deposit and obliteration.

It is, however, in the class of diseases arising from purulent absorption that the most strongly marked therapeutic results should be found, because in them are found conditions of genesis analogous to those which were shown in experiments on animals.

Drs. Sestini and Ferrini, of Turin, have administered the hyposulphite of lime with success in several cases of tuberculosis in the third stage, with nocturnal fever and abundant and fœtid expectoration. Polli has already made an important series of observations, which all tend to demonstrate the serious modifications in the augmentation of the general vitality which take place in the nature and quality of the matters expectorated in the height of hectic fever. In each case, the diagnosis of the disease is established on the general symptoms and on the direct signs of the anatomical change furnished by auscultation and percussion. Dr. Rodolfi says that with the sulphites of magnesia he has been able on several occasions to combat violent attacks of fever with shiverings, in tuberculous patients arrived at the third stage, when this fever of consumption had been treated without success by preparations of quinine. Brunetti, of Constantinople, combats zymosis of the respiratory passages by preparations of sulphite of soda and hyposulphite of lime.

Zalloni and Parascheva, of Syra, congratulate themselves on having been the first to employ sulphites of soda, which have yielded surprising results at the commencement of tuberculosis. Dr. De Pietra Santa states that he has seen the excellent effects of hyposulphite of lime on the second and third stages of pulmonary phthisis. It is well known that in this terrible disease there is a time, in the cavernous stage, when the treatment can only aim at patching up certain morbid phenomena and prolonging

the life of the patient. At that time. the pus which is not entirely eliminated by expectoration is absorbed into the organism; and this purulent infection is shown by the nocturnal fever, the diphtheritic irritation of the buccal and gastro enteric mucous membrane, the colliquative diarrhœa, the nocturnal sweats, and the general emaciation. To prevent this decomposition of pulmonary tissue and to combat the putrid absorption, logic bids us have recourse to therapeutic agents capable, first, of protecting the blood against the catalytic action of the pus which penetrates into the circulatory torrent; secondly, of thus diminishing the purulent exudation in the pulmonary cavities.

It is here that clinical observation has stepped in to demonstrate that alkaline hyposulphites are perfectly indicated to prevent this general decomposition; to arrest the deleterious action engendered by the purulent focus in the blood; to moderate the partial phlogoses of the pulmonary tissue; and, finally, to induce the phenomena of deoxidation. By protecting the blood against the catalytic action of the pus which penetrates into the circulatory torrent, they diminish by so much the purulent exudation in the pulmonary cavities.

As to the hyposulphite of lime, independently of the qualities which it shares with the salts, its congeners, it offers a certain and convenient means of introducing lime into the body, with the effect of favoring the transformation of tuberculous into cretaceous or plastic matter. This salt, the hyposulphite of lime, penetrating into the blood, thus fulfils the double indication of opposing itself to the effects of purulent absorption, and of effecting a calcareous saturation of the organism. The hyposulphite of lime, associated with the phosphate of iron, is indicated when complications of the phenomena of dysmenorrhœa, chlorosis, and general weakness, are met with.—*London Medical Record.*

The Etiology of Puerperal Fever. By Robert E. Huntley, M.D., Jarrow-on-Tyne.

I am induced by the letters signed "Obstetricus" and "Junior" to offer a few observations on the above subject.

In the *British Medical Journal* of August 5th, 1865, I gave details of a number of cases of puerperal fever, which had occurred in my practice a few months previously. It was manifest that the outbreak was in some way associated with me, for it was exclusively confined to my practice, notwithstanding there were four other practitioners in our town. I took all the precautionary measures usually adopted to eradicate the disease, such as baths, changing of clothes, but all to no purpose. I eventually went over to Ireland, and remained there six weeks, after which time I returned, in the confident hope that no chance of my transmitting the disease remained. Nevertheless (and "Ob-

stetricus" should note this), the first two or three cases that I attended showed unmistakable signs of infection, and I was compelled, by the serious consequences, to cease the practice of this branch of the profession for a further period. From my sad experience, then, it was apparent that the obstetrician should relinquish practice during not less than two months. But, when such fearful consequences are involved, it is safer to overestimate rather than underestimate the time; and such was the advice I gave to a midwife who recently lost, to my knowledge, not fewer than four cases within two or three weeks. The frequent ablutions which medical men necessarily undergo make it absurd to suppose that for weeks the *materies morbi* can be communicated by the hand of the accoucheur; and, when a change of clothing takes place, we may reasonably conclude that all chance of infection from this source is avoided. As further proof of the fallacy of this belief in the production of the disease by this means, I may cite the rarity of our ability to trace puerperal infection distinctly to these causes.

I left under the charge of my *locum tenens* several cases when I went from home, yet not a single case occurred among all those he attended in my absence. I find that there is a chimerical fear prevalent among physicians regarding this method of contagion, which oftentimes impels them to act inhumanely. Lately, I was called in to see two cases attended by the midwife above mentioned. I found that the friends had already sought the assistance of medical men, who decidedly refused to attend, because the cases were of this nature. I and my assistant took charge of these patients (one of whom died after a few days; the other, though still under treatment, is slowly recovering); and I see no reason to regret placing them on our list, as no evil consequences have followed to any of the confinements since attended by us. Hence, and from repeated experiences of this kind, I infer that such a mode of contagion is rare. That it does happen occasionally, I have no doubt; but, with due caution, as by ablution on leaving a patient thus affected, and by allowing some time to elapse between visiting patients where there is a possibility of infection, the risk is exceedingly small. The cases related in the *Journal* of February 13th, by Dr. Fenton, would seem to disprove this opinion; but it is by no means certain that the bedclothes in the instances related by him were the source of contagion. I rather incline to the belief that the midwife afforded the fell influence, or was the source from which the poison was derived. Otherwise, burn the bedclothes and set the woman free.

From these considerations and facts, it seemed plain to me then, and I see no reason why I should change my opinion now, that we must look for some other means of communication of the *materies morbi* than that generally accepted by the profession; and, guided by the experience then gained, I advanced the only

theory which commended itself to me, viz., that the contagious influence was somehow intimately associated with the *person* of the obstetrician, and not with his surface or clothing only. It is true that I was unaware of any morbid condition then existing in my own constitution which would be likely to give rise to any morbid exhalation, and fortify me in the belief entertained. Still it is possible that, unobservable to myself, there may have been present some defect in the assimilating process, some error in the secretory or excretory systems, which would generate a morbid material capable of producing septicæmia. It may be, however, that the toxic matter is not generated *de novo* in the system; but, being received from external sources in the first instance, becomes propagated by some unknown means. What I seek, however, mainly to establish is, that the clothes theory is untenable. As to whether the obstetrician can communicate this disease without vaginal examination, I have no definite opinion, not being able to call to mind any instance in which it has happened, but, with views such as mine, such an occurrence would appear by no means unlikely, indeed, nearly as likely as if the whole process of delivery had been under the superintendence of the accoucheur, *i. e.*, presuming that he had been at any time in close contact with the patient.

It is of vital import that the obstetrician should recognize at the very earliest stage the true character of the malady; and the points to which, in my opinion, attention ought to be directed, are the various causes from which puerperal fevers originate. These may conveniently be divided into three classes.

1. Those originating in the patient herself, as general unhealth giving rise to inflammation of the uterine and peritoneal structures, or such as spring from intrauterine causes, putrescence of the infant or of the other contents of the uterus.

2. Such as spring from atmospheric influences, and give rise under ordinary circumstances to fevers, as scarlatina, typhoid, and typhus.

3. That lamentable and malignant form mysteriously communicated by the accoucheur.

Cases of the first class are frequently easily diagnosed, though cases will arise from this cause which may baffle the most expert. As regards the second class, a good, and perhaps the only reliable, guide, is the prevalence in the district of fevers of an epidemic nature. I am of opinion, however, that the epidemic or typhoid form of puerperal fever does not always coexist with the prevalence of typhus or typhoid, but that there is some meteorological condition conducive essential to its production. In all these cases, though the *materies morbi* may not be identical, yet blood-poisoning is the result, and the symptoms are only modified by the constitutional peculiarities and condition of life of the patient.

It has occurred to my mind that, assuming this theory of the

production of puerperal pyæmia by the obstetrician, as asserted by me, to be true, it would be matter for serious thought, whether, in the event of a second outbreak in the practice of one man, he should not entirely relinquish obstetric practice, the second event arousing suspicion that there might be some special disposition existing conducive to the production of this fatal disease. I should so act myself.

As most important in preventing the spread of the disease, it is evident that some efficient and organized system of espionage is imperative.

I have become aware in my short experience that many deaths have occurred through want of knowledge and due appreciation of the gravity of the consequences likely to follow from persistence in midwifery practice, in the vain hope from day to day that the disease will disappear. Ignorant midwives, and young practitioners who may not have been taught to recognize the peculiar features of this special form of disease, are very prone to fall into this error. Now that officers of health are (or ought to be) generally established in every district throughout England, it should be deemed by the Privy Council one of the chief duties of such officers to investigate the true nature of any epidemic of puerperal fever coming to their knowledge, and to consult with, or advise, the propagators of the mischief. A stumbling-block would, I suspect, be often placed in his way by the fact that the most important means of information—the books of the Registrar of Deaths—do not always furnish a true record of the cause of death, when it is puerperal pyæmia. This is no idle statement, but one which I have verified by reference to the returns in this district, when I have found only two deaths returned as from this cause during two months, when I have had satisfactory knowledge of the occurrence of at least eight. The tendency to observe secrecy in such cases is a fact intelligible, and to some extent, perhaps, excusable. From this point of view alone, I regard the late Sanitary Act as a wise measure, calculated to prevent much human suffering, and to save many valuable lives.

When, ten years ago, I enunciated the theory I have expressed above, the proposition did not elicit any discussion. Possibly it was deemed unworthy even of notice; yet I feel confident that in the future it will be recognized as by no means so improbable as superficially it may appear to be.

There are many occult problems connected with the etiology of disease, which suggest themselves every day, that are not less strange than that now propounded, and yet they meet with favorable consideration. The theory of transmission from extraneous sources, I grant, at first sight would seem most probably true; yet, in the minds of those who may unfortunately have the opportunity that experience affords, I feel certain that grave doubts of its validity will arise.—*The British Medical Journal.*

Subcutaneous Injection of Strychnia in Incontinence of Urine.

Dr. Kelp, director of the lunatic asylum at Wehmen, near Oldenburg, describes the results of subcutaneous injection of strychnia in two cases (*Deutsches Archiv für Klin. Medicin*, Band xiv, Heft 3 and 4). One case was that of a girl aged 16, who was admitted with *melancholia stupida*, and was discharged cured at the end of about a year and a half. She was small and feeble, and had been badly cared for at home. From childhood, she had suffered from nocturnal incontinence of urine. Small doses of strychnia, given internally, produced no effect. But, after the injection of one-sixteenth of a grain of nitrate of strychnia dissolved in water (one grain in two drachms) in the sacral region, the enuresis ceased, and did not return for some days. The injections were repeated from December 17th, 1867, to April 3d, 1868, whenever the enuresis returned; it appeared at gradually increasing intervals, and at last ceased. The dose of strychnia was increased to one-sixth of a grain. During the whole time not more than four grains were used. For a year after her discharge, she resided near the hospital, and was occasionally troubled with incontinence of urine, which was removed by a repetition of the remedy. She has since married, and has not come under Dr. Kelp's notice; from which he concludes that the enuresis has not returned. The second patient was an imbecile girl aged 20, who had suffered from enuresis from childhood. The treatment was the same as in the former case, and the result was as good; but Dr. Kelp does not know whether the result is permanent, as he has not heard of the patient since she left the asylum. The treatment was continued for about two years, during which about ten grains of strychnia in all were used. The intervals between the attacks of enuresis varied from some weeks to a few days. The usual dose was one-seventh or one-eighth of a grain; but, even when this was used daily, no general action on the system was observed.—*The British Medical Journal.*

Cadaveric Poisoning.

Dr. Odenius, of Lund, in vol. vi of the *Nordiskt Med. Arkiv*, agrees with Neumann, Hüter, and Cöhnheim, that infection from the dead body may take place through the sound skin through the medium of the follicles. The sweat-glands, however, in consequence of their structure, are less favorable to this than are the others. He observed some time ago that, after making *post mortem* examinations, the hand of himself and his assistants became covered with pustules; and at length it was found that this arose from the friction of the hands with cocoa-butter, which had stood in the room for a long time and had become rancid. In 1871, he made an experiment on himself, which was trouble-

some but convincing. After examining a recently dead body from pneumonia, he perceived the next day a painful swelling on the anterior surface of the forearm. There was no wound—not the slightest laceration; but at the necropsy a little fluid from the body had come into contact with the arm, and had been pressed on it by the wristband of his shirt. Sloughing of a portion of the skin took place at the part. While the disorder was at its height, Dr. Odenius observed that movements of the muscles of the forearm were attended with rigors, which he attributed to the increased entrance of pyrogenous material into the lymphatic vessels. During the next year, he had several opportunities of making similar observations on himself and his assistants. He remarks that similar symptoms of poisoning are produced by attendance on difficult labors, which require long continued operations and manipulations. In any case, in order to produce the effect described, there must be a certain predisposition, a fine skin, etc., or a continued and close contact of the infective material with the cutaneous follicles.—*The British Medical Journal.*

Action of External Application of Water, etc., on the Cerebral Blood-Vessels.

Schüller, of Laubbach, has made a number of experiments on rabbits to ascertain the effect on the cerebral vessels of the external application of warm and cold water, etc. His paper is published in the *Corresp.-Blatt der ärztl. Vereine in Rhineland, Westphalen und Lothringen*, No. 14, 1874. The animals were trephined, and in course of time the cervical sympathetic on one side was removed. Some of them were curarised. The following were the results which he obtained. 1. Dilatation of the vessels of the pia mater is produced by laying compresses soaked in water at a temperature of 50 Fahr. on the abdomen, and in a more intense degree and of longer duration by immersing the animal in the water. If the action of the cold water be continued for five or ten minutes, the result is gradual contraction of the cerebral vessels, which often lasts half an hour. 2. The application of warm water (95° to 99.5° Fahr.) always produces strong contraction of the vessels, which lasts for a longer or shorter time. 3. Douches on the belly or back generally produce alternating changes in the calibre of the cerebral vessels. 4. The injection of water into the rectum is attended with moderate dilatation of the vessels. 5. The usual result of the cold pack is a gradually increasing and very energetic contraction of the vessels of the pia mater, which often lasts two hours. At the same time, the pulse and respiration become slower, and reflex irritability is reduced. 6. Ice laid on the intact skin of the head produces after some time a moderate contraction of the vessels. 7. Friction of the abdomen always produces contraction. The

results were observed in curarised animals in a less marked degree than in those not so prepared. Section of the vagi has no influence on the changes in the vessels. Schüller makes some further remarks of physiological interest; and, with regard to the practical part of the subject, observes that the use of extreme temperatures is contraindicated in cases of cerebral hyperæmia, anæmia, and congestion, especially where the muscular structure of the blood-vessels is impaired. He attributes the beneficial influence of the various forms of baths partly to the depletion of the cerebral vessels, partly to the establishment of normal tone in the muscular structure of the cerebral vessels, to strengthening of the cardiac muscle, and to improved nutrition both of the nervous centres and of the whole organism. The chief point of interest in Schüller's communication is, that the action of baths is not limited to the temperature and change of substance, but that changes in the distribution of the blood, in respiration, in the action of the heart, and in excitability of the nerves, form part of the physiological results of this agent.—*Allgemeine Medicin. Central-Zeitung*, No. 94, 1874.

Action of Certain Medicines on the Vessels of the Brain.

Schüller (*Berliner Klinische Wochenschrift*, Nos. 25 and 26, 1874) performed experiments on rabbits by removing, with a trephine, a portion of the upper part of the parietal bone without injuring the dura mater, and generally at the same time taking away the cervical sympathetic with the superior cervical ganglion of the same side. He was thus able to observe the vessels of the pia mater, and to notice any changes in their filling. The substances with which he experimented were mustard, nitrate of amyl, ergotin, opium, and chloroform. Small sinapisms produced scarcely any effect on the vessels; the application of large ones, on the one hand, was first regularly followed by dilatation, which was followed by more or less rapidly alternating changes in the calibre of the vessels, and finally by contraction, which often continued an hour and a half after the removal of the sinapism. To explain these phenomena, Schüller supposes that at first the irritation of the sensory cutaneous nerves produces, by reflex action, a partial paralysis of the vaso-motor nerve-fibres (dilatation), and that later on the influence of the peripheral congestion exerts its influence and ultimately gains the upper hand (oscillations of calibre and contraction). After nitrate of amyl, there was a very evident dilatation of the vessels of the pia mater, both arteries and veins. It could be observed even on the side where the sympathetic had been injured, and where there was already dilatation of the vessels; and took place both after section of the vagus and in curarised animals. This action, according to Schüller, depends on the degree of filling of the

vessels, and the capability for reaction of their muscular structure. Ergotin produced a strong and long-continued contraction of the vessels of the pia mater; even on the side where the sympathetic was injured. Schüller regards ergotin as standing in a certain antagonistic relation to nitrate of amyl, inasmuch as the contracting action of the former overcomes the dilating action of the latter. Opium produced first dilatation, then contraction. After the inhalation of chloroform, there was a commencing contraction of the vessels; but very soon they became relaxed, and well marked venous stasis was developed. Nitrate of amyl not only very quickly removed this action on the vessels of the pia mater, but, when chloroform narcosis was carried to a far more advanced stage, under the action of nitrate of amyl, the labored dyspnœal respiration and the pulse quickly recovered themselves, and in a remarkably short time reflex excitability was restored.—*The British Medical Journal.*

The Medical Year 1874.

From the London Lancet.

Medicine is not only a year older since our last summary, but a year wiser and a year more effective in its knowledge of disease and of methods for controlling it.

In Physiology, one of the principal works that have been published since the close of the last year, is Dr. Kline's memoir on the Lymphatic System, in which the distribution of the lymphatics, as well as the arrangement of the epithelium on the serous membranes, is very fully considered; the general result arrived at being that the serous membranes are to be regarded as unravelled lymphatic glands, and, as such, play an important part in the formation of the white corpuscles of the blood. Drs. Foster and Balfour have just published a work that has long been needed—"The Elements of Embryology"—in which the process of development is described, in a singularly lucid and intelligible manner, from the earliest period in the fowl, and which constitutes the first part of a larger work, on the same subject, embracing the history of development in other animals. Dr. B. has also done a good piece of work in following the development of the dog-fish and its allies.

Mr. Parker has continued his elaborate account of the development of the skull, in his lectures delivered at the Royal College of Surgeons. Arnold, Gasser, Rauber, Mihalcovics, Sernoff, von Torok, Merkel, and many others, have devoted themselves to the elucidation of the process of development of various special parts or organs, as the eye, ear, pituitary body, testis, etc. Mr. St. George Mivart has published a brief but useful account of the Anatomy of the Frog. Quite recently, Mr. Barker has published a valuable translation of Frey's Histology. A very interesting

little work has been issued in the International Scientific Series, by M. Marey, on "Animal Mechanism and Aerial Locomotion," in which great ingenuity is displayed in the application of recording apparatus to the registration of the movements of animals, both in aerial and terrestrial locomotion. To the same series Dr. Draper, of New York, has contributed a volume that will be read with interest, on the "History of the Conflict between Religion and Science, in which the learned author, who is well known for his excellent treatise on Human Physiology, has endeavored to present a clear and impartial statement of the views and acts of the two contending parties. There is also a third, consisting of a translation of Oscar Schmidt's doctrine of descent and Darwinism. A popular physiology is contained in "Hinton's Physiology for Practical Use." A complete account of the Physiology of Food and Dietetics has been written by Dr. Pavy. Dr. Carpenter has developed two or three chapters on Mental Physiology contained in the earlier editions of his Physiology, which have been excluded from the later ones for want of space, into a goodly volume, written with his usual clearness and intelligence.

Amongst foreign works may be mentioned the "History of Creation" and the "Development of Man," by Haeckel, the former of which has been translated into English by Lankester, and the latter into French. Hermann has published a fifth edition of his excellent work on Physiology, which is now, we believe, in course of translation into English. Dr. Gustav Le Bon, in a large volume entitled "La Vie," remarkably well illustrated, and M. Coyteux, in another, the "Etudes sur la Physiologie," have both treated fully of human physiology. Hitzig has collected his observations and inquiries into the functions of the brain into one volume, which is worthy of careful perusal. Though, perhaps, scarcely capable of being included under the head of Anatomy and Physiology, we must not omit to chronicle the first International Congress of Orientalists, which was held at the Royal Institution in London, in September, and which, under the presidentship of Dr. Birch, was the occasion of several excellent addresses, amongst which those of Max Muller, the President, and Prof. Owen, deserve special mention. Professor Rutherford has been appointed to the Edinburgh Chair of Physiology, and carries with him the good wishes of his late colleagues in King's College, and of his friends in London. We trust that still increasing numbers of students in attendance upon his classes will testify to the ability with which he conducts them. A large number of separate contributions have been made up by individual observers, amongst which it is difficult to select any of pre-eminent value. Braun has repeated Hitzig's experiments on the excitability of the surface of the cerebrum by weak electrical currents, and whilst generally confirming his experiments, and those of Ferrier, adds this novel point, that there are several cases in which there appear to be two centres on the surface of

the hemispheres for the same set of muscles, as, for instance, those of the neck. He also finds that, after shaving off the gray matter from a given spot, the groups of muscles can be made to contract, if the current be applied to the cut surface of the white substance as before, showing that the nerve-fibres really arise from the gray matter, whilst, if the white substance be divided, all manifestation of nervous excitation ceases. Burdon Sanderson has also made experiments upon this point, and has shown that there are points in the corpus striatum of the same side which correspond to those of the same hemisphere. Dr. Ferrier considers that in Dr. Sanderson's experiments only a medullary corresponding to the cortical centre is removed. Nothnagel, from experiments on the thalami optici, has come to the conclusion that they have no relation to voluntary muscular movements, nor to the general sensibility of the skin, but that they are immediately connected with the muscular sense. It is pleasant and encouraging, in connection with the subject of Physiology, to call to mind the meeting of the Russian Association for the Advancement of the Natural Sciences, held at Kasan, a flourishing city in the very centre of European Russia, on the Volga, which possesses a good university. Physiological subjects figured largely in the discussions. We may mention especially Dr. A. Schakowsky's paper on the Amount of Fat in Human Milk, averaging 3 per cent., and the striking reduction in the amount produced by a strictly vegetable diet, till it fell to 0.8 per cent.; Prof. Kowalewsky's paper on the Mechanics of the Movements of the Bile; and the paper of Dr. A. Troitzky, of Kasan, on a method of estimating the rapidity with which a stimulus is propagated under different temperatures, and with various strength of exciting currents. The maximum rapidity of propagation of stimulus is between 68° and 50° F.

The experience of the year has shown that Esmarch's plan of preventing hemorrhage is capable of general application with advantage, and it has been almost universally adopted. Dittel's elastic ligature for the treatment of fistula in ano has been lately highly praised by Mr. Allingham, who has treated a number of cases with it satisfactorily. An interesting correspondence took place in our columns in January as to the credit of priority in the use of the elastic bandage and of the elastic ligature. According to our correspondent, Dr. Gesnaldo Clementi, of Naples, Dr. Grandesso Silvestri, of Vicenza, used the elastic ligature in 1862; and he and Professor Vanzetti, of Padua, together used and described the elastic bandage very much in the same way as Esmarch. The subject of bloodless surgery, by means of the galvanic cautery, has been discussed at length by Dr. Bryant, and the introduction of a new and constant battery with carbon and bichromate of potash for its elements, seems to remove one of the difficulties hitherto in the way of the general adoption of the plan. In connection with the subject of Pyæmia, we may refer to Mr. Lister's practice, as reported upon by Mr.

Gamgee, which serves to illustrate the proverbially diverse effect produced by the same series of phenomena in different minds. The treatment of Syphilis was the subject of an address to the Hunterian Society by Mr. Jonathan Hutchinson, who proved, we imagine, to the satisfaction of all candid inquirers, that mercury is the remedy for that loathsome disorder. The subcutaneous injection of mercury was the subject of an interesting communication by Mr. Cullingworth, but the method was one which has hitherto found little favor in this country. Sir William Ferguson has introduced a modification of the method of operating on cleft hard palate, which seems to promise well; and the same eminent surgeon was able to relieve public and private anxiety as to the identity of Livingston's remains by the recognition of the united fracture of the humerus. Mr. Teale has called attention to the operation of ovariotomy in exceptional cases; Mr. John Wood has contributed to the literature of thoracentesis, and Mr. Reginald Harrison to that of puncture of the bladder by the aspirator. The recent lecture on American Surgery by Mr. Erichsen has set before us many points of interest in connection with the surgery and hospitals of the United States; and Dr. Buchanan's introductory address served to illustrate the activity of our northern brethren. Amongst the feats of surgery for the year, we may mention Billroth's total extirpation of the larynx and epiglottis for carcinoma—the second operation of the kind which he has performed; also his second complete removal of the thyroid gland, and the extirpation of a fibro-cystic tumor of the uterus, weighing sixteen pounds, with the uterus and its appendages, followed by recovery, by Dr. E. H. Trenholm, of Montreal. The "Lancet," of July 25th, sets forth that Dr. Henry Harland, of Wadhurst, Sussex, had extricated a Waterloo bullet from the hand of James Jenner, aged eighty-three. The said bullet had been in the veteran's hand fifty-nine years, and had become inconvenient to him at his work as a gardener! Our pages have contained many illustrations of the happy use of the aspirator in tapping the pericardium and other cavities, and Dr. Maclean speaks of the pus let out of livers at Netley by this instrument in a summer as measuring hundreds of ounces.

Of papers of importance at the Medico-Chirurgical Society, those by Dr. Cunningham on Recent Experience of Cholera in India, and by Surgeon A. Hall on the use of Chloral subcutaneously injected in that disease; by Mr. Mahomed on the Prealbuminuric Stage of Bright's Disease; by Dr. George Johnson on the Laryngeal Symptoms produced by pressure on the Vagus and Recurrent Nerves—may be noted. Much valuable matter has been brought forward at the Medical Society, which shows no signs of decay; and we must not omit to mention the able Lettsomian lectures delivered before the Society by Dr. Broadbent, on the subject of Syphilitic Affections of the Nervous System. The most important papers at the Obstetrical Society have been those of Puerperal Thrombosis, by Dr. Playfair, and

on the Relation of Flexion and Congestion of the Uterus, by Dr. John Williams. We may note also a discussion at a special meeting on the admission of ladies as members of the society, which resulted in a decision adverse to the proposal.

The summer months brought the usual pleasant gathering of scientific societies, in which not a few of our brethren seem to find their holiday. The meeting of the British Medical Association at Norwich was large and successful. The addresses were of a high character, and the Society of the old city was full of worth and hospitality. Amongst the distinguished foreigners was M. Magnan, who essayed to illustrate on dogs the different action of alcohol and absynthe, and in so doing involved some of the spectators in a criminal prosecution, at the instigation of the Society for the Prevention of Cruelty to Animals.

The meeting of the British Association at Belfast was presided over by Professor Tyndall, who landed matter in such terms as to offend the sensitive religious minds of the Irish. He indicated that he saw in matter promise and the potency of all things. It is only fair to add that he has since confessed that when he looks abroad on the beauty of the universe, he cannot resist the feeling that there must be some Being superior to it and more familiar with it than he himself. And so we think. We have great regard for Prof. Tyndall and his colleagues of the British Association; but we trust that there is a Being somewhere in the wide universe to whom even the knowledge of our wisest *savans* appears very elementary. Prof. Huxley's address in Physiology on the Hypothesis that Animals are Automata, and Dr. Hooker's on the Carnivorous Habits of Plants, Professor Redfren's paper on the Effects of Ozone on the Animal Economy, Professor Cleland's on the Morphology of the Brain and the Function of Hearing, Sir Duncan Gibb's case of an old woman of 111, and other communications, are full of scientific or human interest. The Social Science Congress met in Glasgow. One of the best features of the meeting was a very able address from the chair of the Public Health section by Dr. Lyon Playfair. During the sitting of the British Association, congratulations and friendly sentiments were exchanged with the French Association for the Advancement of Science, which met at Lille. The Medical section of the French meeting was well attended, and the scientific work of it was good. The Public Analysts are now becoming an important power in the community. They held a meeting at Cannon Street Hotel, on Friday, August 7th, and in several resolutions showed their mind on questions involved in the working of the Adulteration Act.

The General Medical Council had a long and important sitting in the very hot days of July, from the 9th to the 18th. The business was opened by a very perfect address from Dr. Paget, retiring President. In the pith of the report of the visitors there is claimed a terrible defect in preliminary education.

Several cases of rheumatic fever have been published, in which

almost every conceivable remedy has been given with varying success. The acid treatment and the alkaline treatment, quinine, iron, and propylamin, have all been shown to be valuable, or at least not hurtful, in some cases of acute articular rheumatism. The value, in epilepsy, of bromide of potassium in large doses frequently repeated, was illustrated in a patient under the care of Dr. Ransome, of the Nottingham General Hospital. From Charing-Cross Hospital we published a case showing the value of chloroform in chorea, and from the Royal United Hospital, Bath, another showing the value of chloral hydrate in the same affection. Lastly, we lately recorded a case from the University College Hospital, in which a child was shown to have suffered from an acne-like skin eruption, shrewdly traced by Dr. Tilbury Fox to taking milk from its mother, who was under the influence of bromide of potassium.

In Therapeutics, interest still centres to a very large extent in substances which, as a matter of fact, affect the nervous system, such as alcohol, chloral, belladonna, physostigma, phosphorus, etc. The value of alcohol as a therapeutic agent is still a much contested point. But we have lately had to remark on the great care and moderation with which it is prescribed by the present leaders of medical practice. The care with which a spirit ration was served out, and its effects watched, in the Ashantee war, made the subject very interesting. To say the least, the teeto tallers sustained in that warfare a creditable amount of health, and in sickness showed a creditable amount of recuperative power. The Gold Coast campaign may be held to have proved, in the words of Dr. Parkes, that the rum ration should not be given in greater quantities than two ounces and a half per man daily, and that the time for giving it is not before or during, but after, a march. We may hope that in the future the spirit ration will be, as it was here, an extra to be given when deemed expedient by those best able to judge of circumstances and the wants of men. That remarkable medicine, physostigma, stands credited, on the testimony of Dr. Crichton Brown, at the Medical Society of London, with having cured two cases of general paralysis. Supposing there to have been no error of diagnosis, and no misconstruction of the relation of events in these cases, we should say that this is the most remarkable therapeutical achievement recorded in the year—much more remarkable than the several cases of tetanus apparently cured by chloral. The application of the curative powers of this latter medicine become multiplied. Unfortunately the use of the drug as a tippling agent also increases. Several cases of poisoning by it have occurred. Amongst the purposes for which it is recommended, with some show of reason, is as an anæsthetic in labor, and as a remedy, hypodermically injected, in cholera. In two or three cases of exophthalmic goitre, under the care of Dr. R. T. Smith, very great relief and advantage were obtained by the employment of belladonna. The question of the use of the cold bath in typhoid fever

has been raised by an amateur therapeutist in the "Times;" but there has been no important addition to our knowledge of its value. In the serious epidemic of typhoid at Lyons it was used extensively, but with rather injurious results in the production of congestion, etc., and a corresponding high rate of mortality. The bath used seemed to be at too low a temperature—namely, 68° F., and the use of it was not sufficiently restricted to cases of hyperpyrexia. In November, Dr. Clifford Albutt, of Leeds, discussed the mode of death in the early days of scarlatina, and gave proof—first, of the hyperpyrexia as a cause of death; secondly of the efficiency of cool bathing in averting the tendency to death. The use of ipecacuanha in the form of spray for cases of winter cough and bronchitic asthma is considered by Dr. Ringer.

Dr. Spencer Cobbold has just finished an account of cases illustrative of the treatment of tapeworm, in which he shows at once the Protean effects of this intestinal guest, and the efficacy of discriminate treatment. He evidently revels in the possession of nineteen tapeworm heads from eighteen patients, not only as the guarantee of complete cure, but as a rich accession to his cabinet specimens.

Although the closing year is not likely to be remembered as one in which crime was less in amount than usual, there has, nevertheless, been an absence of crime of a sensational order. Public attention has not been roused by any villainous or subtle atrocity, and medical jurists have not been called upon to unravel any startling mysteries. The great question of identity involved in the Tichborne trial has been settled, and perhaps we may go so far as to say that the Lord Chief Justice has originated a new principle of identification by his axiom that however closely two persons may resemble each other anatomically, yet "no two persons ever were alike within." Certain it is that his masterly dissection of the inward Orton, and his comparison of it with the inward Tichborne, served to carry conviction, when scars, thumbs, ears, twitchings, brown marks, and peculiarities had merely tended to make confusion worse confounded. One of the most important advances in forensic medicine has been made by Dr. Richardson, of Philadelphia, who, by applying a micrometer and the highest powers of the microscope, has shown that a distinctly recognizable difference exists in the size of the blood-cells of man and the other mammalia.

The science of toxicology is threatened with a severe shock from an unexpected source. The practice of cremation has been strongly advocated, and should it come into vogue there will be, of course, an end to "exhumation by order of the coroner." Civil cases involving medical questions have not been numerous. Payment of a life policy has been refused on the ground that material information as to the habits of the assured was withheld, and although a verdict was originally obtained for the plaintiff, the insurance company obtained a rule for a new trial.

The case of Simpson vs. Davey was one of a peculiar and distressing nature, and has roused the sympathy of the whole profession. Dr. Davey has had to pay £500 damages (besides law expenses) because, having unfortunately contracted syphilis at a midwifery case, and having affected with it another patient (Mrs. Simpson), and, further having undertaken to treat Mrs. Simpson free of charge till she was well, he failed to fulfil his contract. We would particularly remind our readers that Dr. Davey has suffered, not on the ground of having infected a patient, but merely on the ordinary ground of non-fulfilment of a contract.

The year will be remarkable in epidemiological annals, first, for the reappearance of plague in no less than three different localities of Asia and Africa; and, next (we may more reasonably hope), for the cessation of the long-continued prevalence of cholera in Europe. Early in the present year plague broke out in Mesopotamia, on the lower Euphrates; somewhat later it appeared in the Regency of Tripoli, district of Bengazi, North Africa; and still a little later in Western Arabia. The scene of the outbreak in Mesopotamia is memorable in ancient history as being within the confines of Babylonia, not far distant from the ruins of the great city. Here, among the Afigi Arabs, who occupy the first of the great marshes which exist on the east bank of the river, and of which a series extends to the junction of the Euphrates with the Tigris, the disease appeared. Spreading from the marshes, it extended along both banks of the river, attacking the different towns and villages as high as Hiliah. Traveling westward, also, it passed the marshes of Hindieh, on the west bank of the river, the scene of the outbreak of plague in 1867, and attacked the two cities, sacred to the Shiite Mahomedans, of Meshed Ali (Nedjef) and Meshed Hussein (Kerbela), on the border of the great Arabian desert. It is estimated that this outbreak carried off 4000 persons out of a population of 90,000. The outbreak of plague in North Africa was first recognized at Merdj, a village twenty hours distant from Bengazi. It is stated to have appeared first among a Bedouin tribe encamped near the village, then to have extended to the village, and to have subsequently spread among the tribes inhabiting the district. Here the outbreak was preceded by and occurred at a time of famine, and when the country about Merdj had been converted into a vast marsh by a protracted rain. A French physician, Dr. Laval, unfortunately contracted plague in investigating this occurrence, and died from the disease. The outbreak is now reported to have ceased, but the loss of life it has caused is not yet known. The district of Bengazi was affected with plague in 1851. The scene of the outbreak of the plague in Western Arabia is in the mountainous Assyr district, North Yemen. Here, also, it is stated in a recent report, that the disease is at an end. This outbreak would seem, so far as is yet known, to have been very circumscribed, but of its nature no doubt appears to be enter-

tained. Plague, it is said, had not been known in Yemen since 1816 until this appearance.

The probable cessation of cholera on the Continent relieves Europe from an incubus which has been weighing heavily upon it for ten years. The disease has been uninterruptedly present in Europe ever since its extension from Egypt to the southern coast of the Continent in 1865.

Port Sanitary Authorities are increasing in number and importance, and their influence in checking the importation and perpetuation of epidemic diseases is being acknowledged and appreciated. The systematic inspection of vessels immediately after arrival, the removal and isolation of the sick, the examination of the drinking water, the closet accommodation and state of bilges, the disinfection of the clothing, the inspection of foul cargoes, and other miscellaneous duties, are found to be within the scope of work of a port medical officer. At the beginning of the current month, a large body of Russian and German emigrants imported small-pox into the Thames, and although the sick and suspected were at once separated from the healthy, the latter proved to be infected, and took the disease to Liverpool, where, however, it was successfully circumvented by the local authorities. The half-yearly reports issued by the Corporation of London, as sanitary authority of the port, show the large amount of sanitary work that has to be done on this as on other rivers. Scurvy has exhibited a slight tendency to increase in our mercantile marine. Seven official inquiries have been held by the medical officer of the Board of Trade during the year on account of outbreaks of this disease; and in one of these the master of the ship was prosecuted and fined for non-compliance with the sanitary clauses of the Merchant Shipping Act. Some progress has been made in the ventilation of ships of all classes; and much attention has been directed to this important subject, chiefly with reference to ironclads and to school ships.

The Sanitary Commission during the year has issued twenty-one reports—viz., four on the causes of certain outbreaks of fever, six on the drainage and general sanitary condition of important towns and districts, and eleven on sanitary matters of general interest, such as inquiries into the condition of the dwellings of the poor, and the influence of chemical manufactures on health, etc. The outbreak of typhoid fever at Cambridge was the subject of our first report. The disease appeared late in the autumn of last year, and, after slowly making its way along a certain line of drainage, culminated in a severe outbreak in the new buildings of Caius College. We attributed the spread of the disease to the abominable condition of the town drains, and to the percolation of sewage matter into the wells; and suggested that the cause of the outbreak at Caius College would be found in some defect in the main drain, by which contaminated excretory matter or sewer gas had found access to the College buildings. Dr. Buchanan, some weeks later, found that a portion of fœcal mat-

ter had been sucked up from this drain, and had contaminated the water supply of that part of the College where the cases occurred. Our inquiry into the health and management of the children at the Brentford Union Workhouse, led us to the conclusion that the state of our workhouses is rapidly reverting to the status quo ante bellum 1866, and served to increase a growing feeling in favor of boarding out pauper children. The new drainage works at Brighton were the subject of a lengthy report.

Although we have no such important contribution to chronicle this year as that of Surgeon Lewis's very remarkable discovery of filaria circulating in the human blood, the members of the Indian and British medical services have contributed their quota to the scientific work of the year. The Ninth Annual Report of the Sanitary Commissioner with the Government of India contains a remarkable paper, illustrated by some fifty-nine colored drawings, on the floating bodies in the air of that country, by Dr. D. D. Cunningham, of the Indian Medical Service, who has been employed under Government, in conjunction with Dr. Lewis, of the British army, in the investigation of cholera in India. Dr. Cunningham has also, in addition, made many miscellaneous æroscopic observations in connection with the presence, development, and influence of bacteroid and other bodies found in the air of certain localities, and in that of sewers, from which he draws many interesting conclusions, one of which is that no connection can be traced between the number of bacteria, spores, etc., present in the air, and the occurrence of any disease, nor between the presence or abundance of any special form or forms of cells, and the prevalence of diarrhœa, dysentery, cholera, ague, etc. Dr. Vandyke Carter has prosecuted and published his researches into the affection known as mycetoma, or fungus-disease of India. A Commission composed of the medical authorities at Calcutta have instituted an exhaustive series of experiments into the nature and effects of Indian snake-poison, as compared with that of Australian snakes—the Australian tiger-snake, for example—which have established the correctness of the results previously arrived at by Dr. Fayrer, and proved the inefficacy of the ammonia treatment of snake bite.

The Ashantee war gave several naval medical officers opportunities of distinguishing themselves, and these received their rewards in early and well-deserved promotion.

In surgery we have to note the issue of a third edition of Dr. Pirrie's book, and a work embodying the observations and lectures of Mr. Cæsar Hawkins. We must not omit to mention with satisfaction the papers in the "Lancet" on Hospital Construction, by Dr. Sutherland and Captain Douglas Galton. Our tribute to the merit of Dr. Barnes's work on the Clinical History of the Medical and Surgical Diseases of Women has been fully endorsed by the Profession. A new edition of Meiggs' and Pepper's Diseases of Children, and Dr. West's well-known work on

the same subject, deserve a passing word. We reviewed at much length a number of works on the Anatomy and Diseases of the Ear, by Politzer, Hinton, Turnbull, Allen, and others. A new edition of Dunglison's Dictionary of Medical Science, large and valuable as it undoubtedly is, would still be benefitted by careful revision. In Mental Physiology, Dr. Carpenter's "Principles" deserves to be named as an important contribution to that subject. In Sanitary Science and Army Hygiene, we have had many works in connection with the late Franco-Prussian war, Lex's and Roth's contributions, and Dr. Guy's Lectures on the Sanitary Aspects of War. Pettigrew on Animal Locomotion has excited some controversy.

The Hospital Sunday movement grows and extends everywhere. In London, auspiciously begun in 1873, it expanded in the present year, both as regards the sum raised and the number of contributing congregations.

The roll-call reveals many gaps in the professional ranks. Early in the year, Mr. Wormald, senior member of St. Bartholomew's surgical staff, died at a ripe age; and Mr. Thomas Dickson closed prematurely a promising career at Guy's. Dr. Forbes Winslow had not quite survived his vigorous and devoted energies in medico-psychology when he too was summoned to "the majority." Medical physics owes much to Dr. Neill Arnott, medical diagnosis to Dr. A. Kilgour, and the limitation of criminal responsibility in courts of law to Dr. Winslow. The Bombay army boasts of few abler medical officers than Dr. John Maclennan, who discharged with equal promptitude and effect duties apparently diametrically opposed; but a yet severer loss than his was that of Cruveilhier, the great anatomist, physiologist and surgeon, a worthy disciple of Dupuytren, and long one of the brightest ornaments of the great Parisian school. "Literary labor," says his compatriot and eulogist, M. Bardinet (himself just numbered with the dead), "hospital practice, patients at home and abroad, faculty engagements, lecturing, and so on, absorbed eighteen or nineteen of the twenty-four hours, and this herculean work was carried on for many years. Rarely has a man of science been so thoroughly kind and considerate to his patients; rarely have the latter more warmly reciprocated a welcome so sympathizing." Truly "his works do follow him," for few anatomists of the century continue to fill a greater or more honored place on the book-shelves of the learned. About the same time England—and, indeed, the whole civilized world—began reluctantly to persuade themselves that the mightiest missionary explorer since Las Casas or Xavier had died in David Livingstone—died indomitable to the last, after a life which more than any other realized Arnauld's noble rejoinder when entreated to rest—"Rest! we shall rest through eternity!" The arrival of his mortal part on our shores enabled Sir William Fergusson to identify it by the recognition of the false joint which Livingstone had asked Sir William to examine in London some seventeen

years before. The honor of a tomb in England's Walhalla, Westminster Abbey, was accorded to him; but his monument

> "ære perennius
> Regalique situ Pyramidum altius,"—

will be that disenshrouded Africa with its slavery suppressed, and its moral and religious darkness replaced by Christian science.

Dr. Anstie's death was the cause to us of almost domestic grief, and threw our pages, which he had so much enriched, into mourning. We need not recount again his virtues,

> "Since Heaven, what praise we offer to his name,
> Hath rendered too authentic by its choice."

We must here leave our attempt to exhaust the history of medical labor. We can only, as busy men, snatch fragments from the accumulating pile of the stately building of medical science and art. We should especially like to have attempted to do some justice to our foreign brethren, and to our American—we were going to say—fellow-countrymen; but we will content ourselves by referring to our constant attempt to represent in our columns the points of all their scientific and practical work. Witness the Foreign Gleanings, the letters of our Paris Correspondent, and our frequent leaders on physiological or pathological questions. As regards our American friends, what English surgeon did not feel proud as he read Mr. Erichsen's address, winding up with that accomplishment of Dr. Sayre, the removal of the head of the femur fifty-two times, bearing the same relation to Mr. Syme's excision of the elbow that the Reform Bill of 1867 bore to that of 1832. Medicine is becoming more and more an affair of public interest. "Internationalism" has no dread to suggest when it represents the discussion of sanitary questions at Vienna or physiological questions at Kasan. Be it ours in the future not only to hold high the domestic and social interests of the Profession, but also to recognize medical labors everywhere, whether applied to minute and impalpable questions in pathology, or to those great questions which affect the health and happiness of communities.

A Case of Purpura Hæmorrhagica Requiring Transfusion.

During the night of June 4, 1874, I was called to see M. H., æt. 7 years; he had been for some time previous subject to troublesome epistaxis, etc. Both of the child's parents are in good health as well as circumstances; so that the child had had excellent hygienic care. There are four children, the patient being the third in the order of birth, and, with him excepted, all are in good health. There was at the time of my visit an intense general pallor, extending to the lips and internal surface of

the eyelids, giving the patient a waxy appearance, and making more apparent the existing ecchymotic spots; these last were situated behind the ears, at the angle of the mouth, the roof of the mouth, and a large one, of six inches in diameter, over the region of the liver. Blood was noticed oozing from the nose, gums, and pharynx, while the quantity vomited led me to infer that there might also be internal bleeding. The pulse was feeble, quick and rapid; the respiration labored, the tongue pale, moist, and clean. The general expression of the face was anxious to a degree which might even call for the term frightened. The hemorrhage at this time was from the nose, and was sufficient to require applications for relief to both anterior and posterior nares.

As before stated, the history of the patient was that of a previously healthy boy, descended from healthy parents, in every way active and well. There being no obvious cause for the attack, I am disposed to attribute its origin to some interference with normal nerve action, due to the shock from a fall from the second story window upon the brick pavement years before, although no severe external injury could be perceived at the time. This may seem like going a great way in order to find a cause, but I am quite confident that I at one time removed the tibia for necrosis, which had its origin in fully as distant nervous shock, and from my experience in military surgery I think that the nervous shock accompanying and resulting from wounds and exposure eventuated in typhoid conditions, typhoid pneumonia, and phthisis many years after what was denominated complete recovery from the primitive conditions.

The treatment was, of course, first directed towards arresting the active epistaxis, by means of Monsel's salt applied to both the anterior and posterior nares by insufflation, which I employed successfully during the recent epidemic of small-pox in this city, in cases which I at first looked upon as requiring plugging of the nostrils by Bellocq's canula.

Internally, the administration of potass. chlorat., in combination with tr. ferri chloridi, was persisted in, while every attention was given to providing the most concentrated food. Rest was enjoined, with proper regulation of carriage riding in the park during the cool of morning and evening, so as to give as much pure air with as little excessive heat as was possible.

There could be noticed scarcely any improvement, and there were frequent returns of epistaxis, while the persistence of the ecchymotic spots was remarkable. The application of alcohol with brisk rubbing was maintained daily to the spine, and quinine, iron, and strychnia, together with stimuli in food combinations, were freely administered. During the second week, at the suggestion of Dr. Keichline, of this city, the ol. terebinth., gtt. x–xv ter die, was given in sweetened ice-water. At the beginning of the third week there was improvement to the extent of an interval of four days without epistaxis, and as the weather

was oppressively hot, the patient was sent to Atlantic City. He stood the journey well, but in the night bleeding recurred to the extent of alarming prostration; local and constitutional remedies were applied, and, on account of the stomach refusing food from the amount of blood swallowed, he was stimulated as much as he would bear. In the afternoon he was brought back to the city. The bleeding having been so excessive, and the consequent prostration so marked, I told the family, after consultation with Dr. W. W. Turner, that the child could not live except transfusion was practised. I was the more fully impressed with this as a necessity from having been invited by my friend, Dr. T. G. Morton, to witness the procedure in a similar case in his practice, a full report of which he gave in the *American Journal of the Medical Sciences*, July, No. 135, pp. 116, 117. I feel quite confident that nothing else could have saved life in either case.

The condition of the patient on Monday, June 22, 1874, was critical in the extreme: pulse 160, feeble; surface cool, respiration panting; general lassitude, with disposition to drowsiness, amounting nearly to stupor. Transfusion of f℥iv of blood from the father yielded a result beyond our most *sanguine* expectations; in short, recovery began from that day, progressed, and at this writing, Nov. 2, is established. It is proper to state that on the third day following the transfusion, violent epistaxis recurred. This was arrested by plugging the nares, with the aid of Bellocq's cannula. There was no internal medication, excepting quiniæ sulph., gr. iv daily, and the following, at the suggestion of my friend, Dr. A. Frické:

℞. Tinct. nucis vomicæ, f℥ss,
Acid. phosphor. dilut., f℥ii,
Aquæ destillat., f℥i,
Syr., f℥ii.—M.
S.—Teaspoonful ter die.

The diet maintained was of the most concentrated character, such as juices of meats, eggs, milk, and cream, with a liberal supply of champagne daily.

Without entering into the consideration of the minute physiological conditions of the blood, and the agencies which affect it, I take it for granted that the blood is the medium that perpetuates both molecular and general growth, and through which repair is instituted: From what little I have seen of transfusion and its effects, I see no reason to oppose the views made public more than two hundred years ago, but rather to endorse the quaint words, "the blood may, if it takes, be of mighty use to man's health, for the amending of bad blood by borrowing from a better body." If the loss of blood causes a depression or a lowering of the vital forces, an extra supply should certainly be invigorating.

Respecting the method of transfusion, a rule applies to this as to all other operations in surgery, viz., the simplest is always

the best. Undue haste in any part of the procedure must be avoided, and all preparations should be carried on away from the parties concerned: it is well to make every provision for what may occur in the way of accidents, rather than wish for things not at hand when, perhaps, too late. Hot and cold water, ice, old muslin, sponge, spirit of ammonia, stimulants, an operating or a pocket case of instruments, in addition to those directly required in the transfusion, with at least two competent assistants, may be placed upon the list as requisites. The syringe should be in perfect order, for this is *the* instrument in this operation, and I certainly would not advise any one to run the risk of being equal to the emergency and try to manufacture a syringe for the occasion from any glass one he may find about the house, for only a good blower could succeed in this.

The apparatus used for receiving and defibrinating (it should be termed "oxygenizing") the blood may be described as follows. It is not intended to be referred to as possessing any special merit over and above those already presented. There is scarcely *any* surgical apparatus which may not be extemporized, and *successfully*, when occasion requires and ingenuity suggests, but it is often a point to be able to connect luxuries and necessities. The receiver should be of a shape easily adapted to receiving the blood direct from the donor, and, being graduated, measures accurately the blood drawn, doing away with the necessity of transferring from one vessel to another, and serving as well for extra venesection should the quantity fall short of that deemed requisite. It can be readily and cheaply made, and, as I believe the air-tight syringe to be the best instrument for transfusion, the receiver should nicely fit the nozzle, and allow all the blood to be drawn into it. The application of all these means is the most apparent in that this operation is, in one respect, similar to that of tracheotomy for croup—it is a last resort, allowed when there is little or no hope of anything else succeeding as life-saving. Therefore it is that all apparatus and all procedures should be as efficient as possible, and have economy of time as a principal feature.

I think it would have been simply impossible to employ the direct method in the case of this child, for even an ordinary amount of quiet could scarcely be obtained, and the use of an anæsthetic was not to be thought of. Proper care being exercised in the use of the syringe will give greater prospects of successfully transfusing blood free from any air whatever than by any other means that I know of. There are some conditions of the patient at the time of this operation, connected with if not dependent upon it, which it is well for all to know. The breathing becomes quite irregular, the efforts amounting to quite a struggle; the surface becomes cool and pallid, followed by rigor of unusual severity; the pulse is feeble and quite irregular, and fluttering. The patient repeatedly asks for water, and ice is taken with avidity even during the rigor; all the conditions are

those generally accompanying a severe nervous shock. The treatment at this stage embodies the usual applications for restoration—artificial heat, rubefacients, plenty of air, fanning, ammonia, stimulants, etc.—*and must positively be persevered in until full reaction is established.* These symptoms seem somewhat alarming to one performing the operation for the first time, and might lead to the supposition that air had entered. I think they can all be explained by the fact of the heart being called upon suddenly to accommodate its action to a considerable quantity of blood after having been greatly depleted. Again, the shock may be greatly aggravated if the blood is introduced at too high a temperature; 100° is sufficient, or one or two degrees above may be allowed, to compensate for cooling in the handling. This last will be but slight if the syringe is (as it should be) immersed in the same water which is used for heating the blood.—*Philadelphia Medical Times.*

Endocarditis from Malarial Infection.

It is said that an alteration of the blood, produced by paludal infection, may, as in variola, be the cause of endocardial lesions. Among the various species of ulcerous endocarditis it would appear that we must now reckon another as the effect of this form of poisoning. That which follows rheumatism becomes generalized in the entire circumference of the mitral orifice, and gives rise to the formation of a tissue which has a tendency to become organized, like a cicatrix, and thus produces more or less considerable stenosis of the cardiac orifice, and consequently death from impeded circulation. The endocarditis which occurs in puerperal conditions produces, on the contrary, an exuberant vegetative tissue, the organization of which is impossible, and its detritus may carry infection throughout the entire organism. According to Lancereaux this new species of endocarditis is especially localized in the valves of the aorta, and attacks, by preference, those who have been poisoned by paludal miasm. This endocarditis is anatomically represented by tumefactions located at the valves of the aorta, and sometimes at the mitral valve, with prominent vegetations, free or covered by fibrinous deposits. Histologically, they consist of small, round, embryonic elements, formed at the expense of the connective tissue. When these vegetations undergo a granular fatty degeneration, aneurisms, lacerations, ulcers of the valves, and molecular detritus are formed, which may give rise to embolisms, etc. The symptomatology does not differ from that of the other alterations of the aortic valves; a differential diagnosis is therefore, almost impossible. A double souffle, with its greatest intensity at the base of the heart, and prolonged to the aorta, and throughout the thorax, is the constant sign. It coincides with cardiac hypertrophy,

hepatic hyperæmia, without œdema of the extremities, and with characteristic splenic engorgement. A few slight chills, a paroxysmal fever, vomiting and diarrhœa, anorexia, and dejection are, according to Lancereaux, the general symptoms which assist most in the diagnosis. The adynamic character of the phenomena, the lividity and the pallor, without convulsions, are the most important complements. Its course is more rapid than that of rheumatic endocarditis. A little more than a year suffices for the evolution of the disease, and the fatal termination is not caused by any impediment to the circulation, but rather by an intoxication of the blood; hence the fever and the other phenomena not observed in the former. The prognosis is, therefore, very grave, for therapeutics cannot prevent the softening and the detachment of the vegetations. The septicæmic phenomena should be combatted with tonics and antiseptics. Quinine betters the symptoms decidedly.—*Annuar. de Sci. Med. and Lo Sperimentale,* January, 1875.

Salicylic Acid.

We have received from Professor Horsford the following abstract of two papers just received from Professor Kolbe, containing the results of experiments made at Leipsic with salicylic acid.

In the lying-in hospital at Leipsic, salicylic acid has been employed to the exclusion of carbolic acid since July last for disinfection of the hands, in vaginal douching, application to ulcera puerperalia, etc., in solution in water of one part in three hundred to one part in nine hundred, or as a powder mixed with starch in proportion of one part in five. This use of salicylic acid has thus far been attended with such successful results that it is recommended in the strongest terms for use in obstetric practice, by the authorities of the hospital.

Professor Kolbe suggests that physicians, and especially hospital physicians, should study the action of salicylic acid as a medicine, whether and in what quantity of larger or lesser doses it will influence scarlet fever, diphtheria eruptions, syphilis, dysentery, typhus, cholera, etc.; and whether it may be used against pyæmia and the bites of dogs; also whether it may not be used advantageously among horses, cattle, and sheep to prevent glanders, foot-rot, mortification, and so forth.

Kolbe, to prove the innocuousness of salicylic acid, took for several consecutive days half a gramme (seven and a half grains) daily in water, one part to one thousand, without the slightest observable unpleasant effect. After an interval of eight days he took, for five consecutive days, one gramme (fifteen and a half grains) daily, and then for two days one and a half grammes (twenty-three grains) in alcohol each day. The digestion was

perfectly normal; no trace of salicylic acid could be found in the urine or fæces. (The test is perchloride of iron, which gives an intense violet color.) At no time was there the slightest discomfort.

The experiment was repeated by Professor Kolbe and eight of his students, all at the same time. Each took on the first day one gramme, and on the second day one and a quarter grammes, of salicylic acid. Not one of them was able to observe the slightest derangement of any organs.

The acid in diluted solution is employed to wash the feet to prevent the offensiveness arising from the butyric, valerianic, and other related acids in sweat. It is also used as a constituent in tooth power, and for a liquor to wash the mouth.

Professor Wunderlich, of the University Hospital, Leipsic, recommends a medicinal preparation of salicylic acid for internal use, consisting of

Acidi salicylici	1	gramme,
Olei amygdalæ dulcis	20	grammes,
Gummi Arabici	10	"
Syrupi amygdalæ	25	"
Aquæ florum aurantii	45	"

Kolbe proved by experiment in the bath that the salicylic acid is very little if at all absorbed through the skin.

C. Neubauer (a pupil of Professor Kolbe) has experimented with salicylic acid to determine the quantity necessary to arrest fermentation in solutions of sugar and in new wine. He found that one gramme of salicylic acid is adequate to make 0.98 gramme of press yeast (weighed dry) in ten litres (about ten quarts) of new wine incapable of fermentation.

Kolbe found that $\frac{1}{2000}$ of salicylic acid would keep river or pond water in casks perfectly fresh (the experiments continued four weeks in a warm room) where without the acid the water acquired unpleasant taste. This quality will make the salicylic acid serviceable in preserving water on long sea-voyages.—*Boston Medical and Surgical Journal.*

On Micro-Organisms in Suppurations—Their Influence upon the Course of Wounds, and the Means of Opposing their Development.

At a recent meeting of the Académie des Sciences, M. P. Bouloumie read a note upon this subject, from which we condense the following:

1. Pus from any collection not in direct or indirect connection with a wound never contains micro-organisms, with the exception of certain minute brilliantly refractive points often joined two and two. Pus from a wound, whatever its nature and what-

ever the dressing employed, has always afforded micro-organisms, usually endowed with movements which become more appreciable when the pus is diluted. In pus from an abscess in the vicinity of a wound, the number and variety of organic forms observed depend upon whether the abscess is in a tissue continuous with or only in contiguity to that in which the wound is situated. In the first case the forms are numerous and varied, in the latter few and simple.

The movements of these organisms, which are hardly perceptible at the moment the abscess is opened, become much more so after exposure of the pus to air, and especially if the latter is diluted. The elements observed in pus, besides the pus-globules, the white and the red blood-globules, are almost constantly the same; they are composed of mobile and immobile single granules, chaplets of the same, etc., and are described at length by M. Boulouinie. All these elements are surrounded by an amorphous, fatty, granular substance escaped from the pus in process of destruction.

2. No dressing will absolutely prevent the presence of micro-organisms in wounds, although certain applications may modify their number and activity. Alcohol and glycerin appear to be the dressings which repress most powerfully the vitality and mobility of micro-organisms. Lister's dressings do not prevent their appearance in the products of suppuration.

Raw cotton dressing, made with care, the wound having been cleansed with dilute alcohol and covered immediately with lint soaked in glycerin, permits the development of organisms only in such small quantities as to lead one to believe them contained in the dressing at the time of its application.

3. The micro-organisms above described do not exercise an equally morbific influence upon the wound under all circumstances, or upon the individual. They may exist in the products of suppuration without hindering cicatrization or injuring the patient's health. They may, however, at times invade the parts in the vicinity of a wound, and give rise to abscess in the neighborhood. They may, by means of the lymphatic or venous system, invade a healthy organism without doing anything more than provoking reactions and eliminating dejections. They may invade an organism already profoundly affected, and may develop septicæmia; first by their toxic action, then by the virulent action of the elements disorganized by them.

4. Among the agents used for dressings, it is necessary to choose those which exercise a favorable action upon cicatrization, and which, at the same time, are opposed to the vitality and proliferation of the micro-organisms. From this point of view, alcohol and glycerin ought to have the preference. They arrest the life of those vibriones which require water, by depriving them of their water of constitution.

5. It is by proper dressing, very often, that the first step in infection is prevented, and it is by an intelligent hygiene that

the chances of generalization on the part of the morbific ferments may be diminished, should they escape beyond the limits of the wound.—Abstract from *Bull. Gén. de Thérap.*, January 30th, 1875. X.

Vulpian on the Action of Jaborandi and Atropia on Perspiration.

M. Vulpian (*Progrès Médical*, February 13, 1875) says that it has been experimentally ascertained that atropia checks the flow of saliva from the submaxillary gland by paralyzing the terminal filaments of the chorda tympani. Heidenhain has shown, further, that the vaso-inhibitory (or vaso-dilator) fibres contained in the chorda are not affected by the drug; for excitation of the glandular filament of the chorda (*i. e.*, faradisation of the lingual before the glandular filament is given off from it) accelerates the circulation through the gland quite as well after the administration of atropia as before it; in the former case, however, it no longer succeeds in causing a single drop of saliva to flow from Wharton's duct. Lastly, we may conclude that atropine does not act directly upon the secreting cells; for the same slight exudation of saliva is caused by electrical stimulation of the cervical sympathetic in a curarised dog, whether it be under the influence of atropine or not.

Now if either muscarin or jaborandi be injected into the femoral vein of a curarized dog, profuse salivation ensues; this may be immediately arrested by the subsequent injection of atropia. What we already know of the action of the latter alkaloid, authorises us to conclude that both muscarin and jaborandi excite salivation by stimulating the terminations of the secretory fibres of the chorda tympani, and not in virtue of any action on the sympathetic.

By putting together what is known about the influence of atropia and jaborandi on the salivary secretion, and about their mutual antagonism as regards the perspiration, Professor Vulpian endeavors to throw light on the nature of the influence exerted by the nervous system on the latter function. After referring to the experiments of Bernard, who showed that section of one cervical sympathetic in the horse was followed by sweating of the head and neck on the same side, while stimulation of the upper end of the divided cord arrested the secretion, he suggests that the sweat-glands may probably stand in much the same relation to the vaso-motor system as the submaxillary gland. Moreover, since atropia and jaborandi do not influence the submaxillary secretion through the sympathetic nerve, their corresponding action on the perspiratory function should not be attributed to any stimulation or paralysis of vaso-motor fibres. We may legitimately suppose that the functional activity of the sweat-glands is controlled by some nervous mechanism analo-

gous to the secretory filaments of the chorda tympani in the case of the submaxillary gland.

He adds a hypothesis concerning the mode of action of the chorda tympani. It is not needful, he thinks, to suppose that its terminal filaments are in direct relation with the secreting cells. We may regard the secretion of the submaxillary gland as being under the control of intrinsic inhibitory ganglia. The degree of activity of these ganglia is inversely proportionate to the functional activity of the secreting tissue. The glandular filaments of the chorda tympani are intimately connected with these regulating centres; stimulation of those fibres arresting the action of the ganglia, while their paralysis leaves the ganglia to operate unchecked. Jaborandi and muscarin paralyze the inhibitory ganglia; they do what may be done by electrisation of the chorda tympani. Atropia, on the other hand, exalts the normal activity of the ganglia, its action being analogous to section of the chorda tympani.—*London Medical Record.*

Langley on the Action of Jaborandi.

Under the title of "Preliminary Notice of Experiments on the Physiological Action of Jaborandi," Mr. J. N. Langley, of St. John's College, Cambridge, sends a very interesting and most important communication to the *British Medical Journal* of February 20. He finds that, injected under the skin, jaborandi excites through its influence on the spinal cord, tetanic convulsions similar to those of strychnia. In dogs and rabbits this effect was not observed, though this difference may depend on the dose.

The drug, introduced into the jugular vein, at once lowered the pulse in dogs and rabbits. The pulse was reduced to one-third its previous rate. In the frog the drug arrested the heart in diastole, but atropia caused the heart to beat again. He concludes that jaborandi stimulates the inhibitory fibres of the pneumogastric, but does not rapidly exhaust them. In the dog and rabbit, jaborandi caused slight decrease in arterial pressure.

Injected under the skin of a frog's back, it dilated the arteries, capillaries, and veins, and retarded, nay, even arrested the circulation of the web of the foot, whether the sciatic nerve were intact or divided. Injected into the jugular vein of a rabbit, after section of the sympathetic of one side, jaborandi first contracted and then dilated the vessels of the ear; the dilatation was greater than on the side where the sympathetic was left uncut. He concludes, therefore, "that paralysis of the normal vaso-motor influence does not thus seem to be one of the most prominent characteristics of jaborandi."

As jaborandi caused increased secretion of the submaxillary gland after section of the chorda tympani, he concludes that

the drug acts on the periphery of the nerve. It rapidly exhausts the gland. Section of the sciatic nerve appeared to have no influence over the secretion of the skin of the leg excited by jaborandi.

Mr. Langley points out the similarity between the action of jaborandi and of physostigma.—*London Medical Record.*

Best Mode of Making Suppositories.

In the opinion of the writer, the best mode of dispensing suppositories with dispatch, insuring at the same time a perfect distribution of their medicinal ingredients, avoiding all foreign matter for the purpose of hardening, and giving the satisfaction to know that the cones will melt at animal heat, is the following, which I offer to the readers of the *Journal*, hoping it will be of benefit to those pharmacists who have experienced trouble and loss of time in their preparation:

Take of cacao butter a sufficient quantity, powdered in a wedgewood mortar by first striking the butter gently until it is broken up into quite small pieces, a little care being required so as not to strike too hard, otherwise the friction produced would have a tendency to soften the butter, making it a little more difficult to manipulate; then add the medicinal ingredient, and rub all together, forming a plastic mass to be rolled out into a suitable length, and cut up into as many pieces as suppositories have been directed, each piece to be formed by the fingers and a spatula into a conical shape. It is advisable to sprinkle a little lycopodium over the fingers to prevent contact of heat from the fingers, which would soften the mass during the necessary manipulation. If made in winter, when cacao butter is much harder, by the addition of one drop of glycerin to each suppository, a mass can be formed in a much shorter time.—*American Journal of Pharmacy.*

Treatment of Abscess in the Knee-Joint by Subcutaneous Pneumatic Aspiration.

As the pneumatic aspirator is not yet in general use among medical men, the following brief note of a case, in which its use resulted in complete success, may prove interesting and instructive.

Mrs. S., aged 26, sent for me on January 26th. She informed me that a month previously, she had fallen down during the frost and struck her left knee against the curbstone. Since then she had suffered intense pain in the joint, which had gradually increased; for several nights she had had no sleep; four

days back she had a rigor; her temperature at noon was 99.2°; pulse 80; tongue a little coated. On examining her knee, I found that it was greatly swollen, exquisitely tender to the touch, and she could not bear the least movement. There was distinct fluctuation, and I had no doubt that fluid was in the cavity of the joint. As she had not slept for several nights, I ordered her twenty-five grains of chloral at bedtime, but the pain was so intense that it gave her no sleep. I asked my partner, Dr. Gibbings, to see the lady with me, and he agreeing with my view of the case, I determined to try pneumatic aspiration. The lady would not hear of this being done without chloroform; and as she was in very great pain, and we were anxious to further examine the joint, Dr. Gibbings administered the anæsthetic, and I passed the second largest of Messrs. Weiss & Son's needles into the joint, when about two ounces of thick pus, mixed with blood and broken-down-looking tissue, passed into the exhausted reservoir. The patient quickly recovered from the chloroform, and said the joint felt much easier. I put her on a back splint, and gave a grain of opium at bedtime. She slept well, and the next morning was quite free from pain. I kept on the splint for a fortnight, and then began careful passive motion. She is now well, and has gone to the country. The knee feels weak, and is a little stiff, but this inconvenience is gradually passing away.—Frederick H. Daly, M.D., in *The British Medical Journal.*

Affecting Dissecting Room Scene.

The janitor of an Indianapolis medical college was deeply affected on recognizing his mother-in-law on the dissecting table. His grief was the more poignant from the fact that he had himself carried the stolen corpse up three flights of stairs.—*The Medical Record.*

Bromide of Potassium as a Caustic, Anæsthetic and Curative Application to Malignant, Suppurating Tumors.

Peyraud (*Il Raccoglitore Med.*) had a cancroid of the face which produced excruciating pains, and which could not be extirpated. He determined to sprinkle the surface with finely pulverized bromide of potassium, after doing which the pains diminished in intensity, and the growth of the fungous granulations was checked. This treatment was continued for four weeks with excellent results, and during this time the tumor entirely subsided. In this case the bromide exerted an anæsthetic influence on the terminal fibres of the sensitive nerves, diminishing the pain, and acted also through its vaso motor peculiarity, by contracting the vessels of the tumor and thus diminishing its volume. It

acted finally as a caustic, and destroyed the superficial tissue; upon this point Peyraud advises that in order to obtain the full effects of the bromide, the surface should be gently washed off just previous to the sprinkling of the powder.—*Allg. Wein. Med. Zeit.*, February 23, 1875.

ARTICLE V. *Via Medica; an abstract of an Address delivered before the Graduates of* 1875, *at their Public Commencement Exercises, in St. Patrick's Hall, March* 19*th*, by S. M. BEMISS, M.D., Professor of the Theory and Practice of Medicine, and Clinical Medicine, Medical Department, University of Louisiana.

Gentlemen: The ceremony which calls us together to-day, is a seemly and becoming one.

Whenever new candidates for membership in a cultivated profession knock at its portals for admission, and are found worthy, it is proper that a committee be appointed to meet them at the threshold and bid them welcome to the honors and privileges to accrue from their new position. In conformity to such a custom, the Faculty of your Alma Mater has to-day devolved this important function upon me.

Gentlemen: This day is to each of you an epoch so full of importance and consequence, that the duty I attempt to execute is a most responsible one.

Whenever the one of your teachers who may be delegated to perform this duty can succeed in making the occasion intrinsically one of instruction and interest, it must afford the new communicants a twofold satisfaction. First, that they shall be able to revert to it in after years, as the interesting epoch at which the consummation of long-fed hopes was effected. Second, that they shall remember it as an occasion of actual accretion of knowledge, and consequently of improvement. A speaker capable of achieving such an end, may well exclaim with the syren of old, "You shall leave us much delighted, and with an increase of knowledge."

At the last anniversary one of my learned colleagues gave a lecture on Medical Education, which, because of the severity of its analysis and correctness of its conclusions, will find a prominent place in the medical literature of the day. Neither opportunity nor qualification is afforded me for the performance of my

duty in a similarly scientific manner. I have therefore chosen to endeavor to entertain you with some account of the road over which you this day commence your journeyings.

The subject of my address is "Via Medica"—the doctor's pathway. It would require too much time to attempt to give a full panorama of this road, with its hills and ascending grades—its boggy places, its rough pavements—its easy going bye-ways that invite the traveller to wander from the true path. I shall therefore confine my sketches to mere glimpses of the studies of the physician—of his divergences from the true path, and of his diversions and recreations while *en route*.

I. The Studies of the Physician.—Very soon the young physician realizes the fact that if his student days are in name ended, his life of real study has but just begun. He learns that our palace of art is very unlike that "palace of art" which Tennyson built as a "lordly pleasure house for his soul." On the contrary, the conscientious members of our profession labor earnestly and incessantly—perpetually seeking for new truths and a higher state of perfection.

The first point, in respect to which the young physician, who is ambitious of preferment, should desire to perfect himself, is the application of his scholastically acquired knowledge, to the actual cure of disease. Every one must admit, that an amount, greater or less, of real experience is necessary to confer upon a physician that adeptness in cure—that *savoir faire* in dealing with disease, which attends the most successful practitioners. There is, however, a marked difference in the amount of experience which enables different debutants to reach the same plane of proficiency. Perhaps failure on the part of those who lag behind, is more often owing to lack of proper method in their attempts to reduce science to art—to convert their hitherto didactic medicine to applied medicine, than to any personal inaptitude on their part.

In truth, as far back as any history of human philosophy can be obtained, a great difficulty to overcome has always consisted in the inability to determine the abstruse relations between pure science and the material world.

Formerly, under the teachings of what was called "the philosophy of identity," a belief seemed to linger in the mind of man that "enough of the attributes of the Deity had been bestowed upon him to justify him in attempting the role of infinite intelli-

gence, and he made vain efforts to solve the nature of the material by philosophic abstractions of the human intellect." But medicine is a practical profession, and not a speculative one. Learning, however vast the accumulation, does not endow the possessor with power to cure disease. But one great desideratum which unfailingly brings in its train the highest degree of skill, is learning united to practical wisdom. Accumulation of knowledge is the foundation of the physician's edifice—practical application of that knowledge is the superstructure.

It is not possible to cast any very correct estimate of the amount of knowledge the physician has accumulated at the date of gaining his diploma. I would, however, venture to state that he possesses a competent knowledge of the human body; of its histology and chemistry; of its physiology, and that display of forces which constitutes the dynamics of a healthy system. He is acquainted also with the precepts of surgery; likewise with the rules which should govern his conduct in reference to those processes, both normal and abnormal, which are connected with one of the two great epochs of human life which chiefly concern the physician. He can generally describe, as the books do, the symptomatic phenomena of a given disease. He can recite the names of the various drugs and their respective doses which are recommended for the cure of a given disease But bring him face to face with some terrible attack of sickness—tell him the conflict is his, and the issue is dependent upon his skill—place the whole wealth of therapeutics at his right hand, and bid him select his weapons, and his experiences will be very much those of old David—he has not proved these new weapons. Now, it is precisely in respect to this point, that you will have the most abundant and perpetual cause for congratulating yourselves that your course of study has been taken at a great hospital, in which actual exhibitions of the behavior of disease under the physician's management have been afforded you. I may even go beyond this ground for congratulation, which makes you only spectators of the fray, for each of you may truly say in reference to these illustrations of practical medicine, *pars fui!* But priceless as these privileges assuredly have been, they cannot have developed any young physician so much that there is not ample room for growth before reaching his professional maturity.

The study first in point of importance to the young practitioner is the careful analysis and record of his cases. This analysis

should, in each instance, be made to represent as far as possible the whole morbid process, and the items and sum of means used to antagonize, or thwart, or, as must sometimes occur, to promote and encourage certain features of these diseased processes. Without doubt, every new alumnus now present is able to diagnose the great majority of ordinary diseases, and to give each its correct name and place in scientific nosological classifications. But the difficulty in respect to the descriptions of diseases as found in the text-books, or as heard in your lectures, consists in this, that however perfect the delineations may be drawn, they still fail to represent the activity, the power and mode of doing mischief, which should always be attached to the idea of a morbid process.

It is as if a painter had pictured a lion so accurately, that every spectator shall at once see and recognize the object intended to be represented. But he is quiescent. To realize his enormous power, we must see this monarch of brutes seize his victim, and hear the crunching of bones between his resistless jaws. So with disease: we must witness its modes of attack, and endeavor to lay bare those processes by means of which it renders life impossible. The difference in the morbid processes of various diseases constitutes that difference between them individually, which justifies us in giving them substantive names, and in creating a methodic nosological arrangement.

These leading facts should enlist the young physician's attention as primal points of study. We all know that in war it is of the utmost importance to a successful general, that he should as quickly as possible learn the plan of campaign and tactics his enemy is about to pursue, together with the precise character of the destructive weapons he employs.

The knowledge thus indicated is, in either case, only obtainable by careful observation. Having been most carefully instructed in respect to the character of observation necessary to constitute a proper clinical record of a case, no further admonitions are required except to mention, that one source of thoughtless error often spoils a case-report. This is, the substitution on the part of the physician of his own subjective convictions for actual observations. The physician is obliged to bring into use each one of his senses in detecting and diagnosing disease. But he must test and prove these various senses until he can register them as thoroughly trustworthy. For example; the tactile sen-

sations in regard to the fever heat of scarlet fever, often lead the observer to place it at a higher mark than actual measurement shows it to reach. Therefore, until these senses are so thoroughly educated as to constitute them servants, sufficiently trustworthy to be depended upon, the practitioner should prove them by repeated observations, or by such other means of proof as may be brought to bear upon the case.

But let the physician be ever so accurate, and patient, and persistent in taking his observations of disease, he is forced to remember that these observations are merely notations of symptoms. It has been appropriately said that symptoms are the outcroppings of disease. True, but still the sum total or character of outcroppings may fail to represent the sum total or character of morbid conditions which underlie them. It often occurs that a frightful array of symptoms is in reality a manifestation without danger, while an unnoticed and apparently insignificant matter may betoken the greatest peril. The whirlwind passed by, but God was not in the whirlwind—the earthquake and the fire passed by, but God was in neither. After the fire, a still small voice indicated His Majestic Presence. So it is with disease: the quiet, latent symptom, often indicates the presence of a fearful morbific potency which is attacking the strongholds of life. The stormy tempest of convulsions is a source of far less concern to the practitioner than the "still small voice" of that faint crackle, scarcely audible to the ear, which informs us that a vitally indispensable organ has become the seat of inflammation.

A precise knowledge of the relations of symptomatic phenomena to the morbid processes with which they are coincident, would bestow upon its possessor the highest attainable degree of acquirement in regard to diagnosis and pathology.

Thus far our profession has failed in its efforts to formulize or elucidate some of the most striking phenomena connected with certain morbid processes. All our learning, and means of observing, and skill in the employment of those means, have failed to explain the behavior of many specific poisons when introduced into the human system. A speck of small-pox virus, so minute as to elude all our senses, gains admission to our systems, and silently multiplies itself, until in fourteen days it produces a loathsome inflammation of the skin. After recovery, the person thus affected is rendered so insensible to this poison that he

enjoys complete immunity from future attacks; and yet neither he, nor any one who examines him, can point to the modification or change which endows him with this exemption. These are marvels which, for anything we now know, may continue to be classed with the unknowable. But while the actual *modus operandi* of a morbid process can no more be laid open to our view than the physiological processes of the system, we have learned enough, respecting them to enable us to formulize rational and curative measures of treatment. In respect to some diseases we say, the process cannot be arrested until it reaches a certain point, or culminates in a certain event. In other instances we say, the process has no definite period for the consummation of its course, and is susceptible of arrest by appropriate remedies.

In the former case, we simply endeavor to protect nature in those evolutions which tend to bring the process through its inevitable cycle This we accomplish by imitating those circumstances, conditions and surroundings, that had relation to the most favorable cases, which are termed "typical cases." However, the skillful physician does not allow his admiration of nature to prevent his interference when any untoward circumstances either retard or intensify the morbid process.

When, on the other hand, the morbid process is entirely accidental as to duration, we employ our energies to bring about its arrest and retrogression.

I may truly say, that within the whole compass of medical science, there is no department of study more full of absorbing interest, or more pregnant with important consequence to success, than that of the morbid processes of various diseases. As previously observed, the sum total of these various processes in each instance represents that morbid entity, or substantive presence, with which we figuratively invest disease.

Next, as an important branch of study, comes the application of various material substances to the cure of disease—"Therapeutics." The meaning of the old Greek verb from which our term "therapentics" is derived, did not directly imply the administration of drugs. Therapeno: I wait upon; I serve. I am the more careful to put some stress upon this original use of the word, as it releases us from the sense of obligation to medicate, which might otherwise attend its employment. Therapeutics is one of the weak points of medicine, and must of necessity con-

tinue to remain an imperfect branch of our art. The very intricate character of the problems which therapeutics is expected to solve, prevents the demonstration of that certainty which should attach to a positive science. For example, in therapeutics the nature of the problems may be thus stated: Given; certain agents which are known to exert specified influences upon the human economy—modifying its forces—changing its secretions and excretions—composing certain organs to physiological rest—exciting certain others to additional action. Given; different diseased states which alter and perturbate, in an almost endless manner, the regularity of action of those agents. Given, lastly; a finite mind to determine in respect to the selection of the agent, both as to quality and quantity, and to determine likewise in regard to the precise present conditions of the body he designs to act upon. Surely, few problems can be presented to the human mind which require so much knowledge and such clearness of judgment, or which involve greater responsibility. It is therefore well said, that the power of the physician is in no sense neither omnipotent or supernatural—he only subordinates forces, forever and everywhere existent, to the purposes of curing deviations from health.

But, while I feel disposed to point to these difficulties in their full magnitude, I am able to whisper many encouraging words in regard to their surmountal. For instance, even if we properly postulate the statements that, metaphysically considered, there can be but only one corect prescription for any given derangement of health—that this right prescription is absolute truth, and all others are departures from the truth; yet the results of actual practice are not subordinate to so strict a ruling. Nothing is more common than for physicians who have a "perfect agreement in regard to the pathology present in a case, to differ in their therapeutics." No truth is, indeed, better understood in our profession, than that diverse measures of treatment may be based upon the same principles, and successfully employed to achieve the same results. To borrow the expression of a recent author, "the human economy is a complex organism whose upward recoil may be brought about by impulses in more than one direction." These admissions, however, do not successfully controvert the statement that in all given cases, some one course of treatment more nearly approximates the standard of absolute truth than all others. Our constant endeavor should therefore

be to attain a line, at least, closely parallel to truth. Another difficulty connected with therapeutics, important to be noted by the new alumnus, rests in the necessity of accommodating our therapeutical measures to the stage of the morbid process present at the hour of prescribing. A certain consensus between the disease and remedy must be constantly preserved. The opium given to cure the first stage of pleurisy is needless, or even hurtful in its second stage. It is not to be doubted that great discredit has been thrown upon therapeutics from sheer neglect of this principle.

The study of Hygiene is an important one to the physician. Its great aims are as beneficent in purpose as the cure of disease. These are, to point out those conditions on which health depends. The fulfilment of such a promise involves on the one hand information respecting those circumstances, conditions, influences and agents, whose presence is essential to human health. On the other hand information respecting those circumstances, conditions, influences and agents, whose presence is detrimental to human health. The former part of this twofold duty is better performed than the latter. That is to say, while hygiene has taught us inestimable lessons in regard to those rules whose neglect is very surely followed by deterioration of health, it has thus far failed in determining the nature of those influences and agents, which are so deleterious to health, that they devastate various parts of the world in the form of sweeping epidemics. But while this latter is confessedly the weak point of hygiene as a science, it is yet that part of it which, as a study, is likely to reward investigation by rich and wonderful discoveries.

At present we know but little of the actual nature and entity, or of the toxic mode of action, of those specific poisons which give rise to epidemics of cholera, yellow fever, or others of that class of diseases to which the term zymotic is often applied. Prior to the present epoch, the mode of studying these causations of disease was by assembling facts concerning the circumstances immediately attending their appearance, spread, and destructive action, and then to base a hypothesis upon these facts; very much as Newton built up his theory of the attraction of gravity by observing the falling apple. The energies of the profession are now turned to an attempted solution of the difficulties respecting the intrinsic nature of these poisons by efforts to establish their essential characters, through the certainty of

objective study. The general belief is, that the minuteness of these mischievous enemies of mankind constitutes the chief obstacle to their successful study. This is no new doctrine. The ancient idea that epidemic diseases originated from mineral or terrestrial exhalations, was, centuries ago, disputed, and the animalcule theory suggested as a substitute. During the middle ages, an hypothesis was prevalent that the infinitely small were man's worst enemies. Cuvier made this hypothesis more tangible, by speaking of the frightful profusion (*richesse effrayante*) of insect life.

At present the great desideratum is, to determine first the truth of these hypotheses. Second, if it be found that minute organisms, whether animal or vegetable, do occasion all this sickness and death, to determine what their essential nature and habitudes of life are, in order that we may scientifically formulate their mode of destruction. While ignorant respecting these points, nothing known to science warrants us in stating that our so-called disinfectants are capable of destroying any of these morbific agents, in the area of the atmosphere.

II. The Doctor's Divergences—The Bye-paths of Medicine—Errors of Belief and Practice.—Although it is written, "for of the Most High cometh healing," no disclosures in respect to any special means of cure have ever reached us from a supernatural source. The spittle and clay, the pool of Bethesda, and the seven dippings in the Jordan, illustrate miraculous interference with disease, instead of the intrinsic power of therapeutical agents. If we compare Theology and Medicine in respect to revelation, we perceive that their positions are almost entirely reversed. The student of theology finds it difficult to make his sublime lessons square with scientific formulations. Sometimes, in truth, the apparent, or real conflict between them is so great, that he retires from the field of contest, either doubting or a confirmed skeptic. In medicine, the nature of the contest is completely changed. Unaided by revealed truths, the physician seeks to force investigation into the hidden penetralia of nature, and strives to lay bare that secret and wonderful chain of relationship, which has placed the forces of the human body subject to the influence of a vast number of agents, distributed through the whole universe. The accomplishment of such a task is so difficult, that one might well implore aid of that omnipotent and

omniscient Being to whom alone this mysterious construction is, a perfect harmony and a perfect science.

In former times, physicians attempted to turn the prevalent philosophy of their various epochs to their own advantage, in affording means of development of medical knowledge. If philosophy taught that abstract questions were susceptible of elucidation by assuming premises, and basing upon them wordy arguments and ingenious deductions, physicians were wont to apply this, *a priori* method, in attempting to solve the great problems of their profession. This method is now altogether rejected. We now demand for the support of our hypotheses, a stated number of facts which shall agree with one another, at least in their most essential particulars. The facts of nature constitute the crucial test of our theories.

If I have stated these premises correctly, it may seem to some wonderful that erroneous belief could enter our profession. For if our motto really be "prove all things, hold fast that which is good," the exclusion of erroneous belief would appear to be provided for. But alas for human shortcomings! mankind is scarcely able to reach so high as to grasp truth unmixed with earthy error. The most common source of erroneous belief in medicine, may be safely alleged to depend upon such imperfect collations of facts that they fail to represent truth. In a very few instances, these assemblages of facts are the offsprings of violent intentions to mislead; when, for example, in support of narrow doctrine or a pet hobby, the physician selects from his facts such only as answer his purpose. But it more often happens, that groups of facts accidentally collocated, cause some medical minds to frame false hypotheses. For example, it has occurred during the recent session, that a number of cases of pneumonia were admitted to the hospital in conditions of cure so advanced that complete recovery was near at hand. These cases had passed through the dangerous stages of the disease, without the least care or treatment.

Now it is obvious, that a belief based upon these observations alone, would hold that complete absence of care and treatment was a proper course to adopt in reference to this dangerous disease. This would be a grave medical heresy, as correct statistics and observations abundantly prove.

If we follow those practitioners who have made imperfect collections of facts, from which they deduce false doctrines, we find

that after numerous failures in their illy matured efforts to cure disease, they undergo a natural transition from false hypothesis to a wavering faith, which afterwards culminates in absolute unbelief in medicine as a science, or as an art.

"The formation of correct hypothesis and pure faith, in medicine, is done by submitting carefully gathered facts and experiences to a calm and well matured process of thought." Only one thing is likely to invalidate conceptions of theory elaborated in this manner; that is, a subjective imperfection of the mental organization of the observer and thinker, which prevents correct conclusions.

It is necessary to observe, that it is a grievous misfortune that, from whatsoever cause the medical heresies of the practitioner spring, their mischievous effects immediately reach their patients. The theologian may preach error and yet practice all the observances of a pious life; the physician either puts his erroneous belief at once into practice, or he perpetrates deception by prescribing in opposition to his own convictions. Other erroneous opinions, generally attended by absurdly erroneous therapeutics, result from those acrobatic antics (as they might be termed), which seem as natural to the exercise of some wrongly constructed minds as the sawdust and spring-board appear to be to some physical organizations. I suppose that all of the learned professions comprise examples of this class of persons. They are, indeed, frequently remarkable for scholastic acquirements; brilliant and showy in professional or social deportment, yet crotchety, and forever chasing some fanciful vagary or unfruitful abstraction. Who is able to estimate the vast aggregate of injury inflicted upon the professions of theology and medicine by some of the glittering but misleading lights contemporary with ourselves?

Gentlemen—I make the assertion that the real presence and constant activity of a competent stock of strong, sober common sense, is more necessary to securing successful results in the practice of medicine than in the other learned professions. I make this assertion with an earnestness based upon a knowledge of its truth, but with becoming humility, for I know that a different ruling is often held.

III. The Doctor's Diversions.—The pleasant places and shady bowers along his pathway, which invite him to loiter and refresh himself.

In every profession, or avocation, man requires his periods of rest and recreation. For fear that cupidity or other causes should interfere with the regular recurrence of such periods, the authority of divine edict has set apart every seventh day, as one upon which the din of human occupations should cease, and rest from toil should prevail. But Sundays are not for the doctor. His labors are classed with the undignified but benevolent function of lifting the unfortunate ass from the ditch. Pliny is authority for holding that "pleasant discourse, jests, conceits, and merry tales, *melliti verborum globuli*," are the sole nepenthes of Homer. According to the decision of a recent cynical writer, a doctor must practice so much self-abnegation as never to partake even of his own agreeable medicines of this description. "A merry doctor! why one might as well talk of a laughing death's head—the cachinnation of a monk's memento mori. This life of ours is sorrowful enough at its best estate; the brightest phase of it is sicklied o'er with the pale cast of the future or the past. But it is the special vocation of the doctor to look only upon the shadow; to turn away from the house of feasting, and go down to that of mourning; to breathe day after day the atmosphere of wretchedness; to grow familiar with suffering; to look upon humanity disrobed of its pride and glory, robbed of all its fictitious ornaments—weak, helpless, naked—and undergoing the last fearful metempsychosis from its erect and godlike image, the living temple of an enshrined divinity, to the loathsome clod and inanimate dust. Of what ghastly secrets of moral and physical disease is he the depository! There is woe before and behind him; he is hand and glove with misery by prescription—the ex-officio gauge of the ills that flesh is heir to. He has no home, unless it be at the bedside of the querulous, the splenetic, the sick, and the dying. He sits down to carve his turkey, and is summoned off to a *post mortem* examination of another sort—he knows nothing of the quiet comfort of the sleek-headed men who sleep o' nights. His wife, if he has one, has an undoubted right to advertise him as a deserter of bed and board."

The delineator of this picture threw into the coloring too much lampblack, in the way of horrors and sadness. Still it is an important question as to how the physician may best recreate himself, and wherewithal he shall seek to amuse and refresh himself during his hours of leisure. The sports of the field may invite him, or with quaint Izaak Walton he may wish to sing,

"I in these flowery meads would be;
These crystal streams should solace me;
To whose harmonious bubbling noise,
I, with my angle, would rejoice."

These recreations may in themselves be innocent enough, but they comport rather badly with medicine. People somehow, have got an idea that the apostolic precedent should still prevail, in so far that the occupation of fisherman must be abandoned when a higher vocation is assumed.

Gaming is so severely tabooed in medicine that but few physicians ever engage in it. It is scarcely probable that any member of our profession will ever become so skilled in the mysteries of cards as to be able to produce a text book on draw poker, like a recent representative of this great country at a foreign court.

Politics is a recreation to be classed with the lowest character of gaming, and equally to be avoided. Indeed, a political doctor is generally a failure, either as it relates to the service of his patients or his country. Yet it must be remembered, that important crises occur in the chronology of all human governments in which individuals of all professions and avocations should become aroused to the defence of imperilled rights, and merge every other consideration in the one great sentiment of patriotism.

There are certain circumstances, readily enough to be apprehended, which impose restrictions upon the physician's enjoyment of many of those amusements and relaxing pursuits whose beneficial influence is quite available to members of the other learned professions. It may be said of many of them, that as a rule, they are simply not miscible with the physician's pursuits and habits, without sensibly colliding with his interests. This is essentially true of all those recreations which require absence from his place of business.

Rush advised his students to get possession of land as early as possible, perhaps in order that they should feel so independent of professional fees, as to be under no temptation to build up medical accounts.

It is certainly true, that agriculture and horticulture chime in with our physiological studies better than most other addenda to our profession. Life everywhere in the vast range of nature, is to the physician a part of his science. There is to our view no missing link in the whole serial classification of life.

The study of botany is likewise extremely entertaining, as well as beneficial to the physician. What a wealth of enjoyment must be conferred upon the practitioner, who is able to identify and analyze all of those remedies which are produced in his own vicinity! In almost every plat of flowering plants which decorate the homes of our country, the physician who is acquainted with botany can single out some one, or more, of which he may truly say, these are not alone for ornament; with their aid I am able to succor suffering humanity. Nor must it be supposed that the habits or pursuits of our profession are in the least degree incompatible with the most cultivated and refined appreciation of the purely æsthetic. We are not so "enamored of pus and miasm," that our sense of the beautiful and sentimental may not be most exquisitely preserved. The physician may therefore consistently study botany, simply on account of the delicious gratification of the senses afforded by familiar acquaintance and association with the vegetable kingdom. Why may not the doctor, in his enthusiastic devotion to the beautiful in nature, apply to his own case the lines of the weary poet?—

"Ah! my heart is sick with longing,
Longing for the May—
Longing to escape from study
To the young face, fair and ruddy,
And the thousand charms belonging
To the Summer's day."

Another means of recreation to the physician, consists in studies with the microscope. The objections to it, as a means of recreation, are that it requires so much time, and imposes so much labor to obtain gratifying results. The pleasures and revelations it is capable of bestowing, are best described by one of its most devoted disciples. There are surpassingly beautiful creations too minute for the unaided human eye—music which the human ear can never catch—an infinitude extending downward, as well as that which reaches upward; for as we descend the scale of the microscopical world, such wonderful perfection is developed that we have reason to believe that it extends beyond man's powers of investigation.

Thus I have endeavored to give a brief abstract of the mental employments of the practitioner of medicine. I have been careful to make no reference to that portion of the *res gestæ* of our travel along the figurative *via medica*, which concerns our social relations in such a manner as to come within the purview of

morals, or ethics. The alumni present are the compeers of the whole medical profession, and with myself, there is neither privilege, appearance of necessity, nor the desire to give expression to admonitions upon such points.

In conclusion, I ask permission to call your attention to the influence which the emotions of the physician sometimes appear to exert over his coolness and skill. I am moved to make this request, first, because it happened that I have recently read a paper on what is termed the sympathy of a physician; second, because it has but just occurred to me, that a friend to whom I was sincerely attached died under my professional ministrations. The question of a physician's sympathy is therefore a subject which, at present, is very naturally one of interest to me.

"What is sympathy, in so far as a physician is concerned?" If we answer the question in its widest sense, we can state that "the studies of the physician are so broad as to include the vital phenomena of all living beings, and he has sympathy for all, for he sees that all are interwoven into the same inextricable web." Therefore, in respect to this broad exercise of sympathy, he is fitted to declare with Coleridge—

> "He prayeth best, who loveth best
> All things both great and small;
> For the dear God who loveth us,
> He made and loveth all."

But the physician's sympathy with his patient is a more restricted and personal outgoing of the heart. If mutual attachment subsists between the parties, the physician realizes a deeper sense of responsibility as it respects the treatment he pursues. In the ordinary routine of his professional duties, he never forgets the vast responsibility which he assumes when he undertakes to protect his fellow beings in their possession of the inestimable gift of life. But when the patient is endeared to him by the ties of personal friendship, it becomes a more trying crisis. Yet it should not distract his mind, or affect his capability to prescribe. The physician should consider sympathy to be a mere "compassionate feeling for his patient, rather than such a degree of feeling with him as to incapacitate the medical attendant for viewing the patient as a mere subject for medication—a deranged machine which he sets about repairing with the perfect indifference of the watchmaker over his broken piece of mechanism." This self control is not the result of hardening

one's sensibilities, or of acquired insensibility to ordinary endearments of friendship, but it is obtained through practice of a moral discipline which is often admirable to behold.

Gentlemen: Every one now within the compass of my voice, has lived as a contemporary with a great and good man, who commanded an army which fought bravely although to all human foresight, vainly. This general had sons enlisted in the army which he commanded. All who ever knew him could testify that a father's heart never beat with more tender sympathy for his offspring than did his. Yet he could order his sons under fire, and expose them to danger in the same impartial manner in which he treated the sons of other fathers in his command. This example is for the emulation of the physician—this sympathy is the proper expression of the physician's sympathy. It is an emotion which, tender as it may be, should put no restriction upon the employment of either the highest degree of skill, or of the greatest energy. Instead of striving to repress sympathy for our patients, let us encourage its growth. It is a divine attribute: "Lord, behold, he whom thou lovest is sick." It is a sentiment whose well-springs are so hard by the fountains of the crystal river of life, that the physician who experiences it in its fullest expansion, may hope to be vouchsafed an appearance of the vision which blessed Abou Ben Adhem.

"Abou Ben Adhem (may his tribe increase)
Awoke one night from a dream of peace,
And saw within the moonlight in his room,
Making it rich and like a lily in bloom,
An angel writing in a book of gold:
Exceeding peace had made Ben Adhem bold,
And to the presence in the room he said,
'What writest thou?' the vision raised its head,
And, with a look made of all sweet accord,
Answered—'The names of those who love the Lord.'
'And is mine one?' said Abou. 'Nay, not so,'
Replied the angel. Abou spoke more low,
But cheerly still, and said: 'I pray thee, then,
Write me as one that loves his fellow-men.'
The angel wrote, and vanished. The next night
It came again, with a great wakening light,
And showed the names whom love of God had blessed,
And lo! Ben Adhem's name led all the rest."

Gentlemen: In behalf of your faculty, and in my own humble name, I bid you now an earnest God-speed—an affectionate adieu.

PROCEEDINGS OF THE NEW ORLEANS MEDICAL AND SURGICAL ASSOCIATION.

On Homœopathy. By John Dell'Orto, M. D.

The questions proposed by the Committee for the discussion on the subject of Homœopathy to-night, are fourteen, which I will divide in two parts. In the former I will investigate the subject upon its scientific import. In the latter I will consider the social contact, intercourse between Homœopaths and Allopaths, and between them and the public.

To the first part I will answer with books in hands, as it is a very arduous labor for me, so little acquainted with your language, to open a discussion upon a subject so difficult, and so interesting, and for the study of which I have lacked leisure and opportunity; therefore, gentlemen, I need all your indulgence and patience. To the second part I will express candidly my personal opinion, as a physician, as a man, as a gentleman.

The better to facilitate the understanding of my paper, allow me to invert a little the order of the questions, and commence by No. 3—

"What is the cause of the origin, growth and success of Homœopathy?"

It was toward the end of the last century, that such a confusion reigned in the art of healing, that a great necessity was felt by every physician to reform the whole medical literature. From Cullen down to Broussais we perceive this tendency to abolish the already superannuated humoristic doctrine of Galen, and to enter a new era of progress. Medical science followed the philosophical spirit of the age, and that remarkable progress of ideas, which led to a change, and modification of the religious, as well as the political conditions of society. Notwithstanding the talent of such distinguished gentlemen, as Hoffman, Stahl, Boërhaave, Haller, Brown, Laennec, Bichat, Barthez; notwithstanding their endeavors to keep pace with the reforms of their times, they still viewed with fond and lingering gaze the past, from which they parted.

The cabalistic and spiritual theories of the old vitalist school of Paracelsus and Van Helmont, as well as the materialistic doctrines of Boërhaave, were still ruling over materia medica; and the art of treating diseases was reduced to mechanical administrations of medicines without any precise knowledge, either of them or of disease. An irrational combination of drugs was poured into the stomach of the patient, and man was considered as a machine, his stomach a chemical laboratory. The more enlightened physicians were disgusted; a good movement toward truth had already been effected by the modern anatomical, and physiological theories, but it was then incomplete.

In the midst of this chaos and anarchy, two new—then origi-

nal—and regular systems appeared in Europe; one born in Italy was called the doctrine of counterstimulation, headed by Rasori; the second born in Germany was called Homœopathy, of which Hahnemann was the founder. A man of talent, a hard laborer, a thorough scholar, and profound thinker, Samuel Hahnemann, disappointed with the inadequacy of the schools to explain the great principles of medicine, had just given up the practice of his profession for the more quiet and peaceful studies of the closet. One day, while translating the materia medica of Cullen, he arrived at the chapter on quinia, and found that the explanation given of the therapeutical virtue of this powerful drug, was so contradictory, and based upon so ridiculous an hypothesis, that he decided to experiment upon himself and test its effects. He took forthwith a strong decoction of that substance, and affirmed that after a lapse of time he had experienced symptoms similar to those of an intermittent paroxysm. He repeated the experiment, and as he averred with the same result. This property of quinia to produce fever in man in a state of health called to his memory the adage of Hippocrates "*vomitus vomitu curatur*," and by way of induction he came to the conclusion that the great secret of the cure of diseases must be in those properties of medical agents to produce phenomena analogous to those of the disease. To make sure of the fact, he tried the greatest number of active medicaments and poisons, and obtained the same result. In order to establish the counterproof, he administered those medicaments to sick persons, and the clinical result gave to the law its last sanction. It was a long, and tedious labor of over thirty years, during which time about one hundred medicines were experimented upon and tested. The promulgation of such a simple and plausible theory attracted proselytes to Hahnemann, and soon he was surrounded by many pupils, with whom he founded a new school, based on the following principles, which I will review per *summa capita*, as well as I could understand them in the several books which I consulted:

1st. There is in nature a law that rules the worlds physically and morally—the law of universal harmony. On account of this law, life is an action of appropriation, through which all external modifying agents that surround human beings, accomplish and perform in themselves a *summum bonum*, a harmonious, perfect, and absolute condition, which constitutes health.

When this harmony, this accord between man and nature is interfered with, diseases arise therefrom; disease, which is the perversion of health, an absolute disorder, an accident independent from nature.

2d. Diseases are the virtual and dynamic alterations of those functions which constitute health. "Dynamic" is the vital force, immaterial and intermediary between matter and spirit, to which all our vital actions are to be referred.

3d. Diseases are known by their causes and their symptoms.

The causes of diseases are occasional, and they may be divided

into local and general. The local causes produce acute diseases, the general, chronic

As the vital force is one and general, and as diseases are alterations of that force, so, according to Hahnemann, all diseases must be general; there cannot be any local disease.

Chronic diseases are the most numerous, and their causes constitute the nosology of the main portion of human infirmities; they are syphilis, psora, and sycosis.

The virtuality of diseases is that special *modus vivendi* of every one with regard to the dynamic alteration, and as symptoms are the only sensible manifestations of diseases, so each disease is an individuality composed of a peculiar specific combination of symptoms

4th. If we admit that by virtue of the general harmony of the universe, the Creator has provided human beings with physical means of subsistence, such as vegetables, fruits, and animals, why should we not admit, also, that he has prepared, in the natural order for each disease its therapentical corresponding remedy.

It is the duty of physicians to study these therapentical remedies, and find the medical agents, the dynamic, specific properties of which will influence the vital force, and destroy the disease in its succession of symptoms.

5th. To know the specific properties of medicaments, they must be tried by experimentation.

There are two kinds of experimentation; experimentation on man in state of health, and in sickness; the first is called pure or direct, the second clinical or indirect.

From the pure experimentation of medicament, we are led to the conclusion that the specific character of medicaments is to produce peculiar and constant morbid actions or artificial diseases of the same order as the natural ones.

Applying this principle to the clinical observations, we infer that an artificial disease produced by medicaments has the property to cause the natural disease, which it most resembles, to disappear—*similia similibus curantur*.

6th. Medicaments do not act by virtue of their chemical and physical, but rather by virtue of their physiological properties on that dynamic force, which we have here above referred to. As a force cannot be weighed and judged by its quantity, so medicaments, in order to produce this dynamic action, must be divided in doses infinitely small, and follow the same laws that rule miasma, virus, and epidemics, because by the extreme division obtained through dilution and trituration, the activity of medicaments increases. Hence the reason of infinitesimal doses.

Herein lies all the creed of Homœopathy, which has seduced so many intelligent minds. The general principles it has agitated in regard to the new way of studying medicaments; the desire of the medical profession to get rid of the old trite doctrines and form a new, more simple and pure materia medica,

were the causes of its growth. To the then empirical practice of bleeding and purging without any fixed basis, a milder therapeutic was substituted, and nature left alone and untrammelled, was enabled to perfect quietly a cure, and thus a prestige of success seemed to have been awarded to the new school.

"Is there any truth in this doctrine?" Yes.

"How much truth is there?"

Let us see, and study it point by point.

1st. "*There is a general law that rules the world, the law of universal harmony.*" Nothing more true than this principle. No man who lifts his head and looks around can fail to perceive the harmony, the beauty, the perfection of nature. He feels it in the smallest particle of the universe, everywhere he finds the greatness, the majesty of the Infinite Being. But the conclusions which the homœopaths draw therefrom, and apply to health, are erroneous. Between the harmony of nature, and that harmony, that order, which constitutes health, there is a great difference. The first is perfect, absolute, the other is relative. Certainly in regard to disease, health is order, it approaches nature more than the disease, which is disorder; but it is not absolute when compared to nature.

How can health be an absolute good, since we have the germs of diseases in ourselves? germs transmitted into and through us from generation to generation. Though unaccountable, these facts are daily observed, health being differently noted out to different individuals; it is a blessing dependent upon and limited by social status, habits, avocations, climates, etc. One man enjoys good health, with his pulse beating from 70 to 75 per minute, as we see in the warmest climates; another is in a high fever, when it beats only 65 or 70. One is born with hereditary tubercles in his lungs, and still he can enjoy good health and live many years, so long as there is no softening down of these tubercles.

2d "*Diseases are alterations of the dynamic force and are accidents independent from nature.*" It may be impossible for men to demonstrate mathematically the existence of this force—it is shrouded in the mystery which envelopes the acts of all life, animal and vegetable—but since we are compelled to have recourse to hypothesis in order to explain the phenomena of physics we must admit it, but it must be well understood. The dynamism of Hahnemann is insufficient to explain the causes of all diseases, and the secrets of nature. According to my opinion the theories of Dumas de Montpellier and Trousseau on this subject approach nearest to truth, by establishing this physiological point of great importance, that is, that every organized being for the maintenance of its existence, possesses two forces often antagonistic to each other in their actions; the force of assimilation, and the force of vital resistance.

The force of assimilation is that property by which the organic bodies appropriate to themselves, and convert into their own

proper substance, those alimentary matters, more suitable to the constitution of each. This may correspond to the "force" of homœopathy.

The force of vital resistance is the other property belonging to the same organic beings, by virtue of which they persist in their existence, the struggle for life until their natural term arrives, offering a vital resistance to the causes of alteration and destruction to which they are exposed. Almost confounded with the force of appropriation in the inferior beings, the force of vital resistance exists in the highest degree in man, and manifests itself by phenomena with which physicians must be acquainted. It is in the knowledge of these two forces that lie the secret of the physiological acts of health, of the constitutional causes of some diseases, of the pathological alterations, and of the therapeutical actions of some medicaments. It is in them that resides the *vis medicatrix naturae*, the *vita superstes in morbis*, through which only diseases are cured.

When at the bed side of our patients we do not find such a vital resistance, our treatment is a failure. See how the dynamism of homœopaths is *distant* from us; they who pretend to create every thing, even nature itself. Nature, they say, has nothing to do in diseases; nature, which is so perfect in health, is useless, powerless in diseases. "That gross nature"—I quote the very words of Hahnemann—"which is unable to approach the lips of a wound, as an intelligent surgeon does, unable to cure by itself, fractures, hernias, etc., is, in many instances, a source of danger; and left alone without the support of active therapeutics, will destroy tissues by suppuration or mortification, etc."—How far this principle is fallacious on account of its absolutism, it is needless to demonstrate. To fix the seat of disease absolutely in the dynamic force, to consider them as mere accidents independent of nature, to neglect the study of vital resistance, is as great an error as to consider health an absolute good. Nothing is absolute in medicine except, perhaps, anatomy and surgery.

3d. "*The causes of diseases are occasional, and accordingly diseases are divided in acute and chronic.*" We agree with homœopaths in the causes which produce acute diseases, but we cannot admit syphilis, psora and sycosis, as the original sources of chronic diseases. We are willing to acknowledge with them that the chronicity of a disease is, in many circumstances, due to constitutional causes, taints, or diatheses; but they must admit with us that the pathological alterations in the tissues affected by disease, though effects in themselves, are often causes of chronicity, and that in the study of these pathological alterations the physician many times finds a great help for his diagnosis and therapeutical indications. The fallacy of homœopathy on this point, lies in withholding from pathological causes the importance which they deserve.

I will not detain you long in proving the absurdity of the general division of diseases in acute and chronic. Nevertheless I

must confess that reading the theory of Hahnemann on psora, I have found many true, interesting, and beautiful ideas worthy of perusal and study, and applicable to practice.

"*Each disease is an individuality, composed of a combination of symptoms, that can be imitated by medicaments.*"

A disease does not consist only in a simple combination of symptoms, but in the general principle which links them together, and which constitutes the cause of its objective manifestations. If it was not so all diseases would resemble each other in their symptoms. The fever, the angina, and the eruption of scarlet fever, are symptoms by which we can suppose the existence of the disease; but these symptoms alone do not constitute the disease; they are only manifestations of that morbid, specific principle which is the whole disease—scarlatina. Several symptoms may fail to appear, sometimes there is only one, and yet the disease exists whole in that single symptom. We have many instances in our every day practice that prove the truth of this assertion. Take an ulcer in the leg; here is a symptom which represents many different diseases, from a simple ulceration produced by external causes to syphilitic, scrofulous, mercurial ulcerations, each manifested with only one symptom, but very different in their principle. In the different types of intermittent fevers, how often we have but one stage, viz.: that of chill, or of profuse perspiration, frequently a single paroxysm of neuralgia, sometimes hemorrhage, and still the disease is there in the miasma which exists even in the apyrexia during the intermittence, and which we must attempt to destroy with the antiperiodics, and not by treating symptoms by symptoms.

4th and 5th. "*Every medicament has the property to produce on man in the state of health peculiar morbific action. This action is constant and cannot fail unless some antidotes destroying or neutralizing the effects of the first be simultaneously administered.*"

This definition of medicaments, though it may be true in regard to poisons, is too absolute. If health was really an absolute good, we could admit the constancy of the specific action of all medicaments tested on man in a state of health; but as health is relative to many circumstances, so the results of pure experimentations must be also relative to the same circumstances, and cannot be so constant as to be reliable in their clinical application. There are, according to Trousseau et Pidoux, in the three orders of nature, many substances that cure several diseases by modifying, exciting, etc., certain physiological acts, without producing any morbid symptoms, when given to a man in a state of health; such is an infusion of peppermint after an indigestion—a few grains of rhubarb in a simple constipation or dyspepsia, Peppermint cures the indigestion; rhubarb cures the constipation and dyspepsia, yet these medicaments taken by man in a state of health cause no morbid action, they cure by virtue of *contraria contrariis.*

We have, according to the same authors, another class of

medicaments which, if administered to man in a state of health, present two kinds of phenomena, primary or physiological, and secondary, or therapeutical, or poisonous. They first modify the physiological functions, and in some cases produce quite a healthful action They excite afterwards one or more morbid actions and even poisonous effects; take for instance, opium, which is sedative in its physiological action, and stimulant in its poisonous effect, procuring a quiet, pleasant sleep in the first, narcotism and stupor in the second. This is the kind of medicaments to which the definition of Hahnemann is more applicable, and which are very useful in the treatment of symptoms. But from their pure experimentations we can only judge but of one class of their action, viz.: that which is healthy and physiological; with regard to the application of the other action, the poisonous one, to the clinical cases, we cannot foresee it from their effects on man in state of health, until therapeutically applied.

There is a third class of medicaments (following the same classification of Trousseau and Pidoux) called specific, which produce on man in the state of health morbid poisonous actions alone; they have no healthy, physiological properties, such as arsenic, mercury, iodine, etc.; and from these effects alone we cannot infer their curative properties in diseases. Who can foresee the antisyphilitic action of mercury from its alterative effects obtained by pure experimentation? Who can foresee the antispasmodic effects of valerian from its smell, or action on healthy man? To appreciate, to know the curative antisyphilitic properties of mercury, the curative antispasmodic properties of valerian, we must apply these agents to syphilis or nervous spasm:

"*Each medicament causes symptoms similar* (according to Hahnemann), *analogous* (according to the modification of modern homœpathy) *to the natural disease to which it corresponds, and cures by virtue of similia similibus.*" It is a generally admitted fact, that some of the physiological symptoms caused by certain medicines are analogous to some symptoms of natural diseases. With quinine we can produce some symptoms of an intermittent fever; with mercury we can produce alterations analogous to many of the various external manifestations of syphilis; and so with belladonna, in regard to scarlatina, etc.; but there is a great difference between those physiological actions of medicaments and the general principle which is the cause of the symptoms of diseases. With quinine you will never produce anything like miasma, nor like that intermittence which is the pathognomic character of an intermittent fever. What resemblance is there between that fever, that peculiar rash, that feeling of dryness in the throat produced by belladonna, in a healthy man, and the morbid specific principle which we call scarlatina? What resemblance is there between scrofula and the effects caused by iodine preparations? What resemblance is there between a mercurial poison and the syphi-

litic virus. Does a poison act in the same way as a virus? I will return to this question elsewhere.

Let us now admit, with homœopaths, that all medicaments administered to man in the state of health cause constantly morbid symptoms, but, when applied to the disease, which they most resemble, is their dynamic action so constant as they say? Does the same medicament given in the same disease in different persons produce the same results? The clinical facts prove every day the contrary; and it is precisely in this difference that resides the art of the physician—that is, to know how far the action of a medicament extends, in order to discriminate what share it had in the cure of disease, and what the *vis medicatrix naturæ* had. How many instances have we of diseases cured, without any application of medicine, by only the force of nature. There is already in Europe (let me quote the following passage from the Materia Medica of Trousseau and Pidoux) "a large city, where homœopathy having reigned almost exclusively for several years, and being totally abandoned, the practical medicine has taken another face. In the hospitals of Vienna, acute diseases, left to themselves, are rather guided in their march than actively treated."

6th. "*The actions of medicaments are due to their physiological effects on the dynamic force, not to their physical and chemical properties.*" In this principle there is a great truth, which has changed the whole face of therapeutics, and has compelled physicians to study and seek the causes of diseases and medicaments in the living body itself, and not outside of it, as they were wont to do before with the old iatro-mechanico-chemical doctrine—"*Medicaments must be divided in small doses, in order to prove more active, and become a pure and spiritual force able to place itself in contact with the immaterial and spiritual principle of the disease, and destroy it.*"

Although our understanding fails to account for the increased activity which medicines are supposed to acquire in their extreme division by means of trituration, dilution, or succession, nevertheless we cannot deny the extreme divisibility of matter, and must acknowledge the fact that many of them (the poisons) penetrate the economy in as small quantities as virus. But when absorbed, and brought into the current of the circulation, do poisons really act as virus? No; between a virus and a medicament, between a virulent disease and poisoning, there is a very important difference. Poisoning is only an accident, which has its cause outside of the economy, and its effects are almost constant; while viruses have their causes in ourselves, and when they take their development, it is because they find congenial principles which act as seeds, assimilating them to the whole substance of the organism, multiplying infinitely, and becoming hereditary and contagious; but where they do not find these congenial principles, or germs, they have no effect, or very little. This explains perfectly well the reason why many persons, even

of delicate constitution, pass through epidemics of all kinds, without ever perceiving their influence. Poisons do not act in this way; they are never epidemic, contagious, or hereditary.- So, let us conclude, that the reason of the infinitesimal division of poisons is not the same as the reason of the infinitesimal division of viruses; then the artificial diseases produced by poisons cannot be similar to virus, miasms, and epidemics. The experiments of Spallanzani and Fontana do not prove anything in favor of *similia similibus.*

"*What good has the system of homœopathy accomplished in Medicine?*"

"*What good has it accomplished in pharmacology?*"

In creating his new Materia Medica, based upon the principle of *similia similibus*, Hahnemann had endeavored to explain and utilize an old idea, which had been already foreseen but neglected by Hippocrates, Demetrius, Paracelsus, and Stahl. His doctrine, considered in this general view, does not deserve certainly any ridicule, because he wanted to apply to general diseases and general remedies the theory of the fact, observed every day in practice, of the treatment of certain local inflammation by direct toxic application of irritants, which cure by causing another therapeutical inflammation in the stead of the primitive one. And, if in his intended explanation of this law, he has overreached the mark—if its therapeutical interpretations by homœopaths did not correspond entirely to the greatness of the principle—yet we cannot deny the good that this system has accomplished in medicine and pharmacology, by calling the attention of physicians to pure experimentation, and resting the art of curing upon this therapeutical truth, that is, that a medicament, in order to be specific, must act upon the very seat of the disease. A real fever of experimentation was thus excited among physicians, and all medicaments were immediately tried, more thoroughly studied, and their applications more rationally made. The question of the infinitesimal doses led professional men to reflect upon and correct many errors. Since then chemists gave more care to medicinal preparations. From that long series of experimentation of over three-quarters of a century (in which homœopathy had a great share, and for which they deserve the praise of all impartial men)—from that long experimentation this other therapeutical fact was brought to light, that medicaments produce in the organism two kinds of symptoms, primary and secondary, usually contrasting with one another. Such a medicament that produces purging and diarrhea, leaves afterwards an obstinate constipation. Another, coffee, e. g., which causes insomnia, is afterwards followed by an uncontrollable feeling of sleepiness. It is upon this contrast that homœopaths established, with their different doses and dilutions, some therapeutical applications which, though in many instances may be dreams of their imaginations, in some others were crowned by the most undisputable results. How do they pretend to cure

some dysenteries?—with colloquintida. I have myself seen successfully treated by an allopath, several diarrheas of children with tincture of sulphur, and yet colloquintida and sulphur are purgative. These are facts, which are waiting their explanations from farther studies, and more quiet and impartial observations. In the meantime, we must grant to homœopathy the merit of having evoked such issues, the decision of which will eventually confer benefit upon medicine.

Second Part.

"What would be the death-blow to homœopathy? Which is the best policy for us to follow with the general public, in order to counterbalance the manœuvres of homœopathy? Is there any instance of a thoroughly educated physician having tried homœopathy, or adopted it at the outset of his career? In our friendly social intercourse with homœopaths, what would be the best policy to adopt?"

Gentlemen: Against doctrines, against creeds, there cannot be any death-blow; if they are wrong, they will fall by themselves—they will find in their own fallacy the cause of their death. But if they are right, if they are true, they will stand like that theory of the movement of the earth, and they will pass to posterity in spite of opposition and intolerance. The lessons that are true in religion and politics, must also be true in science. The death-blow of homœopathy will come from the opposition of doctrine alone to doctrine—of facts alone to facts: it is the only way by which we may come to an understanding, and to a better feeling between one another. If their doctrine cannot stand the logic of our criticism, they claim facts—they claim success; ours is the burden—the duty of refuting these by practical demonstration. Let us join and work with them; I mean with those who are really physicians, and thoroughly educated; and there are some honest and well-meaning gentlemen in their ranks. Every one must have some friends, some school-mates, who have adopted, *bona fide*, homœopathy at the outset of their career, and are working for the same purpose as we do—the welfare of humanity. I could speak of many of them, but I will limit myself to name a well-known German in America, Dr. Constantine Hering, of Philadelphia, whose fine works and experiments, on the poison of the Trioecephalus Lachesis of the West Indies, gave him a world-renowned and well-deserved fame.

Let us join with these gentlemen, and study fairly the same facts in the same clinical rooms—there, at the bedside of the patient, where all preconceived ideas must fall before the reality of a man who is sick, and wants to be cured: he little cares whether we cure him by virtue of *similia similibus* or *contraria contrariis*—all that he asks for is health, and that in the readiest and surest way possible.

Homœopathy has proclaimed, that medicaments must be given

in the greatest state of purity. Let us put in practice this principle, without adopting their ridiculous doses. Let us reduce our prescriptions to simpler formulas. When, according to our diagnosis, we come to the indication of an active medicament, e. g., strychnine, let us give it alone; we will be more able to judge of its effects, and to control them, than when combined with other substances. Let us imitate them in their rigorous dietetic system. Let us be as strict as they are with our druggists, in the prepartions of active medicaments—some of their mother-tinctures, as they call them, are very good and strong.

We laugh at their minute doses; do we not sometimes go too far with ours? Did we ever obtain, for example, in our practice, poisonous effects by laudanum in *cholera infantum* in very small children? I believe I did once; and since that time I am very careful in giving opiates to children. We say that we must imitate, that we must help nature in the treatment of diseases; but sometimes we over reach, and fall on the extreme opposite of homœopaths—and our extremes are more dangerous than theirs! Many times we want to please our clients too much—especially ladies. We make use too often, and for trifles, of our hypodermical injections of morphine, and we do wrong, because we accustom to the use of the drug; but when the time comes that they are really sick, and there is a true indication of it, then the usual doses are insufficient—we are compelled to increase them, and the consequences are less than satisfactory!

Let us then work together, and take out all that is wrong in both systems, and retain only what is good; it will finally turn to the advantage of everybody, of ourselves, of the profession, of the public. Let us be united, and instead of wrangling with one another, let us prepare to fight together the ignorance, and prejudices, from whatever class of society they may arise, and counterbalance the manœuvres of all charlatans and quacks, no matter to what school they may belong, and expel them from our sanctuary. The man who would put himself at the head of such a movement of conciliation, will have deserved well of science and humanity.

Questions in relation to Gynæcological Subjects, with Answers. A paper read before the New Orleans Medical and Surgical Association, by Wm. P. Brewer, M.D.

The questions to be mentioned in this paper having been presented by the committee as subjects for discussion this evening, and myself appointed to lead off, I will do what I can to answer them. I must say in starting, however, that they are open questions, and are now being discussed *pro and con* by those shining lights of both hemispheres who have made a special study of such subjects—and but little of real value from the experience or original

thought can be expected from the general practitioner like myself. But, of my limited stock of knowledge I will give freely, hoping to be enlightened by the deeper learning and more extended observations of my worthy fellows of this Association. I will further state that the subject matter embraces too extensive a field for anything like a satisfactory article—in condensed form even—in the limited time we have for such discussions. I would advise, that the whole matter be referred back to the committee for a division under the different heads, so as to make it a subject matter for discussion at several meetings. The interrogatories, in some respects, are somewhat vague, and leave much to imagination to determine the exact object to be arrived at in this discussion. We will now take up these questions *seriatim*, and discuss them.

On Menstruation.—1st. What are the best means of bringing on the retarded menses, when the possibility of pregnancy is out of the question?

2d. Is the period of the menopause really a critical period?

We must not take it for granted that the mere circumstance of a girl having passed the usual period when menstruation commences in a majority of cases, is proof of retarded menstruation, which calls for interference. The date of puberty varies greatly, some having the menstrual flow earlier, and others later in life, as we find some children teething earlier, and others later. As a rule when we find an absence of menstruation in an apparently healthy girl we will usually find an absence of some of the other signs of puberty, and it would be advisable to let well enough alone for the present, and watch further developments. There are some cases on record where without any apparent menstruation impregnation took place. These cases are, however, very rare. Sometimes, though rarely, absence of menstruation is owing to some defect which may be relieved by surgical interference, but, more frequently all interference is useless, as the condition depends on either absence or defective formation of the uterus or ovaries. Frequently this condition (retarded menstruation) follows the various diseases incident to parturition. These are cases of retarded menstruation in which the physician can do good, and to them I will now draw your attention. In them we find two classes of symptoms which are apparently very different in general character, but approach each other in their essential causes. In the one, there is a condition of apparent plethora, and in the other of anæmia, with the tendency of the first to pass into the second—with the change sometimes rapid. Thus we see a fine, healthy girl to all appearance, developing into a woman—the rounded form—the fullness of bust, etc., but one thing is lacking to complete the change, and that is the important one of menstruation. In place of which we find her begin suffering from headache, flushed face, back-ache, hypogastric pain, constipated bowels, furred tongue, and other symptoms of grave constitutional disorder, increased at regular

monthly intervals. After a few returns of these symptoms scant menstruation accompanied with pain, commences; to be absent again for several periods, or we find hemorrhage occurring from various mucous membranes. For the first few months there may be no serious disturbance of health, but after an interval, shorter or longer, we find a general letting down into a chlorotic condition to take place. In the other cases, or those of anæmia, the general health, which is at no time good, becomes poorer and poorer as the period of puberty approaches, while all outward signs of this period are developed. Now in both of these cases we ask ourselves what are we to do to improve the general tone, and give strength to the body to successfully bring on that change of the female economy demanding a discharge of a certain amount of blood from the body, which takes place at puberty, and lasts for a period of about 25 or 30 years. As the symptoms accompanying retarded menstruation show that the equilibrium of circulation has been disturbed, and our endeavors should tend to a readjustment, therefore, our efforts to the establishment of menstruation by remedies must be directed to the improvement of the general health, rather than those which act immediately on the sexual system. Accepting the fact of the necessity of building up the system when there is general debility of it, we are called upon to use tonics in the widest acceptation of the term. Not alone tonic medicines, including the various preparations of iron, but in addition, good healthy exercise, pure, bracing air, pleasant company, early hours for retiring and rising, morning trips up mountain sides, or the more homely but healthy occupations—use of the dusting brush and broom, milking cows, morning rambles, enjoying the sweets of beautiful nature, rides in an open carriage, baths, etc. All, however, short of fatigue. In constipation of bowels give some mild aperient, as for example, watery ext. aloes. If preparations of iron increase the sluggishness of the bowels add some aperient salt. I have used with benefit the following, as advised by Dr. West: ℞ ferri. sulph. gr. ix; magnes. sulph. ʒiij; acid sulph. dilut. ʒss; syr. auranti. ʒiv; aquæ carui ℥vj. ℳ s. ℥j. ter. die.

If the patient can afford the expense, I would recommend a trip to the Chalybeate Mineral Springs, Va. Should there be apparent plethora, the same treatment, dispensing with the iron preparations and pursuing a more active purgation would be advisable, together with shower bath, and horseback riding. Should there be at any time an attempt at menstruation we must at once employ those remedies which have a direct action on the generative apparatus, viz: rest in bed, electricity, hot hip baths, with or without mustard, according to amount of pain; stimulating diuretics, etc., all to be used with due amount of caution. When pain is very severe leeches or cups applied to hypogastrium sometimes relieve the pain, and bring on the flow. Should there be much congestion of uterus cupping the organ itself is sometimes beneficial. As for the treatment to be used where retarded

menses occur from some disease following parturition, all I have to say is, treat the cause of retardation, and not symptoms.

In answer to the second question under this head, (on menstruation), I will say the period of the menopause may or may not be a critical period in a woman's life—the danger being dependent entirely upon the general condition of the patient; and whether the cessation of the menses has been sudden, producing grave constitutional disturbance, or whether it has been gradual without such disturbance. According to Dr. Cohnstein, in the Deutsche Klinick, 1873, No. V, we have—

1st. The mean duration of menstruation is 31 years.

2d. In 76.0 the menopause is gradually established (from one month to two years); in 24.0 it comes suddenly, principally when the sanitary conditions are unfavorable.

Those who have had the first child towards the age of 36 to 46 years, generally cease to be regular after a normal evolution of puerpural phenomena.

As regards influences which hasten the menopause, the author cites, "tardy appearance of courses, celibacy, rarity of deliveries, abstention from suckling. Thus the greatest duration of the courses occurs in women who menstruate early, who bear more than three children, suckle them themselves, and are delivered again between the 38th and 42d years."

At the menopause, we will frequently find diseases develope themselves of which we have had no suspicion, or we may have to deal with a profuse uterine hemorrhage, (not menstrual), endangering life. Also, at this period, uterine cancer is apt to be developed in those with a cancerous tendency. In practice, I have seen three cases of uterine cancer develope at this period, and pursue a rapid course.

On the Uterus.—1st. Is the empty uterus contractile?

2d. If so, what are the best means to make it contract?

3d. If not, when does it begin?

4th. What are the best means to bring into play that contractility?

By empty uterus, I take it for granted is meant one in which there is an absence of the product of conception, and of any uterine tumour. In answer to this question I am of the opinion it is contractile

How frequently in menorrhagia do we find clots being expelled from the uterus. Now, without contraction how would it be possible for their expulsion to occur? Also, in cervical endometritis, in abrasion of the os and cervix; and in other diseases, what caused the sensation of bearing down pains so often complained of, if it is not contraction? According to some authorities, in every copulation and sexual excitement, the neck of the uterus becomes erectile, and throws out a fluid, after which, the organ becomes flaccid with lips soft, and this act answers to the erection of the penis. "Some doubt still exists, however, as to the precise source of nervous supply. All agree that the chief

supply is from the sympathetic system—the hypogastric, renal, an inferior aortic plexuses being all believed to contribute. An idea generally entertained is, that the sacral nerves send some filaments to the cervix, but this has been denied by Dr. Snow Beck, who failed in his dissections to discover any single filament proceeding from this source."—Leishman. Owing to the intimate nervous connection between the uterus and other pelvic organs what will irritate one, will, (if irritation is kept up for a sufficient length of time,) by reflex action produce uterine contraction. Irritation of the mucous surface of the organ, of the os, or cervix, and irritation of vagina if long continued will produce uterine contractions—also, irritation of mucous membrane of other parts in nervous connection, or *rapporte* with uterus will produce contractions, as for example fissure of anus. Those remedies which act by irritation of the nervous system of this organ will, if administered sufficiently long or in large doses produce contractions, as for instance, electricity and the application of cold, etc. This action in the empty uterus is not so well marked as in the gravid one, because the degree of sensibility is greater in the latter than in the former, and it is for this reason also, that many remedies which act on the gravid uterus have no apparent effect on the empty organ. I will now refer you to the following translation of the result of the experiments of Prof. Cyon on innervation of this organ: "Prof. Cyon, (Pflügerer's Archiv., Band viii, Heft 6 and 7, 1873,) communicates the results of experiments made on animals on this point. Dogs and rabbits of different ages were used; some were put under the influence of curare before experimenting, others not. The following are the chief results arrived at: 1. The uterine plexus is the most important, if not the only, motor nerve, which can produce effectual movements of the uterus by the irritation of its peripheral ends. (Irritation of the central ends only gives rise to severe vomiting). 2. Irritation of the central ends of the *first* two sacral nerves produces in a reflex way powerful uterine movements, which cease after the uterine plexus has been cut through. (Irritation of the peripheral nerves gives rise to powerful contractions of the bladder and the rectum). 3. Irritation of the brachial, crural, median, sciatic nerve, etc., gives rise to no peristaltic movements of the uterus, but only causes a slight rigidity and paleness. 4. The effect of the irritation of these nerves disappears if the aorta has been previously compressed; but irritation of the central ends of the sacral nerves still causes, even after the closure of the aorta, peristaltic movements of the uterus. 5. Suffocation through continued interruption of respiration causes powerful peristaltic movements, probably through direct excitation of the involuntary muscular fibres by the accumulated carbonic acid gas.—*The Obstetrical Journal of Great Britain and Ireland*, No. xiii, April, 1874, p. 66."

On Impregnation.—1st. In cases where pregnancy would be dangerous to a patient, and the physician prohibits copulation,

is he justified in informing his patient of the means of preventing impregnation?

2d. If so, which are the best preventive means?

3d. Is there a period in the months when impregnation is more likely to take place?

4th. Is there any position during copulation that favors impregnation?

In answer to the first interrogatory, I would say that it is a question for each individual practitioner to decide, as there is both a moral, and religious theorem involved applicable to each of us. For my own part, I would, if such a condition existed, advise a separation of beds, (which is giving information of one of the preventive means of pregnancy), and if I found the female pregnant, and knew she could not with safety proceed to full term would unhesitatingly produce abortion, repeat advice, and if not minded, would ask her to seek the advice of some one else. With the various appliances within the reach and knowledge of the laity to prevent the ejection of male semen into the vagina, I think any information on the subject unnecessary. If I should, however, take the other view of the question, I would provide those appliances, and order the female to thoroughly wash her vagina out with cold water, and would further order an acid wash to be used immediately after, with a view to the destruction of the vitality of any remaining semen. Though, if the theory advanced by Beck as to the manner in which spermatozoa reach the ovum is correct, (and his argument is plausible), no means other than preventing the semen coming in contact with the mouth of the uterus will be of any avail, and that can be done only by forming a barrier to the entrance of semen at the time of copulation.

3d. In answer to the third interrogatory I will say, impregnation is supposed to take place most frequent ly within seven days following after the menses have ceased to flow.

4th. Is there any position during copula ion that favors impregnation?

If we take the relation of the uterus to the vagina in a woman lying on her back, we find at the point of junctnre an obtuse angle formed, with the angle looking upward and with the mouth of the uterus looking downwards, and into the angle or cup. Now, after the ejection of semen from the male organ the mouth of the uterus dips as it were into a basin filled with seminal fluid, and whether the spermatozoa is sucked up, as advanced by some, or whether it flaps itself up into the uterus, as advanced by others, we find the mouth of uterus occupying a position favorable to either performance. A vaginal examination with speculum has been made immediately after copulation, and the mouth and neck of uterus were found as stated above, bathed in, and completely surrounded by seminal fluid. My authority is a medical gentleman of high and acknowledged attainments, and who made the examination immediately after copulation to satisfy

himself on this point. Such being the case, we perceive that the horizontal back position, while being the most natural position, is about as good as any.

On Pregnancy.—1st. What is the best method of reckoning the time of pregnancy?

The following method of reckoning the time of pregnancy is quite accurate, and answers all practical purposes: Supposing impregnation to take place within the seven days following the last day of menstruation, and the period of pregnancy to be 280 days, we reckon from last day of menstruation flow, nine months forwards, or, more simply, three months backwards, and add to the date seven. As for example: if the last day of menstruation was September 1st, nine months forward would bring us to June 1st, of the following year, or 273 days + 7 days = 280 days, or shorter, by counting three months back from September 1st, gives June 1st, to which add 7=June 8th. In this calculation we do not allow for the liability of impregnation a few days before menstruation, or in the interim. To make the calculation more certain we can correct it by comparing with quickening. This usually takes place about the 17th or 18th week.

2d. Are the beatings of the fœtal heart so very constantly audible that we can infer pretty safely from their absence or weakness, when looked for, for the first time, that the child is dead or in danger?

No, for frequently we are unable to detect the beating of the fœtal heart though the child is living, and will be carried to full term; and then be delivered living. In conversation with those who have paid particular attention to this subject, they stated that they have, at times, failed to detect the beating of the fœtal heart, though, afterwards, a living child was born. And frequently the mother can feel the fœtal movements, though the pulsations of the heart are inaudible. Gentlemen, I have individually paid so little attention to this subject that I am unable to give an opinion from personal observation.

3d. Can we determine the sex of the child by the beating of the fœtal heart?

As an answer to this question I refer you to the following result of 100 observations by Drs. Strong and Steele, made with a view of determining the sex in utero; who, after a rigid analysis, thus formulized their statistics: (1) In the majority of cases male fœtal hearts are slower than female. (2) 132 fœtal pulsations per minute is the average which constitutes a dividing line between the sex. Below this, sixty-eight and forty-sevenths per cent. are males, twenty per cent. are females, eleven and three sevenths per cent. are doubtful. Above this, fifty-three and one-third per cent. females, twenty six and two-thirds per cent. are males, twenty per cent. doubtful. (3) The most accurate observations are made during the last four weeks

of gestation. (4) The rapidity of the heart's action is increased in proportion to the feebleness of the fœtus. (5) Calcareous or fatty degenerations of the placenta render the pulsations feeble and irregular. (6) In some cases it would be possible to diagnose diseased conditions of the placenta from careful observations of the fœtal heart.

Of fifty cases examined consecutively, twenty-seven gave birth to female children and twenty-three to males. The lowest rate observed was 118; it occurred but twice; once each in male and female child. The highest rate noted was 180, occurring three times, twice in males, and once in the case of a female.

The average rate of the male pulse was 136 3; of the female, 137; of both sexes, 136.7. Considering the latter as the dividing line between the both sexes, a pulse at and below this rate may be referred to males, and above it to females. In twenty-six cases the sex was correctly predicted, and in twenty-four, an error was made. If the cases be excluded where there was unusual activity of the fœtus, the average rate of the male pulse will be found to be 133.6; of the female, 136.2; of both sexes, 134.7.—*The Medical Examiner*, August 15th, 1874.

4th. Is there any means of determining the sex in utero?

By the pulsation of the fœtal heart, (not always certain) and by examining the genitals with hand introduced into the uterus after the rupture of the bag of waters.

5th. Is there any means by which the sexes can be produced at will?

A series of observations were made by M. Thury, a French veterinary surgeon, with a view to discover, if possible, whether, and in what manner the sexes could be produced at will in animals. The result arrived at was: that when the male had connection with the female in the beginning of heat in the female, the offspring were females; and when had towards the termination of heat the result was a male. These results were published; and others who were engaged in raising animals pursued the same course of observations and, verified by the results, obtained the truthfulness of the theory advanced by M. Thury, from his observations. From observations made by medical men with regard to the developement of sexes in the human subject, the result arrived at was that in a conception taking place at an early period after the menstrual flow had ceased, the product will be a female; and the farther removed from that period (always omitting four or five days anterior to the following monthy illness), the more likely is it that the child will be a male." We generally find that where there is a difference of fifteen years, more or less, between the ages of parents, with the father the elder of the two, the children are usually males, and this is accounted for on the ground that the father has not the same amount of desire as a younger man; some time usually elapses after the flow before copulation takes place.

A Case of Double Gonorrheal Ophthalmia which Presented Peculiar Features. By Dr. V. Grima.

Gentlemen.—I wish to mention a case of double gonorrheal ophthalmia that has presented peculiar features. The case is that of a man who called at my office, stating that both of his eyes had become red and swollen, and were running matter profusely since the preceding evening; furthermore, said that he was suffering from acute gonorrhea, and felt assured that *contamination* was the cause of the eye trouble. He was very much alarmed and requested to be sent to my hospital wards, stating that he had no one at home to assist him in attending to his own case. I ordered him to the Eye-Wards immediately, with strict recommendations not to allow any one around him but a well trained nurse, for fear of further contamination in the ward. The only prescription was that he should dip his face in a basin of water as often as he could, every fifteen minutes, if feasible, and wink his eyes in the water so as to thoroughly wash the matter out. During the intervals I directed him to keep constantly a wet rag over both eyes. The prescription was followed for three days and nights, the case improving rapidly in the mean while. During that lapse of time I touched the lids twice with nitrate of silver (10 grs. to oz.), everting the lids thoroughly and washing away the medicine immediately. I then had the patient to stop the trying experiment and rest; he went on improving gradually for about a week, when one morning I found, at my visit, that his eyes looked entirely well; not the slightest secretion was to be seen; the gonorrhea had also stopped. He then called my attention to another form of trouble; both of his knees were swollen and painful, so were his elbow joints and wrists. I prescribed iodide of potash internally and iodine paint locally. The trouble lasted about ten or twelve days; the knee joint being largely swollen by intrà-articular fluid. He then gradually, and rather rapidly, got well, but again complained of his eyes, which had become very painful; on examination I found that he was suffering from rheumatic iritis. The patient finally recovered, the whole trouble having lasted about six weeks.

As a great deal has been said by every body concerning gonorrhea, rheumatism, and *metastasis*, I thought I would bring this case before the Association. I shall abstain from any comment for the mere reason that any one can see the peculiar features of this case.

NOTICES OF NEW BOOKS

Transactions of the American Ophthalmological Society—Tenth Annual Meeting.

I. Under the heading, "*Astigmatism Produced by Tenotomy of*

Recti Muscles," Dr. Henry D. Noyes, of New York, gives the history of an interesting case (moderate myopia, with slight astigmatism in each eye, accompanied with diplopia), the various features of which, more especially the result of surgical interference (repeated tenotomies), will prove instructive to all who are called to deal practically with similar defects. We have to refer the reader to the original paper for the details of this case, which is an illustration of the influence of tenotomy on the muscular condition of the eye, and upon its refraction—a muscular condition in which there was, at the first, defective abduction for distance of 8°, became converted into defective adduction by repeated tenotomies; astigmatism was increased in the eye so much operated upon. The author calls attention to the common association between myopic astigmatism and muscular insufficiency, and asks if we ought not to set down a certain number of cases of astigmatism as the result of defective muscular equilibrium.

II. Dr. Wm. Thomson, of Philadelphia, reports a few cases of *Conical Cornea corrected by glasses.* "These cases," says the author, "might more accurately be classified under the head of acquired astigmatism, since their histories demonstrate that the optical defects were induced in eyes that were probably emmetropic, by prolonged strain of the eyes in reading with insufficient illumination, when the individuals were rendered vulnerable by causes which produce a general debility of their systems." The optical features of these cases are interesting, and the influence of glasses suitably selected proved to be very satisfactory.

III. On the Atropine Treatment of Acquired and Progressive Myopia. The above title is that of an able but short contribution, read before the American Ophthalmological Society, at Newport, July, 1874, by Hasket Derby, M.D., surgeon to the Massachusetts Charitable Eye and Ear Infirmary, Boston—tables of 36 cases accompany the text.

"The recent therapeutics of progressive and of acquired myopia," says the author, "have received but moderate attention from English writers on ophthalmology. Accepting apparently the dictum of Donders, that the cure of this error of refraction belongs to the class of things, 'devoutly to be wished for,' with unquestioning faith, they have contented themselves with discussing the division of the external recti in cases where weak-

ness of the interni is found, and in going into the general question of prophylactics."

It is beyond doubt that tenotomy of the rectus externus muscle has done good service in the cases referred to; but we must confess that the late investigations into the etiology, process and cause of myopia, have very materially altered previous notions concerning the nature of the *disease*, and its treatment both preventive and curative.

After briefly mentioning the labors of Dobrowolsky and Schiess-Gemmsens, and pointing out to the main features of the subject, the author gives tables of interesting and instructive cases, and concludes his memoir as follows: "The whole subject in fine needs and deserves further investigation, and the collection of much more numerous statistics. As far as our present experience reaches, the following conclusions seem to me, as to others, justifiable."

"That the emmetropic eye, through undue or disadvantageous use, acquires myopia much more frequently than has previously been supposed, and that such acquired myopia is very apt to be progressive, commencing with spasm of accommodation and going on through a state of congestion and irritation, to the structual change characteristic of this error of refraction."

"That the paralysis of the accommodation by means of atropine, persisted in during a period of several weeks, and further, by rest of the eyes, shaded glasses, and, in extremely aggravated cases by local blood-letting, offers a reasonable prospect of preventing the progress of the myopia, in some instances of lessening its amount, and in occasional, recent, and slight cases, of removing it altogether.

"That in cases of progressive myopia it is reasonable to expect positive results from an annual course of treatment similar to the above, carried on during those years which experience has shown to be most favorable to the advance of the myopia, thus enabling the patient to tide over a critical period. It is, perhaps, in its prophylactic action that the most importance is to be attributed to the atropine treatment."

We take it for granted that there are two factors of myopia; two efficient causes of short sightedness: elongation of the antero-posterior axis of the eyeball, and spasm of the ciliary muscle. It follows that when a patient gives evidence of short-sightedness the amount of optical error due to elongated axis may be

separated from that which is due to a condition of spasm of accommodation. In some cases the myopia is produced exclusively by one of the two factors, as for instance: when the refractive condition in an apparently short-sighted eye proves to be emmetropic after paralysis of accommodation is obtained through the influence of atropia; the myopia in such a case is exclusively the result of a spasmodic condition of the ciliary muscle. If, on the other hand, the refraction of the eye should remain unchanged in spite of the action of atropia, we have to admit that elongation of the axis is exclusively the factor of the myopia. In other cases both factors are active to a more or less extent in bringing about the result (short-sightedness) which is due partly to elongation of the axis and partly to spasm of the ciliary muscle. We can easily conceive the benefit to be derived from the use of atropia in cases of *acquired* short-sightedness inherent upon a condition of spasm of the ciliary muscle, which condition can be relieved by paralysis of said muscle through the action of the drug; and, what is more yet, the well known *progressive* tendency of the *disease* may be checked by the removal of the original cause; the condition of spasm which, if allowed to remain *active*, will inevitably bring about the well-known material changes of sclero-choroïditis. In order to avoid a progressive and gradually aggravating trouble, the exact condition should be detected and treatment instituted at the earliest possible stage, previous to any material and permanent changes having occurred in the fundus oculi.

According to Schiess-Gemusens, whose ideas we shall quote from the *Annales d'Oculistique*, 1874, the first stage of myopia may be detected by the following symptoms: Vision first becomes indistinct at *long distance*, the symptoms being often more marked in *one* of the eyes; it follows that the trouble is overlooked as long as the other eye remains sound. In a good many cases, however, the following subjective symptoms occur: a certain degree of photophobia, a disagreeable feeling in the eye, and even pain when the eyes are overstrained by work. Objective symptoms are, spasm of ciliary muscle, slight congestion of the large anterior ciliary vessels; the least work promotes lacrymation, with or without accompanying catarrh of palpebral conjunctiva; frequent winking of the lids also exists sometimes to a very marked degree.

In the incipient stage of myopia, says Schiess-Gemusens, the

fundus oculi is *constantly* altered so as to present a really striking aspect. As a rule, a red and cloudy appearance of the fundus first shows itself to the nasal side of the optic papilla, the redness being in some cases so well marked as to sharply divide the inner from the outer half of the optic disk. After awhile, the whole extent of the papilla may become cloudy, and furthermore the inflammatory process be extended to the surrounding region, the outlines of the disk then disappearing. In some instances, the blood-vessels may be seen partially to disappear as if imbedded in the neighboring retinal tissue, as it is seen to occur in cases of neuro-retinitis. As a rule, in the early stage of the disease, no pigmentary changes obtain around the papilla on the side of the macula. The enlargement of the veins of the retina and the exaggerated curves of the arteries also constitute a constant symptom of incipient myopia. As the disease progresses, the well known changes occur in the posterior hemisphere (sclero-choroïditis).

In the case of hypermetropic individuals working without the assistance of glasses, the author accounts for the above changes occurring through the excessive efforts of the ciliary muscle, and the consequent tension of the choröïd and retina.

IV. Dr. William F. Norris, of Philadelphia, reports two cases of optic neuritis—one from cerebellar sarcoma, the other from lead-poisoning. These cases, though not calculated to throw any *new* light on the subject, are confirmative of the practical bearing of recent investigations into the intra-cranial and perioptic lymphatic spaces.

"Tumors of the cerebellum and *posterior portions* of the hemispheres of the cerebrum appear more apt," say the author (the italics are ours), "to produce choked disk than those of the anterior lobes. The cranium is a closed cavity, and any increase of its contents, if they were all fluid, would press equally in all directions. But inasmuch as the brain is a soft solid, this law does not hold good, and tumors of the cerebellum, bound down by the tentorium, by their direct pressure on the fourth ventricle cut off all communication between the lateral and third ventricles of the brain and the subarachnoid space of the spinal cord, and thus dam up fluid in the former. Moreover, by *compressing the lateral sinuses of the dura mater, they readily cause leakage of the watery parts of the blood, and are thus specially prone to increase the subarachnoid fluid.*" We have marked this last phrase of the

author with italics, in order to call attention to some obscure cases in which patients having in all probability died from brain trouble, post mortem examination only shows considerable engorgement of the sinuses of the dura mater and large increase in the fluid contents of the ventricles, the symptoms during life being as follows: stupor, with accompanying paralysis of one or more of the cranial nerves, more especially of the motor oculi and abducens nerves (which run into the walls of the cavernous sinus), together with an *excessive* dilatation of the pupil, which is expanded to a much greater extent than is usually found in non-complicated cases of paralysis of the motor oculi nerve, and is suggestive of irritation of the sympathetic filaments given off by the carotid plexus; also congestion of the large veins of the retina, which are black and tortuous, no structural changes obtaining in the optic disc or surrounding portions of the retina. In such cases, the absence of the material signs of optic neuritis should point out to a condition of mere congestion of the sinuses and dropsy of the ventricles likely to have occurred under the influence of some general cause.

V. Syphilitic Gumma in the Ciliary Body. By Edw. G. Loring, jr., and H. C. Heno, New York. This is an interesting observation of a disease which is usually considered as one of very rare occurrence. The small number of cases that have been recorded (three cases, says Loring) go to show that the trouble is very seldom to be met with in the countries where experienced specialists are to be found within easy reach of each case; it will be observed that such countries are peopled most exclusively by white inhabitants. Now, on examining how things stand in countries where both white and black people are to be found in equal numbers, it is ascertained, as far at least as my experience goes, that gummy productions *in the iris* are equally frequent in both races, but the ratio is entirely different as concerns the *ciliary region* of the eyeball and corresponding episcleral tissue; white people will show syphilitic gummata in the iris, but very seldom in the ciliary region (ciliary muscle, ciliary processes and episcleral connective tissue of ciliary region).

The clinical features are not always the same in all cases where specific gummy productions appear in the ciliary body; there seem to exist different forms of the disease. It is not generally known that in many instances it appears to be connected with a peculiar form of pustular *syphilitic conjunctivitis.* I have once

before called attention to the association of these two clinical features of gummy productions in the eye. An attempt at a description of the trouble may be read in the *New Orleans Medical and Surgical Journal*, Vol. 1, No. 1, Art. V. (*Contribution to the History of Ocular Syphilis*, by Dr. V. Grima). I have recently observed a case in a *white* woman, in which the gummata originating in the ciliary portions of the *iris* were accompanied with the peculiar pustular conjunctivitis above mentioned.

VI. Charles S. Bull, M.D., New York, reports *two cases of interesting syphilitic lesion of the eye.* The first case is described as an external growth involving the sub-conjunctival and episcleral fascia and sclera, accompanied by an intra-ocular growth. It is evidently a case of specific gummy production similar to to those I have alluded to in the above paragraph. From the position of the two growths in this case (the extra-scleral being just over the intra-ocular growth), "it seems not improbable," says the author, "that the two are directly connected through the sclera." That such is the condition is more than probable, according to my personal observation of like cases. I have seen an opening in the two growths communicating freely through the sclerotica.

The second case mentioned by Dr. Bull is one of choroïditis disseminata, in which the peculiar arrangement of the pigmentary deposits in the retina and choroid showed unusual ophthalmoscopic appearances.

VII. On Color-tests for Ametropia, based upon the Chromatic Aberration of the Eye, by John Green, M.D., St. Louis, Missouri. The author takes as his starting point the following experiment of Wollaston, who "looks through a prism at a small lucid point, which, of course, becomes a linear spectrum. But the eye cannot so adapt itself as to make the whole spectrum appear a line; for, if the focus be adapted to collect the red rays to a point, the blue will be too much refracted, and expand into a surface; and the reverse will happen if the eye be adapted to the blue rays; so that, in either case, the line will be seen as a triangular space."

Dr. Green finds that the emmetropes and myopes describe the spectrum of the lucid point as triangular, with the apex red; the hypermetropes see it with the apex violet.

"In measuring myopia by this method," says the author, "I have succeeded best by directing the attention to the red apex of the spectrum, the weakest concave glass, through which the apex

appears as a sharp red point, being the measure of the myopia for red rays. Adding to this the correction for the brightest part of the spectrum, viz: the yellow, a correction which I have found to be about $\frac{1}{14}$ we have the measure of the actual myopia in the meridian corresponding to the direction of the refracting edge of the prism. Turning the edge of the prism then through any angle, in a plane perpendicular to the axis of vision, and again measuring the ametropia in the new meridian, we detect any difference in refraction, and by thus examining one meridian after another we may work out even very complex cases of asymmetry."

Hypermetropia is measured by the same process, with convex glasses. The experiments mentioned, and their practical results, are simple and interesting.

VIII. Hasket Derby, M.D., of Boston, reports a case of *Sympathetic Ophthalmia, persisting after Enucleation*, in which recovery was only obtained after *removing extremity of optic nerve and surrounding tissues.* It is desirable that all instances of sympathetic ophthalmia persisting in spite of either *partial* amputation or *total* enucleation of the eyeball should be carefully recorded, and the circumstances of each case thoroughly investigated. We are not as yet sufficiently informed as to the *process* of sympathetic *influence;* what is the relative importance of diseases of the anterior hemisphere and posterior hemisphere of the eyeball as causes of sympathetic ophthalmia; and furthermore, what is the influence of any pathological condition of the surrounding tissues in cases where the entire eyeball has been removed? *Painful* diseases of the *anterior* hemisphere in one eye very often promote irido-cyclitis in the other eye; the *active* influence in such cases seems to be the *pain;* every one knows that when an eye has been destroyed by *anterior* staphyloma, it may prove harmless to the other eye, as long as no *acute* stage intervene; any *painful* condition of the destroyed eye is sure to *influence* the sound eye. Dr. A. Desmarres, of Paris, was in the habit of amputating the *anterior* hemisphere of the destroyed eye in cases of sympathetic influence, in order to do away with the *influence* of *pain.* On the ground that diseases of the anterior hemisphere are accompanied with pain, which pain I consider to be the main agent of sympathetic influence, it strikes me that amputation of the anterior hemisphere should be advisable in the majority of cases. According to the above teaching, I have experimented on several

cases of sympathetic ophthalmia originating from diseases of the *anterior* hemisphere of the destroyed eye; considering the *iris* as *the sensitive* organ in the anterior hemisphere, I have removed the diaphragm entirely in several cases, and succeeded in checking the disease without any further mutilation. Cases of a destroyed and even diseased but *painless* eye, causing no trouble for years and even for the whole lifetime of the patient, are very numerous. I have removed an eye recently that had been destroyed since childhood, patient being now sixty years old; a very acute and extremely *painful* condition was suddenly started in the destroyed eye by *spontaneous (pathological)* and *complete* dislocation of a *chalky* lens; such condition very rapidly gave rise to alarming sympathetic symptoms in the well eye; the destroyed eye had to be removed *during the acute stage.* Should I have removed the *anterior* hemisphere alone in this case? I am satisfied that such partial amputation, provided it had been made behind the iris and ciliary processes, would have checked the trouble. The question as to the advisability of performing the operation for excision of eyeball *during the acute stage* has recently called the earnest attention of practitioners; it well deserves further investigation, inasmuch as some cases have been recorded where death followed as apparently resulting from the operation. We must confess that some cases, such as the one above mentioned will allow of no delay. The question is in like cases to determine what are the *limits* of *harmless* and *effective* surgical interference; further experience is needed to solve the problem.

IX. Dr. Arthur Mathewson, of Brooklyn, proposes *a new method of treating Blepharospasm* "with the hope that it may be given a further trial." We shall certainly put the method to a test; the device is indeed very simple, and has proved successful in giving "prompt and complete relief in a case that had long resisted other means of treatment. A band of rubber, about a line in breadth, half a line in thickness, and an inch in length, was attached by one end to the surface of the upper lid, near its lower edge, at the middle of its horizontal length, a strip of isinglass plaster, notched so as to adapt itself accurately, being applied across the band, and the whole covered with collodion and allowed to dry till firmly adherent. Then the band was stretched upward to an inch and a half, so as to elevate the lid moderately, and fastened to the forehead in the same way. The band kept in place, and was borne without inconvenience for

twelve days, till during the struggles of an epileptic attack, it was loosened. The blepharospasm was then found to be entirely overcome, and has not returned" The author had previously seen the same plan successfully employed by Dr. VanBibber, of New York, in a case of ptosis.

X. J. F. Noyes, M.D., Detroit, Mich., proposes, *a new method of operating for strabismus*, which he describes as follows: "The patient being fully under chloroform or ether, the lids secured wide open by a speculum, a horizontal incision or slit is made in conjunctiva directly over the tendon, sufficiently long, through which the tendon is lifted out on a blunt hook. The tendon is then divided quite near to its insertion on the ball, leaving enough end or stump, so that the other end of the divided tendon can be carried under it lapped and secured by sutures. The amount of shortening thus effected must by actual measurement equal the deviation to be corrected. If it be found necessary to do this, a portion from the end of the tendon may be cut off before carrying it under and lapping as already described.

"The surfaces of the tendons where they come together in lapping, I need hardly say, should be previously freshened by a knife, or the tendon, if it can be done, may be divided in the first place obliquely. Two stitches or sutures, only, are sufficient to secure the ends of the tendons thus placed until they grow together, using a curved needle for this purpose; it is passed first through the conjunctiva, and then from beneath, through the tendons, thus placed on one side and tied, and then in the same manner on the other side and tied. On the third or fourth day after the operation, or longer, the sutures can be removed with safety."

The author claims the following as the new features and main points:

1st. The correction of the deviation or squint is made only on one eye, and by one operation, and is effected without disturbing the point of insertion or attachment of the tendon on the ball.

2d. The tenotomy is made on the opposing or elongated tendon.

3d. The shortening necessary to correct the squint or deviation is made by lapping the ends of the divided tendon, and is applicable to all cases.

XI. H. Althof, M.D., of New York, contributes an interesting *Clinical Study on Canthoplasty.* "The value of the procedure

may be stated in a few words; in trachoma, diphtheria, and blennorrhœa. In trachoma and phlyctœnular conjunctivitis and keratitis, its principal merit lies in shortening the attack, preventing relapses, relieving suffering quickly, and supporting powerfully the efficiency of other remedies. In diseases of the lids requiring operations it greatly improves the chances of the latter. In ulcus serpens (Saemisch) and diffuse keratitis (Hutchinson) it may become a desirable support of the treatment so far in use. Adding to this the fact that it is very easily done, that it can never do any harm, and never causes any disfiguration if properly performed, I am sure it will not be entirely overlooked as a contribution to ophthalmic therapeutics."

"About the performance of the operation I do not need to speak. If the lids are well separated, a simple cut down to the malar bone producing a wound of the outer integument not larger than 8 mm., will, in most cases, fully relax the presure of the lids. Slight modifications of this first act must be left to the discretion of the surgeon. The greatest neatness is of course, required in uniting the conjunctiva to the skin. Three sutures are generally sufficient, but the operator must judge for himself how many he thinks proper to use.."

Lectures on the Respiratory Organs, Heart, and Kidneys. By Alfred L. Loomis, M.D., Professor Pathology and Practical Medicine in the Medical Department of the University of the City of New York, etc. 8vo., pp. 549. New York: William Wood & Co.; 1875.

These lectures were delivered to the medical class of 1874 in the University of New York City, and are published from phonographic reports, as the author states "with unimportant alterations."

The work is divided into forty-four lectures, of which twenty-three are devoted to diseases of the larynx and lungs; twelve to those of the heart, and nine to those of the kidneys. Without undertaking to give an analysis of the subject matter, we shall now consider some special topics selected for purposes of illustration and occasionally of criticism.

Following two lectures on various forms of bronchitis proper, we find one devoted to asthma and whooping-cough. The lectures are strictly didactic, exceedingly systematic, and, though

delivered extemporaneously, must have been carefully elaborated. The author expresses in his preface an intention of publishing lectures on other medical subjects at some future time. We trust that this volume will be received so favorably as to encourage him to persevere in an undertaking so well begun.

In regard to asthma the notable point of interest is the treatment. "Different cases are relieved by very different remedies. The different remedies may be divided into three classes—*depressants*, *sedatives*, and *stimulants*." The first class is composed of nauseant remedies; the second mainly of narcotics, while the third consists chiefly of coffee and alcoholic preparations. Opium in full doses is the narcotic most relied on; the mode of administration preferred being the hypodermic injection of morphine with or without atropine. He observes—"During the intervals the treatment must be altogether hygienic, as there are no remedial agents, the use of which, at such times, will prevent the recurrence of the paroxysms." Following the suggestion of a medical friend, we have for several years found great satisfaction in the preventive use of small doses of muriate of ammonia with muriate of morphia, three or four times daily for a week or more. Asthmatics are content with experiencing some inexplicable alterative effect from the alkaline muriate upon the bronchial mucous surface, and the medical man need not ask to be much wiser about its precise mode of action.

Some credit is probably due, also, to the morphine; but how much belongs to each element properly in the prescription, we cannot say, not having tried them separately for this complaint.

Dr. Loomis believes in no specifics for whooping-cough, and is rather less sanguine about treatment than most medical teachers, restricting medication to sedative and antispasmodic remedies. Belladonna is, of course, included, but with less prominence and faith than are displayed by its great advocate, Trousseau.

Four lectures are occupied with the important subject of pneumonia. In conformity to the recent German pathological doctrines, he recognizes three distinct types, viz: (1) *croupous or lobar pneumonia*, always acute; (2) *catarrhal* or *lobular pneumonia*, sometimes called broncho-pneumonia, which may be acute or chronic; (3) *interstitial pneumonia*, or fibrous induration of the lung, corresponding to the chronic pneumonia of some writers.

Dr. Loomis adheres to the prevalent opinion, that croupous

pneumonia rarely attacks children under five years of age, and adds that it is rare also between forty and sixty years of age. No reason is given for such exemptions, and we are unable to supply the omission; indeed, the statement is not according to our observation, if the crepitant râle can be considered a diagnostic sign of this disease.

As regards treatment, the ancient plan of venesection followed by antimony and calomel is disapproved. The first expedient is believed to retard recovery, though it may mitigate such early symptoms as pain and dyspnœa; while the latter agents are not accredited with any control over the progress of the disease. Aconite and veratrum viride do not receive as much credit from the author as they enjoy with the profession at large, though he acknowledges that they may slow the pulse and lower the temperature; but he thinks that these advantages are more than counterbalanced by the gastric disturbance and general prostration which they are very apt to produce. The German treatment by cold compresses to the chest is mentioned with disapproval. Temporary relief of pain and dyspnœa is admitted by thus reducing the temperature, but the prolonged use of cold applications he reasonably apprehends would result in extension of the inflammatory process.

The author's plan has the merit of simplicity, and is free from the objections urged against the spoliative and depressant plans. All cases of a severe type are treated with quinine in doses varying from thirty to forty grains daily, and this course he finds to lower the temperature and shorten the febrile stage, though he admits that it does not arrest the progress of the pneumonia. For deficiency of heart power the use of alchoholic stimulants is recommended. The following remark is in precise conformity to general authority: "*Counter-irritants* in the early stages of pneumonia are not only useless, but greatly increase the distress of the patient." This is a traditional faith, which no one claims to have confirmed by actual trial, but accepts as implicitly as he does the history of Jonah. A considerable experience in the practice of early blistering, confirmed by some testimony from others, has completely controverted this tradition in our own mind. We find vesication over the affected region in the stage marked by the crepitant râle to be followed by rapid subsidence of pain, dyspnœa and fever, and almost invariably by early resolution without consolidation or bloody sputa.

On the subject of "acute catarrhal or broncho-pneumonia," Prof. Loomis is in close accord with Niemeyer. This disease "in its acute form is almost exclusively confined to childhood; while the chronic form furnishes the anatomical basis of almost all pulmonary phthisis; in either case it is a secondary affection." It is the form known as lobular pneumonia, having commonly originated in bronchitis, often associated with whooping-cough and measles, generally preceded by collapse of circumscribable portions of lung substance from obstruction of the bronchioles, and attended with copious exudation of changed epithelial cells into the air vesicles—such in brief is the idea of its pathology.

The author's plan of treatment is quite similar to the one recommended in croupous pneumonia. Having objected to the German application of cold compresses, he advocates the free use of quinine, which he believes, has the properties of lowering the temperature and arresting cell-development. Other remedies are to be used according to indications.

The subject of interstitial or fibrous pneumonia hardly needs our notice. If the reader is familiar with Niemeyer's work, he will find himself at home with this author.

In regard to the pathology of pulmonary consumption, Prof. Loomis discards the doctrine of Laennec, with the fundamental idea of tuberculosis, and adopts the recent German doctrines as enunciated by Virchow and Niemeyer. The following quotation from page 214, expresses the leading features of this theory:

> It seems to me that we have reached this point with regard to the anatomical changes of pulmonary phthisis, viz.: that all these changes, call them tubercular if you choose, can be arranged under the head of inflammation; and that they only differ according to the primary seat and character of the inflammatory process.
>
> In one class of cases the primary changes are in the cavities of the alveoli and bronchi, and are epithelial and cellular in their nature.
>
> In another class of cases the primary changes occur in the bronchial and alveolar connective tissue. These connective-tissue hyperplasias may be nodular, linear, or massive; they may occur in lung-tissue that has already undergone a change of the epithelium in the air cells, or they may occur as an independent development.
>
> Again, in still another class of cases, the primary changes may occur in the lymphoid elements of the lung. Hyperplasia of the lymphoid elements is almost always associated with connective-tissue hyperplasia, and the little masses or nodules formed as

the result of these two changes have all the anatomical characters of what are ordinarily termed tubercles; it is certainly impossible with the microscope to distinguish one from the other.

Accordingly the following classification of the different varieties of phthisis is made: (1) Catarrhal, (2) Fibrous, (3) Tubercular. The first variety is regarded as identical with chronic catarrhal pneumonia, in which a cheesy transformation of the effused cell-products has taken place. Under favorable circumstances this substance may soften and be expectorated, and restoration of the lung tissue to its former condition take place; or, if the cheesy nodules be of small size, they may become encysted; but generally an inflammatory destruction of tissue follows softening, and cavities are formed.

Fibrous phthisis bears a similar relation to interstitial pneumonia; but, as the latter is usually associated with catarrhal or croupous pneumonia, so this form of phthisis is connected with the preceding variety.

The tubercular variety of phthisis is regarded as uncommon, and the author observes: "I am not prepared to say it is or is not necessary that there should be a cheesy nodule prior to the development of tubercle, but I am strongly inclined to the opinion that tubercles should be included among inflammatory growths." One or more of these forms of phthisis may exist at the same time, and it is then generally impossible to determine which was primary.

On the etiology of phthisis the author seems disposed to attach little weight to special hereditary predisposition, though he fully recognizes an inherited feebleness of constitution, which predisposes to this disease under exciting causes; and he maintains that this feebleness may also be acquired. More importance is attached to anti-hygienic influences, especially in early life, and much also to climate. But the local or exciting causes are regarded as more operative than the general causes just mentioned, and under this head he includes bronchitis, pneumonia, pleurisy and mechanical irritants. This reasoning fully accords with his notions of its pathology—that its starting point is rather inflammation from some external assault which an enfeebled constitution is unable effectually to resist, than perversion of function in the lymphatic system.

In distinguishing between the different varieties the thermometer is made a test. When the temperature never rises above

100°, it is presumed to be of the fibrous form; when it ranges from that to 103°, especially with evening exacerbations, the catarrhal form is presumed to exist; when a still more elevated and constant temperature is maintained, the tuberculous form is indicated. Another point of distinction between the catarrhal and tubercular varieties is the fact that wasting in the former is preceded by a bronchial catarrh, while in the latter it is the forerunner.

The views on the pathology of pneumonia and pulmonary consumption, to which attention has just been directed, are those of the German school, and have been adopted by Dr. Roberts, whose work on the Theory and Practice of Medicine was noticed one year ago in this journal. The labors and writings of German investigators in medicine are attaining a prominence at this day which has previously been enjoyed by French workers in the same field. But this preëminence belongs rather to pathology than therapeutics. For practical results in medicine, as in other matters, British and American ingenuity bears the palm.

Before proceeding to the consideration of those affections of the kidney, which are named, from their distinguished investigator, Bright's diseases, the author devotes a lecture to acute uræmia. The symptoms, whether occurring under the form of convulsions or of coma, are attributed to failure of function in the kidneys, and urea is regarded as the toxic agent. It is somewhat remarkable that the pathology of this disorder is here disregarded, the condition of the kidneys leading to arrest of their functions being scarcely alluded to in this lecture. The most interesting point in this connection, however, is the author's treatment, which consists chiefly in the hypodermic use of morphine, particularly in that form marked by convulsions. His plan, and the reasoning on which it is based, may be stated in his own words:

First.—That morphine can be administered hypodermically to some if not to all patients with acute uræmia, without endangering life.

Second.—That the almost uniform effect of morphine so administered is, first, to arrest muscular spasms by counteracting the effect of the uræmic poison on the nerve-centres; second, to establish profuse diaphoresis; third, to facilitate the action of cathartics and diuretics, especially the diuretic action of digitalis.

Thus morphine administered hypodermically, becomes a powerful eliminating agent.

For uræmic coma no treatment is designated.

In the classification of Bright's diseases, the author deviates from Niemeyer and adopts Grainger Stewart's modification of Virchow's arrangement, as follows:

First.—A form in which the anatomical changes are inflammatory in their nature, and commence in the uriniferous tubules. This form has been designated *parenchymatous nephritis*, or the inflammatory form of Bright's disease.

Second.—A form which is non-inflammatory, in which the anatomical changes commence in the walls of the blood-vessels. This has been designated the *amyloid form* of Bright's disease

Third.—A form in which the anatomical changes commence in the intertubular tissue. This form has been designated the *cirrhotic form* of Bright's disease.

Of the numerous classifications by different authors which we have observed, this is the most simple and lucid and admirably suitable for class instruction.

The author thus enters upon the consideration of the first variety:

This is the inflammatory form of Bright's disease, and by far the most common form of the disease. It may pursue an acute or chronic course, be of short or long duration, and vary both in the character and intensity of the inflammatory processes.

If it passes through its entire course, it may be divided into three stages: *first*, a stage of inflammation; *second*, a stage of degeneration, either fatty or granular; *third*, a stage of atrophy.

The first stage of parenchymatous nephritis corresponds to that form of Bright's disease usually denominated acute albuminuria, or acute desquamative or tubular nephritis. I prefer acute parenchymatous nephritis to any of these terms.

The second stage commences with obstruction of the urinary tubules by the products of the earlier stage of inflammation. These epithelial deposits then undergo fatty transformation. By encroachment on the vascular system of the kidney its nutrition is gradually impaired, and so the third stage of atrophy is produced.

In the amyloid form there is no acute or inflammatory stage. Its course in brief is thus indicated:

In tracing the changes which occur in the kidneys, or in any other glandular organ that is the seat of amyloid degeneration, we find that the walls of the minute arteries are primarily affected; then there is added changes in the secreting tubes or cells; lastly, the organ undergoes atrophy.

For the sake of convenience in description, the anatomical changes which take place in kidneys which are the seat of amyloid degeneration, may be divided into three stages.

First, a stage of degeneration of the walls of the vessels; *second*, that in which is added to the changes in the blood-vessels, changes in the uriniferous tubules; *third*, a stage of atrophy.

The third form of Bright's diseases affects the interstitial structure, and is stated to be not divisible into distinct stages. The gradual increase of the interstitial substance encroaches on the tubular and vascular structures, and thus produces atrophy and loss of function. With regard to the nature of this intertubular affection, the author inclines to the belief that it is inflammatory, but does not speak positively.

The following remarks show how difficulty may arise in the study of renal affections:

The cirrhotic kidney will rarely be met with unassociated with other degenerative processes. The same may be said of all forms of Bright's disease. You will rarely meet with an amyloid kidney which is altogether waxy. Any form of kidney degeneration may be engrafted upon any other form; as, for instance, a cirrhotic change may be developed upon a tubular inflammation—a tubular inflammation upon an amyloid or a cirrhotic degeneration. Indeed, a great source of confusion in all forms of Bright's disease, is the change in the tubules, which for the most part are inflammatory in their nature. At an autopsy, the evidences of amyloid and cirrhotic degeneration may be present in the kidneys, and in addition, in a large proportion of cases, there will be found evidences of recent tubular inflammation, which may have been the direct cause of death. Under such circumstances, the tubular inflammation is secondary to the cirrhotic or amyloid degeneration.

* * * * * * * * * * *

In conclusion I will state, that although as you examine a kidney which is the seat of the combined forms of degeneration that we have been considering, you will sometimes find it difficult to determine the primary seat of the lesion, yet, when you take the etiology and clinical history in connection with these anatomical changes, there will be little difficulty in settling the question.

Among the causes which induce parenchymatous nephritis pregnancy is enumerated, resulting in puerperal eclampsia. This renal disorder is not attributed by the author to pressure on the renal veins, but to augmentation of function imposed on these organs and the irritation produced on the uriniferous tubes by the passage of an increased amount of excrementitious matter. Niemeyer, while agreeing that the convulsions are due to uræmic poisoning, regards the pathological condition of the kidney as that of amyloid degeneration rather than inflammation. The latter view seems to us less probable, from the fact that puerperal

eclampsia is obviously connected with an acute rather than chronic affection of the kidney, as both the convulsions and the albuminuria usually subside immediately after delivery.

It is to be regretted that pathological anatomy has not given a solution to this question and set it definitely at rest. Admitting what seems to be the most probable interpretation of the eclampsia, viz.: uræmia, and likewise the author's mode of accounting for its production, something must be demanded for the puerperal state, as predisposing women to such a nervous explosion; for, apart from this state, albuminuria is not commonly associated with convulsions.

To show still farther the importance attached by the author to the presence of an excess of urea in the blood, the following is quoted:

Before proceeding to detail these symptoms, let me remind you that the presence of urea in the blood in abnormal quantities has very much, if not entirely, to do with the phenomena which attend the development of the different forms of Bright's disease.

We venture the opinion that the above proposition is rather too sweeping, though prepared to admit that the nervous symptoms in Bright's diseases are entirely attributable to uræmia. But dropsy certainly is quite independent of this cause in organic diseases of the liver and heart, and tubular nephritis may as readily cause vascular obstruction in the kidneys as cirrhosis in the liver or valvular derangements in the heart.

The drain from the blood in albuminuria is not regarded by the author as a matter of much moment, but Niemeyer remarks, on chronic Bright's disease: "We have recognized the loss of albumen from the blood as the immediate cause of most of the symptoms of the disease, and hence our most important task by far is to cover the loss of albumen by a diet rich in protein substances, and by appropriate medication."

It seems to us that both are partly right, but that neither view is broad enough to comprehend the whole field of the subject. While the nervous derangements may be fairly attributable to uræmia, and the wasting and prostration to loss of albumen, it seems clear to us that the dropsy is due to vascular obstruction. No uniform or exclusive mode of treatment would, therefore, be appropriate.

The ætiology of renal maladies has an important bearing on diagnosis and treatment, and on this point the views of the

author are expressed decidedly and clearly. The amyloid form of degeneration is attributed most frequently to syphilitic infection of advanced stage, and the cirrhotic to the gouty or rheumatic diathesis or to abuse of alchoholic drinks.

In the treatment of the early stage of parenchymatous nephritis the following indications are laid down:

First: The elimination of urea.

Second: The removal, as rapidly as possible, of the inflammatory products which obstruct the uriniferous tubules.

Third: To counteract the effect of urea upon the nervous system.

The fulfillment of the first two indications by the vigorous use of diaphoretics and purgatives is mentioned with disapproval. Digitalis, in the form of infusion, and in large doses is recommended, and it is supposed to operate by its tonic effect on the heart, thereby overcoming the obstruction in the kidneys and enabling them to eliminate the urea which had accumulated in the blood. The treatment for the third indication has already been mentioned in connection with uræmic poisoning. The plan of treatment in the latter stages of this malady is essentially tonic.

In the amyloid form, when the disease can be traced to a specific cause, as is usual, anti-syphilitic treatment is recommended; but in the last variety no special plan is mentioned. For all forms of Bright's disease permanent residence in a warm climate is regarded as advantageous.

It now remains to make some general remarks on the work as a whole. From a somewhat careful examination our impression is decidedly favorable. While the views presented are fully up to what is actually known on the subjects treated, the doctrines are judicious and safe. At the same time they are presented with unusual clearness, and with sufficient positiveness to command confidence. In this volume we miss particular directions for physical diagnosis, which are of the utmost importance in the study of the class of diseases here treated. This want has been supplied by the author in a separate volume. The two, however, would not together make a volume of excessive size, and we are decidedly of the opinion that it would be more satisfactory to the reader to find the whole subject matter within the same covers. We would also suggest that medical writers should be not less careful than others, not to fall into grammatical errors. Examples of some of the most common mistakes occur on pages 229,

322 and 503, and more might probably be found by a critical reader. The publishers have acquitted themselves, in their own department, in a manner worthy of full commendation.

S. S. H.

Transactions of the Texas State Medical Association, Sixth Annual Session, 1874, *held at Dallas, April* 7, 8, 9 *and* 10, *to which is Prefixed the Proceedings in Houston, in* 1872, *and Proceedings in Waco, in* 1873, *not hitherto published.* Pp. 210.

Of the proceedings of the meeting in 1872, the only portions published are the minutes and the address of the president, Dr. D. R. Wallace. The latter consists chiefly of a retrospect of the history of medicine. At the close he takes occasion to invoke legislative aid to define the status of the medical profession, and afford protection against quackery. We have long been of the opinion that the British plan is the best, leaving the matter mostly in the hands of the profession. The great desideratum is an organized body of medical practitioners, to which admission can be gained only through a uniform examination independent of the medical schools. A register of its members should be regularly published, of which each one should have a copy, and thus they would be so generally distributed, and so accessible to the public, that no one need be ignorant whether a practitioner's name were on the list and he be duly qualified. This plan would afford sufficient protection to the public and to the profession. Many people employ irregular and unqualified practitioners from preference, and it is as impracticable to suppress quackery as drunkenness, prostitution, or panperism. Another difficulty in the way of legislative aid would be the demand of the homœopaths, the eclectics, and other exclusive schools of practice, for equal recognition and protection. Refusal of their claims would result in a clamor about persecution, and thus they would gain the sympathies of the masses as martyrs, for opinion's sake.

This plan would tend to make medical qualifications uniform throughout the country, and would render practicable the gradual elevation of the standard of medical education. The only body capable of originating and executing such a scheme is the American Medical Association, and it is probably useless to look for such a move as long as it remains under the control of

medical teachers, who are naturally more interested in the prosperity of their schools and their own reputations than in the welfare of the profession at large. Opposition might, also, be expected from practitioners of acquired reputation. They would not submit to a new examination, and would be jealous of a respectable organization from which they were excluded. On the whole, then, we see no other course open but to flounder along the old way and let the public find us out as best they may.

Following the minutes of the meeting of 1873, is a report on Hæmaturia Miasmatica, by the same Dr. D. R. Wallace. He quotes freely from other writers on the pathology of this malady, but is not clear himself on this point. The most noticeable feature of the paper is his assertion that there is a class of cases to which quinine is not applicable, inasmuch as it is really the exciting cause of the hæmaturia. No less remarkable is his reasoning, expressed as follows:

Now, in the absence of any other explanation of the fact that the sulphate of quinine does bring on, is occasionally the exciting cause of hæmorrhagic malarial fever, this hypothetical one is given: From some part or parts of the economy already belabored by the enemy (malaria), and upon which an extra amount of innervation has been determined, to assist it in its efforts to sustain itself, the quinine, by its equalizing properties, diverts such extra nerve force, depriving such part or parts of the means of support that nature has provided, and in this manner the way is open for an accession of the disease.

We leave comments to those more familiar than ourselves with the natural history of this malady.

Following the above is a paper by Dr. B. Powell, of Houston, on Cerebro-spinal Meningitis treated with bromide of potassium and preparations of ergot. To this are appended letters from Doctors Leonidas Hudspeth and J. J. Burroughs, of Houston, corroborative of the writer's views and experience. The remedies named were not the only ones used, but were the principal ones, and extraordinary success is claimed from this mode of treatment. The mode of action recognized is the same as laid down by Professor Hammond, as follows: "Bromide of potassium constringes the vessels of the cerebral mass." "Ergot constringes the vessels of the spinal cord." The plan of treatment may be briefly stated in the writer's own words:

I do not propose to make any comments, nor do I deem any necessary, except to state that generally the disease yielded

gradually, and that the doses of both drugs required augmenting after ten or twelve hours, for the first two days, after which time it was expedient to lengthen the interval between doses, or diminish the dose.

Mild purgatives were employed in every case where the bowels were not fully opened; vesicants were sparingly used, mercury ditto, opium not at all, nauseants ditto; quinine, in one case, in large doses. *Patient died.*

In view of the great fatality attending the ordinary modes of treatment of this disease, the above plan deserves further trial.

Dr. A. R. Kilpatrick contributes a report on Eclampsia, which consists of an account of six cases in his practice. In one case the convulsions appeared almost at the beginning of pregnancy, and continued throughout gestation. Although urged by the woman's parents to procure abortion, and though satisfied that this proceedure would have stopped the convulsions, Dr. K. refused and let her go to term. However, she recovered. The old resort of venesection is his principal reliance in this malady, and in one of the cases related, this was pushed by successive bleedings to the extent of five and a half pints! and the patient recovered.

The induction of premature labor is not recognized as a remedial measure, though it has been shown by Barnes and others to be practicable, safe to the mother, and, when near full term, to afford the best chance for viability to the child, while the continuance of eclampsia is known to be generally fatal to the latter, if not to the former.

The address of the President, Dr. R. T. Flewellyn, is on the history of medicine, and chiefly its antiquities.

Among the minutes of the meeting of 1874, a prominent place is occupied by the report of the committee on legislation. A long code of articles to regulate the practice of medicine was adopted and recommended to the legislature for enactment. The provisions are, for the most part, very desirable—too good in fact to be realized, while human nature remains so imperfect and perverse as we find it in this nineteenth century.

The President's Annual Address for 1874 was delivered by Dr. D. F. Stuart, of Houston. Several important and desirable measures are advocated, more or less practicable in their nature. On the trite subject of medical education a plea is made for the Medical School at Galveston. Legislation for the repression of quackery is invoked, which, in our opinion would be about as

effectual as the standing enactments against profanity. The truth is, our unwashed sovereigns are fond of humbug, and we might as well attempt to deprive them of whiskey as of patent medicines; the suppression of charlatanry is an event about as probable as the extinction of priestcraft. The sanitary system of the state receives a share of censure, inasmuch as the members of the Board of Health have been appointed rather for their political, than their scientific qualifications. His plan is to endow the State Medical Association with corporate privileges and invest it with the functions of a State Board of Health; also, to divide the State into districts, each under the sanitary management of a competent physician, subordinate to the State Board. A state medical journal is considered a desideratum. A good one would undoubtedly be useful to its readers, and the only obstacle in the way would be the want of a sufficient number of paying subscribers. Some very admirable remarks are made about the observance of the code of ethics. It is worthy of notice that the whole text of it is printed at the end of the volume, and there is reason to presume that Texas physicians will be more cognizant—not to say more observant—of its precepts than those of Louisiana.

Dr. Greensville Dowell, of Galveston, reports "that he has invented a plan for the radical cure of hernia." The principle of the operation is not new, however, being the old and exploded device of exciting adhesive inflammation, which easily effects a closure of the hernial outlet, that is sure afterwards to return as before.

Dr. Thomas D. Wooton, of Paris, reports a successful operation for multilocular ovarian tumor. Its most remarkable features are shown in the following:

The outer cyst wall was very thick; so much so as to suggest, when it was incised, the idea of being the uterus. The estimated weight of the tumor and contents, fifty pounds; supposed weight of patient, after operation, not to exceed seventy-five pounds.

Two points suggest themselves to me, as being of special interest in this case. First, the history of its development did not furnish the ordinary physical signs of an ovarian tumor; doubtless, an expert might have diagnosed the case earlier, but with me it was difficult. The second point of interest was the utter absence of all medical treatment to abate the progress and growth of the tumor.

Dr. Alex. W. Acheson gives an interesting report on the health of Denison in 1873. This town was laid out in Septem-

ber, 1872, and in three months had a population of 3,000. Owing to the high price of lumber, suitable house-room could not be provided, and the over-crowding exceeded that of the worst parts of New York city, reaching the extent of 500 inhabitants to the acre. Such a condition, joined to an almost total lack of sanitary precautions, led to the inevitable result of a terrible harvest of disease and death. One of the maladies, characterized as "Our Scourge," broke out in the latter part of the summer of 1873. The symptoms of the disorder were markedly choleraic, but, in the cases examined *post mortem*, a congested condition of the liver and spleen was discovered. The pulse was generally quickened to 140–160 beats, with a sluggish capillary circulation, while the eyes were injected and jaundiced. Most of the subjects were men of intemperate habits. The most remarkable feature in this connection is the fact that some persons were attacked who lived several miles from the city and had not been there. The duration of the disease was from four to twelve hours, and the mortality was fully ninety per cent. The question in the writer's mind was, whether the malady was true cholera, malarial diarrhœa, or a complication of cholera with pernicious intermittent fever. It seems to us that the disease in question is not attributable to any single cause, but to the combined influences of malaria, dissipation, bad food, and especially to a scanty breathing space poisoned with decomposing animal and vegetable matters.

Dr. John H. Pope gives an historical account of the yellow fever at Marshall in 1873.

Dr. E. Palmer contends that the fever which visited many towns of Texas, in 1873, was not true yellow fever. Three cases seen by him at Calvert are stated to have presented features of periodicity. Satisfactory evidence is adduced that black vomit is not pathognomonic of yellow fever, but occurs likewise in hæmorrhagic malarial fever. A point on which particular stress is laid, is the fact that no less than seven of the cases of black vomit at Calvert had previously had yellow fever; for he lays down the dictum—"Whoever denies the perfect immunity from a second attack of yellow fever, acquired by having passed through the ordeal of one attack (so long as he remains in a southern climate), can have had no practical experience in the nature and operation of the disease."

On this last point we think Dr. Palmer is too strenuous.

Numerous secondary attacks occurred in New Orleans in the great epidemic of 1867, some having had it in 1853, and other previous epidemics.

The feature of periodicity in a number of cases is not irreconcilable with the theory of yellow fever. Leaving out of account the view of complication of malarial with yellow fever, as a disputed point, it is evident that an individual may be seized with a malarial fever during the prevalence of yellow fevor, and before he has had time to recover from the former he may be attacked with the latter.

The subject of yellow fever is continued by Dr. Dowell, who gives an account of the cases treated by him at Galveston and Calvert. His plan of treatment seems to be peculiar—certainly is unlike anything in use here. Small doses of quinine, sometimes combined with Dover's powder and calomel, are given through the febrile stage. Brandy is his favorite stimulant. All wines, and especially champagne, are condemned. His views on the etiology of yellow fever are curious enough for quotation:

"I think the slaughter house below the city and the privies were the cause of the fever at Calvert.

I have the best of reasons for saying that *Yellow Fever* is fed from *animal filth*, and any place *entirely free from it will never have an epidemic* of yellow fever. Animal filth breeds fomites and animalcules; they, coming in contact with the blood, set up a fermentation in the body, and every drop of blood is changed before the patient is convalescent."

The moral of which is, that the people of Calvert should adopt a strict vegetable diet and abolish privies; and that the yellow fever animalcules should be starved out by cutting off their allowance of animal filth.

Dr. T. J. Heard, of Galveston, contributes some remarks on yellow fever, chiefly in regard to treatment, which is so far judicious as he deprecates any active method of medication.

Dr. J. T. Matchett, of Benham, gives his views on the mode of propagation of yellow fever. Reverting to the close of the Mexican war, he alleges that yellow fever was not brought from Tampico and Vera Cruz to New Orleans on the return of the troops in 1847, avering that "not a single case of yellow fever was known to occur in New Orleans that year, though it developed itself in some of our men after reaching the city." This last statement is certainly incorrect, as our mortuary records give 2804 deaths from yellow fever in 1847. He is convinced "that it is generally

of local origin, and, although portable, never spreads unless food, in abundance, is supplied to support its germs;" but what is the nature of its local origin, he does not clearly inform us, though he intimates subsequently that it may be kept in check by rigid sanitary regulations. It might, also, puzzle him to answer why local causes are not similarly operative, one year with another, and why the habitual violation of sanitary rules is only occasionally attended with a yellow fever outbreak.

A paper follows, by Dr. R. H. Harrison, of Columbus, on the epidemic fever which prevailed there in 1873. The circumstances attending are minutely described, from which it appears that the fever was greatly aggravated by the bad sanitary condition of the town from a recent overflow of the river, leaving the ground covered with mud and dead fish. Periodicity was noticed in a large portion of cases by this observer, and especially in the fatal ones. Albuminuria was not very marked in the severest cases, and never unless the urine was tinged with blood. It was also noticeable that a considerable number of people liable to yellow fever escaped, though they visited and waited on the sick. From these facts it appears that, if yellow fever was present at all, it must have prevailed to a limited extent at Columbus, and that most of the cases were an aggravated form of malarial fever.

It is encouraging to note vigorous signs of vitality in the medical body of a state which was recently regarded as almost out of the world, while in Louisiana there seems no prospect of a state organization, such as exists in the others with hardly an exception. The general standard of the papers in this volume is not very high, to speak candidly, but the contributors are working men, and energy and industry are sure to bear good fruit when trained in the proper direction. S. S. H.

Examination of the Urine. By George B. Fowler, M.D., Examiner in Physiology, College of Physicians and Surgeons, New York, etc. 12mo., pp. 80. New York: D. Appleton & Co.; 1874.

This little book, though not exhaustive of the subject, contains what is most needed by the general practitioner, and is more convenient for ordinary use than larger ones. Besides being

clear in the directions given, it has a few wood-cuts, showing the microscopic appearance of various urinary deposits.

S. S. H.

Note on Salicylic Acid. By Edward R. Squibb, M.D. Read before the Medical Society of the State of New York, February 2d, 1875, and by permission printed in advance of the Transactions. Pp. 10.

This substance, although known by chemists for about thirty-five years, has only within the last two years come into notice as an antiseptic and antiferment. Until quite recently it was prepared only from the willow, the poplar, and some other trees and plants, but it is now procured by the chemical action of dry carbonic acid on the phenate of sodium heated to 212°–482°. The resulting salicylate of sodium is treated with hydrochloric acid, by which the salicylic acid is precipitated in crystals of a light brown color. This acid is very sparingly soluble in cold water, but much more so in hot water; and after cooling it will hold one part in from 250 to 500. By the addition of a small portion of one of the neutral salts it is rendered much more soluble. The usual plan is to use three parts of phosphate of sodium, by which one part of the acid is made soluble in fifty of water.

The following quotation shows its therepeutic applications, by which it appears to possess great advantages over the irritating carbolic and cresylic acids:

"It is used for medical and surgical purposes either dry or in solution. When used dry it is sprinkled on to wounds, ulcers, or dressings in the form of very fine powder, in very small quantities, either simply powdered, or mixed in various proportions with some diluent, such as starch. When used in simple solution either for spraying surfaces, or for washes or gargles, it is used in tepid solution of about one part to three hundred parts of water. When stronger solutions are required for washes, gargles, or to moisten dressings, one part of the acid and three parts of phosphate of sodium to fifty parts of water have been used. When applied to wounds it appears immediately in the urine.

Its alleged advantages over all other antiseptics are: First, that it is far more powerful and effective in smaller quantities; and secondly, that it is, in all quantities necessary for complete effectiveness, entirely devoid of irritant action upon the living tissues. It is not caustic nor corrosive in any quantity, and never produces inflammation. In large quantities it may be irri-

tant and painful, but yet rarely surpasses a stimulant effect, while it appears to be quite neutral in the very small quantities which are yet thoroughly effective. Thirdly, it is said to reach and prevent processes of decomposition which are beyond the reach of all other antiseptics or antiferments. These processes are of two kinds, namely—vital, or those in which living organisms have an important part, such as that produced by yeast and many of those which occur in putrefaction; and chemical, or those which occur independent of vitality, as the production of the volatile oils in mustard and bitter almonds, the effect of diastase, etc. Now, while carbolic acid and other antiferments are azymotic, or completely arrest or prevent fermentations of the first kind, they are powerless with the chemical processes. Salicylic acid is said to be more effective with the vital ferments, and equally effective with the chemical.

Fourthly, in quantities said to be thoroughly effective, it is entirely odorless and tasteless, and harmless, while it has no poisonous effect in any reasonable quantity." S. S. H.

Dysmenorrhœa. By Jno M. Johnson, M.D., President Atlanta Academy of Medicine, late Prof. of Physiology and Pathology in Atlanta Medical College. Read before Atlanta Academy of Medicine, Jan. 12, 1875. Pp. 18.

The essay opens with remarks on the physiology of the female generative organs, some of which are not quite in accordance with the ideas generally accepted. He dissents from the doctrine that ovulation in the human female occurs monthly, and adduces as evidence the fact that pregnancy may occur without menstruation. His idea is thus stated:

My belief is that ovulation is not regulated by periods, but by the generative power of the organ, and that ovulation may occur once a month or once a year, and always independent of any other causation than its own inherent force.

After examining the views of the leading gynæcologists of the day, Dr. Johnson proceeds to state that he attributes the causes of dysmenorrhœa largely to errors in management during the period of childhood, having reference to dress, exposure to vicissitudes of temperature, exercise, habits of employment, diet, etc.

On the pathology of the malady he is not very clear, but strongly inclines to the view that it is mainly a disorder of function, connected with the general state of the health, and often traceable to imperfect nutrition. This is in opposition to the

doctrines of writers who maintain that it is due to organic lesions or mechanical obstacles, and who consequently treat by surgical methods. His treatment is in accordance with these views, the local part consisting mainly of warm enemas, sometimes medicated with laudanum, and warm vaginal douches. "Uterine Surgery," he says, "should only be resorted to after everything else has been tried." S. S. H.

Report of the Vaccine Department of the New York Dispensary, for the year 1874. By Frank P. Foster, M.D., Director of the Vaccine Department. Pp. 15.

The most important point in this report is the claim made for the superiority of dried bovine lymph over all other forms of stored vaccine. This is supported by several tables and by the testimony of various individuals, among whom we notice Drs. C. B. White and S. C. Russell, President and Secretary of our Board of Health. S. S. H.

Letter to a Commi tee of Citizens on the Proposed Schuylkill Drove Yard and Abattoir. By John H. Rauch, M.D., Treasurer of the American Public Health Association; late Sanitary Superintendent of Chicago, with medical opinions on the subject.

The object of this pamphlet is to show the impropriety of locating such an establishment in the midst of a thickly populated locality within the limits of the city of Philadelphia. The reasons adduced are of a sanitary and hygienic nature, and are too indisputable for comment. S. S. H.

EDITORIAL.

The Journal.

This number will complete the second volume of the new series of the NEW ORLEANS MEDICAL AND SURGICAL JOURNAL. The first issue of a periodical under this title occurred in May, 1844, and was under the editorial charge of Dr. Erasmus D. Fenner and Dr. Hester. Since the period of its original establishment

the great war of the States has transpired, and with its general damaging effects upon literature and the spread of learning, has occasioned temporary suspension of its publication.

In July, 1873, the Editor was induced to commence the publication of the present new series, simply because he supposed that the lack of a medical periodical in the metropolis of the South was a hiatus disagreeable to be endured by all who took pride in the promotion of Southern Medicine. In the salutatory to be found in the first issue of the "New Series," these personal desires and intentions of the Editor, are fully set forth. For two years the Editor has labored in support of the purposes originally announced. With how much, and what character of success these labors have been crowned, can be in part developed by myself, and in part determined by the criticisms of others. First, in a financial point of view, the JOURNAL has lost money for the Proprietor. This untoward result is in all probability to be properly attributed to that remorseless depravity which in the face of the utterance "Let us have peace," has yet maintained in active operation the most injurious warfare against all interests and honest occupations in that section from which the JOURNAL should expect to derive its support. I suppose it to be the world-wide experience of our profession that aught which affects the financial prosperity of a community deeply concerns them. Somehow, we seem to belong with those more tender leaves of the general foliage which are earliest blighted by the frosts of financial adversity. However correct the explanation may be, the statement is frankly true, and will, I trust, be carefully pondered by the friends of the JOURNAL.

In regard to the success of the JOURNAL as a medical organ, seeking to subserve and promote the great ends of our profession, others must decide. It has been my endeavor to make its influence conducive to the welfare of the profession at large. So steadfastly have I held this object in view, that all medical politics, school interests, and cliquism, have been most sedulously excluded. In endeavoring to realize the full fruition of this catholic spirit, I have even forborne to attack many flagrant violations of professional ethics to which my attention was called by letter, or otherwise. I mention this matter, that those who have called my attention to these delinquencies may understand why I have suffered them to pass without notice. Whether such a degree of neutrality is justifiable in medical journalists,

standing in the positions of watchmen upon the walls of the profession, is a question for my readers to answer.

I have never been guilty of such immodesty as to claim that the NEW ORLEANS MEDICAL AND SURGICAL JOURNAL was either the best medical journal in the country, or the cheapest. The skilled labor and material requisite for the publication of a journal, command larger prices in the South than in the North, and herein lies one of the principal difficulties to be encountered in medical journalism in this section.

The Editor now appeals to the profession to increase the patronage and support of the JOURNAL to the extent, at least, of meeting its expenses. One hundred new paying subscribers will fully compass this end.

Texas State Medical Society.

We learn from the secular papers that the late meeting of this body has been unusually harmonious and profitable. Reports upon many interesting subjects were read and referred to the Publishing Committee, who are instructed to furnish one copy of the report of the proceedings to each Medical Journal in the United States. We can only afford space for a few clippings from the "*Democratic Statesman*," of Austin, in order to show our readers how industriously and efficiently our Texas brethren are pushing their work:

Dr. J. M. Fort, of Paris, offered the following preamble and resolutions:

WHEREAS, The continued prosperity and success of this association, and the benefits resulting therefrom to the profession being in a measure dependent upon the efficient and hearty cooperation of the county societies; and

Whereas, It is the desire of this body to encourage the organization of societies in every county throughout the State of Texas, and to encourage and foster the interest of those already organized; therefore, be it

Resolved, That the physicians in each and every county in the State, not having an organized medical society, be and are hereby earnestly requested to form themselves into such organizations as early as practicable, and send delegates to the next meeting of this body.

Resolved, That each county society represented in this association be requested to forward to the secretary, through its delegates (or by mail), an annual report, embracing a synopsis of its

proceedings during the year, a health report of the county in which it is located, and such other matters as may be regarded by the society of general interest to the profession.

Dr. Robertson, of Washington, offered the following resolutions:

Resolved, That in view of the fact that yellow fever is reported to exist on the extreme Southern coast, in an epidemic form, at this early period of the season, it is the imperative duty of the government, the profession and the people to promptly adopt all proper sanitary rules and regulations for the protection of the health of the people.

Resolved, That whatever difference of opinion there may exist as to the efficacy or benefits that may result from quarantine regulations and laws, all agree to the beneficial results of properly established and enforced intelligent sanitary rules. In mitigating it, it does not prevent those epidemics that so fearfully scourge our race in those localities subject to their visitation.

Resolved, That the Medical Association of this State, anxious as it is to do all in its power to prevent the appearance of yellow fever and other diseases, do advise and earnestly urge upon the authorities and people of all localities subject to malignant diseases, the prompt adoption and rigid enforcement of proper sanitary regulations by the appointment of boards of health, through whom the proper rules for ventilation, fumigation and disinfection can be enforced.

Resolved, That a committee of five be appointed by the president to report a system of sanitary rules, to include the best and cheapest disinfectant, rules for ventilation, fumigation and drainage, for the information and adoption of the different localities in the State.

Dr. Matchett, of Waco, offered the following resolution:

WHEREAS, In the triumph of American Independence, we recognize and acknowledge the aid of Deity, and a new era, in the rapid development and propagation of Medical Science during the last century; therefore, be it

Resolved, That the President of the Texas State Medical Association is hereby requested, and authorized to appoint a committee of five of its members to prepare and present to the Centennial meeting, to be kept in the archives of the same, a history of the progress and science of medicine, and especially that part that pertains to, and interests Texas.

Adopted.

To the President and Members of the State Medical Association:

Your Committee on Ethics beg leave to make the following report: As you have adopted the code of ethics of the American Medical Association, and as nothing has been placed in our

hands upon which to predicate a report, we ask, therefore, most respectfully, to be discharged.

E. P. M. Johnson,
Brown,
Ashbell Smith.

Dr. Wallace offered the following resolution, which was adopted:

Resolved, That the Texas State Medical Association recommend the establishment of a National Board of Health, to consist of the heads of departments of the Treasury, War and Navy, with the view of determining some practical method of quarantine at ports threatened by the approach of contagious epidemic diseases.

Dr. M. K. Taylor, of the United States Army, arose and made some sensible remarks on the subject. His views were endorsed by the Association.

Dr. Wallace amended his former resolution, which was adopted, as follows:

Resolved, That to the end of securing the establishment of such a National Board of Health by Congress, this association solicit the co-operation of the State Medical Associations of the sea-board States, and of the members of Congress from this State.

Adjourned to 3 p. m.

Dr. Dowell, by permission, presented a paper on hernia—the operation for, and exhibited instruments for use. He showed the advantages derived from his process of operating; also, a paper on urethral stricture, showed a variety of instruments for the relief of stricture, and is satisfied that no patient should die if his self-retaining catheter is used after the operation.

Report adopted and referred to Publication Committee.

Dr. Burt offered the following resolution:

Resolved, That a committee of three be appointed whose duty it shall be to report at the next annual session of this body, on the anatomical and physiological difference between the white and negro races; on the modification of their respective diseases; and the difference in treatment resulting therefrom.

The resolution was adopted.

Among the many resolutions which were passed on during the last hours of the session, the following are the most important:

Resolved, That a committee be appointed on the subject of quarantine, its past results, its present status and its future promise.

Resolved, That a committee of three be appointed whose duty it shall be to report at the next regular meeting of this body, the organization of sections of this association as nearly after the model of the American Medical Association as practicable.

Resolved, That the president of this association appoint one member of this association in the cities of Galveston, Houston, Indianola, Corpus Christi, Brownsville, Austin, San Antonio,

Dallas, Waco, Brenham, Calvert, Marshal, Jefferson and Tyler, who will immediately put himself in report with the mayor and aldermen of these cities, and that he shall watch the introduction of yellow fever, or any other epidemics, and report to this association, through their chairman, a complete history and observations upon these subjects.

Resolved, That the officers and members of the Texas Medical Association be requested to use their influence in the cause of a State university, with a medical department, library, and all things necessary to constitute a great school of medicine.

Resolved, That at the future meetings of the Texas Medical Association, no report of standing committees or communications shall be read before the association which exceeds twenty pages of letter paper.

Resolved, That a vote of thanks be extended to our worthy ex-President, Dr. A. G. Clopton, for the able and impartial manner that has characterized the deliberations of this body during its present session.

Resolved, That the thanks of the Texas Medical Association be tended the editors of the *Democratic Statesman* for printing the proceedings of the present meeting.

Resolved, That a committee of three members be appointed by the chair, whose duty it shall be to memorialize the Legislature for a law requiring the registration of marriages, births, and deaths throughout the State. It shall also be their duty to exhibit the different forms of other States, and from the list to select the one of their choice.

Resolved, That a sub-committee, composed of one from each city represented in this association of over 10,000 inhabitants, be appointed, whose duty it shall be to urge upon the municipal authorities the necessity of establishing city hospitals, and, also, poor houses in each county.

The essayist for the ensuing year is Dr. Greenville Dowell, of Galveston.

Committees appointed:

Committee on Surgery—Drs. C. W. Truehart, B. F. Eads, H. H. Park, J. S. Willis, R. A. Watkins, and J. C. Sinclair.

Committee on Diseases of Women and Gynechology—Drs. T. D. Wooten, C. M. Blocker, W. J. Goodman, and S. F. Starley.

Committee on Arrangements and Invitations—Drs. E. P. M. Johnson, J. H. Pope, B. F. Eads, C. M. Blocker, and L. S. Rayfield.

Delegates to the American Medical Association—Drs. J. B. Shepherd, M. H. Oliver, A. G. Clopton, W. H Park, J. M. Fort, G. C. McGregor, R. A. Watkins, J. W. Stalnaker, L. S. Rayfield, S. A. Owens, J. T. Heard, L. Hudspeth; H. Ryan, J. Cummings, S. G. Haynie, D. Port Smythe, R H. Harris, W. R. Wallace, E. R. W. McCreary, J. Larendon, and J. J. Burroughs.

Censors—Drs. S. F. Starley, five years; A. G. Clopton, four

years; J. J. Burroughs, three years; T. D. Wooten, two years; J. P. Matchett, one year.

Committee on State Board of Health—Drs. Harrison, Hudspeth, Clopton, Taylor, and Sears.

Committee on Climatology and Epidemics—Drs. J. T. Hussey, J. F. Hooks, ... Burrows, J. R. Taylor, R. H. L. Bibb.

Committee on Creation of Library—Drs. D. R. Wallace, D. F. Stewart, J. M. Litten, and S. G. Haynie.

Committee on Indigenous Medical Resources of Texas—Drs. W. E. Saunders, O. H. Seeds, A. T. Morris, and L. E. Underwood.

Committee on Record of Cases—Drs. Greenville Dowell, ... Ryan, ...Palmer, W. J. Burt, J. Cummings, T. D. Manning.

Committee on Publishing, Finance and Claims—Drs. R. H. Harrison, L. Hudspeth, ... Becker, M. A. Taylor, W. A. Morris.

Committee on Science and Progress of Medicine—Drs. J. H. Pope, I. H. Willis, D. F. Stewart, R. N. Swearingen, D. Port Smythe.

Committee on Children's (Diseases and Hygiene, perhaps)—Drs. M. A. Taylor, S. F. Matchett, O. H. Seeds, T. W. Holland.

Petition to Board of Health in regard to Quarantine.

We publish the subjoined petition with the signatures of the petitioners, in order to exhibit the sentiments of a large number of our leading physicians in respect to the true value and expediency of quarantine.

Any one who will give himself the trouble to look through the State ordinances in regard to quarantine and a Board of Health, will perceive that both the Board of Health and the State authorities are so constrained by law, that they cannot exercise any volition or give way to the influence of any personal opinion in the matter of establishing quarantine. Whenever legal notice of an infected port is served upon the Board of Health, the law makes it obligatory upon them to notify the Governor, whose duty, as defined by statute, is to issue a proclamation establishing quarantine in so far at least as commerce with the infected port is concerned. The whole animus and letter of these laws show them to be emanations from the most implicit trust in the efficacy of the quarantine system, and consequently they guard its establishment, and its most relentless administration. Even with regard to the choice of persons who shall constitute the Board of Health, the law stipulates that "the said members shall be selected with reference to their known zeal in favor of a quarantine system." It is even to be

doubted whether much discretion is allowed to the Board of Health upon the very important point of notifying the Governor of the fact that an "infected port" in communication with New Orleans calls for a quarantine. In the matter of fixing the duration of detention at quarantine they possess some powers, but the law explicitly states that the period shall "not be less than ten days." It must, therefore, be conceded that the petition is likely to be devoid of any good results is so far as the immediate abrogation of quarantine usages are concerned. But, as an illustration of the drift of medical opinion, it is a movement which cannot be too highly estimated. Whatever of wisdom—whatever of actual benefit to man in a sanitary point of view may have been accreditable to the practice of quarantine in former days, we are forced to admit its entire infeasibility and impracticability at the present time. The folly exhibited by attempts to blockade the great natural highway of commerce, which the Mississippi river affords, and yet permitting unrestricted ingress of persons and fomites by the general railways which centre here, is too apparent to require discussion. A man with his system charged with the germs of small-pox, or yellow fever, may land at almost any other of our sea-ports, even at New York, and arrive in this city before the period of incubation has terminated. Recognizing these truths, the medical profession desire to do away with restrictions upon commerce, which bestow no compensations in the way of protection against disease. What other means of protection do we propose to substitute? Surely nothing promises such good results as those measures which are practised under the term "Disinfection." We admit that this word is often loosely used, and also admit that in our present state of learning, we cannot employ it with that strict scientific accuracy which would be acquired by a knowledge of agents positively destructive of disease-germs, and yet susceptible of general utilization. But, although such an admission is forced upon us, experience has abundantly proved the success of that warfare waged against disease-germs by the use of what are termed disinfectants. Whether this success is brought about by the direct antagonism of the various agents employed, to the causative materies morbi, or whether in part, or mostly, due to the fact that we accompany their employment by sanitary measures, which alter those circumstances and conditions favorable to the development of dis-

ease-germs, the good results ascribable to the use of disinfectants point to this method as the proper substitute for quarantine. The medical profession of this city includes a member whose experience, accurate chemical knowledge, great zeal, and ingenuity in the application of means qualify him most admirably for attaining the best results possible to be reached by disinfection. I refer to the gentleman who, during the last year, did himself so much credit in this kind of service. If an epidemic seems imminent on account of threatened approach of yellow fever, his employment with a *carte blanche* as to the means to be used, would afford a satisfactory test of the amount of benefit to be obtained by disinfection.

The Quarantine laws of Louisiana demand revision and alteration. It is shockingly absurd to attempt to settle by legal enactments, questions which are still sub judice at the bar of science. This absurdity must be charged against any statute which expressly provides that members constituting a scientific commission shall hold to one, or another opinion, in regard to unsettled questions connected with their duties. The explanation for this curious feature in the statute must be found in the fact, that it was passed shortly after the great epidemic of 1853, when people were bewildered in regard to the best mode of securing exemption from such fearful plagues.

NEW ORLEANS, April 26, 1875.

To the President and Members of the Board of Health of the City of New Orleans.

Gentlemen: The undersigned practicing physicians of the City of New Orleans, beg leave to submit—

1. That after many years of experiment it has been clearly proven that quarantine does not protect this city from yellow fever.

2. That the commerce of this city, upon which her prosperity, and the livelihood of more than half of her population directly or indirectly depend, must continue to be most seriously damaged by the repetition of said quarantine.

In view of these facts we pray your Honorable Body not to recommend in future the imposition of any greater restriction upon vessels arrived from infected ports than to require them to be disinfected at the Quarantine Station, and to remove to hospital at the same point any cases of yellow fever which may exist on board; the detention for this purpose not to exceed *twenty-four hours*, and the unaffected passengers, in the mean time, allowed to proceed to the city:

F. Alpuente, D.M.P., 142 Royal street,
A. Foster Axson, M.D., 178 Annunciation street,
E. D. Beach, M.D., 16 Claiborne street,
C. Beard, M.D., 14 Dauphine street,
S. M. Bemiss, M.D., 558 St. Charles street,
Henry Bezou, M.D., 144 Dumaine street,
C. J Bickham, M.D., 710 Magazine street,
W. P. Brewer, M.D., 603 St. Charles street,
J. Borde, D.M.P., 130 Customhouse street,
D. Warren Brickell, M.D., 14 Dauphine street,
A. Capdevielle, D.M.P., 449 St. Charles street,
J. Carter, M.D., 694 Magazine street,
John J. Castellanos, M.D., 72 Orleans street,
S. E. Chaillé, M.D., University Building,
Alcée Chastant, M.D., 485 Magazine street,
H. C. d'Aquin, D.M.P., 41 North Rampart street,
John Dell'Orto, D.M.P., 243 Julia street,
J. P. Davidson, M.D., 242 Prytania street,
F. H. Dennis, M.D., 245 Baronne street,
E. DeBlanc, M.D., 159 Dumaine street,
C. Faget, D.M.P., 159 Burgundy street,
J. A. G. Fisher, M.D., 168 Felicity street,
J. N. Folwell, M.D., 221 Carondelet street,
Edw'd. Harrison, M.D., 112 Canal street,
Alexander Hart, M.D., 19 Camp street,
F. Hawthorn, M.D., University Building,
D. C. Holliday, M.D., 112 Canal street,
A. C. Holt, M.D., 242 Felicity street,
O. Huard, D.M.P., 117 Royal street,
Wm. E. Kennedy, M.D., 168 Julia street,
T. S. Kennedy, M.D., 168 " "
P. A. Lambert, D.M.P., 117 Royal street,
Thos. Layton, D.M.P., 702 Magazine street,
J. Hampden Lewis, D.M.P., 289 Royal street,
E. S. Lewis, M.D., 70 Frenchman street,
F. Loeber, M.D., 161 Baronne street,
Samuel Logan, M.D., University Building,
J. J. Lyons, M.D., 219 Carondelet street,
Sabin Martin, D.M.P., 96 Burgundy street,
Armand Mercier, D.M.P., 220 Girod street,
W. S. Mitchell, M.D., 10 Carondelet street,
J. T. Moreau, M.D., 118 Marais street,
T. G. Richardson, M.D., University Building,
H. D. Schmidt, M.D , 70 North Rampart street,
M. Schuppert, M.D., 179 Carondelet street,
E. Scratchley, D.M.P., 377 Dryades street,
J. F. Seguin, M.D., 82 Carondelet street,
E. T. Shepard, M.D., 1140 Magazine street,
Howard Smith, M.D., 91 Prytania street,
E. Souchon, M.D., University Building,

J. C. Stickney, M.D., 168 Orange street,
Benj. Stillé, M.D., 206 Prytania street,
F. L. Taney, M.D., 377 Dryades street,
C. H. Tebault, M.D., 469 Baronne street,
J. Touatre, D.M.P., 142 Dumaine street,
J. Trudeau, M.D., 132 South Rampart street,
D. Tureaud, M.D., 132 " " "
Charles Turpin, D.M.P., 41 North Rampart street,
W. H. Watkins, M.D., Rousseau street,
J. H. Wiendahl, M.D., 304 Bayou Road,
W. B. Wood, M.D., 768 Magazine street,
S. S. Wood, M.D., 768 " "
P. Yeiser, M.D., Louisiana Avenue.

Caution.

We are requested by the publishers to insert the following to intending subscribers to Zeimssen's Cyclopædia of the Practice of Medicine:

As this great work progresses, it is possible—from some subscribers breaking up their sets, or from other causes—that occasional odd volumes may be offered for sale. Those who desire the complete work are warned against purchasing these, as the Publishers do not engage to supply parts of sets.

Every subscription *must* be for the *entire work.*

No volumes will be sold separately.

WM. WOOD & CO., *Publishers,*
27 Great, Jones Street, New York.

Centennial Medical Commission of Philadelphia.

This Commission has now been fully organized by the election of the following officers:

President—Samuel D. Gross, M.D., L.L.D., D.C.L. Oxon.

Vice Presidents—W. S. W. Ruschenberger, M.D., U. S. N., and Alfred Stillé, M.D.

Secretary—Wm. B. Atkinson, M.D.

American Corresponding Secretary—Daniel G. Brinton, M.D.

Foreign Corresponding Secretary—Richard J. Dunglison, M.D.

Treasurer—Caspar Wister, M.D.

Executive Committee—Drs. Washington L. Atlee, D. Hayes Agnew, Robert Burns, David Burpee, J. S. Eshleman, Andrew Fricke, N. L. Hatfield, H. Lennox Hodge, W. H. Pancoast, Robt. E. Rogers, J. G. Stetler, L. Turnbull, and Edward Wallace, and the officers.

Arrangements have been made to hold an International Medical Congress early in September, 1876, in Philadelphia, at which discourses will be read upon Medicine and Medical Progress in

the United States; on Surgery; Obstetrics; Chemistry and Pharmacy; Materia Medica; Medical Jurisprudence and Toxicology; Hygiene and Social Science; Medical Biography; Medical Education and Institutions; and Medical Literature. Prof. Stanford E. Chaillé has been selected to deliver the address on Medical Jurisprudence and Toxicology.

The Present Number.

This number has been increased beyond the usual limits, but notwithstanding its additional pages, some valuable original papers and several book notices have been laid over for lack of space. We beg those of our contributors whose papers do not appear in this number, to observe towards us as good a degree of patience as they are able to exercise.

New Remedies at I. L. Lyons, Camp Street, New Orleans.

Salicylic Acid.
Benzoate Lithia.
Hydrochlorate of Apomorphia.
Cod-Liver Oil and Sesquixide Iron.
Cod-Liver Oil and Benzoate Iron.
Cod-Liver Oil and Lact. Phos. Lime.

The house calls special attention to the last mentioned preparation.—[ED.

New Remedies at Frederickson & Harte's, Canal Street, N. Orleans.

Camphor Chloral—syrupy liquid.
Camphenyl—not caustic.
Salicylic Acid—disinfectant, no odor, antiseptic.
Apomorphine—emetic by hypodermic injection in poisoning cases.
Liquor Carbon Detergens—alcoholic solution of coal tar.
Benzoate of Lithia—for gout and rheumatism.
Hyoscyamin.
Aq. Ferri Oxydat.—tasteless solution of iron.
Oleic Acid—*pure.*
Oleate Morphine.
" Mercury, 5, 10, 20.
" " Binoxide.
" Quinine.
" Cinchonine.
" Atropine.
Esmarch's Tourniquet.

Table I---March.

Day of Month.	Temperature.			Mean Barometer Daily.	Relative Humidity—Daily.	Rain fall—inches
	Maximum,	Minimum.	Range.			
1	76.5	68	8.5	29 807	85	.09
2	76.5	70	6.5	29.835	88	.01
3	52.5	45	7.5	30.078	50	.00
4	58.5	48	10.5	29.991	83	.13
5	68.5	51.5	17	29.987	84	.71
6	71.5	58.5	13	29.756	77	.40
7	55	36.5	18.5	30 005	49	.00
8	63	48	15	30 180	58	.00
9	69	50	19	30.190	69	.10
10	68	54.5	13.5	30.181	49	.00
11	67	51	16	30.029	87	3 00
12	78	59	19	30.024	84	.00
13	81	69	12	30.026	83	.00
14	80.5	69.5	11	29.956	78	.00
15	70	50	20	29.825	57	.00
16	63	48	15	30.121	26	.00
17	61	50	11	30.293	36	.00
18	67	50	17	30.155	77	2.10
19	78.5	58	20.5	29.892	84	.00
20	76	70	6	29.843	61	.00
21	67	51	16	30.105	50	.00
22	63	46.5	16.5	30 319	50	.00
23	70	49.5	20.5	30.191	67	.00
24	74	60	14	30.240	85	.00
25	76.5	60.5	16	30.245	79	.00
26	80	60.5	19.5	30.165	80	.00
27	81	66.5	14.5	30.067	78	.00
28	79	67	12	30.038	78	.00
29	81	66.5	14.5	30.022	78	3.78
30	75	65	10	30.030	86	.39
31	80.5	69	11.5	30.035	81	1.10
Mean..	71.22	56.98	14.24	30.053	70.2	Total. 11.81

METEOROLOGICAL REPORT FOR NEW ORLEANS.

Table II---April.

Day of Month.	Temperature.			Mean Barometer Daily.	Relative Humidity—Daily.	Rain fall—inches
	Maximum.	Minimum.	Range.			
1	56	51	5	30.176	77	1.22
2	59	47.5	11.5	30.238	51	.00
3	66	47.5	18.5	30.177	52	.00
4	72	52	20	30.138	64	.00
5	75.5	55	20.5	30.177	74	.00
6	78	61	17	30.204	73	.00
7	80	62	18	30.123	72	.00
8	79	66	13	30.046	74	.00
9	80	67	13	30.055	80	.17
10	81	69	12	29.909	78	.15
11	67	64	3	29.825	78	.09
12	71	58.5	12.5	29.955	53	.00
13	67	52	15	30.107	42	.00
14	72.5	54	18.5	30.113	45	.00
15	77	53	24	30.075	62	.00
16	75.5	61	14.5	30.055	48	.00
17	68	56	12	30.132	34	.00
18	77	54.5	22.5	30.152	59	.00
19	80.5	62	18.5	30.130	66	.00
20	77	64	13	29.960	76	3.27
21	72.5	61	11.5	29.828	83	.02
22	70	63.5	6.5	29.845	79	.85
23	67	49	18	29.895	52	.00
24	72.5	53	19.5	29.868	57	.00
25	76.5	57	19.5	30.068	66	.00
26	76.5	63	13.5	30.012	84	1.91
27	72.5	55	17.5	29.985	53	.00
28	76	56	20	30.020	62	.00
29	81	60.5	20.5	30.073	70	.00
30	82	69	13	29.922	82	.00
						Total.
Mean..	73.51	58.13	15.39	30.040	64.9	7.68

Mortality in New Orleans from April 1st, 1875, to May 2d, 1875, inclusive.

Week Ending	Pneumonia.	Small-Pox.	Consumption.	Total Mortality.
Mar. 7	11	31	19	137
Mar. 14	8	20	15	112
Mar. 21	5	22	16	112
Mar. 28	1	21	17	100
April 4	11	13	10	99
April 11	9	15	12	117
April 17	3	10	10	85
April 25	3	8	15	107
May. 2	3	16	18	114
Totals	54	156	132	983

THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

Edited by S. M. BEMISS, M. D.

[NEW SERIES.]

PUBLISHED BI-MONTHLY, AT $5 PER ANNUM, IN ADVANCE.

Volume II. | JULY, 1874. | [No. 1.

Paulum sepultae distat inertiae celata virtus.—HORACE.

NEW ORLEANS:

JAMES A. GRESHAM, Proprietor and Publisher, 92 Camp Street,

1874.

ADDENDUM.

Solution of Tribasic Phosphate of Lime in Cod Liver Oil.

We beg leave to call the attention of our confrères to the following correspondence, and ask them to call at the drug store of Mr. Jas, 103 Chartres street, and witness for themselves the working formula used in the preparation of the solution to which it relates, as it is not a secret remedy.

ED. KUNEMANN, D. M. P.,
J. G. HAVA, D. M. P.

New Orleans, June 26, 1874.

NEW ORLEANS, June 4th, 1874.

Prof. Joseph Jones, M.D., 403 St. Charles Street:

Dear Sir and Confrère—Your attention is respectfully directed to the accompanying new Pharmaceutical preparation, viz., a Solution of Tribasic Phosphate of Lime in Cod Liver Oil, and we beg leave to express the hope that you will be present at the series of experiments by which the entire process of its manufacture will be illustrated.

We have found that cod liver oil will dissolve two per cent. of Tribasic Phosphate of Lime at the ordinary atmospheric pressure, and by the process recently discovered in this city, is not only rendered more valuable therapeutically, but is also improved in appearance and taste, and is much more readily borne by delicate stomachs.

The phosphated oil may be used advantageously in the treatment of Phthisis Pulmonalis, and other diseases, either singly or in combination with phosphate of iron, arsenic, etc. Not only does this preparation excel all other forms of cod liver oil, in the fact that it contains the tri-phosphate of lime in a valuable form, eminently adapted to assimilation by the blood, osseous muscular and nervous tissues, but it is rendered also less liable to chemical changes by the process of solution, of the bone earth.

The grand objection to all the *emulsions* of *cod liver oil* and phosphates, and hyperphosphates, heretofore prepared by pharmacentists, is that they rapidly become rancid and derange digestion.

Your extensive clinical experience in hospital and private practice, will furnish abundant opportunity for a trial and accu-

rate determination of the therapeutic value of the phosphated oil. We feel assured that it will accord with the important results already obtained by distinguished and experienced practitioners of our city, who have employed it with marked success in the treatment of various diseases, as Phthisis Pulmonalis, Infantile Gastro-Enteritis, attended with great wasting of the tissues and incessant vomiting, Scrofula, and chronic Pleuro-Pneumonia.

Accompanying this letter you will please find a detailed statement of several important cases illustrating the value of the phosphated oil.

It will be observed that in the hands of the physicians whose names accompany the report of cases, the Phosphated Cod Liver Oil proved superior to the oil in its simple state, and to all its other preparations now known to the pharmaceutists.

Your well known interest in everything that relates to the welfare of our Southern land, and your devotion to the advancement of medical science, has led us to call your attention to this preparation, the results of which, actual experience has shown to be of the greatest value in the treatment of many severe and dangerous forms of disease.

Most Respectfully Yours,

ED. KUNEMANN, D. M. P.,
J. G. HAVA, D. M. P.,
A. JAS, PH. D. P.

OBSERVATIONS.

Case I.—Emmanuel Pinson, aet. seven months, was in such an advanced state of marasmus that his skin was wrinkled and dry. He threw up whatever his stomach received, and was suffering from a most exhaustive diarrhœa. His weakness was such that he could no longer take the breast. A few drops of milk had to be constantly administered with a spoon. He took small quantities of phosphated oil frequently repeated. After a few days, marked amelioration. Three months later he had recovered his health, and looked as well as any other child of his age.

Case II.—Another child, three months old, exceedingly slender and thin, affected with a diarrhœa since its birth, was cured after a short time by the administration of the phosphated oil.

Case III.—A scrofulous child, aet. five years, affected with incipient coxalgia, was unable to move his leg, so great were the

pains. Was submitted to external applications of tincture of iodine and internal administration of the soluble phosphated oil. Two months after, he was well, walked easily and without pain.

Case IV.—A child seven years old, born of healthy parents, was suffering with chronic pleuro-pneumonia, resulting in a pleuritic effusion and an abscess of the lungs. The effusion disappeared by proper treatment and the abscess opened in the bronchia. The patient was dwindling away by a low hectic fever and disordered digestive functions. He took the cod liver oil, and after several alternations of better and worse, recovered completely, with a good appetite and good embonpoint.

Case V.—Miss D., a distinguished artist, subject for many years to hemoptysis, had given up the use of cod liver oil, as it always produced diarrhœa. The phosphated oil was well tolerated, and under its use both health and appetite improved.

Case VI.—Mr. A. Krauss, lithographer, also an hemoptysic, was obliged to renounce cod liver oil, his stomach being unable to bear it. He is now taking the phosphated oil, and for the last two months has greatly improved.

ED. KUNEMANN, D. M. P.,
J. G. HAVA, D. M. P.

NEW ORLEANS, June 12th, 1874.

Ed. Kunemann, M. D., New Orleans, La.:

Dear Sir—In reply to your favor of the 6th inst., allow me to say that I have carefully examined the Phosphated Oil, and also, in accordance with your kind invitation, have witnessed the entire process of its preparation.

The following are the results of this investigation:

1st. By the process discovered by Dr. Kunemann and Dr. Hava, of New Orleans, cod liver oil can readily be made to dissolve and hold perfectly in solution *two per cent.* of Tribasic Phosphate of Lime.

2d. The cod liver oil is rendered more fluid and elegant in appearance, and more palatable, more nutritious and digestible, by this process.

3d. The solvent employed appears to effect a chemical union between the Tribasic Phosphate of Lime and certain constituents of the oil, and thus greatly enhances the therapeutical value of the new preparation.

4th. The solvent employed not only preserves the purity and

freshness of the oil, and prevents all decomposition, but it also acts as a gentle tonic to the gastro-intestinal mucous membrane.

5th. The accuracy of the clinical facts presented, illustrating its value in the treatment of certain diseases of the Lungs, Bowels, and Lymphatics, appear to be beyond question, and entitled to the confidence of the medical profession.

Respectfully,

JOSEPH JONES, M.D.,
Prof. of Chemistry, Med. Dept. University of La.

NEW ORLEANS, June 16th, 1874.

Messrs. Ed. Kunemann, D. M. P., J. G. Hava, D. M. P., A. Jas, Ph. D. P., New Orleans:

Gentlemen—If I have been several months before answering to your wishes, it was through the desire of having a full, fair and unbiassed trial of your "Phosphated Cod Liver Oil."

I have been prescribing it, and find it to be a most excellent preparation, especially as regards weak and delicate stomachs, which reject all fatty substances thrown into them. To patients suffering from such organs, it has been a source of gratitude to be able to take and retain the drug.

I am much pleased with it, and think it the best preparation of cod liver oil I have yet used. I highly recommend it to the profession.

Respectfully, Y. R. LEMONNIER,
Visiting Surgeon Charity Hospital.

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—o—

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At the present price of sulphate of quinine, it is sold at about one half the price of that agent, and with the testimony offered that it has equal tonic and anti-periodic effects, and that it is less objectionable, there seems to be no good reason why it should not be universally employed by the profession.

The cut below gives the size of the ounce phial and the form of putting up.

I have given the Cincho-Quinine a fair trial, and am much pleased with it. — S. A. BENNETT, M. D., *New Portland, Ind.*

I find it excellent. It works well. — B. RAWSON, M. D., *Findlay, Ohio.*

I am *more* than pleased with it. — I. I. STRISE, M. D., *St. Augustine, Florida.*

I am so much pleased with it that I make it my *chief remedy* when bark preparations are needed. — J. DENNISTON, M. D., *Ovid, N. Y.*

Have prescribed it as a tonic with uniformly good effects. — H. H. BEEBE, M. D., *Marshall, Wis.*

I use it satisfactorily in all cases as a substitute for the sulphate. — J. A. PERKINS, M. D., *Chestertown, Md.*

I think very favorably of its effects. — J. M. ALDRICH, M. D., *Fall River, Mass.*

It gives entire satisfaction as a remedy. — WATTS & BRADFORD, *Cave Spring, Ga.*

I have tried your Cincho-Quinine in more than *forty cases* of intermittent fever, with as much if not better success than sulphate of quinine. — B. E. POWELL, M. D., *Glasgow, Mo.*

It is an admirable preparation, having all the beneficial effects of quinine, without its objectionable qualities. — E. B. STUART, M. D., *Montgomery, Texas.*

We should hardly know how to practice without it. — MOORE & JONES, *Xenia, O.*

I can strongly recommend it to the profession generally. — J. H. FREY, M. D., *Perry, Iowa.*

I can safely recommend it to my professional brethren as a most valuable medicine. — J. F. MILLER, M. D., *Goldsborough, N. C.*

It has all the advantages you claim for it, and doubtless it will in time supersede the use of sulphate of quinine entirely. — SAMUEL W. COONS, M. D., *Madison, Ala.*

I have used about thirty ounces of Cincho-Quinine, and like it very much. — U. N. MELLETTE, M. D., *Williamsburg, Ind.*

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I am greatly pleased with your Cincho-Quinine. — S. B. ANDERSON, M. D., *Lawrence, Kansas.*

It has all the advantages of quinine, without its disagreeable taste. — C. C. SMITH, M. D., *Redford, Mich.*

I have found it in every respect what you claim for it. — A. H. BRUNDAGE, M. D., *Xenia, O.*

I have used Cincho-Quinine in eight or ten cases, and have reason to think well of its results. — J. C. DOWNING, M. D., *Wapping Falls, N. Y.*

After further continued trial of the Cincho-Quinine, I can safely say that it is a most excellent remedy. — S. A. BUTTERFIELD, M. D., *Indianapolis, Ind.*

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I also beg to add that I am prepared to manufacture at short notice any pharmaceutical preparation which physicians may be unable to procure elsewhere.

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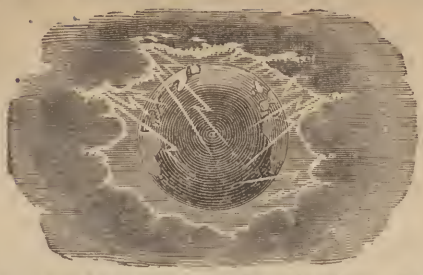

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No. 250 SOUTH NINTH STREET, PHILADELPHIA.

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An Indispensable Remedy in cases of Dyspepsia, Marasmus, Consumption.

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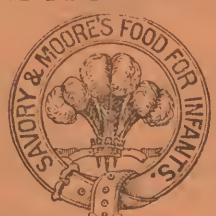

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OF THE

UNIVERSITY OF LOUISIANA,

NEW ORLEANS.

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"On behalf of the manufacturers, Mr. Andrew Blair presented to the College a new Drug Mill, made by the Enterprise Manufacturing Co. of Pa., and a discussion was entered into upon the merits or demerits of the Mill, in the course of which Professor Proctor remarked that he had found it to answer very well for general work, and considered it an improvement on SWIFT'S."

"Mr. Andrew Blair spoke at length in favor of the Mill, and believed it to be the best that had yet been contrived for the purpose of grinding drugs. He exhibited five specimens—Sassafras, Gentian, Senna, Coriander and Liquorice Root—which were ground with just ordinary care, and not sifted. The results spoke practically and favorably for the work of the new comer, and it looks as if the time was approaching when the retail druggist could be independent of the drug miller, and furnish many of his own powders without leaving his store."

"Professor Maish considered the Mill the best that had yet been devised, and thought that one of the prominent advantages was the facility with which the internal working parts could be viewed by simply turning one screw."

OPINIONS.

P. W. BEDFORD, Professor New York College of Pharmacy, writes: "I exhibited your Mill at a meeting of our College. The number present was small, but they appeared much pleased with the work of the Mill. I ground some Seeds, Barks and Roots in their presence, and all expressed their views favorably. I sold two of them at once, and hope to sell still more from it."

P. W. BEDFORD, Professor New York College of Pharmacy, further writes: "Having given your new Drug Mill still further trial, I find it answers an admirable purpose for every want of the druggist in which an iron mill can be used. Have tried various kinds of drugs which may be called representative in their character. I can say, that in the rapidity of its work, and the fineness of the product as compared with the labor expended, it excels any of the other drug mills in the market."

J. F. HANCOCK, Professor Baltimore College of Pharmacy, writes: "I am very much pleased with your Drug Mill, and my young men, who have to work it, are delighted with the change from Swift's Mill. Will shortly give you a lengthy opinion regarding the merits of your Mill, sharply, but sincerely expressed, so that, if necessary, you can use it to your advantage."

JOSEPH P. REMINGTON, Pharmacist, Philadelphia, writes: "The Mill which you have produced for grinding drugs, possesses, in my judgment, many advantages over the mills usually seen in the market. Prominent among these may be mentioned a more accurate adjustment of the working parts, with greater power. The ease with which the whole internal chamber may be exposed to view, and the drawer at the bottom of the Mill communicating directly with the delivery spout, effectually prevents the loss which constitutes the great objection when irritating or dusty powders are to be made."

T. H. PATTERSON, Secretary Chicago College of Pharmacy, writes: "From the examination given your Mill by the members of the College at their meeting, would say that it met with the approval of those present."

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