



stated by Dr. Beaumont, are here confirmed. Thus, M. Bernard found that during fasting the mucous membrane of the stomach is pallid, folded on itself, contains but little blood, and is coated with a thin layer of neutral or slightly alkaline mucus; but directly upon the introduction of any foreign matters, especially digestible ones, into the stomach, its mucous membrane becomes turgid with blood, assumes a more marked degree of sensibility, and the layer of mucus is detached, being displaced by an abundant flow of acid gastric fluid; at the same time manifest movement of the stomach itself takes place. This flow of gastric or digestive fluid, which only takes place when its secretion is solicited by the presence of food, or of some mechanical or chemical irritation, may be influenced by many circumstances. Thus M. Bernard found that although, as was observed by Blondlot, a slight mechanical irritation applied directly to the mucous surface of the stomach excites at once an abundant flow of gastric fluid, yet if this irritation be carried beyond certain limits, so as to produce pain, the secretion, instead of being more abundant, diminishes or ceases entirely, while a ropy mucus is poured out instead, and the movements of the stomach are considerably increased; the animal, at the same time, appears ill at ease, is agitated, has nausea, and, if the irritation be continued, actual vomiting. Bernard has often, in such cases, observed bile flow into the stomach, and escape through the fistulous opening. He has also found similar disorders of the functions of the stomach to result from violent pain in other parts of the body; the process of digestion in such cases being suspended, and sometimes vomiting excited. When acidulated substances, as food rendered acid by the addition of a little vinegar, were introduced into the stomach, the quantity of gastric fluid poured out was much smaller, and the digestive process consequently slower, than when similar food, rendered alkaline by a weak solution of carbonate of soda, was introduced; this effect of alkalies in increasing the secretion of gastric fluid was noticed also by M. Blondlot. If, however, instead of a weak alkaline solution, carbonate of soda, in crystal or in powder, was introduced into the stomach, a large quantity of mucus and bile, instead of gastric fluid, flowed into the stomach, and very often was succeeded by vomiting and purging. When very cold water or small pieces of ice were introduced into the stomach the mucous membrane was at first rendered very pallid, but soon a kind of reaction followed, the membrane became turgid with blood, and a large quantity of gastric fluid was secreted. If, however, too much ice was employed, the animal appeared ill, shivered, and digestion, instead of being rendered more active, was retarded. Moderate heat applied to the mucous surface of the stomach seemed to have no particular action on digestion, but a high degree of heat produced most fatal effects; thus the introduction of a little boiling water threw the animal at once into a kind of adynamic state, which was followed by death in three or four hours; the mucous membrane of the stomach was found red and swollen, whilst an abundant exudation of blackish blood had taken place into the cavity of the organ. Like injurious effects, to a greater or less degree, followed an introduction of other irritants, such as nitrate of silver or ammonia, the digestive functions being at once abolished, and the mucous surface of the organ rendered highly sensitive.—KIRKE'S Report in *Ranking's Abstracts*, vol. iii.

4. *Absorption of Narcotic Substances by the Lymphatics.*—It has been stated by Emmert, Behr, and others, from the results of experiments, that the lymphatic vessels either do not absorb narcotic poisons at all, or to so slight an extent, that no poisonous effects are produced. Henle explains this by considering that narcotic substances in solution are certainly received into the lymphatics by the process of endosmose, but that the walls of the vessels are at once paralyzed thereby, and rendered unable to propel the poisonous solution onwards. BISOHOFF,\* however, has recently performed some experiments on dogs, the results of which would seem to show that narcotic poisons are really taken up by the lymphatics, conveyed by them into the blood, and exert their poisonous effects on the system. Having tied the abdominal aorta below the origin of the renal artery, he inserted into a wound of the leg a strong solution formed of equal parts of nitrate of

\* Schmidt's Jahrbucher, No. iv. 1846, p. 6.

## MEDICAL JURISPRUDENCE AND TOXICOLOGY.

	PAGE		PAGE
61. Retention of a Dead Fœtus. By Dr. Pickett. - - -	532	66. Hydrate of Magnesia as an Antidote in Poisoning. By Schuchardt. - - -	537
62. Duration of Human Preg- nancy. By Dr. Simpson. -	532	67. Poisoning by Strychnia. -	537
63. Nicotine. - - -	533	68. On the Action of Hydrated Peroxide of Iron. By B. M. Fehling. - - -	537
64. Rape on an Idiot. - - -	533	69. Poisoning by Ink. - - -	538
65. Hydrate of Magnesia and Hy- drated Oxide of Iron as Anti- dotes for Poisoning with Arsenic. By M. Schroff. - - -	536	70. Cases of Poisoning. By Che- vallier. - - -	538

## AMERICAN INTELLIGENCE.

## ORIGINAL COMMUNICATIONS.

On Trismus Nascentium. By Ni- cholas Meriwether, M. D. -	539	treated by the Starch Bandage. By George M. Dewey, M. D. -	540
Five Fractures occurring in a Child at the same time; successfully		Repeated Hourglass Contractions of the Bladder during Lithotom- y. By J. L. Peirce, M. D. -	540

## DOMESTIC SUMMARY.

Yellow Fever in Philadelphia in the Summer of 1853. By Dr. Wilson Jewell. - - -	543	Death from Chloroform. By Dr. De Wolf. - - -	552
Pulsating Tumour of the Occiput. By Dr. John Neill. - - -	548	Onanism in a Boy Seven Years Old. By Dr. A. Garwood. -	553
Excision of the Knee-joint for An- chylosis. By Dr. Gurdon Buck.	550	Excision of the entire Ulna. By Prof. Carnochan. - - -	554
Radical Cure of Hydrocele. By Prof. Willard Parker. - - -	551	Lobelia Inflata in Traumatic Te- tanus. By F. Knowles, M. D. -	554
Common Salt as a Remedy for Intermittent Fever. By Dr. J. C. Hutchinson. - - -	551	Felt Splints. By Prof. Frank H. Hamilton. - - -	554
		Aphonia cured by Electro-Mag- netism. By Dr. F. K. Bailey. -	555
OBITUARY RECORD.—THOMAS G. MOWER, M. D. - - -			555

close connection between the extremities of these tubules and the Malpighian bodies, yet he agrees with Bidder, Gerlach, and others, in denying that these vascular tufts lie bare within the cavity of the dilated portion of the tubules. Although he does not recognize the existence of the membranous partition between the tuft and the cavity, described by Bidder, yet he states that the cavity is freely lined, as also is the surface of the vascular tuft covered, by an abundance of epithelium, so that the tuft lies embedded in a mass of epithelial cells (as described by Gerlach).\* He mentions that in the kidneys of the animals (lizards) which he examined, ciliary epithelium, in a state of active movement, was present along the whole length of the urinary tubules, with the exception of at their exit from the gland, and just where they dilated into their terminal extremities within the substance of the organ.†—*Ibid.*

6. *Absence of Corpus Callosum.*—M. PAGET has communicated to the Med. Chirurg. Society, the details of a case in which the middle portion of the fornix and the whole of the septum lucidum were absent, and in which, in place of the corpus callosum, there was only a thin fasciculated layer of white nervous matter, 1-4 inch in length, but of which the fibres extended into all those parts of the brain into which those of the healthy corpus callosum can be traced. Behind the imperfect corpus callosum, the optic thalami, the middle commissure (which was very large), the posterior commissure, and the pineal gland lay exposed after removal of the velum. The lateral parts of the fornix and the rest of the brain were quite healthy. The patient was a servant girl who died of pericarditis. She had displayed during life nothing very remarkable in her mental condition beyond a peculiar want of forethought and power of judging of the probable event of things. Her memory was good, and she possessed as much ordinary knowledge as is commonly acquired by persons in her rank of life. She was of good moral character, trustworthy, and fully competent to all the duties of her station, although somewhat heedless; her temper was good, and disposition cheerful. From these facts Mr. Paget is inclined to regard the functions of the corpus callosum as connected with the highest operations of the mind, especially as in the few cases in which it has been found deficient, or altogether absent, the mind has possessed a moderate average power and capacity for knowledge, and all the lower functions of the nervous system have been perfectly discharged.—*Ibid.*

## ORGANIC CHEMISTRY.

7. *Presence of Copper and Lead in the Bile.*—Last year Bentozzi made the discovery that copper is contained, in a tolerably large amount, in brown biliary calculi; he was never able, however, to detect this metal in the bile itself. By the announcement of Bentozzi's discovery HELLER was induced to pay attention to the subject, and after a careful examination was led to a similar conclusion; namely, that the coloured biliary calculi contain a large amount of copper.‡ The method adopted by Heller for the detection of copper was more simple than the one employed by Bentozzi. Heller directs several of the darkest calculi to be rubbed together into a rather coarse powder, and then burned in a platinum crucible previous to the addition of nitric acid. The ash of the combustion is to be treated with a little nitric acid to remove any remains of carbon, then dissolved in water acidulated with nitric acid, neutralized, and finally tested for copper by the ordinary reagents.

\* Last Report, p. 309.

† The high interest of the above papers must be the excuse for the length at which they have been noticed. They contain much other valuable matter, which it did not appear necessary to notice here, but which will well repay a careful perusal. [Since the above was written, M. Hyrtl, in a strange paper on the "Physiology of the Urinary Secretion," (translated in the Medical Times, April 4, 1846,) denies absolutely the existence of any connection between the Malpighian body or the capsule covering it, and the extremity or any part of the urinary tubule.]

‡ Archiv. fur Chemie und Mikroskopie, vol. ii. p. 238.

## ERRATUM.

Page 257, 20 lines from top, for "two-third inches," read "*two to three inches.*"

16. *On the Internal and External employment of Nitrate of Silver.*—By Dr. HELLER. The different views which have been entertained by writers in regard to the remedial action of nitrate of silver, especially when used internally, have induced Heller to make some investigations on the subject.

Although the nitrate of silver is generally well known and highly esteemed as an external application, yet respecting its efficiency as an internal remedy various opinions are entertained.

Experience has proved the good effects resulting from the local employment of this substance in affections of mucous membranes, of the eye, of the organs of generation, and from its use as an escharotic in various diseases; and the explanation of all this is very easy when we examine more closely into the effects produced by the nitrate of silver in such cases. For example, this salt of silver has the property of coagulating albumen, and with it cellular substance generally, and of precipitating extractive matters: it, in fact, destroys the vitality of these substances, and renders them useless to the system; wherefore they are separated and removed from it as so much waste and dead matter. This salt also decomposes the chloride of sodium, which all secreted fluids contain, as also the phosphates: an insoluble chloride of silver and an insoluble phosphate of the oxide of silver being thrown down. Chloride of sodium and phosphates are contained in all secreted fluids, such as mucus, pus, tears, synovia, lymph, &c.: the fluid also within cells, and that in which the cells themselves float, contain albumen, chloride of sodium, phosphates, and extractive matter. All these various substances are precipitated at once by a solution of nitrate of silver. The cells become completely destroyed and rendered useless to the system, wherefore they are separated from it and cast off. The frequent repetition of this destructive process to the epithelium-cells of a mucous surface by the repeated application of nitrate of silver induces a more rapid and abundant reproduction of these cells, together with an equally rapid and abundant separation of them, and by this means tends to keep in the background the disease which is seated in this membrane. In the fluid which is thus abundantly poured off from a mucous membrane, after the application of nitrate of silver to it, may be found epithelium, chloride of silver, phosphate of the oxide of silver, together with precipitated albumen and extractive matter: we can, therefore, with certainty affirm, that the nitrate of silver does produce some effect by its local application.

When used as an internal remedy, however, nitrate of silver behaves in a totally different manner. Heller performed several experiments with the view of determining to what amount the silver, when employed internally, is taken into the secretions and excretions. He carefully examined the blood, urine and feces of patients who for several months had been taking nitrate of silver in large quantities. He also tried what effect was produced by the direct addition of nitrate of silver to gastric fluid. The results of these experiments are as follows:—

In several cases of epilepsy nitrate of silver was administered internally in doses varying from three to ten grains daily; thus one girl took ten grains daily, and a lad, aged 13, twelve grains daily for three months. In this latter case, therefore, about two ounces and a quarter were administered altogether, yet without the disease being in the slightest degree mitigated, or any other effect on the system being produced. In none of the cases did the skin become in the least discoloured. The blood was several times examined in many of the cases; it presented the character commonly found in the blood of epileptics, especially an increase in the quantity of albumen; but in none of the examinations was there a trace of silver found.

The urine, also, contained not the slightest trace of silver; it was clear and without sediment, possessed its ordinary quantity of chloride of sodium, and contained no ammonia. Since the urine, therefore, contained chloride of sodium, no silver could have been present in it, else would the chloride of silver have been precipitated, and detected as such, which it was not; and since the urine contained no ammonia, which is a solvent of chloride of silver, so also could none of the chloride of silver have been retained in solution; moreover, after combustion and careful examination of the ash, not a trace of silver could be detected. The feces, Heller says, contain, in the form of chloride, the whole of the silver which has been administered. To prove this, nothing more is necessary than that the feces should

whose gall-bladder was small and contracted, and contained a very inconsiderable portion of viscid bile—results which could not obtain were black vomit the product of the liver.

5. The pylorus, in several instances, has been found closely contracted, and yet the stomach contained black matter. •

6. In external appearance, the matter of the black vomit differs greatly from dark-coloured or black bile. The latter is of an homogeneous nature—smooth and glossy—and when examined closely, is found to be of a really black or deep green colour; whereas the former is, as already stated, in the majority of cases, a compound of flaky or granular particles, of a dark brown rather than black colour, floating in a thin, pale, or reddish fluid. Even when the black vomit is of a thick and tarry appearance, it may be distinguished from it by other signs.

7. When mixed with water, the two substances produce dissimilar effects. The bile unites readily to the water, imparting to it a greenish tinge; while the grounds of the black vomit float on the surface of the liquid in the same manner as bran deprived of its mucilage, or rather as mahogany sawdust.

8. If the darkest coloured bile be spread thinly over a white surface, such as the skin, it loses the colour it had in its accumulated state, and appears of a yellowish-green colour; but if the black vomit is treated in the same way, it retains its black, or dark brown appearance.

9. When a bit of paper or white muslin is dipped in these two substances, it is very differently coloured by them. The bile imparts a greenish, the black vomit a reddish tinge to it. We have seen that this fact was long ago pointed out by Warren. Since his time it has been referred to by Monges, Robertson, and others.

10. When bile is added to black vomit, it alters its nature so much as to give it an appearance different from that which it had before.

11. The black vomit differs very much from any mixture that can be made of dark-coloured bile with any of the fluids found in the stomach or intestines.

12. The bile and black vomit differ as to taste. The former always imparts a bitter taste, of greater or less intensity; the other is more or less insipid or acid.

13. The black vomit is sometimes found to adhere to and line those portions of the mucous membrane that are inflamed or congested, leaving the other and healthy parts uncovered. In some cases none of the fluid is found loose in that organ, though a large portion of the internal surface is thickly smeared over in that way. When scraped off, it cannot be made to adhere again in the same manner; from which we may infer that it has exuded from the vessels to which it is attached, and has not been poured into the stomach from some other organ. Bile is never found to adhere in this way to the lining membrane of the stomach, and especially to select in preference

its inflamed portions. It tinges, and mixes with, the mucus secreted by these parts.

14. Although in cases in which black vomit has been discharged freely, or in which, though not ejected, it has been found after death, dissection reveals an engorged state of the capillary vessels;<sup>1</sup> yet in very many, probably the majority of instances, the engorgement of the vessels appears to be lessened or removed by the formation and ejection of the matter. Such was the opinion, long ago expressed, of Dr. Physick.

"The secretion of black vomit," as is remarked by that eminent surgeon, "appears to be one of the most common modes in which violent inflammation of the stomach has a disposition to terminate. Death, however, in general, takes place before it entirely disappears. I have seen many cases which show that the inflammation is diminished by the secretion, of which it will be sufficient to mention the following: On opening a stomach, one-half of it was coated with adhering black matter, while the other half was free from it; on scraping it off clean, and comparing the part underneath with the other half of the stomach which had not secreted any black matter, the difference in the degree of inflammation was very striking, being much the best in the part which had been covered with the black substance. In some cases, where the vomiting of black matter had been considerable in quantity, or continued for several days, the inflammation was found very faint indeed."<sup>2</sup>

This tendency of black vomit to relieve the engorgement of the capillaries has been noticed by very many other writers.<sup>3</sup> Need it be remarked, that the effusion of bile, its ejection from, or its accumulation in the stomach, has never produced any effect of the kind?

15. Facts innumerable may be adduced to show that the black vomit is formed in the stomach itself, and is not poured in that viscus from the liver, or any other organ. "The colouring matter may be, and often is," as Dr. Dickson remarks, "seen in the vessels of the stomach itself, from the mouths of which it may be pressed without difficulty by the finger."<sup>4</sup> The same fact has been recorded by Dr. Arnold, of Savannah.<sup>5</sup> Dr. Evans relates a case in which a large quantity of black vomit was ejected and a great deal found in the stomach after death. "Its mucous coat [of the stomach], washed and examined with care, was of a gray colour throughout, except that it was speckled with innumerable black spots, produced by this black matter, stopping up the mouths of the exhalant vessels, and easily picked out with the point of a needle."<sup>6</sup> Analogous statements are made by Ffirth, p. 37; Kelly, *Am.*

<sup>1</sup> Harrison, N. O. Journ. ii. p. 139; Hayne, Charleston Journ. vi. p. 628; Ffirth, 36; S. Jackson, 78.

<sup>2</sup> Med. Reposit. v. pp. 131, 132.

<sup>3</sup> Rush, Fever of 1793, p. 94; *Ib.* Works, iv. p. 44; Townsend, p. 191; Copland, iii. pp. 143, 161; Parrish, Med. Museum, iii. pp. 168, 188, 190; Lowber, *ib.* v. p. 19; Blair, p. 92; Harrison, N. O. J. ii. p. 139, note; Rochoux, p. 355; Waring, p. 52; S. Jackson, p. 78.

<sup>4</sup> Med. and Phys. Journ. N. S. v. p. 209; Essays, p. 356.

<sup>5</sup> Amer. Journ. iv. p. 318, N. S.

<sup>6</sup> Pages 33, 216, 249-275.



*Journ.* xiv. N. S. p. 382; Arnold, of Jamaica, p. 38; Gilkrest, *Cyclop.* ii. pp. 274, 275; R. Armstrong, 178. "The veins of the stomach," says Dr. R. Jackson, "were generally turgid; the villous coat was abundant at some places, loose and in the act of separating at most; the surface underneath the separated villi was streaked with bright or dark red, or were studded with clusters of points not unlike measles; most numerous at the upper orifice, but not confined to it. In some instances, the mouths of canals were visible at different points of the interior surface, yielding a dark-coloured fluid by pressure," i. p. 79.<sup>1</sup> Dr. Physick, long ago, pointed out that cases occur in which the inner coat of the stomach is as black as the black vomit, and that, in many such cases, no black matter is found in that viscus; an effect evidently due to the retention of the matter in the vessels, inasmuch as the dark colour of the membrane is lessened when the matter is poured out in the stomach.<sup>2</sup>

16. And, finally, the black matter, or one similar to it in its physical characters, and doubtless in nature, has been found at times to be formed in or to proceed from parts where bile in substance cannot, by any possibility, penetrate, and whence it cannot be ejected. It has not unfrequently happened, for example, that the application of a blister, especially in the advanced stage of the disease, has been followed by a copious exudation of fluid resembling in all respects the matter ejected from the stomach. Dr. Monges, a competent authority in such matters, states his having seen this in 1820.<sup>3</sup> A similar fluid has often been found in the bladder.<sup>4</sup> It has been found exuding from the nostrils, fauces, tongue, eyes, ears.<sup>5</sup> It has been discovered in the pleural cavity.<sup>6</sup> Dr. Merrill mentions its having been seen in the peritoneum. The following history of a case which occurred in the practice of the late Dr. Monges, during the epidemic of 1820 in this city, and which I, myself,

<sup>1</sup> The yellow fever is not the only disease in which the above phenomenon presents itself. It occurs sometimes in fatal cases of hematemesis, or in complaints which have been attended with discharges of blood by the stomach or bowels, or both. Portal, in his essay on Melæna, published originally in the *Memoirs of the Medical Society of Emulation*, of Paris, and since inserted in the second volume of the collection of his *Essays*, relates several highly interesting cases of this disorder in which the patients vomited and voided per anum a greater or less quantity of black fuliginous fluid. This, the able and celebrated physician ascertained to be not bilious or atrabilious matter, but altered blood. On dissection of three of these cases, the fluid could be easily forced out of the engorged capillaries. It may not be uninteresting to state, that one of these individuals was no less a personage than the Comte de Vergennes, Minister of Foreign Affairs under Louis XVI., and to whom this country was so much indebted during our revolutionary struggle.—*Mémoires sur Plusieurs Maladies*, ii. pp. 138, 163, 170, 208.

<sup>2</sup> *Loc. cit.* p. 131.

<sup>3</sup> North Am. Journal, ii. p. 59.

<sup>4</sup> Deveze, p. 66; Palloni, *Edin. Journ.* ii. p. 538; Lewis, *New Orleans Journ.* iv. p. 159; Ruzf, p. 19; Kelly, *Am. Journ.* xxiv. N. S. p. 383; Harrison, *New Orleans Journ.* ii. p. 138; Nott, *Am. Journ.* ix. N. S. p. 280.

<sup>5</sup> Lewis, *New Orleans Journ.* i. p. 300; *Ib.* iv. p. 159.

<sup>6</sup> Parrish, *loc. cit.* iii. p. 142; Merrill, *New Orleans Journ.* viii. p. 6.

witnessed, presents an apt and striking illustration of the last-mentioned fact. The patient was an old lady, seventy years of age, who resided at a considerable distance from the infected district. She had for some time previous been confined to her house, and had seen no one who had mixed with the sick, and consequently, had in no way been exposed to any of the real or reputed causes of the disease, of which, indeed, her attack presented few of the ordinary pathognomonic phenomena. She was seized very early in the morning with violent colic, attended with fever, great tenderness over the abdomen; flushed face, &c. She was bled at 10 o'clock; at 11, vomited a large quantity of coffee-ground matter, and died in about twelve or fifteen hours from the commencement of the attack. The next morning her body was examined by myself in the presence of several of the most respectable and experienced physicians of the city, Drs. Otto, Hewson, Gallagher, and Monges, who all coincided in the opinion that the matter vomited, as also that which continued to be discharged from the nostrils, bore all the characteristic appearances of that ejected in the yellow fever—a disease with which they were all perfectly familiar.

“The stomach, as well as the intestines were found to contain a large quantity of a similar substance. The cavity of the peritoneum being likewise filled with a large portion of it, we at first suspected the existence of an opening in the intestines, by which an effusion had taken place. After a careful and minute examination, however, no such opening was discovered. Our attention was now directed to the condition of the peritoneum itself, which was highly inflamed. It was, moreover, found that the substance in question exuded from its surface—the membrane, in many places, especially the portion of it which covers the liver, being coated so thickly with the grounds that these could readily be scraped off with the back of a scalpel.”<sup>1</sup>

With the preceding facts and considerations in view, there can be no difficulty in perceiving the impropriety of regarding the black vomiting as vitiated bile, and in acknowledging its being formed in the stomach itself. Nor is it less certain, that an attentive examination of the subject will lead to a rejection of the opinion which ascribes this fluid to the secretory action of the diseased vessels of the stomach and intestines.

1. Before we can adopt this opinion with any show of reason, it would be necessary to explain satisfactorily how the same fluid, or fluids bearing so strong a resemblance to each other as to be regarded as identical by the most competent judges, can be secreted by the capillaries of parts differently organized, and which in their natural state, or during the course of ordinary diseases, produce fluids of dissimilar appearance or nature. If the black matter ejected from, or found in the stomach and intestines is the result of a secretory process, the same kind of fluid issuing, not only from other portions of the mucous surface, but from the skin and serous membranes must be a secretion too. And yet these parts differ widely from the former, both as regards their organization and the nature of their ordinary products in health and disease.

<sup>1</sup> Monges, *op. cit.* p. 60.

2. Dr. Nott has well remarked, in objection to the theory before us, that the black vomit is most commonly seen in little particles or masses of various magnitude, which could not pass through a secretory capillary.<sup>1</sup>

3. It is admitted that the villous coat and the glands situated beneath it—the intervascular cells and their nuclei—are alone endowed with the secretory power in the stomach. Now facts will be adduced as we proceed, which show that the black vomit is unconnected in its formation with those parts; while by the advocates of the theory of secretion, it is referred to, and, as we have seen, has been detected in capillary vessels which are not endowed with any power of the kind.

4. Such a detection affords no argument in favour of the theory of secretion; for, were the black vomit the result of such a process, it could not be found in the vessels of the circulatory system; for secretions, as is remarked by Dr. Michel, “are never seen in the bloodvessels of a part, not even with the aid of a magnifying power. The function is the result of the vital processes of the parts above mentioned, which elaborate the materials they derive from the blood into different products, then burst and discharge them.”<sup>2</sup> It is evident, therefore, that if black vomit were the product of an operation of the kind, it would not be found collected in the capillary vessels, or that it would not be the means, by its discharge, of relieving the engorgement of these.

5. The very large, and even extraordinary quantity in which the black vomit is sometimes ejected, would alone constitute a strong objection to the theory of secretion; as we can find nothing to be compared to it in the healthy or diseased operation of any regular glandular or secretory structure.

6. The difficulties by which the theory of secretion, as connected with the formation of black vomit, is encompassed, are not removed by calling the fluid a sanguineous secretion, as the process by which such a secretion is produced, and the nature of the parts in which it is elaborated, cannot differ much from what obtains in regard to products having less or none of the characteristics of blood. Neither in the one case nor in the other can the fluid be formed by the capillaries. It may be remarked that, by the advocates of the secretory nature of the black vomit, no stronger reasons in favour of their views have been assigned than that the fluid differs somewhat from ordinary blood, and that it often presents a thick consistence and adheres to the inside of the stomach, forming a black coating of considerable thickness, and that it is found after death collected in the capillary vessels of the affected organ. But, besides that, it can be shown that the black matter is formed under circumstances precluding all idea of its being the result of a secretory process; the facts alluded to may be more readily explained by referring the formation of the fluid to the operation of other agencies.

<sup>1</sup> *Loc. cit.* p. 281.

<sup>2</sup> Charleston Journ. viii. p. 347.

7. The matter of the black vomit bears no resemblance to any known product of glandular action.

8. It is only met with in that state of the system, and in that stage of the disease, when the secretory process in all other parts is generally or often suspended, or greatly impaired.<sup>1</sup>

9. It not unfrequently happens, that the stomach or duodenum contains coffee-ground matter, while the intestines are more or less filled with coagulated blood.

10. In some instances, discharges of black vomit and of blood alternate—in others, again, the black vomit is found mixed with blood in the stomach after death; in another set of cases, coagula of blood are found in the intestines; “the surface having the appearance of the common black matter, while on cutting into them the centre is found to consist of red solid coagulum.” “In such cases,” as Dr. Monges remarks, “in order to adopt the opinion of secretion, we must believe that the same vessels occupied in the secretory process, afforded at the same time passage to a portion of common blood; for we can hardly admit that the mucous follicles are the organs secreting the black matter.”<sup>2</sup>

11. Nor could such a product as the black vomit—supposing it to be a secretion—be thrown out from a surface as little altered in appearance as is the mucous membrane in a large number of instances. There is undoubtedly diseased action going on in the stomach—but usually this amounts to disturbance of function, though not of structure—a condition of parts perfectly compatible with the formation of black vomit. “It does not,” as is remarked by Dr. Michel, who has called attention to these facts—“it does not by any means imply organic lesion, without which destruction of the tissues themselves to a considerable extent, it could not exist as a secretion; for I believe that a *secretion* of such appearance as the vomit, would plainly betoken a state of disease reaching even unto sphacelation.”<sup>3</sup>

Enough has now been said to prove beyond doubt that the black vomit is neither altered bile nor the result of a secretory action on the part of the gastro-intestinal capillary vessels. In view of this, and taking into consideration various facts to which attention is next to be called, we shall be led to the conclusion that it is nothing but blood in a peculiar state of alteration. That it is the product of the stomach itself, when ejected from that organ, or of other surfaces whence it may proceed—the intestinal, urinal, pleural, peritoneal, &c. has been shown. That the black vomit, or a fluid like it, proceeds from the capillary vessels of these parts; that it may be, and has been pressed out of those vessels after death, and is often seen then and during life oozing out of the capillaries of parts accessible to sight, as the fauces, nostrils, skin, eyes, are facts, which, as we have seen, are too well established to be denied, and must of themselves go far to prove the sanguineous nature of the fluid,

<sup>1</sup> Lewis, N. O. Journ. iv. p. 159.

<sup>2</sup> *Op. cit.* p. 61.

<sup>3</sup> *Op. cit.* viii. pp. 347, 348.

especially when we bear in mind the reasons assigned for rejecting the idea of its being the product of a secretory action of those vessels. For, if we refuse to admit that the fluid issuing from or pressed out of these has been secreted by them, we have no alternative than to regard it as blood; the only substance contained in these, whether in health or disease. To the same conclusion we naturally arrive when we take into consideration the fact that the black vomit is the product, in some cases, of gastric inflammation occasioned by the ingestion of acrid poisons, or by other causes; and we know that in inflammation resulting from such agencies—or indeed from any agency—the vessels are engorged with blood, and nothing else; and that therefore what proceeds from them, whatever peculiar appearance it may assume, must be blood also. The same must be true as regards the contents of the capillary vessels in inflammation or congestion of the stomach or other parts in yellow fever. If we find these filled with red or dark fluid, and if their engorgement and dark colour are relieved by the discharge of that fluid, we may safely infer that the latter was blood, for hemorrhage is one of the common modes in which inflammation has a tendency to terminate; and if the red or dark fluid which occasions the relief in ordinary cases is admitted to be of the nature in question, there can be no reason to doubt that, when the effect is obtained in yellow fever, the fluid discharged is of similar kind. Nor is this all. We have seen that black vomit very generally adheres to those portions of the mucous membrane of the stomach or intestines that are inflamed or congested, or from which it was effused, leaving other parts uncovered. In this, we have a striking proof of its analogy to undisputed blood, which exhibits a like disposition to adhere to inflamed surfaces from which it had exuded.

Again: we have seen that vomiting of black matter sometimes alternates with the discharge of pure blood, the ejection of black vomit preceding that of blood, or the reverse; that while the stomach or duodenum contains the former, the intestines are filled with coagulated or fluid blood; that, in some cases, portions of fluid, more frequently of coagulated blood are found, the surface only of which presents the granular appearance.

These facts cannot fail to point out the sanguineous nature of the coffee-ground matter constituting the black vomit; inasmuch as it is not probable, as already remarked, that two distinct fluids—the one blood, and the other a secretion—should be thrown out from the same vessels in rapid succession, or simultaneously, or that the one should be smeared over by the other. Dr. Lewis remarks of the two fluids: “They are thrown up together; run into each other by perceptible degrees, just in proportion to the quantity of blood exhaled, or as it is intermixed with the secretions of the stomach and intestines.”<sup>1</sup> The same writer states the following fact: After remarking that in a large proportion of cases unchanged blood is vomited, he adds: “We

<sup>1</sup> New Orleans Journ. iv. p. 159.

have seen three brothers vomiting at the same time; one of them, slightly coagulated blood, resembling wine lees; another, fresh blood; and the third, the coffee-ground vomit. The young men were all purging blood at the same time."<sup>1</sup> Is it probable, that while in these three cases, the intestines were giving vent simply to blood, the stomach in each should have furnished a distinct fluid? More natural is it to conclude, that this fluid in all was fundamentally the same. It may be stated, in addition, that blood which when ejected from the stomach presented its usual appearance, has been occasionally found to assume the peculiar characteristics of black vomit after being exposed some time to the influence of the atmosphere.<sup>2</sup> Let me remark besides that black vomit imparts to muslin or paper a red colour, very similar to that resulting from dark blood.

That black vomit must be regarded as consisting in great measure of altered blood is still more satisfactorily proved by the fact that the latter may be, and has often been, converted artificially into a fluid bearing the most striking analogy to that ejected in the closing stage of the yellow fever. This fluid, as we have seen, contains usually, if not always, a free acid—the hydrochloric—the presence of which is evidenced by the taste as well as by the effervescence produced with the alkaline carbonates and other chemical tests. Now, if we impart to pure blood the same property by the addition of the aforesaid, or indeed of any other acid, we shall find it to assume, to a greater or less extent, the characteristic appearance of the black vomit. These results were pointed out long ago. Dr. Cathrall, the account of whose experiments has now been before the public more than half a century, while advocating a theory adverse to the one contended for, admits that when blood is combined with such acids, “and the mixture (is) diluted with an infusion of green tea, it resembles in many respects the black vomit;” and that “the odour arising from this combination so much resembles that arising from black vomit which had been kept for several years, that he could hardly distinguish one from the other.”<sup>3</sup> The same experiment, slightly varied, has since been performed, and with similar results, by Dr. Lyon,<sup>4</sup> by Dr. Hope,<sup>5</sup> by Dr. Stevens,<sup>6</sup> by Dr. Riddell,<sup>7</sup> and others.

Dr. Nott, of Mobile, took a few drachms of blood from the heart of a patient dead of yellow fever, and added to it four or five drops of muriatic acid, diluted with a drachm or two of water, and shook them well together; the black colour was produced instantly.

“The same experiment was tried repeatedly on the blood of yellow fever patients, and on that drawn from a patient with pleurisy by cups, and the effect was invariably the same. Any one,” he adds, “wishing to form a cor-

<sup>1</sup> *Ib.* iv. p. 159; *Ib.* i. p. 300.

<sup>2</sup> Rochoux.

<sup>3</sup> Essay on Black Vomit, p. 26. See Trans. of Philos. Society.

<sup>4</sup> Lond. Med. and Phys. Journ. for 1828, N. S. iv. p. 100.

<sup>5</sup> Cited by Dr. Lyon.

<sup>6</sup> On the Blood, pp. 360, 422.

<sup>7</sup> New Orleans Journal, ix. p. 420.

rect idea of black vomit, has only to treat blood in this way, and add a little gum-water or flaxseed tea to represent the mucus of the stomach, and his curiosity will be gratified; and no one can tell the artificial from the genuine black vomit."<sup>1</sup>

"I once, with Dr. Thomas Hunt, of this city," says another distinguished American writer, Dr. Harrison, of New Orleans, "performed the following experiment: A man was brought into the dead-house, while we were there. Upon examination, there was no black vomit in his stomach, but a whitish acid-smelling liquid, amounting to about half a pint. Into this stomach, containing this liquid, some blood from the vena cava was poured. At first, we thought the experiment had failed, and we returned to other investigations. Upon examining the fluid, however, after the lapse of ten or fifteen minutes, it was impossible to distinguish it from specimens of black vomit with which we contrasted it."<sup>2</sup>

A late writer on the yellow fever, Dr. Blair, remarks that, "blood allowed to drop, as in epistaxis, into water acidulated with muriatic acid, forms a very tolerable specimen of some descriptions of black vomit."<sup>3</sup> Dr. T. Y. Simons, of Charleston, while Professor of the Practice of Medicine in the medical school of that city, in 1833, was in the habit of performing the experiment of making artificial black vomit before the class in attendance.<sup>4</sup> Dr. Michel, of the same city, remarks that, by carefully adjusted proportions of acid, it is possible to obtain all gradations of shades in the blood.

"But what is still more satisfactory in the experiment, the blood immediately curdles, as it were, into little flocculi, hardly dissimilar from those of the black vomit. It is difficult always to produce the coffee-ground sediment, since it is impossible to apportion certain particles of blood to those of the acid, as when it oozes in disease from the tissues. I have, however, been able to produce artificial black vomit so perfectly similar in this respect to that fluid itself, that their identity was at once apparent. During my attendance on a case of hæmoptysis, in which slight mouthfuls of blood were coughed up at a time, I obtained about a drachm, which was added to about six ounces of water with a few drops of hydrochloric acid, which, upon being well shaken, developed all the characteristics of black vomit. It was allowed to rest, when the black flocculi formed the usual sediment."<sup>5</sup>

The same effect is obtained by other acids, by chloride of platinum, weak tincture of iodine, and other substances.<sup>6</sup> Sir William Pym relates that, being attacked with hemorrhage from the fauces (in 1806), this was attempted to be stopped by applications of *spt. terebinth.* Having swallowed much blood and turpentine, he threw it up, and was struck with the resemblance it bore to black vomit.<sup>7</sup>

The change in question, and from the same cause, has been observed under other circumstances. Dr. Carswell, in experiments made several years ago, on

<sup>1</sup> On the Pathology of Yellow Fever, *Am. Journ.* ix. N. S. p. 282.

<sup>2</sup> *New Orleans Journal*, ii. p. 148.

<sup>3</sup> *Yellow Fever of British Guiana*, p. 81.

<sup>4</sup> Michel, *Charleston Journal*, viii. p. 343.

<sup>5</sup> *Ibid.* p. 343.

<sup>6</sup> Riddell, *loc. cit.* p. 420.

<sup>7</sup> *Bulam Fever*, p. 223.

the chemical dissolution or digestion of the walls of the stomach after death, found that, in cases in which the coats of the stomach were dissolved by the action of acids, a peculiar change in the colour of the blood contained in the veins of that organ was found to have occurred. It was observed to accompany the chemical dissolution of the walls of that organ, and though less frequent in its occurrence than the latter, was not less obviously the effects of the same chemical cause, for there was no discoloration of the blood when there was no softening or dissolution of the coats of the stomach; 2, the discoloration of the blood was observed only in the vessels situate near the parts softened, and 3, both changes were, *cæteris paribus*, nearly in the same ratio as to degree and extent.<sup>1</sup> The same able physician elsewhere calls attention to the fact that if we kill an animal—a rabbit, for example—or open it while alive, and during the process of digestion, the fundus of the stomach is always found to contain a quantity of gastric acid; and remarks that, “if arterial or venous blood is allowed to flow into the stomach from some of the neighbouring vessels, so soon as it comes in contact with the digested portion of the food, or that in which the gastric acid is most abundant, its natural colour is almost immediately changed to deep brown or black. The rapidity with which this change takes place in the colour of the blood, as well as the degree and extent to which it proceeds, varies with the strength of the gastric acid and the quantity of the blood on which it is made to act. When,” he continues, “the digested acid fluid, or the gastric acid alone is removed from the stomach in which it was found, and is put into another stomach; or other organ, the vessels of which are conspicuous and filled with blood, this fluid very soon undergoes the same change of colour which we have said takes place when it is poured upon the digested food, and consequently, when in immediate contact with the chemical agent by which this change is effected.”<sup>2</sup>

But this must suffice. Well may it be remarked that the production, in this manner, of a fluid, the properties, colour, and appearance of which are in so remarkable a manner similar to black vomit, certainly constitutes a most ingenious experiment;—one, indeed, so far as ocular demonstration of a general character is concerned, which is perfectly conclusive. (Michel.)

Evidently satisfactory, however, as the closeness of the analogy of the artificial black vomit to the true is, and recognized though it be by individuals—not one, but many, whose familiarity with the characteristics of the latter could not but be a warranty of the correctness of their opinion—it has been denied; and there are not wanting those, among intelligent observers, too, who maintain the possibility of pointing out a difference between the two fluids. In evidence of this dissimilarity, Dr. Kelly remarks that, while the true black vomit has a flaky appearance, “like smoky mica,” the artificial has more of

<sup>1</sup> Rech. sur la Dissolution Chronique ou Digestion des Parois de l'Estomac après la mort. Journal Hebdomadaire, 1830, No. 87, p. 350; No. 91, p. 517; Cycl. of Pract. Med. iii. p. 99.

<sup>2</sup> Cycl. of Pract. Med. article Melanosis, iii. p. 100.



the coffee-ground appearance; the addition of a certain quantity of sulphuric ether to the former will cause it to float on the surface, "having the appearance of a dark cobweb or flake of dead matter;" while, by this means, the artificial is made to sink to the bottom, never more to rise. Nitrate of silver changes the black vomit "to a cream-white colour, every dark speck being removed, while the artificial substance retains its colour, being precipitated to the bottom of the vessel. Besides, if black vomit consisted of blood changed by the action of an acid, it is difficult to account for those cases in which, at the same time that that fluid is ejected from the stomach, pure blood is discharged from the bowels; inasmuch as we can see no reason why the acid secretions acted upon the blood in the stomach and not in the intestines.<sup>1</sup>

Plausible as all these objections may appear, they will be found, on close examination, to be but little calculated to shake our belief on the subject of the close analogy in question. If Dr. Kelly has not been able to obtain the flaky deposit in the artificial black vomit, and found that the latter has more the coffee-ground appearance, Dr. Michel arrived, as we have seen, at different results, and indeed found it "difficult always to produce the coffee-ground sediment." Dr. Not<sup>2</sup> also remarks that, by the addition of acid to blood, we do not obtain coffee-grounds, "which are due to the coagulation and agglomeration of particles." Besides, even were it true, that these coffee-grounds are alone procured, the analogy would still hold; for the real black vomit often presents no other appearance; and all that could be said on the subject is, that by the means in question we can imitate one of the forms of black vomit, and not all. If, in some cases, black vomit is ejected from the stomach, and pure unaltered blood from the bowels, it is, as we shall see, simply because the effused fluid does not encounter in the small intestines, whence those discharges take place, the same modifying agencies it meets with in the stomach. Furthermore, whatever may be the difference as regards the effects of sulphuric ether and nitrate of silver on the real and artificial black vomit, the question of the identity of these is set at rest by the results of microscopical examination; for an account of which we are principally indebted to Dr. Riddell,<sup>3</sup> of New Orleans, and Dr. Michel,<sup>4</sup> of Charleston. In speaking of the appearance exhibited by the artificial substance, the latter gentleman says in conclusion, "the blood was so dissolved as to colour the mass as usual, and the entire aspect was, in every particular, so completely analogous to black vomit, that an observer must inevitably be deceived as to whether he is examining the true or artificial product, for they are microscopically the same." Similar results have been obtained in this city.

But admitting that, notwithstanding all that has been adduced in favour of the opinion that the black vomit consists in altered blood, doubts on the subject may still be entertained, all difficulty must cease when the fluid is submitted to a chemical, and especially to a microscopical examination.

<sup>1</sup> Am. Journ. N. S. xiv. pp. 384, 385.

<sup>2</sup> Ibid. ix. p. 282.

<sup>3</sup> New Orleans Journal, ix. p. 420.

<sup>4</sup> Charleston Journ. viii. p. 344.

For the purpose of settling the first of these questions, the chemical composition of the fluid, and ascertaining how far its peculiar characters depended on the presence of blood, I placed in the hands of my friend, Professor Rogers, of the University of Pennsylvania, several specimens of it, for which I was indebted to the kindness of another friend, Dr. Cain, of Charleston, and subsequently other fresh portions obtained in this city during the prevalence of the epidemic of last summer. The results of this examination I here subjoin, by transcribing, with permission, a letter addressed to me by Professor Rogers:—

PHILADELPHIA, Sept. 12, 1853.

MY DEAR DOCTOR: The four specimens of black vomit which you handed me last autumn for examination, gave the following results:—

They came, you remember, from Charleston, and were labelled by us for reference, A, B, C, D.

Albumen was detected in all excepting A, and this was not examined for it, since it was impossible to free the liquid from the large amount of dissolved colouring matter which rendered it opaque. Sulphuric acid was present in all. Chlorine was present in all. Iron was detected in the ashes of all. Alkaline bases were found in the ashes of all. Lime was detected in all. Hydrochloric acid in the free state. The liquids were quite dilute, as their specific gravities, here given, show. Therefore, as the quantities furnished scarcely admitted of so minute an analysis, they were not examined for phosphoric acid.

Sp. gr. of A, 1.016; B, 1.005; C, 1.004; D, 1.003.

I have just finished testing the liquid you left with me on the 9th, specimens E. and F., and have found in it the following substances:—

Albumen.

Sulphuric acid, in a state of combination.

Chlorine, “ “ “

Alkaline bases, “ “ “

Earthy phosphates.

Iron.

Hydrochloric acid in the free state.

These substances, although not the sole ingredients of blood, are yet all of them, with the exception of free hydrochloric acid, constituents of that liquid—a fact which, taken in connection with the other characters, and especially the microscopic appearance of the liquids, gives strong evidence that they contain much altered blood. Indeed, the presence of several of the substances enumerated, as albumen, iron, and sulphuric acid, seem not to admit of any other explanation, since it could scarcely be possible that in that stage of the disease they were the results of any food remaining in the stomach.

I am, very sincerely, your friend,

R. E. ROGERS.

As regards the specific gravity of the specimens examined by Professor Rogers, it varied, as will be seen, from 1.003 to 1.016. In the examinations by Dr. Michel, of Charleston, it varied from 1.010 to 1.027—water being reckoned as 1.000. When kept until decomposition began, and bubbles of air and gases were produced, this changed very much, being sometimes as low as 0.986 or 0.962. The decanted liquid, without the solid parts, gave 1.000 to 1.015. A specimen examined by Professor Hume, and which was remarkably black, had a specific gravity as high as 1.032.<sup>1</sup>

<sup>1</sup> *Op. cit.* Charleston Journ. viii. pp. 334, 335.



Stackler, vomiting of pregnant women, 524.  
 Startin, glycerine in squamous eruptions, 222.  
 Stevens, extra-uterine fœtation, 279.  
 Stewart, new instrument for cauterizing urethra, 265.  
 Stokes, report of Mount Hope Institution, 183.  
 ———, slow pulse from cardiac disease, 218.  
 Stomach, digestive powers of, 463.  
 Stomach, spontaneous rupture of, 493.  
 Stoltz, vagino-labial hernia, 505.  
 Stratton, excision of head of humerus, 235.  
 Strangulated hernia, reduction en masse, 336.  
 Stribling's report of Western Asylum of Virginia, 187.  
 Strohl, diseases of workmen engaged in making lucifer matches, 525.  
 Subclavian, ligature of, 235.  
 Sulphate of iron for excessive perspiration, 469.  
 Sulpho-cyanogen, in saliva, 529.  
 Supra-renal, thymus and thyroid bodies, 195.

## T

Tapping in hydrocephalus, 511.  
 Taxis, new method of applying, 509.  
 ———, injurious effects of pressure in, 510.  
 Taylor, intra-uterine perforation of placenta, 260.  
 Teale, injurious effects of pressure in the employment of taxis, 510.  
 Theobald, catheter left in bladder, 540.  
 Thrombus of vulva, 522.  
 Thoracic percussion, 223.  
 Thrombus, on the metamorphoses of, 193.  
 Thyroid and crico-thyroid arteries, anatomy of, 193.  
 Thymus and thyroid bodies, 195.  
 Toms, on treatment of pneumonia, 490.  
 Tourtual, new muscle of posterior nares and palate, 194.  
 Triple birth, results of auscultation at, 520.  
 Trismus nascentium, 279, 353.  
 Trousseau, abuse of alkaline remedies, 220.  
 ———, rheumatic paralysis in nurses, 222.  
 Tubercular deposits, minute structure and chemical composition of, 485.  
 Tubercle, description of, 469.  
 Typhus, urine in, 202.

## U

Ulcers, mode of curing obstinate, 250.

Upas tree, 530.  
 Urethra, polypi of, 237.  
 ———, new instrument for cauterizing, 265.  
 Urethral discharges, calomel in, 238.  
 Urinary calculi, formation and discrimination of, 198.  
 Urine in Typhus, 202.  
 ——— in dropsy, analysis of, 467.  
 Urinary diseases, treatment of, 211.  
 Urostealith, 202.  
 Uterus, prolapsus of, cured by excision of portion of vagina, 506.  
 ———, incision of neck for contraction of that organ, 518.  
 ———, pregnancy during prolapsus of, 524.

## V

Vagino-labial hernia, 505.  
 Varicose aneurism, treated by pressure, 378.  
 Velpeau, thrombus of vulva, 522.  
 Vincent, disease of neck following ligature of carotid, 233.  
 Virginia, Eastern Insane Hospital report, 454.  
 Vomiting, long continued, with excessive secretion of ammonio-magnesian phosphate, 487.  
 Vomiting of pregnant women, 524.  
 Vulva, thrombus of, 522.

## W

Waller, placenta prævia, 259.  
 Water pepper, tincture of, in amenorrhœa, 279.  
 Weisse, bifurcation of aorta, 197.  
 Western Asylum of Virginia, report, 187.  
 White, ligature of carotid, 234.  
 Wilson's Outlines of Naval Surgery reviewed, 423.  
 Wood, treatment of Bright's disease, 480.  
 Woodward's report of Massachusetts State Lunatic Asylum, 184.  
 Wooten, trismus nascentium, 279.

## Y

Yates, ligature of basilic vein, 509.

## Z

Zwicky on the metamorphoses of the thrombus, 193.  
 Zabriskie, *sanicula Marilandica* in chorea, 374.

## TO READERS AND CORRESPONDENTS.

The following works have been received:—

Essays, Anatomical, Zoological, Surgical, and Miscellaneous, Reprinted from the Philosophical Transactions; Transactions of the Medico-Chirurgical Society of London, Dublin Philosophical Journal, Dublin Hospital Reports, Report of the British Association, and the Dublin Medical Press. By ARTHUR JACOB, M. D., F. R. C., S. J. Prof. Anat. and Phys., &c. Dublin, 1845. (From the Author.)

Scrofula; its Nature, its Causes, its Prevalence, and the Principles of Treatment. By BENJAMIN PHILLIPS, F. R. S. Assistant Surgeon to the Westminster Hospital. Illustrated with an engraved plate. Philadelphia, Lea & Blanchard, 1846. (From the Publishers.)

Animal Chemistry, with reference to the Physiology and Pathology of Man. By Dr. J. FRANZ SIMON, F. Soc. for the advancement of Physiological Chemistry. Translated and Edited by GEORGE E. DAY, M. A., and L. M., Cantab. Licentiate of the Royal College of Physicians. Philadelphia, Lea & Blanchard, 1846. (From the Publishers.)

The United States Dissector, or Lessons in Practical Anatomy. By WM. E. HORNER, M. D., Prof. Anat. in the University of Pennsylvania. Fourth Edition, with numerous Illustrations. Edited by HENRY H. SMITH, M. D. Philadelphia, Lea & Blanchard, 1846. (From the Publishers.)

A Treatise on the Diseases of the Heart and Great Vessels, and on the Affections which may be mistaken for them: comprising the Author's view of the Physiology of the Heart's Action and Sounds as demonstrated by his experiments on the motions and sounds in 1830, and on the sounds in 1834-5. By J. HOPE, M. D., F. R. S., &c. Second American, from the third London Edition. With Plates. With notes and a detail of recent experiments. By C. W. PENNOCK, M. D., &c. Philadelphia, Lea & Blanchard, 1846. (From the Publishers.)

A summary of the Transactions of the College of Physicians of Philadelphia. From April, 1846, to Aug. 1846, inclusive. (From the College.)

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French Cookery. The Modern Cook, a practical guide to the Culinary art in all its branches, adapted as well for the largest establishments as for the use of private families. By CHARLES ELME FRANCATELLI, Pupil of the Celebrated Carême, and late Maître d'hotel, and chief cook to her majesty the Queen. With numerous illustrations. Philadelphia, Lea & Blanchard, 1846. (From the Publishers.)

Human Physiology. With 368 illustrations. By ROBLEY DUNGLISON, M. D., &c. &c. Sixth Edition, greatly improved. In two volumes. Philadelphia, Lea & Blanchard, 1846. (From the Publishers.)

The Medical Formulary: being a collection of Prescriptions derived from the writings and practice of many of the most eminent physicians in America and Europe. To which is added an Appendix containing the usual dietetic preparations and antidotes for poisons. The whole accompanied with a few pharmaceutical and medical observations. By BENJAMIN ELLIS, M. D. Eighth Edition, with numerous additions, by S. G. MORTON, M. D. Philadelphia, Lea & Blanchard, 1846. (From the Publishers.)

Elements of Materia Medica and Therapeutics. By EDWARD BALLARD, M. D., Lond. Phys. to St. Pancras Royal General Dispensary, &c., and ALFRED BARING GARROD, M. D., Lond. Physician to the Fore St. Dispensary, and Lecturer on Mat. Med. and Therap. in the Aldegate School of Medicine. With additions and

alterations. By R. EGLESFELD GRIFFITH, M. D. Philadelphia, Hogan and Thompson, 1846. (From the Publishers.)

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The writings of Hippocrates and Galen, Epitomized from the original Latin Translations. By JOHN REDMAN COXE, M. D., &c. &c. Philadelphia, Lindsay & Blakiston, 1846. (From the Publishers.)

A Practical Treatise on the Diseases of Children. By JAMES MILMAN COLEY, M. D., &c. Philadelphia, Haswell and Barrington, 1846. (From the Publishers.)

A Review of Homœopathy, Allopathy, and Young Physic. By L. M. LAWSON, M. D. Lexington, 1846. (From the Author.)

A Supplement to the July number of the St. Louis Medical and Surgical Journal, containing an examination of the testimony appended to the "Remarks of Dr. THOS. J. WHITE," published in said number of that Journal. By THOMAS REYBURN, M. D., of St. Louis. (From the Author.)

A Manual of Examinations upon Anatomy and Physiology, Surgery, Practice of Medicine, Chemistry, Obstetrics, Materia Medica, Pharmacy, and Therapeutics, to which is added a Medical Formulary. Designed for students of Medicine throughout the United States. By J. L. LUDLOW, A. M., M. D. Second edition, revised and enlarged. Philadelphia, Ed. Barrington & Geo. D. Haswell, 1846. (From the Author.)

Small Books on Great Subjects.

No. I. Philosophical Theories and Philosophical Experience.

No. II. On the Connection between Physiology and Intellectual Science.

No. X. The Principles of Criminal Law.

No. III. On Man's Power over himself to Prevent or Control Insanity.

No. IV. An Introduction to Practical Organic Chemistry. With references to the works of Davy, Brande, Liebig, &c.

No. V. A Brief View of Greek Philosophy up to the age of Pericles.

General Catalogue of Medical Graduates of the University of Pennsylvania; with an historical sketch of the Origin, Progress, and Present State of the Medical Department. Third Edition. Philadelphia, 1845. (From the Faculty.)

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- The American Journal of Pharmacy. Published by authority of the Philadelphia College of Pharmacy. Edited by JOSEPH CARSON, M. D., Prof. Mat. Med. in Philad. Coll. of Pharm., &c. July, 1846. (In exchange.)
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#### ERRATA.

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CONTENTS  
OF THE  
AMERICAN JOURNAL  
OF THE  
MEDICAL SCIENCES.  
NO. XXIV. NEW SERIES.

OCTOBER, 1846.

ORIGINAL COMMUNICATIONS.

MEMOIRS AND CASES.

ART.	PAGE
I. Remarks on the Use of Quinine in Florida, and on Malaria and its influence in that State; being the substance of a Report made to the Surgeon-General U. S. Army. By R. S. Holmes, M. D., Med. Staff, U. S. A.	297
II. Account of a Blighted Fœtus of the third month, having the umbilical cord extensively coiled around the right knee and lower third of the thigh, discharged with a living child at full term: with some reflections connected with the questions of Superfœtation and Spontaneous Amputation. By A. Lopez, M. D. (Read before the Medical Society of Mobile, Alabama, May 6, 1845.)	309
III. Report of some Operations performed during a late cruise in the Pacific. By Ninian Pinkney, M. D., Surgeon U. S. N., (Communicated through Dr. Thomas Harris, Chief of the Bureau of Med. and Surg., U. S. N.)	330
IV. On the Reduction of Strangulated Hernia, "en masse" or "en bloc." By Geo. C. Blackman, M. D. Newburgh, Orange County, New York.	336
V. Remarks on Trismus, or Tetanus Nascentium, and on its identity with Traumatic Tetanus in the Adult. By Wm. O. Baldwin, M. D., of Montgomery, Ala.	353
VI. Cases of Epilepsy. By H. Campbell, M. D., of Uniontown, Pa. (Communicated by Prof. Horner.)	370
VII. Obstetrical Cases. By William M. Hudson, M. D., of Chester, Meigs Co., Ohio.	372
VIII. Cases illustrating the use of Sanicula Marilandica in Choreia. By J. B. Zabriskie, M. D., Physician to the King's Co. Almshouse.	374
IX. Intestinal Obstruction from novel causes. By J. F. Peebles, M. D., Petersburg, Va.	376
X. Case of Varicose Aneurism, successfully treated by Pressure. By Wm. Johnston, M. D., of White-house, N. J.	378
XI. Wound of Abdomen, Expulsion of Intestines, and Cure. By J. Gibbs, M. D., Claiborne, La.	379
XII. Case of Death from Inhaling Chemical Fumes in a Sulphuric Acid Chamber. By Edward A. Mans, M. D., of Baltimore, Md.	380
XIII. On a remedy (the Ambrosia Trifida) for Mercurial Salivation. By Wm. Robertson, M. D., of Harrodsburg, Ky.	382
XIV. Case of Cæsarian Section. By Brodie S. Herndon, M. D., of Fredericksburg, Va.	386
XV. Case of Labour complicated with Prolapsus Uteri. By Augustus K. Gardiner, M. D.; Memb. of Massachusetts Med. Soc.; Physician to N. York City Dispensary; District Physician to N. York Asylum of Lying-in Women.	387

## REVIEWS.

ART.	PAGE
XVI. On Disorders of the Cerebral Circulation; and on the Connection between Affections of the Brain and Diseases of the Heart. By George Burrows, M. D., &c. &c. 8vo. pp. 220. London, 1846. - - - - -	391
XVII. Outlines of Naval Surgery. By John Wilson, (D.) Surgeon R. N.; Late Surgeon H. M. S. Vanguard. 12mo. pp. 134; Edinburgh, 1846. - - - - -	407

## BIBLIOGRAPHICAL NOTICES.

XVIII. Animal Chemistry, with reference to the Physiology and Pathology of Man. By Dr. J. Franz Simon, Fellow of the Society for the advancement of Physiological Chemistry, &c. Translated and Edited by George E. Day, M. A., and L. M., Cantab., &c. Philadelphia, Lea & Blanchard, 1846: pp. 295-717: 8vo. - - - - -	426
XIX. Fifth Annual Report of the Registrar-General, of Births, Deaths, and Marriages, in England. - - - - -	427
XX. Experimental Researches on the Post-mortem Contractility of the Muscles, with observations on the Reflex Theory. By Bennet Dowler, M. D., (Reprinted from the New York Journal of Medicine, for May.) 8vo. pp. 39. New York, 1846. - - - - -	440
XXI. Das Blei und seine Wirkungen auf den Thierischen Körper. Ein Beitrag Zu dessen Charakteristik für Aerzte, Chemiker und Techniker. Von Dr. Ferdinand Rumpelt. 8vo. pp. 268. Dresden and Leipzig: 1845. On Lead and its Action on the Animal Body; a contribution towards determining this action for the use of Physicians, Chemists, and Artists. By Ferdinand Rumpelt, M. D. - - - - -	443
XXII. Ueber eine Neue Geburtszange zur Extraction des in Beckeingange Stehenden Kinskopfes. Mit abildungen. Von Dr. Th. Hermann, arzt und wundarzt erster classe in Bern. 4to. pp. 50. Bern, 1844. Description of a new Obstetrical Forceps for abstracting the child's head when engaged in the upper strait of the Pelvis—with Plates. By T. Hermann, M. D., Physician and Surgeon of the first class in Bern. - - - - -	448
XXIII. Theoretisch en Praktisch Handboek der Mechanische oogheelkunde het zien, het middle waardoor wij zien, den optisch-Physiologischen werkring der ooggen, de eigenschappen en gebreken des hulpglazen en de wijze van derzelve aanwending bij de Mechanische oogzehten; door. J. A. Hess, geotroijeerd gezigt en oogheel kundige te Middelburg. 8vo. pp. 273. Middelburg, 1842. Theoretical and Practical Manual of Mechanical Ophthalmology, containing an account of the Organs, Physiology, and Mechanism of Vision, and of the proper construction of the glasses adapted to remedy the various forms of defective vision. By J. A. Hess, Optician in Middleburg. - - - - -	451
XXIV. The Writings of Hippocrates and Galen. Epitomized from the original Latin Translations. By John Redman Coxe, M. D. - Multa renescentur. 8vo. pp. 681. Philadelphia, 1846. Lindsay & Blakiston. - - - - -	452
XXV. 1. Sixth Annual Report of the Superintendent of the Insane Hospital (Augusta, Maine) 1845. 2. The Annual Report of the Eastern Asylum, in the city of Williamsburg, Virginia, for 1844. 3. Report of the Committee of Regents, of the Physician, and of the Superintendent of the Lunatic Asylum of South Carolina, for 1842. 4. Report of the Committee on the Lunatic Asylum (of South Carolina), including the reports of the Regents, Superintendent and Physician, 1845. 5. The Twenty-Second Annual Report of the Officers of the Retreat for the Insane, at Hartford, Connecticut, May, 1846. 6. Report of the Maryland Hospital (for the Insane) for the year 1845. 7. Report of the Trustees and Superintendent of the New Hampshire Asylum for the Insane, June, 1846. - - - - -	453

ART.	PAGE
XXVI. Human Physiology, with three hundred and sixty-eight illustrations. By Robley Dunglison, M. D., &c. &c. Sixth Edition, greatly improved. Philadelphia: Lea & Blanchard, 1846. 2 vols. 8vo: pp. 651-694.	458
XXVII. Small Books on Great Subjects. The Connection between Physiology and Intellectual Science. From 2d London Edition: pp. 85. Lea & Blanchard, 1846.	
On Man's Power over himself to Prevent or Control Insanity. By Rev. John Barlow, M. A. Lea & Blanchard, 1846: pp. 54.	
An Introduction to Practical Organic Chemistry, with reference to the works of Davy, Brande, Liebig, &c. Lea & Blanchard, 1846: pp. 66.	459
XXVIII. The United States Dissector, or Lessons in Practical Anatomy. By Wm. E. Horner, M. D. Professor of Anatomy in the University of Pennsylvania. Fourth Edition; with numerous illustrations. Edited by Henry A. Smith, M. D., F. C. P. P., &c. Philadelphia: Lea & Blanchard, 1846: 12mo. pp. 666.	406

## QUARTERLY SUMMARY

OF THE

## IMPROVEMENTS AND DISCOVERIES IN THE MEDICAL SCIENCES.

### FOREIGN INTELLIGENCE.

#### ANATOMY AND PHYSIOLOGY.

	PAGE		PAGE
1. General Physiology of the Spinal Cord. By Volkmann.	461	stances by the Lymphatics. By Bischoff.	464
2. Theory of Cell-Development. By Kölliker.	462	5. Minute Anatomy of the Kidneys. By Dr. Bidder.	465
3. Digestive Power of the Stomach. By M. Bernard.	463	6. Absence of Corpus Callosum. By M. Paget.	466
4. Absorption of Narcotic Sub-			

#### ORGANIC CHEMISTRY.

7. Presence of Copper and Lead in the Bile. By Heller.	466	8. Analysis of the Urine in Dropsy after Small-pox. By Dr. Heller.	467
--	-----	--	-----

#### MATERIA MEDICA AND PHARMACY.

9. New method of making Iodide of Iron, in a state of Purity. By M. Kop.	468	Perspiration. By Professor Lipich.	469
10. Sulphate of Iron for excessive		11. Tubercle. By Hasse.	469

MEDICAL PATHOLOGY AND THERAPEUTICS AND PRACTICAL MEDICINE.

	PAGE		PAGE.
12. On the Capacity of the Lungs, and on the Respiratory Movements with the view of establishing a precise and easy Method of detecting Disease by the Spirometer. By John Hutchinson.	469	21. Hemorrhage from the Ear following Suppressed Menstruation. By M. Alibert.	486
13. Influence of the quality of the Milk of the Nurse on Health of the Child. By M. Girard.	474	22. Excessive Secretion of the Ammonio-magnesian Phosphate by the Kidneys with long-continued Vomiting. By Dr. Golding Bird.	487
14. New Researches on the Œdema of new-born Infants. By M. Henry Roger.	474	23. Neuroplasty or Ganglionic Alteration of the Peripheral Nerves. By M. Serres.	488
15. Ulceration of the Gums in Children occurring in an Epidemic Form. By Dr. James Duncan.	475	24. On the Treatment of Pneumonia. By Dr. A. T. Thomson.	490
16. On the Internal and External employment of Nitrate of Silver. By Dr. Heller.	476	25. Acidity. By Dr. Robert Dick.	493
17. Means of Preventing Bed Sores. By Dr. T. Purefoy.	477	26. Spontaneous Rupture of the Stomach. By Dr. Morici.	493
18. On the Diagnosis of Bright's Disease, and its relations to Albuminous Urine. By G. Owen Rees, M. D.	477	27. Pathology and Treatment of Aphonia. By John Bishop.	494
19. On the Treatment of Bright's Disease of the Kidney. By Dr. Alexander Wood.	480	28. Diagnosis of Disease of Mitral Valve of the Heart. By Dr. J. J. Furnival.	497
20. On the Minute Structure and Chemical Composition of Tubercular Deposits. By Dr. J. Hughes Bennett.	485	29. Case of Congenital Cyanosis of forty years' standing, depending upon Patulous Foramen Ovale and Obstruction in the Pulmonary Artery. By Robert J. Spitta.	501
		30. Ammonia in Asthma. By M. Guerard.	502
		31. Hydrocele or Serous Cyst in the Rectus Abdominalis. By Dr. Mayne.	503

SURGICAL PATHOLOGY AND THERAPEUTICS AND OPERATIVE SURGERY.

32. On Displacement of the Lower Fragment in Fracture of the Surgical Neck of the Humerus. By M. Debrou.	504	40. Injurious Effects of Pressure in the employment of the Taxis for the reduction of Hernia. By P. P. Teale, Esq.	510
33. On a New Variety of Dislocation of the Humerus. By Prof. Roser.	504	41. Treatment of Panaris by Mercurial Ointment. By Dr. Martin.	510
34. Vagino-labial Hernia. By Prof. Stoltz.	505	42. Hydrocephalus — Tapping — Cure. By Dr. James Edward.	511
35. Two Cases of Prolapsus of the Uterus cured by Excision of a Portion of the Vagina. By M. Leloutre.	506	43. Treatment of Wounds of the Arteries of the Superior Extremity by Compression and Forcible Flexion of the Forearm. By M. J. Fleury.	512
36. Resection of the four External fifths of the Clavicle. By M. Chaumet.	506	44. Œsophagotomy. By M. Delavacherie.	513
37. On Spermatic Discharges. By Mr. B. Phillips.	506	45. Unusual Injury of the Elbow-Joint. By Mr. R. W. Smith.	514
38. New Method of applying Taxis. By M. Grynfeldt.	509	46. Contracted Cicatrix from a Burn. Autoplastic Operation—Considerable Improvement. By Mr. Carden.	515
39. Ligature to the Common Basilic Vein. By Walter Yates, Esq.	509		

## OPHTHALMOLOGY.

	PAGE		PAGE
47. Successful case of the Employment of Iodine in Removing Sparks of Iron from the Cornea. By M. Reiniger. - - -	516	Esq. - - - - -	516
48. Cases of Melanosis, with Observations. By Holmes Coote,		49. Entozoon in the chambers of the Eye. By M. Alessi. -	517
		50. Amaurosis from Concussion of the Retina—Recovery. By W. B. Page. - - - - -	518

## MIDWIFERY.

51. On Incision of the Neck of the Uterus in cases where, from the contraction of the organ, delivery is difficult. Ry Dr. Laborie. -	518	Labour. By J. Y. Simpson. -	521
52. Results of Auscultation at a Triple Birth. By Prof. Naegele. -	520	54. On Thrombus of the Vulva. By Prof. Velpeau. - - - - -	522
53. Fractured Sacrum impeding Labour—Delivery by Craniotomy in two successive Labours. By J. R. Gibson, Esq. - - - -	521	55. Vomiting of Pregnant Women. By Dr. Stackler. - - - - -	524
54. Influence of Galvanism upon the action of the Uterus during		56. Pregnancy and Delivery during complete Prolapsus of the Uterus. By Ancell. - - - - -	524
		57. Imperfect condition of the Os Uteri, with Malformation of the Vagina—Treatment during Labour. By S. H. Davis. - - -	525

## MEDICAL JURISPRUDENCE AND TOXICOLOGY.

58. Diseases of Workmen engaged in making Lucifer Matches. By Dr. Strohl. - - - - -	525	anogen in the Human Saliva. By J. L. Smith. - - - - -	529
59. New test of Prussic Acid. By Mr. Richard Austin. - - - -	528	63. Poisonous Fish. By Mr. Jameson. - - - - -	530
60. On the Natural Period of Delivery. By Dr. Le Ray. - - - -	529	64. Upas Tree. By Mr. Brooke. -	530
61. What constitutes the intent to commit murder. - - - - -	529	65. Spontaneous Combustion. -	530
62. On the presence of Sulpho-cy-		66. Alcohol. By M.M. Bouchardat and Sandras. - - - - -	531
		67. On the Blood in bodies killed by Strangling. By Ciccone. -	531

## MISCELLANEOUS.

68. On the Composition of air at different heights in close apartments. By M. Lassaigne. - - - - -	532	69. Means of neutralizing exhalations of Sulphuretted Hydrogen. By M. Faucille. - - - - -	532
--	-----	---	-----

## AMERICAN INTELLIGENCE.

## ORIGINAL COMMUNICATIONS.

Address to the Medical Profession, in relation to the objects of the National Medical Association, by the committee appointed for that purpose. - - - - -	533	der, in drawing off the Urine for Retroversion of the Uterus. By E. W. Theobald, M. D. - - -	540
Legal Medicine. By Washington L. Atlee. - - - - -	535	Case of Injury of the Foot.—Amputation, excessive hemorrhage from the stump. By J. C. Butler, M. D. - - - - -	541
Case of a Catheter left in the Blad-			

## DOMESTIC SUMMARY.

	PAGE		PAGE
Reports upon the Hemostatic Vir- tues of the Brocchieri Water and Ergotine. By Drs. Smith and Sinkler. - - - - -	542	the United States for the Session 1845-46. - - - - -	546
Excision of the Elbow-Joint in a case of Caries of the Articular Extremities of the Bones. By Dr. Gurdon Buck, Jr. - - -	544	Early Pregnancy. By Dr. J. B. Walker. - - - - -	547
Statistics of the Medical Schools of		Ectrotic treatment of Small-pox by Tincture of Iodine. By Dr. Sam- uel Jackson. - - - - -	547
		Medical Rank in the Navy. By Mr. George Bancroft. - - -	547

THE  
AMERICAN JOURNAL  
OF THE  
MEDICAL SCIENCES

FOR OCTOBER, 1846.

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ART. I.—*Remarks on the Use of Quinine in Florida, and on Malaria and its influence in that State; being the substance of a report made to the Surgeon-General U. S. Army.* By R. S. HOLMES, M. D., Med. Staff, U. S. A.

DISEASE in Florida may be said to assume always a remittent form; and quinine being the most efficient anti-intermittent known, that medicine is of incomparable value in the treatment of the diseases of that state.

The experience of physicians in the south of France, in Italy, and in the southern States of the Union, shows that a much larger dose of quinine is necessary to check a fever in those countries, than in latitudes and regions where malarious influence is not so powerful.

From experience in Florida, one is almost disposed to believe, that there is a certain rule which though apparently too mechanical in its operations, to some extent is applicable; it is this,—in proportion to the amount of miasmata in the system as shown by its effects, so must the dose of quinine, for the cure, be increased or diminished.

The largest amount of quinine I have ever given at a single dose has been eighty grains; this is the extreme dose: the average quantity is about twenty grains.

Let us commence with intermittent fever. The first question I always ask a patient who complains of this disease concerns the state of his bowels. I am careful never to give quinine with the intestines in a torpid or gorged condition, for its effects then seem to be comparatively lost. I have given it, however, when the bowels were actively purged and irritated. It seems not

to be governed by the general rule that medicines will not affect the system properly when the bowels have not been well attended to. Having then in a common intermittent fever freely opened the bowels, if they have not been already purged, by means of oil, magnesia, calomel, or rhubarb, I give the quinine, generally on the evening of the day on which the purgative has been taken, fifteen grains, say, as a general dose, *at once* (not in divided doses), in water with a few drops of vinegar or of some acid, so that it may be readily dissolved. Suppose this to be given in the afternoon at 3 P. M., the patient having had an attack of intermittent on the same day, commencing at 9 o'clock A. M. and ceasing at 2 P. M. The fever, being a quotidian, will come on, the succeeding day at the same time, despite the quinine; both the chill and fever will be of a lighter grade, but will be as distinctly marked, and will remain as long; but on the third day the patient will be entirely free from the disease, and will not experience, during any part of the day, the slightest symptom of it. In treating many hundred cases of fever in Florida I do not think I have met with twenty exceptions to this rule in cases of pure intermittent fever. The plan of blood-letting to cure the disease, or aid the operation of quinine, I have never practised, having had no occasion to do so; and being opposed, as a general rule, to blood-letting in this disease. If the bowels are in a good condition, I give the quinine immediately after the fever has abated, generally on the same day; in this as in every other disease I have encountered in Florida, always following the rule of checking it instantly. If the bowels are loose, I give the quinine combined with about twenty drops of laudanum, for it will often operate as a laxative, the tendency to which is easily checked. Occasionally, however, I have found a case where this dose (fifteen grains) was not sufficient, owing to what other cause than the idiosyncrasy of the patient, I never could determine; other things being attended to, however, I have never seen twenty-five grains check a simple intermittent fever. Nor do the effects of this agent seem to become inert on the system. Time after time, I have given the same number of grains, at every successive attack, to the same patients and always with the desired results. The worst cases of intermittent are those in which the disease manifests an obstinate disposition to return; this return is, I think, most frequent on the seventh day, then the eleventh, fourteenth, twenty-first, and so on. A patient comes to you complaining of a chill: you may remember that he was lately on the sick report, and inquire when he had the last attack; he answers about a week ago; you examine your books and find this is the seventh day; you administer the quinine on the day he reports to you, not permitting the disease to continue an hour longer than possible; the day after he has a slight fever and chill, and on the third day of his admission he is entirely free from disease, and is returned to duty, with the injunction (if you entertain certain views of the case) to report to you early on the sixth day, in order that you may anticipate the seventh; you administer



the quinine in a fifteen grain dose at once, twenty-four hours before you expect the chill, and no symptoms of fever follow on the succeeding day; but on the fourth day after, namely, on the eleventh day, the patient has another fever and chill, as severe as any he has had heretofore; the disease destroyed in its habit of attack on the seventh day, has taken to the eleventh, and your second dose of quinine has been given for naught. If you follow up the attack and administer for the *eleventh day*, in anticipation, you will in all probability find that the disease has again shifted, either following or preceding that day; hence the rule which after many trials I have adopted, never to give quinine in order to anticipate an attack of intermittent fever, on a *critical* day; I have pursued the opposite system, and with such uniformly bad results, that to this rule I now most strictly adhere.

I never give the quinine during the fever or chill, simply on account of the difficulty of retaining it on the stomach at that time, and not that I believe, provided it was retained, it would operate less effectually, or produce injurious consequences.

On my arrival in Florida, knowing nothing of southern diseases from practice, and being stationed alone at a distant and unhealthy post, I learned the rules by experience alone, guided by which I have since successfully administered quinine. I practised on northern precepts, annoying the patient without arresting the disease, by a continued succession of two grain pills; occasionally at long intervals checking the disease by these means, but much more frequently vexed for weeks by the continued sickness of the soldier. I rose finally to ten grains, and continued to give this quantity at once; I more frequently succeeded by this practice, but not yet to my satisfaction. If the patient had a quotidian, I gave the quinine on the day of the chill; if a tertian, also on the same day; if successful with the ten grains, I was surprised to find the quotidian manifested itself in a modified degree on the succeeding day, and on the next was entirely absent; whilst of the tertian on the third day, not a symptom appeared; hence I drew the rule which I have often verified since, and from which I have never varied, or had any reason to doubt, that the full effects of quinine are not manifested until about eighteen hours after its administration. Finally, convinced that large doses of quinine are necessary in the south, I increased my minimum dose for intermittent fever to fifteen grains given at once.

Congestive fever is of course a disease of much greater danger than the one we have been contemplating, and appears to be the disease proper in which the powerful and successful effects of this agent have been most apparent. This disease is remittent in its character; the grade of its violence, to the most casual observer, will appear much greater than any stage of intermittent fever; there is complete prostration; the faculties are benumbed. the countenance anxious and haggard, and the mind desponding; all these symptoms are often present an hour after the first attack, and when you see the patient for the first time. Here it is evident two plans of treatment

must be pursued simultaneously:—to rally the patient for the time being, and to provide for a periodical return. Frictions, mustard poultices, and stimulants, will answer the first design; but I know of no other agent than quinine, in the materia medica, that will do for the second; you cannot wait for its operation, if the prostration of the patient is so complete, that you dread the return of the chill, lest it prove fatal; and in the first few minutes after seeing the patient, I have given him from thirty to sixty grains of quinine in one dose, in water or brandy; the brandy for the present contingency, but the quinine for one several hours ahead.

If the attack is sudden; as a severe one of this kind generally is, and if the patient has not been sick previously, I have never found much difficulty in having the quinine retained; if it is not retained, and time cannot be spared, I give it by enema. I have given the quinine in the highest stage of the fever, and in the lowest of prostration; never permitting any existing state of inflamed bowels or stomach to deter from its administration, or lessen the dose in which I would otherwise have given it; I have not lost a patient from inflammation following congestion, and where the quinine has been given as here mentioned. I have not seen a patient die who survived after twenty-four hours from the time of attack. The fatal cases are those in which the quinine has not been given in a proper quantity, or where it has not been thrown into the constitution for a sufficient length of time to reach the disease.

Ten distinct cases of congestive fever might be described, and exactly the same symptoms might be seen in every case; yet not two of the ten would be similar in violence. During the last year of my army medical practice in Florida, I probably treated fifty cases of congestive fever; yet two of these cases were so overpowering in their effects, the shock was so tremendous, and the termination of life so speedy, that the system seemed as if inoculated with some violent and consuming poison; death in both cases took place before the lapse of twenty hours from the first accession of the disease. In such cases as these, "distrust is cowardice and prudence folly;" the symptoms seemed to be running into death speedily. In the first case I gave eighty grains of quinine, twenty grains of calomel, and three grains of opium, after giving my usual dose of twenty grains of quinine when I first saw the case, and when the symptoms were trivial. (There are no cases that require more constant attention than these; the change of symptoms is sometimes so hurried, that from comparative health the patient will in thirty minutes time be nearly beyond relief.) The congestion was on the thoracic viscera; the patient was perfectly conscious of his situation, and an hour after he was taken, persisted in asserting that he was "a dead man;" he complained of scarcely any pain; he felt a slight inconvenience about the breast, but his breathing was quick, and deep, and laboured; his chest, to the touch, was hot and dry; his legs, and head, and feet, of a proper temperature, until towards the close of the paroxysm,

when these parts became, as they often do, of an icy coldness, (which must be carefully watched, and heat and friction applied;) his locomotion was easy, and until an hour before his death, he retained his reason, and powers of volition; he could not be induced to keep in his bed, but perpetually tossed to and fro, or jumped up to walk across the hospital floor; his pulse was not much accelerated; in the worst cases it does not reach over eighty-five or ninety; it is generally full and laboured, and easily compressed under the fingers; his stomach was in a good condition, and he easily retained anything on it; his bowels had not been moved since the attack, but readily answered to a large dose of oil given some three or four hours after the quinine was administered; his skin was dry: the secretion of urine was apparently stopped; I succeeded in getting a few ounces of blood from the arm, though it is against my practice to bleed in this disease; I cupped the breast freely, I kept warm fomentations to the legs and feet, and bowels, with friction kept up at intervals, when the hot and wet cloths were not applied; yet, not one of these means seemed to stop for an instant the deadly onward course of the symptoms; the quinine, of which he had taken one hundred grains, in four hours' time, did not affect him in any single particular that I could discover; involuntary discharges came on shortly before death; the body became prostrate, and but for the deep and heavy breathing, accomplished at long intervals, lifeless: the pulse was scarcely perceptible at the wrists, and indistinctly at the carotids; the heat of the chest to the touch was perfect to the last, and even the skin externally was suffused with blood; the extremities became as cold as dead flesh, the jaws were relaxed, the head thrown far back, and the arms extended at right angles to the body; I counted thirty to forty-five seconds between several of the final inspirations; and the powerful dragoon soldier had to yield at last, his robust strength and apparent powers of life not being competent for such an emergency.

In such cases as these I can compare the effect to no other operation that I have a knowledge of, than the overwhelming shock occasionally communicated by a severe wound; there is apparently the same want of a proper cause to account for death; no lesion of any vital organ may be perceptible; and the senses, and almost the strength of a perfect man, are retained to the end. A new reading of a great poet, may perhaps help us to explain the cause: the nerves are "jangled out of tune."

This patient had been five years in Florida; he had never been sick for a day's time before this; he was a large, strong and active man. The place where he died, Fort White, on the Santa Fe river in Florida, was always famed for its severe malarious diseases.

In a post-mortem examination of this case, these were the appearances:—The viscera of the abdomen were in apparently a sound and healthy condition; the contents of the cranium were in a healthy state; the skin around the whole circumference of the chest was discoloured by extravasated blood;

the vessels of the heart were deeply engorged with blood; the auricles and right ventricle were filled; and the lungs bled as if a sponge had been cut that was soaked in blood; the whole blood of the body seemed to have found a common reservoir in the thorax.

The foregoing case affords a good example of the rule by which we are guided in giving quinine in congestive fever. It is probably as well to give it during the intervals of the paroxysm, but we never wait for that time in cases of the slightest emergency; experience has shown that it is unnecessary, and I have frequently given thirty or more grains, where the fever was at its height, with the happiest effects. If the patient in the foregoing case could have surmounted the first attack, I have no doubt the quinine would have modified, or checked any subsequent one: but it was necessarily given too late to control the symptoms, and there always will be cases in which the excess of the paroxysm cannot be foretold.

No term was ever more properly applied to denote a disease than this term congestive; it has been said there is congestion in other fevers besides this; so there is, but it is a congestion of as different a type as that between typhus and intermittent; it is a congestion the end of which, however slight may be its onset, tends to a fatal termination; nor is it brought on purely by the time of the paroxysm; any quick exciting cause, be it ever so trivial, the entrance of a stranger, the firing of a gun, some one bursting suddenly into the room, even the visit of the physician, will throw the blood from the extremities, or more properly perhaps prevent its due propulsion into them; the lips will become livid, the tip of the nose and ears cold; the feet and legs cold, the cheeks blanched; and yet the patient, strange to say, will be unconscious generally that a change has come over him; all this too, independent of the regular periodic time, when the true paroxysm comes on. In the intervals he will be torpid, morose; his mind dull, slow in gathering up his thoughts, his pulse laboring and full and slow, his bowels sluggish, or if irritation has set in upon them frequently purged; the secretions from his liver, kidneys, and skin, improperly carried on, or almost checked: if the congestion is on the brain, he will complain of dull pain in the head, if on the thorax, his breathing will be hastened; if on the abdomen, inflammation of the small intestines will probably soon set in, if the disease is not speedily checked. During the paroxysm the prostration is so great that the patient will not make an effort for hours to move or turn in his bed; his feet, legs, hands and head will be all cold to the touch; yet he will not complain of being cold; over the point where the congestion exists, the heart will be some degrees above the natural standard: you can scarcely get an answer to your questions, the patient looking you in the eye, and thinking for some seconds before he can collect his own thoughts and yours; his countenance is distressed and peculiar, and in many cases an experienced eye could tell by glancing at the patient, what is his disease; now in a case of this kind, if you have an aversion to large doses of quinine given at once; if you say

this agent produces great excitement in the brain, and there are already intense congestion and pain there, or that it produces irritation of the bowels, when there is congestion and probably irritation already existing; if this is the mode in which you reason, the probability is, that within the next few days your patient will die; for this fear of quinine, or of a large dose, *why* is not exactly known, has frightened many a patient into the grave, and will in time to come.

I never think of these objections without remembering a case that lately happened in the south, with the particulars of which I was acquainted. A person in whose recovery great interest was felt by a large circle of friends, was attacked by a dangerous remittent disease, with signs from the first day of inflammation, and congestion in the stomach and intestines. So great was the inflammation, as to lead one to doubt whether it did not take precedence of the disease peculiar to the climate, under which the patient was suffering; the evacuations from the bowels were frequent; the pain so great on pressure that the weight of the bed-clothes could scarcely be borne; the throbbings of the abdominal aorta could be as distinctly counted at the umbilicus as over the course of the carotids; the consequent weakness peculiar to all such irritations of the small intestines was great: this irritation had held out against all active agents, for six days, with scarcely any abatement; but conjoined with this, was the disease under which the patient was said to be suffering; towards nightfall of every day, the extremities would become cold, while the heat of the abdomen was always above its natural standard; the crown of the head, the lips, the fingers, the feet and limbs would be all cold and livid; this would be succeeded by a fever during the night, which would at last attain its acme about ten o'clock on the ensuing morning; during the day, the patient would lay in an exhausted state, until the evening again would bring about the same alarming cold stage; from some access of which so great was the prostration, that it might reasonably be feared a rally could not be made. The physician who had from the first attended the patient, a gentleman of skill in his profession, but impressed with the idea of the irritating power of quinine, had directed his whole efforts towards the irritation of the intestines, letting the fever run its course unchecked, lest the irritating power of the agent in dispute would aggravate every symptom. The question was asked him on the seventh day, how long he thought the patient could exist, with the fever at such a height as it had now attained? His answer was, not for forty-eight hours; and how soon would the simple irritation of the intestines terminate in death, if as uncontrollable as it had heretofore been? "Probably in three or four days." Why not then run this risk of inflammation, check the fever instantly by a large dose of quinine, and have but one disease to contend with? With great hesitation, forty grains of quinine were allowed to be given in two doses in twenty-four hours; the next paroxysm of the fever was trifling, and on the ensuing day banished. Every effort was now directed to the inflam-

mation, which proved severe and obstinate, but from which, in the course of six weeks, the patient recovered.

The following is an analysis of the points of practice spoken of in the preceding remarks which I have always followed, with such results as to justify me in believing in their propriety.

Every periodical disease is to be checked immediately. Quinine, as a remedy for periodicity, is to be given regardless of any existing state of inflammation. Never give quinine in divided doses when directed for the immediate cure of a periodical disease. To be certain of the operation of quinine in a constitution with which you are not acquainted, it must be given eighteen hours before the desired result. In emergent cases it may be given in the lowest state of prostration, or the highest grade of the fever. As a general rule fifteen to twenty grains will be necessary for an intermittent, and thirty to fifty, for a congestive fever. Never give quinine for the cure of a periodical disease in anticipation, when the periodicity exceeds five days.

In small doses quinine is a tonic, in larger doses its tonic is quickly followed by its stimulating property; but in grave periodical diseases I am disposed to think its sedative effect the one which tells most certainly; but to procure this in any perfection it must be given in a large dose; and here it seems to me has arisen the great discrepancy *in times*, at which quinine has been given; any dose of quinine that produces sedation, generally goes through a stimulating process; if this process happens at the onset of an intermittent fever, the fever will in all probability be checked by it; and hence you have cases in which quinine checks a chill when given immediately before it comes on; but it will be seen from the preceding remarks, concerning the difference in the cure of a quotidian and tertian, that this immediate cure by stimulation is not so certain or effectual as the subsequent one, which I think is accomplished by the sedative effect of this article; in this sedative effect, I think, consists the great anti-periodic power of quinine. Tartar emetic in large doses is also a powerful remedy against periodicity by the same property; opium and belladonna also by the same, and alcohol, piperine, capsicum, etc., by the possession of tonic and stimulating effects. If the sedative properties of tartar emetic could be procured independent of its tendency to irritate, I can think of no remedy for serious periodic diseases surpassing it, yet this property is not produced in any great degree by small doses of the article, nor does it follow the exhibition of the like doses of quinine.

It is the continued small doses of this agent that harass and irritate. A patient at the north takes one or two grains almost daily for weeks, until he has swallowed sixty or more grains in the course of a month, and then is indignant at the idea of a fifty or sixty grain dose being given at once in Florida, which suffices for the same period. As large an amount of calomel may be taken at once with but little bad effect; but give it in divided doses,

and the result is irritation, fever, and pyalism. Small continued doses of tonics, like quinine, cannot be given long with impunity. The most natural conclusion, if you would give quassia for weeks, would be that your patient would finally reject his food; his stomach would be debilitated instead of strengthened, and placed in a proper state to take on inflammation.

There is one great source of error to a northern physician, in reading the accounts of the large doses of quinine given at the south: this consists in the supposition that these doses are often repeated, while the truth is, the large dose generally suffices for the cure of the disease. It is the nature of intermittents of course to continue their attacks, when the patient resides in the same atmosphere that gave origin to the disease.

The result was as follows, in ascertaining how much quinine had been given to the ten men at a post in Florida, (the average number of men being 220,) who had taken the largest and most frequent doses:

Lewis,	grains 220	in 6 months and 7 days.
Wise,	grains 135	in 6 months and 8 days.
Sheriden,	grains 95	in 5 months.
Nash,	grains 140	in 3 months and 27 days.
Smith,	grains 150	in 5 months and 2 days.
Johnson,	grains 150	in 5 months and 6 days.
Bowers,	grains 80	in 4 months.
Thomas,	grains 120	in 5 months and 6 days.
Chapman,	grains 100	in 4 months and 13 days.
Lord,	grains 70	in 3 months and 26 days.

The names of these men were taken from the hospital books; they had taken more than any other ten men in the command; and it is somewhat singular that on the day this table was made out, not one of these men was on the sick report, nor had any one of them been on for several days previous. Under peculiar circumstances greater quantities of quinine may be taken by an equal number of men in the same time, though I think this is not an unfair representation of the amount given in Florida. You give fifteen grains for an intermittent, and the disease, for the time at least, is checked; with fifty grains at once in a congestive fever the like result is accomplished.

In diseases of this climate, eminently periodic in their character, and of a highly dangerous type, it will be apparent that the plan of giving quinine in divided doses cannot be entertained. The disease is one in which you cannot afford to lose so much time; many cases of fever are presented in which you are convinced the patient will succumb to a second or third paroxysm; the first of these may, with great certainty, be expected at the end, or before the lapse of twenty-four hours; and one would be operating by the rule of books, without ever consulting the evidence of his own senses, who would continue to give quinine in small doses up to the very

hour of the expected and dreaded paroxysm. It has been seen that it requires eighteen hours at least for the full effects of quinine to be manifested; yet many physicians will give ten or more grains, in two grain doses, commencing about ten or twelve hours before the expected highest grade of the paroxysm; it is evident that scarcely a grain of quinine is here brought to bear on the fever: but will not the third paroxysm be checked by the ten grains, if that quantity is sufficient? It probably may, but be the quinine a tonic, a sedative, or a stimulant, you want its power in one of these respects concentrated on the fever. It cannot be thought strange that the practice at the north of giving quinine in divided doses, for bilious, remittent, and congestive fevers, should prove so unsuccessful. Though the fevers there do not require such large doses of this article as at the south, they require very different ones from those that are now given. I have known in Chester county, Pennsylvania, in a small district, one-fourth the number of patients die, who were seized with a pure miasmatic, congestive fever, differing from that of Florida only in its lighter grade; yet the disease, as treated by army surgeons in Florida, with quinine, was one of the least mortal, probably not more than one in forty cases proving fatal.

I had an attack of congestive fever in December, 1841, a month after I came to the territory, with which I lingered for three weeks on the verge of the grave, at a distant post, and with no better medical assistance or advice than I could give myself. A stranger to the powers of quinine, I took it in grain doses, for a fever which I am in the habit of checking now in a day's time, by drachms, instead of grains, of this great agent. The congestion was on the brain, the pain so intolerable that the slightest motion could scarcely be borne; the intolerance of light and sound perfectly tormenting to the senses; the muscular system weak, and languid; the eyes and cheeks cadaverous, and after a few days deeply sunken in. How often, subsequently, with all these symptoms at their height, have I given forty or fifty grains of quinine,—have seen its effects on the brain, aggravating for the time every symptom, or occasionally but slightly affecting the disease for some hours; and then, as the sedative effects came on, have beheld the patient drop into a composed sleep, his skin become moist and natural, (no better diaphoretic than this agent, in many states of the system,) and awake in six or eight hours, a man really free from disease; this may appear all exaggerated to those who have never seen congestion, or its treatment in this manner; but to any one who has, I appeal whether this description is not unvarnished truth.

This agent is essentially opposed to the periodic effects of malaria, nor are these effects by remission always apparent; nor do we give quinine only for fevers. They comprise but a small class of the diseases that remit; and this remission may be so slight, or brief, that neither the patient nor physician can discern it; and the degrees are of all grades, from the slightest



alleviation of the symptoms, to the enjoyment of almost perfect health. We know of no disease occurring in a malarious region where remission may not be suspected, for there seems to be no disordered action that cannot be affected by this powerful agent. If the physician in the south can be blamed for his too great enthusiasm for the specific (if there is such a thing) for all these ills, he has certainly chosen a handmaiden worthy of his worship. To one accustomed to look at the slow and languid operation of medicines, in fevers at the north, and the want of faith with which they are so often given, the operations of this medicine appear miraculous: they are only equaled, when it has fair play, by its certainty.

The immediate effects of a large dose of quinine are, buzzing, and murmuring in the ears; a partial deafness, which often continues for twenty-four hours; a great sense of fullness about the head, and often a dull pain across the forehead; there is generally more or less excitement or partial delirium of the mind, without exciting the spirits in any degree. I have seen patients under the effects of quinine wander, and talk incoherently, as if from the influence of alcohol. Occasionally it will produce a pricking sensation in the skin, and a quivering in the muscles of the fingers and eyelids. One patient I knew who was always made perfectly insane, and beside himself so that he had to be confined, even by the administration of five grains of quinine. I have never seen any of these symptoms last much longer than twenty-four hours.

Many more severe effects have been remotely ascribed to quinine or rather to the large doses of it as given in the south, such as the enlargement of the liver, and spleen; inflammation of the stomach and bowels of a sub-acute character; a dropsical condition of the body; palpitation of the heart, etc. These effects were known to the world as sequelæ to intermittent and other fevers of a miasmatic origin, before quinine was ever given in its present doses, or in fact before it was known. I have never found any occasion to believe that quinine gives origin to inflammation. Chronic affections of the liver and spleen are incident to hot and malarious climates, without disease even as a cause. I believe them to be often the pure result of malaria. Enlarged spleens are quite common, chiefly with those who have suffered from intermittents, but frequently with persons who have never been sick, though living in a malarious district. There is one effect of quinine which I would not mention were it not proper to say whatever one believes and has seen: it is the soothing and quieting action that it exerts over the stomach and intestines. I have never seen this mentioned, but it has been frequently forced on my observation.

The effects of malaria are most evident in the convalescence of disease. This is a matter accomplished with great difficulty in the most sickly regions of Florida; and chiefly if the disease has been long in its continuance. From the effects of a severe fever, or dysentery, a patient will remain for weeks in *statu quo*: when on any day you might expect to find him well,

until at last you examine the pulse attentively, and find that hectic has set in; the patient has night sweats; the bowels are loose; pain in the abdomen and head; emaciation comes on; the tone of the stomach is weakened; the secretions deranged; the skin improperly exerting its offices; the assimilating power acting improperly, and the *vis vitæ* irreparably shattered. This state of things I have only seen at the most unhealthy posts, especially at Forts Macomb and White, where I was first stationed, when I came to the territory. I have sent patients from there to the general hospital at Cedar Keys, in this condition, of whom a causal observer would have said, they will not live to reach the island; yet no one could fix on any disease these men had: they were convalescing, and under the better atmosphere of the sea shore, they nearly all recovered.

One circumstance has often attracted my attention, in cases of diseases from miasmatic origin in this country; that is, the abnormal actions of the heart: its beats, if the patient has been weakened by disease, are so tumultuous and diversified, but afford withal so little sign of organic lesion, that if not acquainted with the former condition, your prognosis will be most unfavourable. I have seen, however, but few permanently bad results from this cause. If you strengthen the patient by tonics, and remove him to a more healthy spot, even in Florida he will recover. I look upon this as a deranged miasmatic nervous action in that organ, for which change of place and tonics afford the best cure.

I believe there is scarcely a person even in good health in a miasmatic region, who is not subjected in some degree to the effects of the unseen agent around him. His rest will be broken at night; his appetite will not be so good as formerly; he will not enjoy that feeling of full health he has been accustomed to; his system will be attacked from time to time by the offshoots of the diseases preying on others around him, though he will not be sick, and may escape with a like freedom from disease during all his sojourn in the country: but the whole constitution seems in some degree to labour under the influence of malaria; slight wounds that would heal elsewhere in a few days time, must here undergo the slow process of suppuration and granulation, and even this does not come on easily, the wound remaining for days without any visible advance towards a cure. You cannot persuade a slight incision of the skin to heal by adhesive inflammation; and I have repeatedly seen wounds in habits that were to all intents healthy, assume a deep burrowing suppuration. I was in the habit at first of discharging patients when the wound had closed by granulations, if not on an important part of the body, and not interfering with common duties; but on the slightest exercise or excitement, inflammation would set in afresh, in surrounding parts; the granulations would assume an unhealthy appearance; suppuration, if it advanced, would be slow in progress, and of an unhealthy form: perfect rest, good diet, occasionally tonics, and poultices or stimulants to the part, were necessary for a cure. Many of these wounds,

especially about the fingers, ran rapidly into deep-seated inflammations. A miasmatic constitution of the atmosphere is particularly favourable to the formation of whitlows. I treated these by deep incisions, and generally, some hours after, sprinkled the part over with calomel; but the first incisions scarcely ever proved sufficient; the suppuration would extend beyond them. The pain of these whitlows will take on regular remissions.

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ART. II.—*Account of a Blighted Fœtus of the third month, having the umbilical cord extensively coiled around the right knee and lower third of the thigh, discharged with a living child at full term: with some reflections connected with the questions of Superfœtation and Spontaneous Amputation.* By A. LOPEZ, M. D. (Read before the Medical Society of Mobile, Alabama, May '6, 1845.)

I PRESENT to the society this evening, a specimen of a *Blighted Fœtus*, whose history is as follows :

*Louisa*, a coloured woman, the mother of other children, was delivered by a midwife on the 10th March, 1839, of a *healthy living child* of perfect development, and at the full term of gestation. On Tuesday the 12th, I was summoned in great haste, and on my arrival found the conclave of old women terribly astounded by the discovery of a new subject, which had been thrust aside unobserved, on the day of parturition, among the soiled clothing and discharges from the uterus. Upon examination, it proved to be the specimen which I now exhibit. To all appearances it had attained its *4th month*, although it is with difficulty that we can accurately specify the early fœtal age, owing to the uncertainty as to the time of conception, and because, we all know how much the growth of different fœtuses varies. The skull is so entirely compressed as to expand its proportions and bring the opposite parietal surfaces in close contact. The entire body is likewise much distorted and flattened, doubtless from the pressure exercised upon its plastic nature by the uterus and the other child, for so long a period prior to its expulsion. There is not the slightest decomposition, and its aspect was even less unfavourable, before it had undergone such long maceration in the alcohol, necessary for its extra-uterine preservation. The membrane which you see was found separately discharged. I obtained no information concerning the placenta, but an additional interest is afforded to the case, from the fact, that the *umbilical cord, no larger than a small thread, will be seen entwined around the right knee and the lower third of the thigh.* It was much more so when I first possessed the specimen, but on its transportation from South Carolina to this place, the bottle was broken, the spirit evaporated and the entire surface covered with a thick mould, so

that in my effort to clean it, the upper portion of the cord from the umbilicus to its attachment upon the thigh was unavoidably detached. I have said that this circumstance added interest to the case, and I so consider it, because, independent of other physiological questions to which such sports of nature give rise, and to which I shall presently refer, there is superadded, the problem of what some writers denominate "*spontaneous amputation*" in utero, from this condition of the cord as well as from other causes purely hypothetical. The upper extremities are very much deformed and displaced by the pressure, so that the left shoulder (with its scapulary connections) is entirely twisted over the back of the neck, giving it the appearance of arising from the same origin with the right.

I have given you the history—and you have the specimen for examination. Perhaps, in the true spirit of medical philosophy, as well as the common sense view of the question, it would be wise to leave it at this stage of its notoriety—because, to take one step further in speculation, is neither more nor less than to plunge into the same limitless, unfathomable sea of uncertainty, in which the ablest and most ingenious minds have invariably been lost, whenever they have attempted to solve the enigma of conception and generation. "Man is fearfully and wonderfully made" are the words of the inspired man of God, and he did no more than declare a truism, derived from the first incomprehensible creation of our race out of the "dust of the earth," to the present day, at which the same creative will, of the same Omnipotence, sends forth his creatures to fill the globe through the instrumentality of animal copulation. Trace this wonderful problem from the most insignificant mite, that owes its brief tenure of existence to equivocal generation, up through the almost endless chain, to *Man!* and say how much wiser are we now for all the labour of the mightiest intellects—the war of words—the rancour of controversy, and the arrogance of short-lived dogmas to which this high and as yet intangible question has been subjected? We breathe, we cry, we feed and perish! This is the sum total of our knowledge, and it is all we are likely to know of how a child is begotten, developed or perfected, because it is self-evident that it is the pleasure of the Creator, in his wisdom, to conceal what he listeth from our scrutiny.

Yet aware of all that I have said, I must be pardoned for trespassing upon your time, as I desire to submit to the society a few remarks upon four subjects collaterally belonging to the specimen now upon your table.

*First.* Is it a blighted fœtus of one conception?

*Secondly.* Can a dead fœtus be retained to full term with a living one?

*Thirdly.* Is this an instance of superfœtation?

*Fourthly.* What would have been the chances for spontaneous amputation had the fœtus progressed to maturity with the present condition of the umbilical cord?

These are all questions of laudable curiosity—nothing more; because, grant that we ever should, by the triumph of intellectual industry, tread

the labyrinth in which they are now concealed—*cui bono*? The discovery could not avert the issue, it could not abate one jot of human suffering, nor could it shed one additional ray of lustre upon those immutable truths of physiology, pathology or chemistry, whose results are *practically* applicable to everyday purposes. This is the age of utilitarianism, and unless I am much deceived, we are every day completing its ascendancy over the incitements to man's industry and ambition. The people now demand servants—they are tired out with philosophers.

Let us revert, then, to our first question—*Is it a blighted fœtus of one conception.*

I cannot consent to settle this either too summarily or presume to do it too definitely, inasmuch as other circumstances of an imposing character oppose such peremptory decisions. But in advance of further inquiry it may not be amiss to establish the fact that the discharge of *blighted fœtuses* is not of unusual occurrence, and that they do occur, both alone and simultaneously with others more mature. To the former class I shall for the present confine myself and I refer the society to the records of such similar cases as I have from 1839 to the present time collected. I will endeavour to condense their histories as much as possible.

1. The *Chevalier Galbiati*, in a communication to the *Med. Chir. Society of Naples*, Nov. 29, 1834, states that cases where the fœtus dies during utero-gestation and is retained to the full term, are not uncommon. He therefore reports the following. (*Gaz. Méd. de Paris*, Aug. 1835, from *Observ. Medico.*)

(a) The *Marchioness of M.* was affected during the 4th, 5th and 6th months of pregnancy with moderate discharges of blood from the womb. At the 4th month, the uterus was of equal volume as at the third. At the 6th the same as at the 5th, but less than at the 3d. During the 7th month hemorrhages more abundant, followed by pains and the *expulsion of a fleshy, fresh organized body* in which was contained a *fœtus shrunk and atrophied as if plunged in alcohol, apparently not over two months old.*

(b) The *wife of the Chevalier M.*, in the *third* month of gestation, fell from a carriage; slight hemorrhage; no pain; by proper means abortion prevented. She proceeded to the 5th month increasing in volume. At the 6th month the uterus opened, accompanied with hemorrhage. Size scarcely equal to the 3d month: pregnancy doubted. At 7th month, another flow, but there being no sensible development, he concluded against pregnancy. At the 8th month she was allowed to resume ordinary diet, hitherto forbidden through fear of abortion. Undue indulgence produced another hemorrhage, resulting in the *expulsion of a shriveled fœtus about three months old.*

(c) *La Marchesina G*—examined a short time previously to the 3d month, pronounced pregnant; increased to the 4th and 5th month. At the 6th month, traces of blood, and the uterus of less size than at the third. It was therefore decided that pregnancy had ceased to progress. At the 7th month hemorrhage and *expulsion of a fœtus, as large as the fruit of an almond, supposed to be less than three months old.*

2. *Dr. Porter*, of New London, Connecticut, reports a case of retention

of a dead fœtus in utero from the 5th month; and its expulsion at full term. *It was free from putrefaction.*

3. Dr. Hays, Editor of the *Amer. Jour. Med. Sci.*, reports in that periodical (Aug. 1837, p. 535) the case of a fœtus blighted between 3 and 4 months old, expelled at full term, *entirely free from putrefaction*, the umbilical cord several inches long, and as *small as a thread*. Nine months antecedently the mother conceived; ceased to menstruate, and suffered all the attendant circumstances of conception. About the period of quickening flooded profusely; this ceased; she progressed and quickened, after which the breasts became flaccid, her size diminished, and she carried the fœtus to full term.

4. In the *Lond. and Ed. Monthly Jour. of Med. Sci.*, for October, 1841, is reported a case taken from *Journ. de Chir. et de Med. Prat.*, for May, 1841, in which Dr. Brett of Guingamp, relates an instance of *delivery at full term of a fœtus dead 4 months previously*. It was six inches long, skin rather reddish; *epidermis not removed by friction*; it appeared to be 5 months old.

These facts demonstrate that the human embryo is frequently from some cause, blighted or arrested in its progress towards a perfect and mature development, and that notwithstanding its privation of those elements essential to its well-being, or, as the French would express it, "*faute d'aliment*," it still can be retained in the uterus, and preserved from the decomposition incident to all dead matter, whether animal or vegetable. And it is further proved, that this organ of exquisite sensibilities and diffusive sympathies—which feels itself (so to speak) called upon to dislodge all offensive and foreign substances, ay, even its own nurtured offspring, whenever its innate susceptibilities notify it of the necessity—encourages the presence of a substance whose extinguished vitality, one would suppose, might prove not only a source of irritation to itself, but prejudicial to the integrity of the healthy and living fœtus. How and why is this? I shall, before I conclude this paper, be compelled to refer to its solution, but with the faintest of all faint hopes of success.

I now pass to the next branch of the subject:—*Can a dead fœtus be retained to full term with a living one?* The specimen now on your table answers affirmatively. Is this an isolated case, or are they not rather of frequent occurrence? I will offer no apology for consuming your time with the details of such analogous cases as I have from time to time harvested, for my own instruction. The case then is not isolated, nor of rare occurrence, where a *fœtus blighted at early age is retained and expelled at full term with a living child*.

Dr. Desfermon, of Paris, communicated to the *Med. Soc. of Emulation*, a case where the first child was born healthy and well developed, and with a perfect placenta, *to which there adhered a mass which proved to be a second child with its secundines*. It was a *flattened fœtus* apparently arrested at the third month. The mother thinks the duration of her pregnancy was ten and a half months. (*Bulletin des Sci. Méd.*, July, 1829.)

Dr. Colombe delivered a woman in 1827, naturally, at full time. The placenta attracted his attention—it was divided into two distinct parts,—

very different in their volumes and anatomical relations. The membranes of the defective placenta were entire and contained a fœtus apparently four months old, deformed and flattened, skin shriveled, pale, and slightly livid.

N. B. About the third month the mother was much fatigued and suffered from anorexia, pains in the abdomen, hips and pelvis. (*Oper. cit.*)

Dr. Ingleby, Lecturer on Midwifery, &c.,—favourably known as the author of an excellent work, entitled "*Facts and Cases in Obstetrical Medicine, &c.*" reports in that work the following account: In a case of pregnancy a healthy placenta was expelled by hemorrhage. In the membrane adherent to the uterine surface, there was found a four months fœtus enclosed within its membranes. It was flattened but not putrid, and in connection with the same placenta there had just been expelled a healthy, full sized child.

In the *London Lancet*, Oct. 30, 1841, Dr. Streeter relates a case to the Westminster Med. Soc., as occurring in the practice of Dr. Hughes. One fœtus was alive at full term, the other blighted, having perished apparently at the third month—consequently retained nearly six months after its death. It was probably expelled during the labour a few minutes before the placenta. It had undergone very little decomposition and was squeezed quite flat.

Collins, (*Practical Midwifery*, p. 317,) records three cases where one fœtus was blighted at an early period and retained until the other was nearly or completely developed.

Cruveilhier (*Anatomie Pathologique*) relates one case.

The most remarkable case, however, of any that has met my eye, is recorded by Dr. Robert Lee, who saw a case of triplets, two of which perished at the third or fourth month, were retained to the full term of gestation and then expelled, attached to the placenta of a living child.

M. Menard, of Nantes, relates (*Lond. and Edin. Monthly Journ. of Med. Sci.*, Oct., 1841, from *Journ. de Loire Infer.*) the birth of one fœtus five months old, dead and atrophied, the other, living, healthy, and at full term. The first he describes as "putrilage cremeux," in detached pieces, the remainder of which had finally to be extracted by the fingers, in one dry, soft, and flattened mass. Two hours afterwards a healthy, living child was born.

Dr. Perkins, of New London, in a letter to Dr. Porter, May 16th, 1840, relates as follows. On the 18th April, 1840, he delivered a woman of a healthy male child, full term, weight nine pounds, the patient did well. That night she expelled a fœtus enveloped in the membranes; it was between four and five months old, entirely undecomposed, of natural colour, perfect in form for that age, except the head, which was compressed.

Mauriceau (*Dis. of Women with Child, &c.*, Ed. 1688, p. 44,) gives the case of a young woman delivered at the usual term of two children, one of ordinary size, the other dead, in third or fourth month of its age.

These instances, for the present, conclude the second division of my subject, and serve to exhibit one of the irregularities likely to occur in the function of an organ in which we would expect *à priori*, to find an uninterrupted chain of cause and effect, and from here we proceed to our next inquiry, viz.—

*Is the specimen before you a case of Superfætation?*

In order even to approach a satisfactory hypothesis we must be content to take an infinite number of questionable propositions for granted, else, groping as we do, with our eyes hoodwinked, the further we proceed the worse would confusion become confounded.

It were profitless to call your attention to the various theories, which, ingenious and short-lived, gave place to one another through each cycle of human speculation. Yet it may be well to defer further consideration for a while, and summarily inquire what is understood by *superfœtation*.

As early as the time of *Hippocrates* he defined it to be "*a reiterated conception, when a woman being already with child, conceives again the second time.*" Passing down from that epoch to the most recent understanding of the question, we reach the definition given by *Mr. Guy*, in his late valuable work on "*Medical Jurisprudence*," who calls it "*The conception of the second embryo, during the gestation of the first, the products of the two distinct conceptions being born, either at the same, or at different times.*" (p. 109.) Thus we perceive that these two definitions differ only in phraseology, but are identical in substance, so that there exists no difference in the meaning of the term through this vast interval of time, however the learned may have disputed *ad interim*, as to the *modus operandi*, or even its possibility. We will proceed now to examine if there be any instances on record which approximate more nearly to the probable limits of this category, than those cases which I have hitherto cited.

I shall commence with two cases, whose history is more familiar to me than others—both of them having occurred within a short distance of my former residence in South Carolina.

The *first*, I have received from several very old and veritable persons, whose parents lived contemporaneously with the event, and it is the case referred to by *Buffon*, and quoted by all subsequent writers since his day as the "*American Case*."

The *second* occurred at a plantation on the *Pee Dee River*, not more than eighteen miles from my residence, and I have frequently seen one of the children. The history of the "*American case*" is this:—Between Charleston and Georgetown, (S. C.,) on the stage road, there was a small settlement, now uninhabited, but still called, from the event, "*Mulatto Town*." A white man first, previously to leaving his bed at daylight, had connection with his wife—and left her to call his negroes to labour. In a very short time after he sent his negro to the house on some errand, where he found the woman yet in her bed, and he *also copulated with her*, according to some accounts, through her salacious entreaties; others said she yielded to his threats of taking her life, (which, by the way, would defeat conception, if the views of some medical jurists be correct.) Be it as it may, in due time *she was delivered of two healthy living children, one white—the other mulatto*.

*The Pee Dee Case*.—On the plantation of a gentleman, about thirty years ago, there was employed an Irish overseer. He entered the hut of a negro one morning and copulated with his wife, just as he had left her



to go to his work, first having connected with her himself. In regular process of time the negress was delivered of *two healthy, live-born children, one perfectly black—the other a light mulatto*. They were both, I think, alive in 1840, when I left Carolina. The mulatto boy I know was. He was named after his white father, and was a likely, intelligent man, about twenty-five years of age, and the favourite body servant of his master.

*Dr. Walsh*, in his "Notices of Brazil," (vol. 2d page 90,) relates the case of a creole woman who gave birth to *three children at one time, one white, one brown, and one black*. Each child had the features of its respective class.

*Mosely* (Dis. of Trop. Climates, p. 111) reports the case of the "Ohortwood Estate" at the island of Jamaica, where a negress brought forth *two children* at a birth, *one a negro, the other a mulatto*. When interrogated as to an explanation, she answered, that a white man on the estate came to her hut one morning before she had risen and she suffered his embraces almost instantly after her black husband had quitted her.

*Zacchias*, in his "Consilia," (see Guy, p. 110, who also refers to the two preceding cases,) states this case. *J. N. Sobrejus* lost his life in a quarrel, leaving his wife pregnant. *Eight months afterwards*, she was delivered of a dead and deformed child. *One month subsequently*, she gave birth to a perfect living child.

*Foderé* (vol. i. p. 484–6), cites the case of the wife of Raymond Villard, of Lyons. She was delivered on *20th Jan. 1780* of a living 7 months child. On the *6th July following*, (five months after delivery,) she was delivered of a living daughter.

*Beck*, (*Med. Jurisp.* vol. i. p. 222,) reports that *Dr. Maton* delivered a woman of two male children perfectly developed *at intervals of nearly three calendar months*. *Beck* quotes other cases, viz., three at intervals of *one month*, three of *two months*, and one of *four months*.

*Pliny* tells of a servant girl who *copulated the same day with two several persons*. She gave birth to two children. *One resembled the master, and the other his proctor*. (Vide *Mauriceau*, p. 42.)

*Delmas*, surgeon at Rouen, delivered a woman at the hospital, of *two male children at one birth*, at 8th month of gestation. *One was white, the other tawny, placenta united*. She confessed that *she had cohabited with a white man, but twice had yielded to the importunities of a negro, when she supposed she was 4 months advanced*. (*Amer. Journ. Med. Sci.*, Aug. 1828, p. 421.)

*Dr. Jamieson*, of Dublin, relates of a lady who was confined of a healthy child which she nursed, and *seven weeks afterwards*, she brought forth *another of six months gestation*, with membranes entire and placenta attached. It was from 8 to 9 inches long. The first was born 13th Feb., the last 3d April 1841. *The dead fetus had remained in the uterus forty-nine weeks*. (*Dub. Journ. Med. Sci.*, Sept. 1841.)

*John Irvine*, surgeon, British navy, records the case of a woman delivered by *Dr. Burleigh* on the *first of October* of a female child, at full period of utero-gestation; healthy, rather small sized. Placenta expelled in three hours by natural contractions. On the 2d Nov. she gave birth to a healthy large male infant. Placenta followed in two hours. (*Med. Times*, Dec. 28, 1844.)

*Dr. Pertus* (*Rev. Med.*, March 1838) gives the following case. Pregnancy, June, 1837. On 20th September, slight hemorrhage with pains lasting eight days. Examined the coagula and discovered a *fetus 3 months*

old with its annexes, and *subsequently an entire ovum, in which was a fœtus of 5 weeks old.* (Vide *Amer. Journ. Med. Sci.*, 1838, p. 454.)

Smellie ("Cases of Midwifery," *Ed.* 1754, vol. 2d, p. 85) reports a case occurring in the practice of Mr. Campbell. A woman delivered of her first child, followed by severe after pains. *Five days after she miscarried of a fœtus, 4 or 5 months old, still born, no signs of putrefaction, having neither hair nor other signs of having been longer conceived.* Smellie wrote to Mr. Campbell, "what you have written me seems to favour the doctrine of superfœtation, *more than anything I have met with in practice.* But there are instances of extra-uterine fœtuses which have laid whole years in the abdomen without being putrefied."

In the *Providence (R. I.) Gazette*, Aug. 1845, the following case is reported:—"A coloured woman in the town of Bristol was on Friday week delivered of a *black child*, and on the Sunday following of a *white one*. The first was very black, and the last as white as children of white parents ordinarily are. The children have been seen by nearly the whole town of Bristol, and the facts are corroborated by *Drs. Holmes and Briggs*, who attended on both occasions of child-birth."

In the *Amer. Journ. Med. Sci.*, July 1842, p. 220, a case is contributed by Prof. Beck, taken from the *Bulletin de l'Acad. Roy. de Paris*, (Séance, Dec. 21, 1841.) The article is headed *Superfœtation*, but I do not think that it legitimately belongs to that classification. *M. Renaudien* attends a female who aborts in the sixth month of pregnancy; she died under severe symptoms of puerperal peritonitis seven days after. Dissection discovers a uterus with *a double neck and two cavities*, with an ovary, fallopian tubes and ligaments to each. Hence it is inferred that "superfœtation *might have occurred.*" I do not recognize a case to be one of superfœtation, unless the anatomical structure of the receiving parts is normal, and the reiterated deposit of germinal matter is received *cæteris paribus*. It requires no stretch of the imagination, nor tax upon our assent, to acknowledge that with two distinct receptacles and two distinct sources of supply, each should be capable of appropriating to itself its peculiar burden. The genital conformation of numerous animals sanctions this admission, but this is not what a rigid inquiry would demand as definitive. The idea of superfœtation is strictly confined to the possibility (denied by our opponents) of a single uterus receiving the ovum for a second child, *after those actions are set up within the uterus and its appendages, which are declared to be antagonistic to such a result.* I have deemed it proper to notice this case briefly, and I apply it to all such other instances as have been or may be adduced. I do not wish the argument trammelled; it asks only for the irrefragable proof, such as the case I have given from the *Providence Gazette*, and others of like kind, where the *different colours* of the children paralyze theories.

The last case I shall offer is from *Casper's Wochenschrift*, Jan. 28, 1842. A black woman of Surinam copulated during the same night with a negro and an *European*. She was delivered of *two children, one a negress, the other a mulatto*. Dr. Hille, a Dutch surgeon, attached to the army at Surinam, states that in 1841 the children were alive and then eight years old. He adds that at the death of the mother, who died previous to this period, an examination revealed the *genital parts to have been perfectly natural in their structure.*

While preparing this essay I have been informed by a friend, of a case which occurred about 1830 in Virginia. The authority is strictly veracious,

and the parties from whom the information was derived are of the highest respectability and intelligence. A negress at the time of corn harvest was met by a *negro man* in a remote part of the field, who copulated with her. On her way back a short time after, she was encountered by the *white man* to whom she was hired and he performed the same act for her. In due time she brought forth *two children, one perfectly black; the other mulatto.*

Having completed the narration of such human cases as I had collected, I will conclude this part of the question by an *alleged case of superfœtation in a goat*, as communicated to *Dr. Berjaud* by *Dr. Pertus.*

The animal had received the caresses of the ram in the beginning of Dec. 1836 and was supposed to have fecundated. She however continued to manifest a desire for the male, and was gratified fifteen days afterwards. In the beginning of May she brought forth two young, perfectly formed, but refused to suckle them, and they perished. The abdomen remained large, and in 15 days more she brought forth *three others perfectly formed*, and seemed very anxious to nourish these. They lived three days. N. B. The term of gestation in goats is five months, at which period, the first two were born, and the *number of young generally two*, but never more than three.

The recital of cases bearing so similarly upon one subject has perhaps proved tedious, but as they were the result of some years' collection, I thought that while they answered no immediate purposes of utility, they might at least serve as an index to others, who, desiring to pursue the investigation would have their labours thus far abridged.

In reviewing what I have written I find that I have submitted for your inspection a specimen which under any aspect is of curious interest, whether we regard it as a blighted fœtus of one conception of twins, or whether it be considered one of those embryos whose uterine existence depended upon the yet unsettled hypothesis of superfœtation. It is equally worthy of reflection, viewed merely with reference to that singular and unaccountable property of the uterus, by which it is enabled to retain within its cavity for an indefinite period, a decidedly foreign substance, unnecessary to it as an organ, and destined to act no part in the great works of nature. Moreover, this foreign dead matter remains undecomposed, free from putridity, the fate of all other dead matter, animal or vegetable, especially when subjected to two such agents as heat and moisture.

How is this? Upon what depends this antiseptic property in most of the cases so far recorded? Why is it, to use the language of a distinguished author, that "*whatever may be the condition of the dead fœtus, the tenantry of the living one, even up to its maturity, may in no respect be affected?*" It cannot be wholly or scarcely at all ascribed to the preservative qualities of the amniotic fluid, because *firstly*, the component parts of this fluid are not qualified for this end, consisting, according to recognized analysis, of the following elements, "*a peculiar acid called amniotic, with a little muriate of soda and ammonia, and a trace of the phosphate of lime, and is said to be very little more than water having a little gluten in it,*

sometimes serous and pellucid, *at others turbid and offensive.* We are further informed, that while in the earlier months, it is nearly if not quite transparent; *as pregnancy advances it becomes turbid, containing more or less of what appears to be mucus:*—Secondly, we know that in the advanced stages of gestation, when the fœtus dies from any cause, and is retained but a very limited time after death, we find on its expulsion the incipient signs of decomposition, and frequently entire decay and desquamation, which signs are adopted to guide medical jurists in their investigation of criminal cases. Why, we ask, should this unavailing mimic miniature of man escape the influences of destruction, when older embryos, capable of maturity, with increased resources of vitality, undergo the almost universal doom of dead matter?

Another curious point of inquiry is created, as far as it may affect the right of property, vesting upon proofs of legitimacy—and this warrants the question,—*Does or can Superfœtation occur?*—We are constrained to admit, in the premises, that the array of objections to the doctrine are specious, and are urged by some of the ablest physiologists. But it is equally evident that in the midst of their doubts and investigations, facts have stared them in the face of so obstinate and irresistible a nature as to qualify in a very material degree these objections. The admissions to which, from time to time they are driven, are based upon certain contingencies, and I think that the authorities and facts which I have already presented, afford these contingencies beyond dispute. The opponents to the doctrine further feel the difficulty of persisting, as is frequently to be seen when consulting the cases quoted even by themselves, from which, it is evident, they cannot fly, and, in short, I am clearly of opinion, after a mature review of everything they have written against it, that they rather uphold than invalidate the theory. It does not, nor must it satisfy the inquirer after truth, to have the alternative crowded upon his acceptance in the form of a compromise. In science, no such thing is admissible. *It is either superfœtation, or it is not.* We do not receive the substitute of terms, such as “superfecundation,” “cotemporaneous conception,” “secondary ovum,” and the like, because, surely, if connection by coition through means of one man impregnate, it is *one fœtation*; and if another man perform the same act upon the same woman (within prescribed period assigned to gestation), with equal success, it is *super-fœtation*, that is, a second fœtation superimposed upon the first;—so that, with all the distortion of which language is capable, it cannot be construed otherwise, for as I have before said, the cases I have presented substantiate the fact, else, literally, (as in some of my examples,) *black and white* have at last ceased to be proof. Hence there is no good reason for denying to the influence of copulation by one man, that which examples concede to claim of the same act by different men, and even of different colours. Besides, it is common for the husband to cohabit very regularly during the early months of pregnancy;

for in the report which I have given of *Dr. Defermon's* case, this fact is specially stated in evidence, and as the *occlusion of the uterus* and the formation of the *membrana decidua* are amongst the prominent arguments against the possibility of superfœtation, there is a propriety in pushing our inquiries somewhat further, and ascertaining, if possible, at what precise time these *quarantines* of the uterine dominions commence, and at what time their ports are irrevocably closed against foreign entries. It is admitted upon authority not to be slighted, that although the deciduous membrane does exist at the time specified, and notwithstanding the cavities for some wise purpose are in a manner closed, yet neither this membrane lining the uterus, nor the mucus which effects the closure, *although in contact* with the orifices and cells of that organ, adhere so firmly as to prevent the ingress of the semen or even of the "aura seminalis," provided this last be admitted to exist. Nor do they prevent the frequency of hemorrhages during gestation,—nor is the menstrual discharge, to which some women are subject while pregnant, obstructed. But, say the opponents, when the uterus is impregnated, the Fallopian tubes, instead of "running horizontally," to the organ, "lie parallel to its sides," thus preventing them from embracing a new embryo from the ovaria. This might be reasonable, if it applied to a fully developed uterus, but not in early pregnancy, when superfœtation is supposed most commonly to occur. The weakest and last objection urged is, that if superfœtation were true, the new embryo would prove destructive to the first. This objection is weak and generally set down as the most gratuitous of all of their assumptions. Let us return, however, to the closure of the uterine orifices by the causes already named,—and we are prepared to meet this objection by falling back upon old *Mauriceau*. (*Op. Citat.*, p. 44.) He says, "when a woman is animated with an *earnest desire* for copulation, in the heat of the action, she sometimes dischargeth by the passage that terminates in the bottom of the womb, which *being dilated and opened* by the impetuous endeavours of the seed, *agitated and overwhelmed* more than ordinarily, and this orifice *being at the same time a little opened*, if the man's seed is darted into it *at the same moment*, it is thought a woman may then again conceive, *which is called superfœtation.*" "Therefore," he adds, "I am not willing to say that there never is any superfœtation, but that it happens very rarely."

An admission to this extent from *Mauriceau* is of force, as it is extracted from him during researches which might rather be said to be adverse to the doctrine than otherwise. And if its occurrence is granted as being possible in one instance—it fixes the fact. Its frequency is irrelevant. Nor should the period at which he wrote be objected as "*antiquated*," since I am clearly of the opinion, that on this subject and its embarrassments, the light since shed has been greatly disproportioned to the advantages enjoyed by later inquirers, and perhaps in no age have medical men, aspiring to eminence, involved themselves in grosser absurdities, nor

entrenched themselves behind more inconsequential arguments, than have some of our own times.

Again,—all who canvass the broad question of generation, speak of an “*orgasm*” as essential to certain results. Now this is a term and nothing more. Men are in the habit of using different names for the same things, and upon a rigid comparison, I unquestionably prefer the unsophisticated energy of the ancient writers to the hyperclassical and affected confusion of the latter day neologists. For example, on this very subject, where *Mauriceau* speaks of “an earnest desire for copulation,” he at another place (p. 22) says, “now to the end, these different sexes should be obliged to come to this touch which we call copulation: besides *the desire of begetting their like, which naturally inclines them to it*, the parts of men and women destined to generation, are *endued with a delightful and mutual itch, to stir them up to the action*, without which it would be impossible for a man, (so divine an animal,) born for the contemplation of heavenly things, to join himself to a woman, in regard to the uncleanness of the parts, *and of the act.*”

Now these various expressions, no less graphic than true, certainly harmonize with all after opinions, and sustain the litigated points yet at issue in questions of rape, as to whether a woman can be impregnated unconsciously during sleep; or whether she can conceive from forcible copulation, when her weakness “not her will consents;” and many others equally doubtful to the minds of jurists, medical and legal. Why then deny to the uterus the competency of repeating the procreative power under the *orgasm* excited by *two different men* upon the impressionable ova, provided two pre-requisites obtain?

1st. That the act be performed within the time prescribed by the advocates of superfoetation, as well as by its opponents, (who have been forced, at times, to yield certain grounds,) viz., *within thirty days after the first conception*. 2d. That the proposition be true with regard to the anatomical and physiological *condition of the parts in the early months of gestation*. If then the os uteri be capable at this period of affording an exit (as has already been shown) to the catamenia, hemorrhages, and even the expulsion of the ovum, it argues that these supposititious barriers are not so formidable as might be imagined. We are justified in a contrary inference from all observation. Indeed, a still stronger point can be established; because this *membrana decidua*, the very gate of the inner temple, to some physiologists, *might as reasonably be urged against the introduction of any ovum at all*, whether of the first or subsequent copulation, since, according to *Hunter*, the uterus is lined with “*a soft, humid, paste-like secretion*, furnished by the secreting vessels of its lining membrane,” (constituting his deciduous membrane,) “from the earliest trace of impregnation which is to be observed in the cavity of the uterus,—AND EVEN BEFORE THE OVUM HAS REACHED IT.” If then it can keep out the last ovum to suit our

opponents—why it certainly, if Hunter be correct, can reject the first. This adventitious membrane, which, according to my belief, is doubtless destined for *ultimate*, and not for immediate service, upon all good authority “appears to be nothing else than an effusion of coagulable lymph,” having, as Hunter says, “scarcely a more firm consistence than curd of milk or coagulum of blood,” and in the opinion of *Rigby*, (p. 48,) “although much thicker than the other membranes it is much weaker.” *Rigby* also says, “inferiorly near the *os uteri* it becomes thinner.” Again,—“it is much more loosely connected with the uterus during the first months of pregnancy,” “and this is one reason why premature expulsion of the ovum is more liable to take place at this period than later. (*Loco. cit.*)” With all these facts it is no very fanciful conjecture to suppose that the same state of the parts, which is capable of permitting substances, from a slimy secretion, to an entire ovum, to escape, may allow others to enter. Nor is it unlikely that a frail and soluble substance no firmer than a “coagulum of blood, or the curd of milk,” should give way to the violence and heat of orgasm, or to the vehemence and force of an ardent copulation, when both parties are maddened to that state of unconscious and ungovernable passion, to which has been eloquently ascribed the genius of the Athenian orator, who was said to have been “conceived in rapture, and in fire begot.”

My views seem more directly confirmed by another fact, which is, the frequent occurrence of *early abortions*, ay, and later miscarriages, in consequence of undue and violent venereal indulgences.

You may recollect that I had occasion to refer to Dr. Deferron's case, in order to show that this supposed occlusion might be disturbed by regular cohabiting during the early months. Permit me to call your attention now, to another feature in that case; then omitted. The reporter states that “six weeks after conception she underwent fatigue, after which, spots of blood were seen on her linen.” May not those spots have been the result of laceration, or a breach of continuity of this very slender, incipient curtain, no stronger than coagulum, occasioned by a violence of venereal pleasure; within the term prescribed on all sides, for the possibility of a second conception? The thing is not even improbable.

The possibility of superfœtation is also admitted by distinguished physiologists, provided the intervals between the respective copulations be short, (as for instance in the “American case,”) and they further concede, that where the two children are born at an interval of some months, they are not twins, although existing some time together in the mother's womb: (vide *Richerand Phys.*) And in these opinions they are sustained by recorded examples, but more especially, if you recollect, those reported by *Beck*, where the intervals extended from one to four months between the births of mature children. This evidence disarms even *Guy* of his doubts

concerning superfœtation, because he admits that "a long interval, say of four months," favours the doctrine: (p. 113.)

There are other though less weighty objections to our theory, among which is the presumption that there cannot be a case of superfœtation, where there is "one placenta for both children;" and *Mauriceau* goes so far as to declare all such to be cases of "twins of one conception," inasmuch as he believes that out of every one hundred women who have twins, "ninety have but one burden common to both." In this opinion he is at direct issue with *Hugh Chamberlen*, the translator and editor of his book, who in his comment upon this assertion says, "The author is out of his computation, for there are *near as many with two as with one burden.*" Besides, you may call to mind the case I cited from the *Med. Times*, where two children were born, one on the 1st. Oct., the other on the 2d. Nov. following, *each having its distinct placenta*, and as the record of similar cases is by no means rare, it is useless to cite them, for these exceptions, condemning them out of their own mouths, would become too numerous, to promise them the shadow of a general rule. Another weak, though plausible objection is, that "conception may take place in the same ovary, or in different ovaries, from the nearly simultaneous application of semen, *whether of the same man or of different men:*" (*Guy*, 110.) This is a gratuity in speculation not admissible, for, when an unknown cause is attempted to be set up in opposition to a known effect, for which a strong presumptive cause can be assigned, and when both are equally incapable of demonstration absolute, the assent of our reason is justly due to the most rational. Now in my review of the definition and the possibility of superfœtation, I urged that a *second impregnation, superimposed upon a first*, came distinctly within this category, and I care not what the interval of the respective copulations may be, because I maintain that *conception and impregnation is an instantaneous act*. I believe that whenever a consentaneous condition of two individuals exists (*cæteris paribus*) so as to produce the ecstasy capable of arousing the response of the ovarian orgasm to the impulse imparted by the male semen, *that impulse is electrical, immediate and irresistible*. I believe that the venereal torch is applied to materials, combustible and already prepared for their destiny, and that the vivifying power has not to wander about for its election, and incubate the ovum as a hen does its egg, but, that the ripened and expectant germ attracts by its specific, although mysterious affinity, the messenger sent to usher another being upon the stage of human life. What the result may be, remains for future development; it may perchance eventuate in hydatids, in mole, or any other lusus, according to the functional integrity of the respective machinery afterwards called upon to complete the work so beautifully begun.\*

\* *Nisbet* says, "The *retrograde* action of the Fallopian tubes is assisted by a collapse succeeding the excitation which coition has produced."

*Richerand*, arguing for the experiments of *Haller*, says, "In a ewe opened a few



Well then; if I am correct, I am authorized to regard the impregnating act of each man as an act of *fœtation*, and the second, fixed upon the first, as *super-fœtation*, independent of the time. Besides which, the possibility of even a longer duration of time between the acts is not necessarily excluded, for reasons which I have already advanced when I examined the anatomical and *post-coitu* condition of the uterus. Again: the "simultaneous application of semen *by one man*," to different ovaries, I cannot deny, else I would deny the existence of twins; but I do emphatic-

*minutes after coition*, you may see a vesicle larger than the others, torn, with a little wound of which the lips are still bloody." (*Physiology*, p. 477.)

*Broussais*, (*Physiology*, p. 534.) "The stimulation of the vagina produced by coitus being, as we have said, communicated to the whole uterine apparatus, the Fallopian tubes are thrown into a state of erection, by which, of themselves, their fimbriæ are applied to and embrace the ovaries. This is an indispensable preliminary for conception to take place, after which it is necessary that the semen thrown into the vaginal cavity should pass the neck of the uterus, penetrate into the Fallopian tubes, and traverse them so as to reach the ovaries. One of the last mentioned organs is adequate to bring about conception. Stimulated by the semen, it reddens and swells at the most yielding point—its investing membrane is ruptured and gives issue to a drop of gelatino-albuminous fluid, which the Fallopian tube, the fimbriæ of which are still applied to the ovary, takes up, absorbs and conducts, by a kind of peristaltic movement, into the cavity of the uterus."

At p. 537, *oper. cit.* he adds, "Is conception effected at the moment of coition? I do not believe there can be any doubt on this subject."

But *Broussais* is still more explicit in declaring his belief, when commenting upon a case which he cites from *Prof. Lallemand*, (*oper. cit.* p. 551.)

It is the case of a woman who, while in *coitu*, was surprised and alarmed by the opening of her chamber door. After a painful and abnormal pregnancy she died, and the post-mortem examination developed an *extra-uterine fœtation*. The reflections occurring to the mind of *Broussais* from the history of the case, induce him among other corollaries, to put this question. "Did the ovum fall at the instant in the cavity of the pelvis, or, only a few days later?" And he answers himself thus,—"The first case would suppose that the prolific drop (*i. e.* the fluid of the ovum) is absorbed by the tube and carried to the uterus at the very instant of conception." After arguing the converse, he adds, "We know that observers admit that the ovary fecundated by the spermatic fluid, requires a certain number of days to project its ovum, and that it is in order to wait for it, that the fimbriated extremity of the tube, kept in a state of erection by the irritated ovary, remains in apposition with it. But were this really the case, I am surprised that extra-uterine pregnancies should so rarely occur, for during the number of days supposed requisite for the detachment of the ovum, women are often exposed to moral emotions as vivid as that of which I have just spoken, and not less capable of destroying the vital erection of the tube, which maintains its fimbriated extremity in apposition with the ovary." "For if the detachment of the ovum requires so long a period, women can hardly be taken too much care of, during the days immediately succeeding conception."

In conclusion, I will add to these inquiries of *Broussais* another, suggesting itself to my mind, and it is this: Why does not the violent exercise of dancing in which newly married women indulge so immediately at the time when conception occurs, more frequently produce the same extra-uterine accidents?

ally deny that the simultaneous application of semen by *different men* to the same ovaries constitutes a case of twins, and no reasoning can sanction it. Each man is capable of imparting his own peculiar impression, and *then only* can its effects be received, *cæteris paribus*. If he fails, the next man may impart his, and thus in proportion to the single or several success will one or more impregnations be effected, separate and distinct, as such, and each child so begotten has not only its separate father but its separate impregnation. Still more decided is this postulate where the children partake of the distinctive races of their sires, as in cases of *white and mulatto*, or *mulatto and black*. The truth is so palpable that it might even be said to destroy the proverb, "He is a wise child that knows its own father," which would be probable where twins are begotten by one man at one time, but in superfœtation cases, (such as in South Carolina,) where a white man claims one and a black man the other, the child must be a born idiot to mistake its ancestry. To meet such objections fairly, therefore, it would be necessary to canvass them too extensively for time or utility, for we can readily perceive that it embraces the entire scope of the still mooted, unsettled question as to the means by which the semen reaches the ovaria in order to vitalize the sleeping germ of the future man. How are we to reconcile the various opinions seemingly based upon the conscientious convictions of the most elaborate inquiries? The "nearly simultaneous" application of the semen from the same man or from different men must unavoidably be made to depend upon its *mode of entrance*.\* Upon this point, the schools

\* This doubtless is a vexed question, and responsible authorities are arrayed against each other, but I am not satisfied with the objections urged by *Dr. Chapman* in the copious note to his edition of *Richerand*, (1821, pp. 471-474,) for, if he be correct in the premises, it may with great propriety be asked, *to what use is the semen destined?* In organs so manifestly important, and so indispensable to the perpetuation of our race, it cannot for an instant be supposed, that the nice and perfect elaboration of this fluid, and its retention until just at that critical moment, when the Almighty first pleases to consummate his most wonderful work, should have been a gratuitous dispensation, because, in all other of our excretions, periodical discharges are established and their undue retention is attended with pathological results. But the influences of castration would alone resolve this doubt. Destroy the laboratory whence the semen proceeds, and will impregnation be practicable?

Apart then from the assertions made by Haller, Morgagni, Richerand and others, (and the compulsory acknowledgment that once occurring it must be supposed again possible,) *Dr. Chapman's* objections are evidently feeble. He denies the entrance of the semen in utero, because—1st. "The vagina grasps the penis and prevents the projectile force." Now we know that this cannot obtain in all cases, owing to the numerous instances where the vagina is immensely disproportioned to the penis, and no such strangling grasp occurs, and the professor himself admits that "the male organ is endowed with a considerable projectile force."

2d. He objects, because the "spissitude of the semen" prevents its projection the requisite distance. This is readily answered, I think, by a knowledge of the distance to which onanists are capable of projecting it. Again, *Broussais* expressly says that

are divided mainly between the *Ovarists* and the *Epigenesists*. Their arguments spread far and wide, and their conclusions result in uncertainty. *Richerand* and his co-thinkers are of the opinion that although the penis does not enter the uterus, *the semen does*, and it is done, he thinks, at the moment of copulation, when the uterus "from irritation draws together and inhales by *real suction* the semen *which it craves*:" (*Phys.*, p. 47.) You are all acquainted with the multiplied experiments of *Spallanzani*, who asserts that he discovered semen in the uterus upon opening animals immediately after copulation, and *Dr. Good*, than whom there never has been a more indefatigable collector of medical history, after a learned review of all the contending theories, establishes for himself seven positions—the first of which applies directly to the point now in discussion. He says that the experiments from the time of *Harvey* to the period at which he himself wrote, and "particularly during the last half century, have sufficiently established," that "In all ordinary cases the *male semen enters the uterus at the time of coition*." (*Study of Med.*, v. ii. p. 422.)

I am, moreover, disposed to believe that the semen enters the uterus, through the os uteri, for other reasons. It is admitted upon very competent authority (and I particularly desire your attention to this fact), that *whether the ovum leaves its bed or not*, still, *the membrana decidua is formed*, (*Dewees, Syst. Mid.*, p. 64,) and it is distinctly asserted that *the impregnation of the ovaries is all that is required* for the production of this membrane. Two corollaries are deducible from this physiological admission. 1st. The same act which injects the uterus with semen, produces that condition of its tissues preparatory to the formation of the deciduous membrane, and at the same time places the ovaries in that responsive condition essential to conception. The formation of this membrane being, then, the keystone of all objections against the doctrine of superfœtation, how do those who urge them overcome the difficulty interposed to prevent the entrance of *any ovum*, whether for a single or a twin conception? Especially since physiology can affix no accurate time at which the ovum is disengaged from the ovarium, nor the length of time requisite for it, when so disengaged, to reach the portals of the uterus or gain admission within its walls? And, I again desire it to be remembered that this is not

"coition produces in women contractions of the vagina, *which is thereby shortened and approximated to its axis*." (*Physiology*, ed. 1829, p. 534.)

3d. He opposes to the entrance of the semen, the "rugæ of the vagina," which he thinks are "designed as so many barriers, to arrest the progress of the semen," and to this let me add his other objection, viz., the unfavourable position of the os tincæ for such a purpose.

These are ultra-hypothetical, because it can readily be conjectured (and this is all we are permitted to do at *this stage of the inquiry*), that the same anatomical law by which other surfaces are permitted to change their relations for the accomplishment of great functional duties, is as operative here, so that the rugæ may be effaced *pro tempore*, and the os tincæ thrown into a corresponding aptitude.

an argument hinged upon the contingency of an *extra-uterine*, *Fallopian*, or any such abnormal form of embryology, but upon the fact (if DEWEES and others be right,) that the membrane is formed "*whether the ovum leaves its bed or not.*" Hypothetical periods, it is true, have been assumed, for the time at which the ovum, after coition, commences its journey to the uterine cavity,—and upon comparing the amount of testimony, the nearest approach to that time is an average of twenty days. 2dly. I am justified in my opinion of the instantaneous act of conception, because, if the ovum does not reach its destined resting place, nor "*even leave its bed,*" previously to the range of average, why then, the *membrana decidua*, and the mucus, occluding every avenue, being immediately formed without reference to those contingencies, the after chances for peopling the world would be small indeed.

From all I have said, therefore, and with the authorities which I have brought to bear upon my opinions, I feel I can safely assume the doctrine of superfœtation at any period within a reasonable time, not directly contravened by so long an interval between the periods of copulation as to preclude the chances of the semen being received. Having given my objections against the interference of the membrane and the mucus, I will merely add that this membrane is said to have "*no perceptible blood-vessels at that part which is situated near the cervix uteri,—this portion being much more loosely connected with the uterus.*" Again,—"*there is at an early period of pregnancy an angle formed between the decidua reflexa and the decidua vera, and here it, (the latter,) is often extremely thin and perforated with small holes like a piece of lace.*" (Rigby, p. 48.)

It is, therefore, begging the question for our opponents to ask this "*nearly simultaneous application of semen, &c.,*" in the face of their own oft reiterated obstacles to such an event, namely, the occlusion, &c., immediately subsequent to conception.

In conclusion, I will say that I have done no more than sport, as others have done, over a wide field of conjecture,—and after all, when we shall have exhausted ourselves and others, what has been accomplished? Literally nothing,—"*causà latet, vis est notissima.*"

The *fourth* and last question connected with the specimen you have seen is this:—"*What would have been the chances for spontaneous amputation, had this fœtus progressed to maturity with the present condition of the umbilical cord?*"

It is an interesting inquiry, and by no means irrational, if we are to be guided by the many instances in which this intra-uterine operation has been performed with and without the abnormal distribution of the cord as we see here presented. This lesion had occasionally been noticed by authors, among whom were *Richerand*, (*Elem. de Phys.*, p. 477,) *Desormeaux*, (*Dict. de Med.*, vol. xv. p. 404,) *Billard*, (*Mal. des Enfants*, p. 623,) *Murat*, (*Dict. des Sci. Med.*, vol. xvii. p. 70.) But it received no

formal or statistical attention until 1832, when *Dr. Montgomery* published his views on the subject in the *Dub. Med. Journ. Med. Sci.*, vol. i. p. 140, and this having attracted the attention of medical men both in Europe and this country, he was induced to embody the cases which he had seen, with those subsequently coming under his knowledge, in a regular essay now appended to his valuable work on "Pregnancy," and to which I shall be chiefly indebted for many of the citations I will presently adduce.

The agency of the umbilical cord in the production of this phenomenon, is not conceded by all who have either seen, or written concerning it. *Haller* was of opinion, that the malformation always resulted from imperfect development, and not from the removal of a part already formed, and perhaps he was induced to this mode of thinking, from the fact, that there have occurred cases, for which no assignable causes existed. But the propriety of his suggestion is questionable, for the reason, that in many instances, the amputated part has been thrown off some time prior to the birth of the child, exhibiting the limb thus mutilated, and where *the stump was not entirely healed*, either on the foot or the extremity from which it had been separated. On some occasions the bone was not quite covered.

And in objection to the idea that it proceeds from death of the part, I would add, there is no evidence of decomposition which would support such a belief.

I have said that there were cases presenting the deformity, for which no *mechanical cause* could be assigned. I select a few.

1st. *Mr. Watkins* (*Lond. Med. and Phys. Journ.*, vol. 54) relates a case where the *left foot* was removed above the ankle. It was nearly, but *not quite healed*. No marks of putrefaction. The amputated portion and stump appeared in perfect preservation. *Mr. W.* offers no opinion as to its nature or cause.

2d. *M. Chaussier* (*Discours prononcé à l'Hôpital de la Maternité*, 1812) mentions two cases occurring to the fore-arm, and another where the disconnected portions of the hand and arm were lying apart. *The stump was healed*. He attributes it to gangrene, but for reasons to which I have adverted, this would seem to be merely hypothetical, as there is nothing to justify the belief.

3d. *Dr. Tyson West*, of Alford, Lincolnshire, (*Lond. Med. and Phys. Journ.*, 1832, v. i. p. 741,) communicated to *Dr. Montgomery* a case occurring at the Westminster Lying-in Hospital. He accounts for the loss of the amputated portion, by the great consternation produced at the time, but he decidedly thinks that "*the division of the limb was effected by some stricture around it.*" "The stump," he adds, "was partially healed and nicely rounded, an inch and a half below the knee."

4th. *Viel* (*Frorieps Notizen*, bd. xii. p. 26; quoted by *Montgomery*, *Oper. cit.*, p. 219) relates a case where the left foot was separated from the bone, during the fœtal state, and the fore foot was born by itself quite healed.

5th. *Béclard*, (*Bulletin de la Faculté, &c.*, 1817, vol. v. p. 213,) another case of *deep depression penetrating to the bone*, without any ostensible cause. He is of opinion that if the fœtus had remained in utero (it was an abortion)

until maturity, "it would have been born with an amputated and cicatrized leg, the remains of which might have been found in the liquor amnii."

*6th. Dr. Fitch (Amer. Journ. of Med. Sci., No. xxxv., May, 1836, p. 90)* relates a very interesting case which I shall abridge. A woman in seventh month of gestation, had on *17th March* a sudden discharge of liquor amnii. On the *21st*, a *fœtal foot* perfectly formed was thrown off, separated apparently at the ankle-joint. It was in perfect preservation.

On *5th April*, she was delivered of a seven months fœtus. The cicatrix was not complete at the time of birth, but the superior portion of the limb down to the knee corresponded in every respect to its fellow.

*Remarks.*—Dr. Fitch takes occasion in this case, to notice particularly that property in the uterus to which, you recollect, I adverted, as so remarkable in protecting against putrefaction, animal substances accidentally separated from the maternal circulation. Such, for instance, as the blighted fœtus, which forms the subject of this essay. He likewise refers to the decomposition of a small portion that remained a few days after delivery, subjected to the "warmth of the body and *other influences*," which accords with the contrast I instituted between the fœtal preservation and the decomposition ensuing upon retained portions of post partu placenta.

Another character of cases occurs, where the lesion does not depend upon the umbilical cord, but upon "*a preternatural growth of ligamentous cords and bands, and their attachment round and about the fœtal limb with such force as to produce the same results.*"

*Montgomery (Oper. cit.)* gives the case of a fœtus, aborted at fifth month. There were *complete ligaments* surrounding the limbs, consisting of distinct threads passing down from both hands to the legs,—at one end forming a complete ligature around the middle of each hand, preserving a distinct depression where it passed; the *part of the hand below it almost completely undeveloped*. It descended thence towards the legs, which it crossed and surrounded, just above the ankles, so tightly, that *fully two-thirds of their thickness was divided*. No discoloration nor the slightest appearance of disease.

N. B. Four years subsequently, Dr. M. was shown by *Dr. Labatt* another case similar to his own.

*Tragorsky* describes another case. (*Mem. Imp. Acad. Sci., St. Petersburg, 1834. 6th series, vol. iii. pp. 3-7.*)

*Dr. W. C. Roberts*, of New York, in a very interesting monograph upon "*Diseases of the Fœtus*," (*Amer. Journ. Med. Sci., Aug., 1840, p. 383.*) refers to a case such as the one just cited, reported by *Dr. Newnham*, (*Med. Repos. vol. iii.*) where a fœtus had those preternatural ligaments tightly coiled around the *left leg*, which was enormously swelled, creating a deep sulcus.

As to the production of these ligatures, various speculations have been promulgated. *Gurlt*, Prof. Roy. School at Berlin, rejects the notion of gangrene, and thinks they depend upon the history of the formation of the fœtus, being *prolongations of the egg membrane from which the fœtus grows*, whether this skin (or membrane) be taken as the navel, bladder or the annion; but "denies to them the character of organized lymph." (*Montgomery, Op. cit., p. 217.*) He also thinks that they are, afterwards, by the constant motions of the fœtus, twisted into slight, but firm cords or threads, which may involve different portions of the fœtal limbs, so as to "stricture them and cause their separation," producing thereby what has been called spontaneous amputation. From this view of the professor,

Dr. Montgomery dissents, because he believes that whenever this ligamentous cohesion exists between the amnion and the fœtus; "it produces a monstrosity of a peculiar kind, such," he says, "as was observable in the different specimens from which Professor Gurlt deduced his conclusions, where one end of the band was invariably attached to the amnion, while the other was connected with the fœtus." Whereas, in those cases coming under his own notice, "both ends of the ligature were attached to the fœtal limbs."

Neither of these solutions appears to my mind sufficiently satisfactory, nor do they shed much light upon the matter. In fact, Montgomery himself confesses his inability, after five years' deliberation, "to solve the problem of the origin of these ligatures, and still more of their application."

Dr. Simpson, of Edinburgh, (*Dub. Med. Journ.*, Nov., 1836, vol. x. p. 220,) sustains Montgomery in his views, and like him, regards the agent in these spontaneous amputations to be an "*organized lymph*," the result of an inflammatory action, occurring in the fœtal state, which not only produces the special deformity now under consideration, but also other monstrosities, diversified as we know them to be, and recorded in that most interesting and industrious work of Geoffroy de St. Hilaire, *Monstruosités Humaines*. We also know that the fœtus is subject to a variety of *diseases* while a tenant of its temporary prison, and the authorities are laboriously collated in the excellent monograph by *Dr. Roberts* to which I have already alluded.

Having seen that this *spontaneous amputation* does occur, both from causes unaccountable as well as from such influences as we have reason to believe were exercised upon the integrity of the tissues by those ligamentous bands to which I have referred, the inquiry now remains:—*Can the umbilical cord, by its devious distribution, so arrest the circulation, and thus the vitality of a fœtal limb, as to separate during gestation from the body?*

I am disposed to answer affirmatively. *First*. Because the mutilation has occurred, inexplicably as well as mechanically, and under such peculiar conditions as to preclude the idea of congenital deformity; and, *secondly*, Because, cases are recorded where the umbilical cord has been found coiled around the limb, accompanied with such a depressed and indented state of the subjacent texture as to warrant the twofold conclusion, that cases where a separation had been effected, were doubtless due to this cause; and where no separation had as yet been accomplished, to leave a very rational inference of its liability, had the stricture been much longer continued. We are acquainted with the power exercised by ligatures upon polypi and other excrescences, which leaves little room to doubt a similar effect upon the delicate, semi-osseous texture of the fœtal limb.

I shall close this essay by offering, in addition to the specimen upon your table, the history of a few cases *where the cord was found entwining the limb of the fœtus in utero*.

In 1835, *Dr. Adams* sent a specimen to Dr. Montgomery in which the umbilical cord coiled around the left leg of a fœtus in its third month.

The limb was deeply indented, and shortly after this, a case occurred in the practice of Dr. Montgomery himself, (see his work *passim*.) where the cord was entwined around the left limb just above the knee. The fœtus was at its third month.

*Morgagni* (*Alexander's Translation*, Epist. 48, Art. 53, vol. ii. p. 758), records the case of a fœtus between the fifth and sixth months, in which there was a great deformity, especially the feet, but *the left leg was broken*, and he is unequivocally of opinion that it resulted from the binding of the cord around the child's leg.

*Roberts* (*Oper. citat.*, p. 353), refers to a case recorded in *Siebold's Journal*, vol. xvi. No. 2, of the umbilical cord being coiled around the right leg of a fœtus three months old, forming a knot. The development of the leg was prevented, and *worn down to the bone, the integuments remaining unaltered*.

*Dr. Buchanan* (*Amer. Journ. Med. Sci.*, Aug., 1839, p. 522), gives the case of a fœtus four months old. The umbilical cord was singularly twisted around the *neck and thigh*, by which both motion and circulation were impeded. At the point of compression, "nothing more than the integuments intervened betwixt the cord and the bone." Below, the limb was perfectly developed.

The latest case to which I have had access, is one reported by *Dr. Griscom*, of New York, (*New York Journ. Med.*, Jan., 1845.) The lady supposed herself in the eighth month of gestation. Sudden causes produced abortion, and she expelled an ovum containing a fœtus between three and four months old. The umbilical cord longer than usual, *wound around the leg nearly a dozen times*, "as closely as a thread upon a spool."

The authorities and examples which I have cited, are sufficient, I think, to warrant the question which I have proposed to the society, as to what would have been the result in the case of the preparation before them, had not the fœtal growth been arrested by a premature death. From the array of evidence I am bound to conclude that a separation of the limb would have been produced, by that process which has so appropriately been called a "*disjunctive atrophy*." And this supposition is by no means groundless, if we only refer to *Siebold's* case, where the leg "was worn to the bone," and to the declaration of *Dr. Buchanan*, who says in reference to his case, "had the child lived long enough, spontaneous amputation would have been the result." This also was *Béclard's* opinion.

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ART. III.—*Report of some Operations performed during a late cruise in the Pacific*. By NINIAN PINKNEY, M. D., Surgeon, U. S. N., (Communicated through Dr. THOMAS HARRIS, Chief of the Bureau of Med. and Surg., U. S. N.)

CASE I. *Removal of a third of the head of the humerus*.—John R. Burden, the subject of this case, aged 17, was discharged from on board the American whale ship *Golconda*, in the month of Nov. 1841, and sent to the hospital at Lima under the charge of Dr. McLean. Ten months from the date of his entrance into that institution, he came to Callao, by



order of the American consul, to consult me. The statement of the case as detailed by the boy was, that a severe injury had been inflicted upon the right shoulder-joint. Violent inflammation ensued which resulted in supuration. Two fistulous sinuses were formed, one at the superior and anterior part of the shoulder, on a level with the coracoid process of the scapula; the other three inches lower down in a vertical direction. The discharge from these openings has continued from the date of injury to the present moment; general health impaired. The elbow-joint has undergone a degree of contraction, so as to render it impossible to carry the fore-arm beyond a semi-flexed position. Constitutional irritation and symptomatic fever threatened a fatal termination. A probe was introduced into the upper sinus, which entered the socket with facility and came in contact with the head of the humerus. To be certain that such was the fact, I rotated the arm, when the probe was made to move freely between the fingers without being displaced. In consultation with Dr. McLean I proposed the following operation, viz., to cut down upon the joint, examine the head of the bone, and to remove it, as also the glenoid cavity, if they should be found seriously involved in disease. The proposed operation was objected to, upon the ground that it would destroy the patient. All further action in the case was suspended. In the course of a month, the boy gradually becoming worse, was sent by Mr. Prevost, the American consul, to Callao, to be operated upon by me. Upon his arrival I instituted further examination and found that a great change for the worse had taken place. A large tumour had formed upon the posterior part of the arm, and his general health was materially impaired. With the assistance of Fleet Surgeon Suillon, of the French Frigate "La Reine Blanche," I commenced the operation on the morning of the 6th of January, fourteen months from the date of injury.

The boy was seated in a chair; the arm was carried backwards and inwards, and then pressed upwards so as to force the head of the humerus against the superior and anterior face of the capsular ligament. A longitudinal incision was made with a scalpel on a level with the acromion process of the scapula, and carried to below the inferior sinus. This incision was made in a line external to the openings, and distant half an inch. The deltoid muscle was then carefully dissected from its attachment to the capsule of the joint. The coraco-humeral ligament, tendon of coracobrachialis muscle and inner head of biceps were divided. I did not find it necessary to divide the long head of the biceps. The joint being thus exposed, I introduced the fore-finger, and felt a portion of bone which had perforated the capsular ligament, lying external to the joint. Upon further examination, I ascertained that it was attached to the shaft of the humerus half an inch below the capsule. I made a slight enlargement of the opening which communicated with the joint, in order to ascertain the condition of the joint, and to remove the bone with as little injury as possible. With a pair of cutting pliers I removed it; it proved to be a third of the head of the humerus, which had been separated by fracture. It was united to the shaft by ligamentous substance. The remaining portion of head of humerus and glenoid cavity being free from disease, I concluded to leave them alone. The detached portion of bone was carious, caused by being constantly bathed in matter for so great a length of time. A small amount of blood was lost by the division of the anterior circumflex artery. Lips of wound were brought in contact and retained by adhesive strips and interrupted sutures. I now proceeded to remove the tumour from the posterior part of arm, which I was enabled to do without difficulty, a simple

longitudinal incision was sufficient. The extent of disease was much greater than I had anticipated. A portion of it having reached within half an inch of the external border of brachial vessels, I brought the edges of this wound together and applied adhesive strips. A spica bandage was applied and the limb supported in a sling. The patient was put to bed and confined there during the state of febrile excitement, which continued three days. Diet during this period was restricted to rice-water. Cold water dressings were applied to keep down inflammatory action.

*Jan. 9th.* Removed sutures and dressed wound; the healing process by the first intention had taken place.

*10th.* Wounds look well; inflammation has subsided. Bowels confined; prescribed mild purgative; rigid diet to be pursued.

*26th.* Patient is enabled to carry arm across the chest and raise the hand to the head, which he could not do before the operation without creating the most excruciating pain. The wounds have all healed; the functions of the elbow-joint are completely restored, and general health much improved.

*April 1st.* He has now a fair use of the shoulder-joint, and his general health is completely restored.

Ten months from the date of operation, I met with Dr. Garnett of the U. S. N., an officer enthusiastically attached to his profession, and requested him to examine the case. I submit an extract from his letter.

*Oct. 26th, 1843.* "In accordance with your request I examined yesterday the patient upon whom you operated at Callao, and take great pleasure in adding 'my testimony to the entire success' with which the operation has been followed. The patient has now regained the almost entire use of every muscle connected with the joint, and executes with facility all the natural motions of the arm." Eighteen months from the date of the operation the patient returned, as I have been informed, in the sloop of war *Cyanne*, and such had been the improvement that he was enabled to do duty on her voyage from the Pacific.

**CASE II. *Disarticulation at the shoulder-joint.***—On the evening of the 22d of June, 1843, I was called to visit a man in the employment of the English Steam Company, at Callao. I was at Lima, but an express was dispatched to me, and at 8 P. M. I saw the patient. Medical assistance had been called, but no determination resolved upon in the case. Upon examination I found that a severe gunshot wound had been inflicted upon the right arm. The bone was shattered, belly of biceps muscle seriously lacerated, triceps muscle torn entirely through, ulnar and muscular-spinal nerves divided. The fracture of the humerus extended beyond the insertion of the pectoralis major muscle into the os humeri; brachial artery, vein and median nerve apparently uninjured. The accident occurred some distance from Callao. The man had been out upon a gunning expedition, and in passing through bushes with the hand hold of the gun near the muzzle, the trigger was caught by a branch and the load discharged. The whole charge entered the middle of the arm on the inner part of the biceps, external to the brachial vessels, but sufficiently close, as I supposed, to injure them by contusion. Convinced that no effort to save the limb would prove effectual, I determined upon immediate amputation. A large quantity of blood having been lost, I commenced the operation by cutting down upon the subclavian with a view of passing a ligature around that vessel. The patient was placed upon a table in a horizontal position, and the head turned to the opposite side. An incision three inches and upwards was made, commenc-

ing at the external margin of the sterno-mastoid, half an inch above the clavicle and parallel with it. The platysma and fascia being then exposed, a director was passed beneath them and then successively divided. The cellular membrane filling up the triangular space in which the subclavian is lodged, was then torn through by a blunt probe. The external jugular vein was easily avoided. Upon cutting through this cellular membrane, the subclavian vein, distended, rose from beneath the clavicle. In a few moments this subsided. I then proceeded carefully to tear through the fascia which binds down the tendon of the omo-hyoid muscle, and the artery was felt as it rests against the first rib. With Dr. Gibson's instrument I found no difficulty in passing the ligature; the point being directed from before backwards and then turned from below upwards, I kept it close to the vessel to avoid injuring the pleura and nerves. Having secured the ligature and dressed the wound, I proceeded to disarticulate the limb. With a scalpel I made an incision, commencing on the inner side of the arm, at the edge of the deltoid muscle, at the place of insertion of the pectoralis major muscle into the os humeri. This was carried downwards until it reached within three-fourths of an inch of its insertion, and continued in a curvilinear direction to the outer side of the arm as far as the fold of integuments in the axilla. This incision extended through the skin and integuments; the deltoid was then detached and the flap raised. The biceps tendon was divided and the capsular ligament opened; the arm was now pressed backwards, and the head of the humerus dislodged from the socket. Desirous of losing as little blood as possible, I tied the arteries as they sprung. The knife was now placed in the axilla, and by a transverse cut, the limb was by one stroke separated from the body. The arteries being secured, wound well sponged, the edges were brought into contact, and adhesive strips, light dressings and bandage applied; an anodyne was given and the patient put to-bed.

23d. Visited the patient; found him doing well; had rested during the night. Diet, chicken broth. Bowels confined. Ordered ol. ricini ʒj.

24th. Medicine had the desired effect. Pulse slightly excited. Suspended chicken broth, and recommended arrow-root.

26th. Removed dressings. Union by first intention had commenced in the greater part of wound. Re-applied dressings; diet to be continued.

27th. Dressed wound. Healing process advancing favourably. A more liberal diet allowed.

29th. Patient had rested well; in fine spirits; no arterial excitement; renewed dressings. Bowels being costive, I prescribed Seidlitz powders.

July 1st. It became necessary for me to make a visit to Lima on pressing professional business. Before leaving Callao, I called to see the patient, and found him sitting up. The ligatures had come away; the wound had a healthy appearance, and the healing process had rapidly advanced. My esteemed and intelligent friend, Dr. Suillom, offered to take charge of the case during my absence.

On the 4th of July, twelve days after the operation, the doctor was called to see the patient, and found that bleeding from the wound had taken place. Having satisfied himself that it was not arterial, he applied pledgets of cotton-wool soaked in a strong styptic solution. The bleeding not being arrested by this application, the wound was freely touched with lunar caustic, and a firm pressure maintained. Occasional doses of the solution of sulph. magn. with sulphuric acid were administered. The bleeding was temporarily checked by this treatment. An express was sent for me,

and upon my arrival I found the patient in the following condition:—Bleeding had commenced; surface pale; lips blanched; pulse much exhausted; the blood oozed from the wound at its inner border. Parts tumefied and inflamed. I resolved at once, if possible, to discover the cause; to effect which I enlarged the unhealed portion of the wound. My efforts were fruitless. The ligature upon the subclavian was firm. I applied dry sponge, firm compress and bandage; prescribed doses of carb. ferri with nitric acid. On the morning of the 5th, thirteen days from the date of operation, the patient died.

After his death, I was told that the attendant who had charge of him, having left him for a few hours, a friend of the patient's gave him some brandy, which he drank freely of. This created febrile excitement, and before morning the bleeding had commenced.

On post-mortem examination I found the subclavian was thoroughly protected against secondary hemorrhage, by the formation of a firm coagulum united to its inner tunic. The other vessels requiring ligatures were equally well protected. No cause which could account for the bleeding was developed during this examination. It is possible that a hemorrhagic diathesis of an hereditary nature may have existed, though of this I have no evidence.

CASE III. *Hydrocele of the Tunica Vaginalis and Spermatic Cord.*—

Ignacio de los Carreros, of Buenos Ayres, consulted me in reference to two large swellings which had formed; one in the scrotum, the other at the abdominal ring. He consulted Dr. Douglass, a surgeon of respectable professional ability, under whose charge he continued several months. The doctor proposed tapping the swellings, and, at the same time, the removal of the testicle, which, being hard and enlarged, was considered as being in a scirrhus condition. The patient not feeling disposed to submit to so severe an alternative, was desirous of consulting another surgeon, before decided action should be had in the case. I was called in, and upon examination, discovered two large tumours situated as above stated. The one at the abdominal ring did not dilate upon the patient being made to cough; thus showing it not to be hernia, which it strongly simulated. I placed the fore-finger of left hand above the tumour, and with the right hand I made firm pressure upon the swelling in the scrotum; an increase of the tumour at the ring immediately took place, thus showing that a direct communication existed. Two diagnostic marks were absent, viz., fluctuation and transparency. I proposed incision of the tumour at the abdominal ring, and the introduction of a tent; I opposed the removal of the testis, because I regarded it as nothing more than induration, which the excitement of the operation would tend to remove. The patient consented, and I commenced the operation by making an incision over the tumour at the ring, cutting cautiously until the sac was exposed; the superior branch of the external pudic was divided, which required a ligature. A small opening was made in the sac, and then it was freely divided upon a director. The accumulation was considerable, amounting to more than half a pint. I introduced the fore-finger into the sac, and felt the testis; the enlargement and induration were less than I supposed. A tent was introduced and kept in for two days. Ten months from the operation there had been no return of the disease.

I have had occasion to operate for hydrocele of the tunica vaginalis four times; the first case was operated upon by injection. The disease re-

turned in less than three months. The remaining three were operated upon by incision and the introduction of a tent; in every case the operation was attended with success. The mode of operating was as follows:—I grasped the tumour, taking care to exclude the testis, and with a scalpel made a free incision through the integuments of the scrotum and dartos muscle. This exposed to view the tunica vaginalis, considerably thickened, which was opened by a bistoury, cutting from within out. Before all the water escaped, I introduced the handle of the scalpel and upon it passed the tent. This was kept in two or three days; considerable inflammation, accompanied by swelling of the testis, ensued. This state of things is requisite; the testis must enlarge sufficiently, so as to bring in contact the sides of the tunica vaginalis, or adhesion of its surfaces will not take place. In the introduction of the tent, care must be taken not to fold the edges of the tunica vaginalis upon itself. Such an accident may occur, and, if so, the operation would not be attended with success.

When inflammatory symptoms of an aggravated character set in, I adopted the antiphlogistic treatment. The patient unsuccessfully operated upon by injection, was cured by this mode.

*CASE IV. Excision of a portion of the lower jaw.*—Louis Roca, of Lima, called upon me to examine his lower jaw, the right side being affected. He informed me that three years previous, a large gum-boil had formed, external to the first molar tooth, which suppurated. It was opened with a lancet; violent inflammation of the alveolar processes and gums ensued. The discharge of matter continued to increase in quantity; the patient became alarmed and sent for Dr. Reynoso. The treatment recommended having no effect, Dr. Scrivenor was consulted; he used the knife twice upon the diseased parts, but the malady grew worse. Dr. Douglas was then called in; he advised the application of aqua fortis; the patient remained under his treatment for six months. Some time previous to my being consulted, the patient had abandoned all treatment. I introduced two fingers into the mouth and made an examination of the tumour; it was an osseous growth resembling that peculiar exostosis which is often met with in the fangs of teeth. The mouth could not be closed without a lateral movement of the lower jaw. The submaxillary gland was apparently enlarged, and at first sight I was apprehensive that it was attacked with specific disease. Further examination induced me to abandon this opinion, and to regard the gland as merely displaced. The portion of the jaw over the gland was considerably curved, sufficiently so to cause this displacement.

On the 21st of June, 1843, I commenced the operation; the patient was placed in the horizontal position on the left side. The first bicuspid tooth was extracted; a horizontal incision was made extending from the angle of the jaw to the median line down to the bone half an inch below the base, in order to render the scar as little prominent as possible. The facial artery was secured by ligature, to prevent loss of blood. I detached the parts from the external surface of bone, and made a free incision through into the mouth with a sharp-pointed bistoury; the parts attached to the internal surface of bone were also freely separated. The edges of the wound being widely drawn apart, I divided the periosteum on the points at which the section was made and the saw applied. I had no chain saw, but was compelled to use the one in the amputating case. The points for dividing the bone were at the first bicuspid tooth of the same side, and half an inch from the origin of the ramus. At the first point, the bone was sawn through in

a straight direction: this was done without much difficulty. At the second point the direction was oblique, so as to remove less from the superior surface of bone than the inferior. The moment that the saw came in contact with the osseous tumour, I experienced the greatest difficulty in making a section of it. Its hardness ranges between bone and enamel. The portion of bone being removed, the edges of wound were approximated and interrupted sutures and adhesive strips made use of. A small portion of linen was introduced into the anterior angle of wound, to carry off the secretions. Over all a bandage was applied.

The sutures were removed on the third day; the wound had healed by the first intention, throughout two-thirds of its extent. Rigid diet was observed, until the subsidence of all inflammatory symptoms.

Soon after this operation, the ship to which I was attached sailed for the Sandwich Islands; I was compelled to leave the case in other hands. Dr. Simmonds, a surgeon of high promise, took charge of my patients at Lima. Through his unwearied attention, the case of resection of the jaw was attended with the most perfect success; the merest mistake in the medical treatment might have produced a different termination.

Eight months having elapsed, I returned to Callao; my stay was so short that I was unable to see the patient upon whom I operated. I was informed, however, from the best authority, that the operation had been attended with success. A fibro-cartilaginous band had formed between extremities of bone; a calcareous deposit had taken place, by which the bond of union was rendered firm; sufficiently so for the purposes of mastication.

In all of these operations I was assisted by Dr. Suillom, a distinguished surgeon in the French Navy.

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ART. IV.—*On the Reduction of Strangulated Hernia, "en masse" or "en bloc."* By GEO. C. BLACKMAN, M. D. Newburgh, Orange county, New York.

T. B., æt. 64, had been affected with inguinal hernia on the left side, upwards of thirty years. This, though large, had, with but one exception, given him little trouble, having always been easily reduced and retained by no other means than a leather strap. His general health was good. I first saw him in the month of August, 1840. On Saturday, about noon, whilst engaged in raking hay, he was suddenly seized with violent pains in the umbilical region, compelling him to desist from his labour. I was called about midnight. His pulse was quick and hard, his bowels completely obstructed, and he was suffering from nausea and violent retching. There was also great anxiety of countenance. I immediately examined the rupture, and found, to my surprise, that it could readily be returned within the walls of the abdomen. The left inguinal region was exceedingly tender, and he could not bear the slightest pressure. I took about a quart of blood from the arm, and gave him a large dose of castor oil, which produced no evacuation. The bleeding gave but temporary relief. Vomiting of stercoral matter now took place, and his symptoms in all respects became worse. In the morning two neighbouring physicians were called. Active doses of croton oil were administered, followed by tobacco enemas, without the slightest benefit. The symptoms remaining thus obstinate,

after the reduction of the hernia, left us in great perplexity as to the course to be pursued. Recollecting, however, the cases related by Bransby Cooper, in Guy's Hospital Reports, for Oct. 1839, I proposed to make, if possible, the tumour again descend from the abdomen, and to cut down upon the sac, as Mr. C. directs, treating the case as in ordinary hernia, not failing, however, to open the sac. To this our patient at first consented, but finally concluded, "that as he should sooner or later die from his rupture, he might as well go then as at any other time." Nothing could induce him to change his determination.

On Monday, his bowels became tympanitic, his pulse very feeble; mortification took place during the night and he died early on Tuesday morning, being the fourth day from his attack. No means were left untried to obtain an examination of the body, but permission was not granted.

*Remarks.*—"The attention of surgeons has of late been called to the accounts of cases similar to the above, in which the hernial tumour has been returned within the walls of the abdomen, whilst the hernial contents still remain subject to strangulation from the same cause that existed previously to their reduction. Numerous cases have been reported in detail by French surgeons, whilst the pages of the British and American journals have been almost silent with regard to this important variety of hernia. Mr. Braithwaite, commenting on a paper written by Mr. Luke on this subject, to which we shall again refer, justly observes, "that British surgeons appear to have considered the occurrence to be of such rarity, as hardly to have any influence upon the general course of practice. We, therefore, find the surgical records of our own country almost wholly barren of cases of this description, and our best and most approved writers upon the subject of hernia, are altogether silent, or refer to examples taken from the records of other countries, where the occurrence of 'reduction en masse' has excited greater attention." The possibility of this accident has been by many writers of eminence denied. Others have admitted that it may occur, but only in recent cases of rupture, where the adhesions between the sac and the adjacent parts are feeble, and unable to resist the taxis.

The facts which we propose to bring forward will prove most conclusively that both of these opinions are erroneous. We shall endeavour to present them in the order in which they have been recorded.

The first case of this kind is related by Saviard in his "*Observations de Chirurgie*," published in the beginning of the eighteenth century. Saviard was but a spectator, the operation having been performed by a surgeon of reputation for skill, who persisted that he had exposed the intestine, which, after being separated from its supposed attachments, was reduced. The patient died in forty hours, and Saviard examined the body. The stricture existed at the neck of the sac, the latter having been returned unopened, with the intestine, into the abdomen. The report of this case attracted but little notice, and it was not till a similar one occurred to Le Dran, about thirty years afterwards, that the attention of surgeons was arrested, and a violent controversy ensued, which gave Le Dran's account of it great publicity. M. Louis (*Mem. de l'Académie de Chirurgie*, tom. iv. p. 299), disputes the accuracy of his description with much warmth, and concludes his remarks by saying, "Laissons-nous dégrader l'art le plus utile à l'humanité en tolérant des principes aussi défectueux?" The discussion of this question produced quite a division among the members of the academy, and on both sides were enlisted those of high repute as

surgeons. Richter\* defends Le Dran with much energy, and Scarpa justly condemns the conduct of M. Louis, who seems to have doubted the facts presented to his notice, merely because they conflicted with opinions which he had previously promulgated with more zeal than judgment. The particulars of the case which excited this controversy are briefly as follows. The younger Arnaud had reduced a rupture, but the symptoms of strangulation continuing unabated, he sought the advice of Le Dran. This surgeon, after hearing the details of the case, and examining the patient, concluded that the stricture existed at the neck of the sac, and that the whole tumour had been reduced *en masse*. The patient soon died, and the autopsy verified the accuracy of his diagnosis.

Although Le Dran's description of this case was corroborated by the testimony of Arnaud himself, it failed to convince the skeptical, and M. Louis in particular regarded the whole affair as fabulous. De la Faye (*Operations de Dionis*, ed. 5th, p. 324, note A.), observes that a similar case had come under his observation, and Arnaud (*Traité des Hernies*, tom. i. p. 96), gives additional proof of the accuracy of Le Dran's statement. In his *Observations on Surgery*, written more than a century ago, Le Dran has given a complete description of this variety of hernia, and has prescribed its appropriate treatment. He remarks, "when the symptoms that are inseparable from the strangulation of the intestine, subsist after the reduction, the surgeon ought to inquire into the cause. It may proceed from an inflammation of the intestinal canal which remains after the parts are reduced. A volvulus may be the consequence of that inflammation, and of the anti-peristaltic motion that succeeds it. *The strangulation may subsist after the reduction*, (as in the case already mentioned,) *because the cystis hernialis was reduced at the same time.*" After noticing the resemblance in the symptoms of these cases, he endeavours to point out certain circumstances by which they may be distinguished from each other. If inflammation be the cause, he says, the pain will be almost equal throughout the whole extent of the abdomen; if the symptoms depend upon a volvulus, the pain extending over the abdomen will be much more acute in one fixed and permanent point; if they proceed from the reduction of the hernial cyst, he declares that the surgeon cannot mistake, for he will feel a vacuity under the ligamentum Fallopiam, or in the ring; and when the parts are reduced, he will be insensible of the noise generally attending the reduction of hernia, "the whole tumour passing in a heap under the ligament (if it be crural), like a tennis ball."

In describing the case to which we have before alluded, he says, "we found the cystis hernialis in the abdomen, being about three inches in depth and eight inches in circumference, and within this cystis was enclosed half an ell of the intestinum jejunum. Embracing the whole cystis with my hand, I endeavoured to draw out the intestine by pulling it at one end, but the entrance into the cystis was so contracted that it was impossible, and I could not effect it in any other way than by dilating this entrance with my scissors. How could the intestine have been forced out of the cystis, and returned by the taxis?" He then lays down the following rule for the treatment of these cases. An incision must be made where the hernia was; then dilate the ring, or divide the ligament, in order to draw the cystis hernialis back with the fingers or a pair of forceps; open the sac, dilate its entrance, and reduce the intestine. He observes that the sac

\* Programma, in quo demonstratur herniam incarceratam una cum sacco suo reponi per anulum abdominalem posse contra chirurgum Gallum clar. Louis.



cannot be far distant, since it is a part of the peritonæum that lines the inside of the pelvis.

Le Blanc, in his *Nouvelle Méthode d'opérer les Hernies*, published in 1768, has also related an instance of reduction *en masse*, which occurred in his practice.

Scarpa, in his *Traité des Hernies*, referring to the subject, observes, "there is no doubt, that in recent and small inguinal hernia, the intestine, strangulated by the neck of the hernial sac, has been known, in more instances than one, to have been reduced by the taxis, and carried with it the whole of the sac into the abdomen. While the inguinal hernia is recent and not of much size, the cellular substance (between the sac and its coverings) possesses all its elasticity, and hence, the hernial sac and the spermatic cord may easily ascend towards the abdominal ring. I have had occasion to make this observation upon the dead body of a man who had an incipient inguinal hernia. The small hernial sac was capable of being pushed back into the ring with the utmost facility; and in carefully examining the parts, both within and without the belly, it appeared to me that the cellular substance which united the sac to the spermatic cord and cremaster muscle, was disposed to yield equally from without inwards, and in the direction precisely opposite: that is to say, it made an equal resistance to the protrusion and the reduction of the hernial sac. Monteggia has seen a case exactly similar, (*Instituz. Chirurg.*, t. iii. sec. 2, p. 249), although, according to his own account, the hernial sac was not very small, it adhered very loosely to the surrounding parts, and it admitted of being entirely reduced into the abdomen with great facility. Speaking of the practicability of reducing the sac in cases of scrotal hernia of large size and long standing, he considers it quite impracticable, as the cellular substance which unites the sac to the spermatic cord and cremaster muscle, has acquired such a density, that it does not oppose less resistance to the further enlargement of the hernia than to the efforts of the surgeon who endeavours to effect its reduction.

Sabatier, in his *Médecine Opératoire*, whilst discussing the subject of internal strangulations, observes that the most common form depends on the reduction of hernial tumours, in which the stricture is at the neck of the sac. He asserts that this occurs more frequently in inguinal than any other variety of hernia. He relates a case of strangulated congenital inguinal rupture, which was reduced *en masse*, unaccompanied by any gurgling noise at the time of reduction. A bandage was applied, but the symptoms still continued. The abdomen was tense and painful, and the bowels were completely obstructed. The external ring was sufficiently large to admit two fingers. The patient having been made to cough, a round tumour was felt situated at a considerable distance from the surface. Being satisfied that there was an internal strangulation, and that an operation was indispensable, the patient was requested to rise and to move around in order to cause the tumour to reappear. This was accomplished and the operation performed. The hernial sac was thickened and the stricture existed at its neck. The symptoms of strangulation soon subsided, and the patient recovered.

Dupuytren (*Clinique Chirurg.*, tom. iii. p. 553), has met with this accident in eight or ten instances. In two of his patients the hernial sac had been reduced with its contents. The tumour was made again to descend, and the operation successfully performed as in the case of Sabatier. In two of them he was unable to produce a return of the hernia from the

cavity of the abdomen. A round, hard, and painful tumour was felt in the iliac fossa, not far from the superior orifice of the inguinal canal. Dupuytren performed the operation in the same manner, as if a part or the whole of the tumour was without the abdominal walls. In one of them the sac was drawn down with the forceps, and laid open when the strangulation was removed. In the other, the volume of the hernia and narrowness of the inguinal canal compelled him to open the latter throughout its whole extent, when the sac was seized, opened, and the difficulty overcome. In two of these cases, there existed an inguinal hernia on each side, only one of which was strangulated. The symptoms indicated that strangulation had taken place, and that the tumour had been reduced *en masse*, as there still existed all the signs of intestinal obstruction. To add to the surgeon's perplexity, no tumour was to be felt on either side, and there was no circumstance to point out the seat of the strangulation. In the first of these cases, guided by the false statements of the patient, he operated on the wrong side. The operation was repeated, however, on the other side, when the true cause of the obstruction was discovered and removed. In the second patient, Dupuytren cut directly on the seat of the strangulation, and as in the former case, the patient recovered. A case of crural hernia which had existed in a reducible state for twenty-eight years, became strangulated, and the man was brought to the Hôtel-Dieu on the fourth day of the strangulation. In the bend of the thigh was a firm irregular tumour, as large as a hen's egg, extremely painful, and strongly bound down by the crural arch. In the course of the day, the rupture went up suddenly, and with noise, but all the symptoms of strangulation continued, and the patient would not consent to an operation till the twelfth day. The anterior orifice of the crural canal having been exposed by a vertical incision, M. Dupuytren introduced his finger, and felt, at the depth of two inches behind the ring, a rounded elastic body, to which the patient referred as the seat of his suffering. He pulled it by the cellular substance on its exterior surface, the patient at the same time coughing; thus a grayish-white swelling of irregular surface was brought below the crural arch. A bloody fluid spirted out some inches, when the sac was punctured, and the enlargement of the opening disclosed an intestinal convolution of reddish-brown colour. This was now drawn down, so as to bring its neck on a level with the crural ring, and to enable the operator to divide the contracted circle which had confined the parts. Stools passed in twenty minutes after the replacement of the bowels, and the recovery was rapid and complete. The description of this case is copied by Mr. Lawrence, who justly remarks, that the patient owed his recovery, under circumstances that would ordinarily have been deemed desperate, to the sagacity and judgment of M. Dupuytren.

Sanson declares (*Dict. de Méd. et de Chirurg.*, p. 571), that he has met with this form of rupture in three cases. A man was brought into his wards who had been treated for an attack of peritonitis. The patient intimated that he had suffered symptoms of strangulation in a hernia with which he was afflicted, but which he had himself reduced. He was in a moribund state when he arrived, and survived but a few hours. Before his death, Sanson endeavoured, but in vain, to detect the presence of a tumour which he thought must exist behind the inguinal ring. In the post-mortem examination he found the fundus of the sac, which had been completely reduced between the pubis and the fundus of the bladder. He then inquires, "Suppose that I had been called to this man in time, how could I have distinguished the symptoms under which he was labouring, from

those dependent on an internal strangulation coinciding with the reduction of a hernia free from strangulation?" He relates another case, to show the difficulties sometimes attending the diagnosis of this accident. A soldier arrived in Paris in Nov. 1828; shortly after he was seized with violent colic pains, accompanied with nausea and vomiting of matter, at first bilious, then stercoral, and obstinate constipation. Leeches were applied and baths administered without deriving any benefit. During the five succeeding days, the symptoms became more aggravated, with tympanitis of the abdomen and great sensibility to pressure. Inquiries were made of the patient if he had been troubled with a rupture, to which he at first replied in the negative. He finally acknowledged, however, that for several years he had been thus affected, and that he had himself reduced it, though with difficulty, a few hours previously to the manifestation of his earliest symptoms. Although the inguinal region was examined with great care, no tumour could be felt. Indeed there was less tumefaction and pain in this than any other part of the abdomen. In passing the hand over its walls, the convolutions of the distended intestines could readily be perceived. Efforts were vainly made to produce a re-descent of the hernial tumour, and the situation of the patient became desperate. For thirteen days objections were made to the proposals for an operation, which was to consist in laying open the canal and seeking the hernia. Consent was finally given, and Sanson was about to operate, when, in again examining the abdomen, he discovered for the first time, among the projections formed by the intestinal convolutions, a kind of cylindrical column, which descended on the left side towards the iliac fossa, and was lost in the pelvis. Supposing this column to be formed by the descending colon, and that the obstruction might not be far distant from the anus; taking into consideration, also, the fact that the right inguinal region, the seat of the hernia, was the least tense and painful part of the abdomen, Sanson resolved on trying other means before he resorted to an operation attended with so much risk to the patient. The enemata previously administered had returned as soon as given, without bringing away any fecal matter. Hoping to reach the obstruction, he attempted to introduce a large and long gum elastic bougie. At first, he met with great resistance, and was convinced from the sensation made upon the fingers, that the intestine was contracted on itself at that point, so as not to permit the entrance of the bougie. Proceeding with caution, however, and rotating it between the fingers, he succeeded in introducing it the whole length. An injection of olive oil was then administered, which was thrown up with great force. This brought away a slight discharge, and he resolved to promote the tendency to the re-establishment of the passage by the most active means. As the patient was vomiting almost incessantly, no purgative could be given by the mouth. Twenty-four grains of calomel were given in the course of twenty-four hours, but this substance was rejected. For two days frictions of castor oil upon the abdomen, tobacco enemata, and the application of ice to the belly, were tried without success. Recourse was again had to the oleaginous injections, and, as before, they produced slight evacuations. They were repeated with perseverance, and blisters were applied to the inside of the thighs, on the surface of which were sprinkled a few drops of croton oil. A few hours after this application, the patient began to pass some stools, and for several days he continued to discharge an immense amount of fecal matter when all the symptoms subsided. The hernia reappeared spontaneously; it was soft, irreducible, but presented no signs of strangulation.

With regard to those cases of hernia which are reduced *en masse*, Sanson remarks, that it is evident two things must exist; first, the stricture exists at the neck of the sac; second, the inguinal canal must be greatly dilated. He further adds, that we should carefully weigh all the circumstances calculated to throw doubt on the existence of a hernia, of its strangulation, and of its reduction *en masse*, before we resort to an operation which in proper cases will be attended with the happiest results, whilst under other circumstances it cannot fail to aggravate the condition of the patient.

Richerand (*Dict. des Sciences Médicales*) says, that it is rare that hernias of long standing and of large size are free from adhesions, which almost always exist in the sac, or in the inguinal ring. This adhesion of the parts is of course an obstacle to their reduction, which in such cases can only be accomplished *en masse*. This accident, he also adds, sometimes happens in cases where adhesions exist, but the hernial tumour is of small size. Adhesions within the sac generally render the reduction of the hernia impossible, for the sac itself is generally so attached by numerous cellular and vascular bands to the adjacent parts, that without an extreme laxity of the cellular tissue, it cannot possibly be returned into the abdomen.

In the *Lond. Med. Gazette*, vol. i. p. 484, is published the account of a case of scrotal hernia which was brought to the Middlesex Hospital, and placed under the care of Sir Charles Bell. The patient, a male æt. 47, had been subject to hernia for twenty years; five years previously to his admission there had been some difficulty in reducing it. After that time he wore a truss, but continued the use of it for a short time only. On the 7th Feb. his rupture descended; he felt sick and took a black draught which he vomited. Next day he sent for a chemist, who made several attempts to reduce the hernia, but without success. Another surgeon who accompanied him to the hospital succeeded in reducing it, so that the tumour disappeared, and he could even push the point of his finger into the external abdominal ring. The patient expressed himself greatly relieved after this operation. He obtained no evacuation from his bowels, however, from the time that the hernia came down until he was conveyed to the hospital. Cathartics and enemata were administered, and he was bled to the extent of twenty ounces. When admitted, he had constant vomiting; his abdomen was swelled and tympanitic, and exquisitely tender to the touch, particularly at the lower part, on the right side, the seat of the rupture. He had a small, quick, almost a fluttering pulse, and his features were sunk and pallid. The house-surgeon ordered him a dose of castor oil with laudanum, and a clyster. This produced three motions, and he expressed himself as being a great deal better and quite easy. His pulse was fuller, and during the greater part of the day he continued to feel easier than he had hitherto been. But about six o'clock in the evening, it was found that his extremities were cold and damp; and he was restless, complaining of pain in his abdomen. He died that night. The hernia came down repeatedly during the day, and was each time reduced with great facility.

*Autopsy.* The hernia was in the scrotum, having come down shortly before death. All the small intestines were highly inflamed, distended to the utmost, and in some parts loaded with dark fluid contents. The portion of gut which was included in the hernial sac was a knuckle of the intestine ileum, very near its termination in the cæcum. There was a large duplicature of the transverse arch, with a thickened mass of omentum attached to it, which appeared from its form and the old adhesions that

united it, to be the portion which had been reduced five years before, when the rupture had descended. On examining the contents of the hernial sac, there was a fold of distended mortified gut, included within it. At the seat of the stricture, viz., the neck of the sac, the gut appeared soft, as if it were about to ulcerate, and there hung a fold of peritoneum upon the inside, which was loose, resembling an empty bag. Upon squeezing the strangulated portion of intestine, evacuating some of the air which distended it, it was found that the intestine could very easily and effectually be pushed through the external abdominal ring, so as to be hid from the sight. On looking to the inside, however, it was seen that the portion of gut had carried the neck of the sac before it into the abdominal cavity; and the duplicature of the peritoneum which has been described, being unfolded, had formed a new sac for including the knuckle of intestine on the inside of the abdominal muscles. Thus the fold of intestine was pushed through the external abdominal ring, through the spermatic canal, and through that part which is described to be the internal ring, and was reduced within the abdominal muscles, but not within the abdominal cavity. The neck of the sac had been torn off from the internal ring in the efforts of reduction, but continued to grasp the included portion of gut. In the scrotum was also observed another sac which appeared to be that of a congenital hernia. Between this and the upper sac, existed an orifice large enough to admit the passage of the finger.

In a clinical lecture delivered at the Middlesex Hospital, and reported in the *Lond. Med. Gazette*, vol. xiii. p. 923, Sir Charles Bell thus remarks: "But the next thing that occurs in the present case, and which I know you are discussing among yourselves, is the recommendation of removing the stricture without opening the sac. See, then, what took place here: I found upon cutting the crescentic edge or the little wing-like process which goes down to the sheath of the femoral vessels, and which is continued from the lower or inferior edge of Poupart's ligament, that the stricture was not taken off. You saw that I had considerable difficulty; \* \* I found that the stricture was not removed, that it was in the neck of the sac, and not in the tendon. Those of you who were near me must have seen with interest, that the operation appeared to be finished—that the tumour disappeared—that the intestine was removed from the eye; but the case was not satisfactory to me, so that, after it, must have been supposed by you that the operation was completed, you saw me draw the whole portion of intestine down again; that then the assistant took firm hold of the sac, pulling it down so as to be on the stretch; that I passed the director and bistoury again between the gut and the sac, and cut the stricture deep a full inch within the crescentic arch. That which I cut was the edge of the stricture of the proper sac." After noticing the liability of the pressure of a truss to produce this accident he proceeds to relate another.

"A patient was brought into the hospital moribund, and died. He had been operated on by the taxis, and the surgeon was convinced that he had done everything required of him. A tumour was discovered quite within the muscular walls of the abdomen, which proved to be the strangulated intestine within the peritoneal sac; so that the surgeon had reduced the sac and the intestine within it, and the stricture which produced the strangulation being in the mouth of the sac, there was no relief, and the patient died. Here then is the first thing that you will reflect upon when this question is agitated, regarding the propriety of opening the sac. *Always remember that, in certain circumstances, the stricture is in the neck of the sac itself.*"

The cases reported by Bransby Cooper, to which we have already referred, show most strikingly what difficulties may be encountered in the treatment of strangulated hernia, and as it is our object to collect the more important symptoms by which the variety under consideration may be diagnosed, we venture to offer a brief analysis of his report. April 26th, 1839. Mr. Toulman visited Mr. I., æt. 68, who was suffering much pain in his bowels and at the pit of his stomach, attended with much vomiting. Bowels had been twice moved during the day. Mr. T. saw him at 10, P. M. Being aware that his patient was the subject of rupture, Mr. T. inquired whether it was up, and was told that "all was right." The symptoms were, therefore, supposed to arise from indigestion. Rhubarb and magnesia were ordered. Patient passed a restless night; pain referred principally to the pit of the stomach, although it extended in a less degree over the whole surface of the abdomen. Medicine had been rejected and no motion obtained. Skin cool; pulse 60, and soft; no increase of pain of the abdomen on pressure. Epsom salts with the tinct. of senna prescribed. At mid-day, Mr. T. was told that a well-formed motion had been passed. Vomiting and pain still continued, though slightly mitigated. Medicine retained. Dose repeated every two hours. In the evening, symptoms became more urgent. On examination, the right inguinal region, for many years the seat of hernia, and to which a truss had been applied, appeared unnaturally flat, especially when compared with the opposite side. This was supposed to arise from the absorption of fat, on account of the continued application of the truss. The fullness on the left side, however, did not depend wholly on the accumulation of fat, as a small hernia protruded through the external ring, which returned under the act of its examination. Still there was greater fullness on the left than on the right side. A pill of calomel and rhubarb was directed, to be followed by a purgative draught in a state of effervescence, and a sinapism to be applied to the region of the stomach.

28th. Patient much the same; vomiting and pain continuing; pulse calm and soft; countenance free from anxiety, and the tongue slightly furred; no further evacuations from the bowels. Small doses of calomel and opium substituted for the purgatives, and a turpentine enema administered. Mr. T. with his son again examined for outward signs of intestinal obstruction, but nothing was discovered. The cal. and op. pills to be continued. Effervescent draughts and turpentine enema to be repeated. In the afternoon, sickness had somewhat abated. Pain less urgent, but constipation still obstinate.

29th. Had passed a restless night; vomiting now stercoral, and hic-cough. Pain of the abdomen somewhat abated; pulse intermitting, although this was not unusual with him in good health. Owing to the greater fullness on the left side, it was suspected that this was the seat of the obstruction, though the hernia on that was as readily reduced by the taxis, as the one on the other side. The propriety of an operation being now discussed, Mr. Cooper was requested to examine the patient. This was in the afternoon of the 29th. To him the fullness on the left side had not the appearance of a circumscribed tumour like hernia; but of a natural disposition of fat, the absorption from the pressure of the truss on the right side having given rise to the inequality of size. Upon manipulation, however, a somewhat unnatural sensation was communicated as well as that of fullness, in the left inguinal canal. No protrusion could be felt on the right side, but upon desiring the patient to cough, the hernia immediately

descended, but could be reduced again by the most gentle pressure. The forefinger was passed into the inguinal canal by pushing the loose skin through the external ring, and the patient was desired to cough, when complete evidence was afforded of the absence of any descent through the internal ring. The left side was therefore suspected of being the probable seat of mischief. The inguinal canal was opened and a small empty sac alone was found; neither was any light thrown upon the nature of the disease. The symptoms grew worse, and the patient died at 10 o'clock, A. M., on the 30th of April.

The autopsy proved the correctness of the opinion formed during the exploration of the previous day, that on the left side there was no existing cause for the symptoms which had proved destructive to life. The anterior parietes of the abdomen were then turned down, and the position of the intestines carefully investigated. On examining the right inguinal region, the seat of the old reducible hernia, a portion of intestine was discovered in the sac, which proved as easily reducible as it had been during life; but on drawing the intestine out of the situation of the internal ring, it resisted displacement as if some adhesions had retained it there. Looking for the cause of this retention, it was found that a portion of intestine had become strangulated in a small hernial sac, which was situated anteriorly to the larger one, conjoining the reducible hernia. This portion of intestine and its sac which strangulated it, were situated within the cavity of the abdomen, and could not have been relieved even if the exploration had been performed on the right side, unless the dissection had been made on the hernial tumour while protruding into the scrotum.

Mr. Cooper's other patient was a healthy-looking man, æt. 67, who had been the subject of inguinal hernia on the right side for thirty years. No difficulty had been experienced in its reduction, till Nov. 18th, 1834. While at stool in the morning, his rupture descended, and he immediately became sick. Being unable to reduce the hernia, he applied for admission to the hospital. He was then suffering from constant sickness; pulse soft and compressible; tongue dry and furred; skin cool, but clammy; countenance rather anxious. The hernia descended into the scrotum; but was not tender even on pressure. He walked to his bed with apparent ease. The reduction having been attempted without success, he was ordered into a warm bath and subjected to its influence for half an hour, when he became faint, and a further application of the taxis proved apparently effectual, for the hernia was returned into the cavity of the abdomen. He expressed himself relieved, and at his request the truss was again applied. An enema was given, but returned immediately, without producing any evacuation. The urgent symptoms soon returned, his pulse became quicker; he passed a restless night, with occasional vomiting, and the bowels continued obstinately constipated.

*Nov. 19th.* Obstruction still the same: vomiting of fecal matter. Enema of castor oil directed to be given and repeated every second hour, if it produced no effect. At one o'clock, P. M., Mr. C. again submitted him to a close examination, but neither in the seat of the old hernia, nor in any other situation of such protrusions, could he discover anything which would lead him to explore any outlet from the abdomen. The enemata were repeated; sinapisms were also applied to the scrobiculus cordis; and calomel and opium were given, but the patient continued to grow worse, and died at half-past six o'clock.

*Post-mortem.*—Peritoneum generally inflamed. In the lower third of

the small intestines, a knuckle of intestine, to the extent of about two inches, was found strangulated in a distinct sac, lying between the right linea ilio pectinea and the bladder, being placed in the natural pouch of peritoneum, leading to the internal abdominal ring, and which usually forms the sac of an inguinal hernia. The sac, as seen within the abdomen, seemed about as large in size as a large walnut; its aperture which formed the stricture was of considerable size, although the stricture around the intestine was pretty firm. The sac itself was not much discoloured, but the intestine within it was of a dark greenish hue. A firm and reddish margin of omentum adhered to the front of the sac very firmly, and some bridles of adhesive matter confined the cæcum in the iliac fossa. About three inches of the ileum below the stricture, showed evidence of former constriction; the middle part of this portion being slightly dilated, thickened, opaque, and injected; whilst the two extremities were marked by an irregular patch or flake, as if cicatrization had been the result of some former physical compression.

According to Mr. Wade, Mr. C. has met with two more cases of this form of rupture, but the reports of them I have been unable to find. From the cases above described, however, it would seem that Mr. C. was entirely ignorant of the light which had been thrown on this subject by Le Dran, Sabatier, and Dupuytren, for when he suggests the propriety of exposing the inguinal canal and laying open the sac, he does not even refer to the brilliant success which had been attained by those distinguished surgeons.

Mr. Jas. Luke, one of the surgeons to the Lond. and St. Luke's Hospital, on the 25th of April, 1813, read an interesting paper before the Med. and Chirurg. Society, in which he endeavours to show that the reduction of strangulated hernia *en masse*, although rare, is not so infrequent as is generally supposed, and that the occurrence should be considered as coming within the ordinary range of probabilities of surgical practice. Mr. L. asserts that he has met with no less than five cases, and describes in detail the particulars of the operation, which was successfully performed in one of these instances. On laying open the inguinal canal, it was observed that the spermatic cord was clear and unobscured by any superjacent structure, except by a small lobule of fat which overlaid its upper part. While pursuing the examination towards the internal ring, some cellular membrane lying on the inner aspect of the cord appeared more condensed than usual in that situation; which, upon close investigation, proved to be a condensed capsule containing an empty cavity within it, sufficiently large to contain a small egg. It was thought probable that this capsule had formed an investment to the hernial sac, and was now left empty by reason of the reduction that at this time was strongly suspected to have been effected. A finger passed along this cavity entered the internal ring, the large, firm, and defined borders of which were distinctly perceptible. By pressing the finger a little more deeply within the abdomen, the rounded and tense surface of a tumour was readily detected, which after a little exposure was seen to be dark-coloured. It was clear that the hernial sac was now reached, and the fact of a reduction *en masse* was made manifest. It was ascertained by an examination of its surface, that the seat of stricture was at a considerable depth from the internal ring, and probably upwards of two inches from the abdominal parietes.

The subsequent steps of the operation consisted in freely dividing the margin of the internal ring, by which proceeding the hernial tumour was



permitted to be easily drawn out into the inguinal canal and opened, without the danger which would have attended the attempt, had it been made while it lay deeply within the parietes. The sac contained a small quantity of very dark-coloured fluid, and about six inches of small intestine of a dusky red colour, with one small black spot on its surface, the whole being strictured at the neck of the sac. The stricture was clearly seen and easily divided, after which the contents were readily reduced within the general cavity of the peritoneum, and the wound closed by two sutures and adhesive plaster. The extensive division of the abdominal parietes rendered extra precautions necessary against reprotrusions of the intestine, and graduated compresses and bandages were applied over the part. Immediately after the operation, the symptoms of strangulation began to subside, and the patient perfectly recovered.

Mr. Robert Wade has reported a case in the *Lond. Lancet*, for July, 1845, which was attended with considerable difficulty in the diagnosis, but in which an operation was performed with success.

*April 4th, 1845.* He visited Mr. B., an active, healthy man, æt. 75, who had been afflicted with inguinal hernia on both sides for nearly thirty years. The tumours were sometimes as large as a good sized pear, occasionally descending into the scrotum, but were always easily reduced, and generally kept up by a double truss. I was informed, says Mr. W., that two days previous to my visit, Mr. B., on getting out of the bed in the morning, experienced a slight, but momentary, darting pain in the right inguinal region; on going to the water closet soon afterwards, he found that his bowels would not act, an unusual circumstance with him. Towards the afternoon he began to vomit; the fluid ejected soon became stercoraceous, and at my visit had a highly feculent smell and appearance; the breath was also highly fetid. Upon a careful examination, not the slightest appearance of hernia could be detected on either side, nor was there any tenderness on very firm pressure over the inguinal canal, or any part of the abdomen; the pulse was about 90, regular and tolerably full, except during the sickness, which caused great faintness. Cathartic enemata had been administered, croton oil, calomel, and other powerful purgatives had been taken without producing the desired effect. It was evident that there was obstruction of some part of the intestinal canal, and the herniæ were strongly suspected of being its seat. The only reason for this suspicion, however, was the slight darting pain in the right inguinal region, felt on first getting out of bed. The spermatic cord was more distinctly felt on the right than on the left side, and from all the circumstances of the case, Mr. W. was led to the conclusion, that the right hernia, with its investing sac, had been reduced *en masse* by the patient, and that the obstruction existed in the neck of the sac on that side. The symptoms continued to grow worse, although there was not the slightest tenderness on firm pressure in any part of the abdomen, nor could any tumour be felt in the neighbourhood of the rings. The patient was assisted out of bed and made to cough, but although the rupture on the left side was forced down, there was no descent on the right; the attempt could not be persevered in, for he soon became so faint as to fall backwards on the bed, with a pulse scarcely to be felt. Although the symptoms indicated obstruction of the intestinal canal, rather than any serious mischief of the intestine itself, yet taking into consideration the exceedingly depressed state of the patient's nervous system, the extremely harassing sickness from which he had now for some length of time suffered, as well as his rather advanced age, it ap-

peared highly desirable, says Mr. W., that some more efficient means should soon be adopted for his relief. I therefore proposed to cut down upon the right inguinal canal, to lay it open, and endeavour to bring the hernial sac into view, so that it might be opened, and supposing that no stricture should then be found, I would with confidence attempt the operation on the other side. The patient was again assisted out of bed, and made to cough, when the hernia on the left side quickly descended as before, and after a little perseverance in the coughing, assisted by pressure on the abdomen, the hernial tumour on the right side protruded about half an inch through the external ring, but could not be made to advance further. On pressing the protruded parts between the finger and thumb, the patient said he felt a slight degree of tenderness, but so slight as scarcely to be perceptible, and the firmest pressure around the ring on either side did not cause any uneasiness. The abdomen became a little tumid and tympanitic. The patient's legs hanging over the bed, and his head and shoulders supported by a pillow, the operation was commenced by transfixing a fold of the integuments over the right inguinal canal, to the extent of three and a half inches. Some layers of fascia were then divided and the canal laid freely open by a division of the tendon of the external oblique muscles. On opening the sac, which was much thickened and which closely embraced the intestine, about half a teaspoonful of a light brown serous fluid escaped, and a small knuckle of intestine, rather red from congestion, was observed. The finger was then passed within the sac, along the intestine, which at the extent of about three inches, nearly as far as the finger could reach, was found firmly embraced by a membranous band. This was divided by a probe pointed bistoury passed carefully along the finger, when the strangulated portion of intestine was easily reduced. There was no omentum in the sac. The stricture was evidently formed by the neck of the sac, which could be felt completely surrounding the intestine, being very much thickened and puckered at its under part. The wound was dressed, and the patient expressed himself as having obtained immediate relief, especially from his distressing sickness. Bowels acted freely the next day, and he continued to improve, and eventually although slowly recovered. The most unfavourable symptom noticed after the operation was a considerable degree of drowsiness, and the patient required to be roused to answer questions, when he would again relapse into his drowsy state.

From all the facts recorded, it would seem that this accident may occur under three different circumstances: 1st, spontaneous reduction; 2d, as the result of the taxis; 3d, from an incomplete operation. An example of the first of these varieties is to be found in the *Leçons Orales* of Dupuytren. He was called to treat a case of crural hernia, which resisted all attempts at reduction by the taxis, but finally returned spontaneously *en masse*. As the symptoms of strangulation remained, an operation was deemed advisable and successfully performed. Most of the cases have been the result of the application of the taxis without an operation. In addition to those already mentioned we may add an instance which occurred to M. Blandin at the Hôtel Dieu, in which the rupture (inguinal) was of large size. J. B. Demeaux, in his "*Recherches sur l'Évolution du sac Herniaire*," relates a case which was communicated to him by M. A. Berard, which with those before described, triumphantly refutes the assertion of Scarpa, that the reduction *en masse*, can only occur in recent cases of hernia, and in those of small size. The following are the particulars of this case:

A clergyman had been afflicted with crural hernia on the right side for several years, which could readily be reduced, or be made to reappear. The patient had never worn a truss. Whilst engaged in speaking, the rupture descended; the patient himself caused it suddenly to return, when he was immediately seized with violent pains accompanied with vomiting and all the symptoms of internal strangulation. The hernia was made to descend again from the abdomen, but the symptoms were not mitigated. About forty-eight hours after the first reduction, efforts were again made by the taxis to reduce the tumour, but instead of subsiding, the symptoms became aggravated and more alarming. The operation was performed on the third day, and the crural ring was perfectly empty, although it was somewhat enlarged. On the introduction of the finger, a tumour was detected within the abdomen, which the surgeon attempted to draw down with the forceps; in which he but partially succeeded. Discovering in this tumour a hernial sac very much thickened, the operator incised it, when some bloody serum which it contained escaped; the finger was then introduced, which soon reached the intestine itself, and caused the discharge of a large quantity of fecal matter. The operation was not followed with any benefit and the patient survived it only thirty-six hours. The autopsy revealed 1st, the traces of a subacute peritoneal inflammation; 2d, a hernial sac situated chiefly within the abdomen; 3d, the neck of the sac adhering to a fold of the intestine which was perforated on the side of the cavity of the sac.

In the *Bulletin Chirurgicale*, tom. i. p. 363, a singular variety is described by M. Laugier, which consists not only in the reduction of the sac and its contents, but an *arrachement* of the neck, which, detached by the attempts at the taxis, has been returned within the abdomen, maintaining, however, its proper relations with the protruding portion of intestine. The symptoms in his case continued unabated and the patient rapidly succumbed. On opening the abdomen, a fold of intestine about a foot in length was discovered in the hypogastric region, surrounded by a flat circular band, from three to five millimeters in length, and which at no point adhered to the intestine which it strangulated. But that which excited most surprise was the discovery of something resembling the finger of a glove truncated at its summit, which was situated near the orifice of the inguinal canal. Its margin was slightly fringed, and it was evident that the separation at its neck was owing to a circular rupture. It adhered by its base to the upper orifice of the inguinal canal, and it was easy to perceive that it arose from the peritoneum lining the abdominal walls in the vicinity of the ring, which had been drawn inwards by the intestine.

Sometimes, in operating for strangulated hernia, the surgeon mistakes the hernial sac for the intestine itself. Errors of this kind, says Demeaux, have been committed by surgeons of great renown, and if it occasionally happens that the intestine is opened when it was supposed that the sac only was being divided, in other cases, this itself has been detached from the adjacent parts and returned into the abdomen with the intestine which it contained. Such was the case related by Saviard, and as Demeaux remarks, it is probable that many more would have been recorded, had surgeons not been anxious to conceal their errors. This last writer reports two cases of this nature, one of which was communicated to him by M. Denonvilliers; the second occurred to M. Velpeau, and an account of it was published in the *Gazette des Hôpitaux* for March, 1842.

The case on which M. Denonvilliers operated was one of crural hernia. In the first stage of the operation nothing unusual presented itself to his

notice. Arriving at a greater depth, he discovered a tumour which he supposed to be formed by a portion of the intestine; this was isolated from the surrounding parts, its debridement accomplished when it was reduced without difficulty. When the wound was dressed, the surgeon began to entertain some doubts as to the result of the operation. He conjectured that the hernial sac might not have been opened. He waited, however, for a couple of days, when the symptoms continuing unabated, he determined to finish the operation which he had commenced. With the aid of the forceps, he seized the sac and drew it to the surface, when the stricture which existed at its neck was divided. The case of M. Velpeau was likewise one of crural hernia. When he arrived at the sac, he supposed he had reached the intestine, which he thought was deprived of its peritoneal covering. The tumour was detached and reduced. The patient soon died, and on the post-mortem examination, it was found that the rupture had been reduced *en masse*. J. B. Demeaux, in his *Recherches sur l'Evolution du Sac Herniaire*, p. 37, describes another displacement of the hernial sac, viz., its inversion. The facts of one case only have come to his knowledge, for which he is indebted to M. Charles Fournier, *ancien interne* of the Hôtel Dieu. In this, the surgeon supposed he had laid bare the intestine; he isolated it from the surrounding parts, divided the ring and reduced the tumour. The sac was not opened, but by pressure it became inverted. As mortification had already commenced, death rapidly supervened in consequence of the effusion of fecal matter into the cavity of the abdomen.

In the *Provincial Medical Journal* for Feb. 1843; is reported a case which occurred in the practice of Mr. Banner at the Northern Hospital, Liverpool. The patient, a male, æt. 42, had been subject to inguinal hernia for many years, but had always been able to return the gut. The gut had descended two days previous to his admission, when he was seized with great pain in the bowels and constant vomiting; he was unable to return the tumour as usual (which had always descended as low as the scrotum) and sent for a surgeon who bled him, and used the taxis, and at last succeeded in returning the contents of the tumour, which had been the size of an egg. The symptoms of strangulation continued; he was again carefully examined; but the ring could be clearly felt, and there was not the least indication of a tumour. The case was treated actively for inflammation, but the patient died on the second day after admission. On examining the body, a small knuckle of intestine was found just within the inner ring, strangulated by the sac which had been returned with the intestine; this portion of gut was mortified and the peritonæum was in a high state of inflammation.

After this array of facts, the possibility of the reduction *en masse*, can no longer be denied, neither can it be maintained with any degree of propriety that it can occur only in recent cases, and where the hernia is small. Although Scarpa strongly advocated this opinion, the very example which he quotes from Monteggia, goes to disprove his assertions on this subject, as "the hernial sac was not very small." His observations apply only to the empty sac, and on this point we believe there is great unanimity of sentiment among surgical writers. But it should be remembered that we are considering those cases of rupture, where intestine or omentum is contained within the sac, and where adhesions exist between these parts and the neck or fundus of the sac. It seems to have been the opinion entertained by almost all writers on hernia, that shortly after its protrusion, the whole sac contracts such intimate connections with the surrounding parts as to render its reduction impracticable. Arnaud was the first to observe

that these adhesions were not so extensive as was generally supposed. This point has been most fully investigated by a recent writer, to whom we have already more than once alluded, viz., J. B. Demeaux. He remarks, p. 22, that the anatomical researches in which he has been engaged, have enabled him to determine with precision, the adhesions of the sac and its neck with the adjacent parts. Referring to the organization of the neck, and the changes which take place not only on its serous but cellular surface, he says, "the first thing which arrests the attention of the observer, is the diminution and almost entire disappearance of the adipose substance in the cellular tissue, even in subjects truly corpulent; perhaps it would be more correct to call it the transformation of the cellular-adipose tissue into a new substance which contains a large number of blood-vessels. Most frequently we perceive through the transparent peritoneum, around the circumference of the hernial opening, this rich vascular net-work, converging from every point towards the neck, and afterwards spreading itself over the upper part of the sac, so as to be insensibly lost in the cellular substance with which it is continuous. Independently of these vessels, we may also detect a layer of filaments, the nature of which it is not easy to determine; they cross each other in different directions, and I have compared them to the tissue of the dartos muscle. Now this vascular transformation of the sub-peritoneal tissue on a level with the neck, does not extend over the whole surface of the hernial sac. Thus, whilst at the neck of the sac, the peritoneum and the subjacent tissue are most intimately united, the two layers remain distinct at all other points. Besides the cellular tissue, we meet with a layer which I consider analogous to the fascia superficialis; it is the fascia propria of the peritoneum. The vascular tissue on a level with the neck of the sac, is situated between this fascia and the peritoneum. On its external surface this fascia preserves its cellular aspect, and is united to the more superficial parts only, by a loose tissue from which it may be easily detached. Thus in every instance of the reduction *en masse*, the sac draws with it this subperitoneal tissue and the fascia propria. This membrane, thin in its normal state, becomes thickened when a hernia exists, and establishes a kind of demarkation between the sac and its external covering."

Having thus clearly established the existence of this accident, and having endeavoured to throw some light on its mechanism, we proceed in the next place to notice some of the circumstances by which it may be diagnosed. A careful perusal of the cases recorded must convince the reader that this is in some instances attended with considerable difficulty. In the case which occurred to myself it may be said, that the existence of strangulation at the neck of the sac is by no means substantiated. True we have not positive proof on this point; but when the symptoms are compared with those under which the patients of Mr. Cooper and others laboured, it becomes exceedingly probable. We are aware, that in cases where inflammation has been excited previously to the reduction of a rupture, the effect will not cease on the removal of its mechanical cause. Mr. Lawrence refers, in a note, to an example related by Camper in his *Icones. Hern.*, p. 3, in which death occurred from peritoneal inflammation where an inguinal hernia had been returned without delay. But in my patient there was not the slightest evidence of any derangement of the bowels whatever, till he was suddenly attacked in the field with violent pains in the region of his rupture. Again there was a peculiar sensation experienced at the time of the reduction of his rupture, which I have at no other time noticed, and

which has been tolerably well described by Le Dran. Mr. Robert Wade has collected the principal signs, which denote the reduction *en masse* viz., the absence of that fullness of the ring and cord caused by the presence of the hernial sac; an unusual largeness of the aperture, through which the hernia has descended, ascertainable by the introduction of the finger; a fixed circumscribed pain in the neighbourhood of the ring; and in some cases a tumour has been felt which has been tender on pressure, in the same situation, deeply seated within the abdominal muscles.

In the treatment of these cases, the propriety of the proceedings resorted to by Dupuytren and others, can no longer be doubted. It becomes our duty to produce if possible the redescend of the tumour, to expose and divide the sac, with the stricture which exists at its neck. But what are the relations of the hernial sac thus reduced and the openings through which it had before descended. It may be found, says Demeaux, in different situations. In some cases, the peritoneum lining the abdominal walls becomes thrust backwards so as to permit the sac to lie in the subjacent tissue, between it and the aponeurotic layers. In this case, the reduction is complete and the tumour cannot be detected from the surface. These cases are liable to deceive the surgeon, who believing that he has done all that lies in his power, reposes in false security. In these examples the sac is so situated that it is with great difficulty that it can be made to present to the external ring; indeed it is impossible for the patient, by any efforts of his own, to cause the hernia to redescend. At other times the reduction *en masse* may be incomplete, when the rupture may be made to descend with very little effort. Sometimes, says Demeaux, the tumour cannot be reproduced, because the ring is not sufficiently dilated to afford it passage. In such cases a free division of the inguinal or crural canal will be the only alternative, when the sac may be seized with the forceps, drawn down, and the stricture readily divided. The opinions of Demeaux are corroborated by the investigations of M. J. Cloquet, who has probably devoted more attention to this subject than any other writer. He observes, (*Recherches sur les Causes et l'Anatomie des Hernies Abdominales*), "I have accomplished reduction in a mass in more than twenty instances, partly of hernia, either strangulated or otherwise irreducible, partly of empty hernial sacs. It is effected most easily in internal inguinal than in crural, and lastly in external inguinal hernia. I have never succeeded in the umbilical ruptures of adults." He considers this kind of reduction almost impossible in cases of external inguinal hernia, and if accomplished at all, he thinks it must be incomplete, and that the swelling must reappear as soon as the efforts at reduction are discontinued. If the reader has carefully examined the details of the cases which we have collected, he cannot fail to perceive that this accident may happen in cases of external inguinal hernia of long standing and where the sac is of large size. He must also have discovered that the reduction in such cases is by no means necessarily incomplete, but that in several instances the sac was pushed completely through the ring.

ART. V.—*Remarks on Trismus, or Tetanus Nascentium, and on its identity with Traumatic Tetanus in the Adult.* By Wm. O. BALDWIN, M. D., of Montgomery, Ala.

WHILST the ignorant and superstitious mother, in beholding her babe the victim of tetanus nascentium, attributes its production to some incomprehensible necromantic agency; the nature of its cause has, to the educated and enlightened physician, been a subject of almost equal mystery; certainly at least, his attempts to explain it have often been quite erroneous.

Conflicting and discordant indeed have been the numerous propositions entertained and discussed from time to time, relative to the origin and essential nature of this disease. To enumerate, without comment, some of the causes to which its production has been ascribed, I will begin with "influence of stars," "defective clothing of the mother during pregnancy," "vitiating atmosphere and defective ventilation," "irritation of the intestinal canal induced by retained meconium," "excessive purgation," "improper swathing," "influence of cold," "smoke of burning wood," "pouring cold water on the head in the performance of the office of baptism," "applying scorched linen or cotton to the umbilicus," "imprudence in diet in the mother," "defective nutrition to the child," and more recently to imperfect ossification of the cranial bones and "*position*" of the child after birth.

The rehearsal of the various theories or superstructures which have been based upon even the more plausible of these *first propositions*, would consume time unprofitably, whilst most of them are so fanciful and vulnerable in themselves, as scarcely to require a moment's consideration in order to determine their fallacy.

Having lately witnessed a case or two of the disease, I have been led to a more serious contemplation of its phenomena, and, claiming nothing as original, shall endeavour to sustain the proposition of its identity with tetanus in the adult, believing it to be *generally*, if not always *traumatic*, and locating its exciting cause in the umbilicus. In my remarks, which will be mostly analytical, I shall of course be compelled to draw largely from the writings of others. Many of the authors not being accessible in their original form, I shall be compelled to quote them second-handedly, or through others, and supposing them to have been correctly quoted in the first place, I may at times fail to credit the author from whom I obtained them, and simply give reference to the original papers. When this is done, however, it will only be for the purpose of saving time and trouble.

*Trismus nascentium*, although more especially a disease of tropical climates, is not only met with in every climate, but in every season. Co-existent with tetanus in the adult, no country under the sun is exempt from its ravages. In the April number of the *Amer. Journ. Med. Sci.* for 1846, in an article by J. M. Sims, M. D., on this subject, he remarks, "unlike traumatic tetanus, this disease is encountered in every climate and under extremes of temperature," plainly implying the assertion that traumatic tetanus is *confined* to particular climates, which is not sustained by the authorities on this subject. All the authors I have consulted on the subject agree in saying it is more especially a disease of warm climates, but equally unanimous are they in saying it is a disease met with in every place and season, amid tropical suns as well as polar snows. Curling, in his *Treatise on Tetanus*, p. 25, remarks, "tetanus is a disease met with in every part of the globe,"

&c. In Tweedie's *Lib. Prac. Med.*, p. 71, vol. ii., we find a similar remark, and if it were necessary I could multiply corroborative authorities. I am thus particular in making mention and reference to this fact, as I conceive it to have an important bearing upon the points at issue.

Tetanus, at whatever age it attacks, is proverbial for its fatal tendency. In Curling on *Tetanus Nascentium*, p. 106, we read, "Dr. Hancock remarks it is so frequent and fatal in the colonies of Essequibo and Demerara, that at an average estimate it kills one-half of the whole number of infants born there, and the fatality he reckons at least, at ninety-nine in a hundred. Mons. Fourcroy calculates that at St. Dominique, eighty out of every hundred of the negro children die of it before the ninth day. Mr. Morrison, who practised, for several years at Demerara, has never known one authenticated case of recovery." These statements, and other corroborative statistical records show the disease to be more universally fatal in the infant than adult. The discrepancy in this respect, however, is easily accounted for, by the difference in their capacity for resisting morbid impressions. Notwithstanding this difference is admitted, which we could hardly suppose to be otherwise, yet in some instances its mortality in the adult will bear an equal ratio. "Sir James McGregor alludes to several hundred cases (of tetanus in the adult), observed in the campaigns of Spain and Portugal, very few of which were cured. Mr. O'Brien mentions that out of about *two hundred cases* which he witnessed in the Peninsular war, *not one recovered*. Curling on *Tetanus*, p. 21.

The period of attack in traumatic tetanus in the adult, and tetanus nascentium, shows a great and striking similarity of action in the cause producing them. All authors agree as to the time of the supervention of tetanus in the infant, fixing it from the fourth to the fourteenth day after birth, occasionally a few days later, but most generally about the ninth day. This is also about the time allowed by authors for traumatic tetanus to develop itself in the adult, after the reception of the wound. In general the tetanus supervenes between the fourth and fourteenth day after the infliction of the injury; sometimes in the second week is the most common time of all. *Watson's Pract. Med.*, p. 312.

A fact which cannot be reconciled, except with the view of its traumatic origin, is that tetanus nascentium *always* supervenes within the time of the dropping off of the cord, and the time which we would reasonably allow for the perfect and healthful cicatrization of the umbilicus.

For the purpose of comparison, I will as briefly as possible enumerate the most prominent symptoms observed in traumatic tetanus in the adult. Some authors mention various premonitory symptoms as indicating its approach, such as a feeling of weariness, watchfulness, loss of appetite, headache, constipation, pains in the back, &c.; but as all of these are the common precursors of many other species of indisposition, they cannot be properly classed as preceding the attack of one more than another. Larrey has observed that the symptom which undoubtedly announces the approach of traumatic tetanus, is deep and severe pains in the wound extending to the spine, but even this symptom has not been generally noticed by other writers. The first unmistakable symptom, then, indicative of its approach, may be said to be, a feeling of weariness or stiffness about the muscles of the jaws and throat, easily fatigued in the act of mastication and deglutition; also, stiffness and pain in the muscles of the neck, with repugnance to turning the head, sometimes accompanied, but generally succeeded by



twitchings of the muscles of some part of the body. This stiffness in the muscles of the jaws increases slowly, until after a time the jaws are completely closed. Though the closure of the jaws is generally very gradually effected, yet occasionally violent symptoms manifest themselves at the commencement of the disease, and the jaws are suddenly and firmly locked. Other muscles of the body and extremities then partake of the spasm and rigidity; sometimes one set is more affected than another, drawing the body in different directions; violent pain in the præcordia, extending back to the spine; difficult and irregular breathing; frothing at the mouth; a particular state of the countenance denominated by some the true "*risus sardonicus*," produced by the contracted state of the muscles of the forehead; brows and corners of the eyes, the expanded nostrils, alæ of the nose raised; corners of the mouth elevated; eyes prominent and fixed; veins of the face swollen; giving upon the whole a most frightful look and anxious expression to the countenance, and evincing the greatest degree of bodily pain and suffering.

In some instances the tongue is contracted and immovable, and nothing but a thick frothy saliva exudes through the teeth and lips; in other instances it is protruded and often lacerated by the teeth during the severe clonic spasms, producing considerable hemorrhage from the mouth. The larynx is raised, the tone of the voice considerably modified, the act of articulation being always unpleasant, sometimes painful, and occasionally the power of speech lost altogether. The voluntary muscular system generally is in a state of tonic rigidity more or less all the while, sometimes amounting only to a slight stiffness; at others perfectly rigid and "as hard as a board." It has been remarked by some; that the flexor and extensor muscles of the fore-arm and hand are generally unaffected. Clonic spasms come on at various intervals, and sometimes with perfect regularity, increasing in violence and frequency with the aggravation in the other symptoms. I have seen a case of traumatic tetanus in an adult negro, when they came on with regular intervals of two or three minutes, lasting ten or fifteen seconds, the spasms subsiding during the intermissions into a state of semi-rigidity. These spasms came on with a "jerk," as the patient aptly termed it, producing excruciating pain in the præcordia, extending back to the spine, and suspending respiration for the time. In this case the most disagreeable feeling complained of by the patient during the intervals of the spasms, was a distressing sensation of fullness and tension about the præcordial region. The extremities are generally cool, and the surface bathed in a profuse perspiration, especially the head, face and breast. The pulse, generally full or natural at the commencement, becomes small and frequent as the disease advances. Attempts to swallow, and often the effort to speak, change of position, or noise, sudden draughts of air, &c., bring on paroxysms of clonic spasms. The function of the stomach and bowels, occasionally unimpaired, is generally deranged, constipation being a pretty constant attendant. The sphincters of the anus and bladder are generally in a state of firm contraction, giving rise itself to constipation of the bowels and retention of urine, which is sometimes involuntarily and forcibly expelled. Amidst all this bodily anguish and pain, strange as it might seem, the mind still holds her empire, and generally with all her pristine beauty.

The disease has been variously divided, according to the particular sets of muscles which are more especially the seat of spasm—into *trismus*, *opisthotonos*, *emprosthotonos*, and *pleurosthotonos*—tetanus, by some au-

thors being only used to denote that particular form of the disease where the muscles are all equally affected by the spasm—the body being perfectly straight and rigid. These divisions, however, are of no practical value.

These comprise most of the symptoms observed in acute tetanus, but, of course, are subject to various modifications, not only from age, climate, and habits of life, but frequently differing to some extent when all apparent external causes are equal.

From these symptoms the patient may gradually recover; they may terminate in death at an early stage of the disease, during the violence of the clonic spasms, or they may continue for days, increasing in intensity; the breathing becoming more difficult and hurried; the intervals between the clonic spasms shorter; the perspiration becoming more profuse, cold, and clammy; the pulse becoming weaker and more frequent, and, at length, imperceptible at the wrist. The patient sinks generally from the fourth to the eighth day, but more often about the sixth—either from asphyxia, or the system, being worn out by the violence and intensity of bodily pain and suffering, succumbs, completely exhausted. Occasionally, at a late stage of the disease, delirium supervenes. Sometimes the spasms and rigidity of the muscles are ameliorated before death. In the case of traumatic tetanus before alluded to, as occurring in my own practice, and which terminated fatally, the spasms subsided almost completely twenty-four hours before death took place—the patient retaining the full possession of all his mental faculties up to the last moment.

It is not so easy to institute an analysis of the symptoms of the disease as it occurs in the two ages—as in the adult we have the benefit of the patient's own narration of his feelings and sensations, whereas, in the infant, we are deprived of this most valuable index to the disease, and aid in its treatment. The symptoms, however, which are visible to our senses, I will speak of, and draw such inferences as may be deemed legitimate.

In the infant, the first indication of tetanus is manifested to the mother by its fretfulness, and ineffectual attempts to nurse. Taking the nipple in its mouth, and making one or two efforts to draw, it suddenly throws itself off, continuing to fret, and repeating the effort to nurse again in a few minutes, but with no better success. From this *initiator* symptom, taken in connection with the other symptoms which follow, we may reasonably infer the existence of the *same feeling of weariness, stiffness, and pain in the muscles of the jaws, and those of the throat assisting in the act of deglutition*, which the adult describes to us in the commencement of his attack. The symptoms, however, which are first observed by the physician are the stiffness of the jaws, sometimes firmly and tightly closed, at others separated at a short distance and immovable, frothing at the mouth, and muscular twitchings brought on by the most trivial causes. From the countenance of the little sufferer during the clonic spasms, the feeble moan or grunt just as they come on, the interruption to the breathing during the paroxysm, followed by the lusty cry as it passes off, it seems to me that we cannot resist the inference that the *same præcordial pain and agonizing sense of suffocation* are present, so much complained of by the adult patient. In the whole train of other symptoms which follows—such as the various kinds of abnormal muscular action; the character of the breathing; temperature of the surface; profuse perspiration; condition and character of the pulse; extreme nervous impressibility, and "*risus sardonius*,"

there is no difference to reconcile,—for I apprehend no one will contend there is a greater want of uniformity in the symptoms of the disease as they occur in the infant and adult, than is observed in different instances in the adult. I have seen one case of tetanus nascentium answering completely to the description of what has been termed *genuine tetanus* in the adult.

The muscular rigidity was so diffused and uniform, that the muscles of the jaws, body and extremities, were all perfectly and equally rigid—so much so, that the hand could be placed under the middle of the child's back, and it raised up without bending in any way. Authors, on this point, I believe, are pretty unanimous. I will, therefore, only quote one which occurs to me just at this moment. Cullen, in a paragraph upon tetanus nascentium, (*Caldwell's Ed.*, vol. ii. p. 172,) in speaking of the "locked jaw," (which is by far the most uniform symptom present in all instances of tetanus, so far as abnormal muscular action is concerned,) remarks, "but this is not the only symptom, as for the most part *it has all the same symptoms* as the opisthotonos and tetanus so called, and which occur in other varieties of tetanic complaints, above described."

The disease is of much shorter duration in the infant than adult, terminating generally in from six to forty-eight hours after the first symptoms are observed—most often in about thirty, or thirty-six hours after the attack, but occasionally it does not terminate until the third, fourth, or fifth day, or even later, when it is termed *chronic*. Here again we perceive a difference in the disease as it occurs in the two ages—terminating in death, in a much shorter time in the infant than adult, *generally*—the cause of which, however, seems sufficiently obvious to my mind. The *nervous impressibility* of the infant is much more delicate and acute than that of the adult. M. Hall, in his work on the "*Physiology of the Nervous System*," p. 44, in speaking of the impressibility of the "extreme filaments of the excitor nerves," remarks, "these phenomena are most obvious in the lower orders of animals, *and in the very young of the higher orders.*" This fact may also be attributed to the cause before alluded to, as making it more uniformly fatal in the infant than adult—the fact of the young and delicate infant being more easily exhausted by morbid impressions, and not having the power of resisting them to the same extent.

The post-mortem appearances in tetanus in the adult are not always uniform, but the abnormal exhibitions found about the medulla spinalis and its investing membranes, are so generally present, as to entitle them to be regarded as the seat of pathological changes in this disease. I will briefly enumerate some of the morbid appearances which have been detected in adult patients dying of tetanus.

On examining the body of a boy who died of traumatic tetanus on the fourth day after the injury, "it was found that the cervical portion of the spinal cord, to the extent of an inch, had undergone considerable softening, and that the membranes covering this portion were inflamed and much thickened." *Curling on Tet.*, p. 35.

The following are the post-mortem appearances in a case published as an example of idiopathic tetanus. "The body being examined, a great quantity of fluid was found in the theca vertebralis; the membranes enveloping the spinal marrow were of a rose colour, the minute vessels being *turgid with blood.*"—(*Ibid.*)

H. Sandwith, M. D., *London Lancet*, vol. iii. No. iv. p. 322, after having detailed at length a case of traumatic tetanus, remarks, "In this and

other tetanic cases, there was *increased vascularity* of the cellular membrane surrounding the theca vertebralis, together with increase of fluid within the theca, and the tunics, as well as substance of the medulla, were *extremely vascular*.

Baron Larrey states that in the *numerous inspections* of the bodies of the soldiers who died of tetanus, in the Hospitals of Louvain, after the battle of Waterloo, which were made with the greatest care, he *constantly* discovered evident traces of inflammation on the spinal cord, with serous effusion more or less of a *reddish colour* within the sheath."—*Clinique Chirurgicale*, vol. i. p. 88.

Ollivier discovered depositions of a *red and very consistent fluid* in the cellular texture between the dura mater of the cord and the bony canal of the spine in the dorsal region," &c.—*Curling*, p. 39.

"In some few instances *blood has been found extravasated in the spinal sheath*." This was noticed in a case of traumatic tetanus recorded by Dr. Funk. \* \* \* \* In another case of traumatic tetanus, mentioned by the same author, a similar appearance was observed.—(*Ibid.*, p. 37.)

"In an interesting case of tetanus given by Dr. Reid, in the *Transactions of the Association of Physicians in Ireland*, vol. i. p. 113, *great vascularity and an effusion of blood were found round the spinal marrow*."—*Marshall Hall on the Nerves*, p. 214.

In *tetanus nascentium*, as in tetanus in the adult, the anatomical lesions observed after death are not always the same. Sometimes every part of the body is found to present a perfectly normal appearance, and like it too, most generally *spinal* lesions are observed.

"Dr. Goëlis, of Vienna, in the examination of children who have died of this disease in the Foundling Hospital of that city, frequently found an appearance of *increased vascularity* in the substance of, and in the membranes enveloping the upper part of the spinal marrow. The same has been observed by Dr. Thompson, of Philadelphia.—*Curling on Tet. Nascentium*, p. 109.

M. Billard (*Stewart's Ed.*, p. 490), after remarking that he had only witnessed two instances of the disease, both of which proved fatal, observes, he "found on dissection nothing more than an effusion of a quantity of *coagulated blood in the spine*."

In the April number of the *Amer. Journ. Med. Sci.* for 1846, p. 365, in the article by Dr. Sims, before referred to, he found on dissection, in a negro infant having died of this disease, a "*coagulum of blood occupying the spine in its whole length*, enveloping perfectly the medulla spinalis; thicker as it approaches the brain. Spinal veins full of black blood." In the "brain, superficial vessels full of black blood, particularly, posteriorly." Nothing else remarkable in the post-mortem appearances.

In a case of *tetanus nascentium* which occurred in my own practice, I will here simply give the post-mortem appearances. On the 15th March, 1846, I examined the body of a well-formed and fully-matured male negro infant, the property of Judge Goldthwaite, of this city—having died of *tetanus nascentium* a few hours after it had completed its sixth day from birth. Autopsy seven hours after death. Present Drs. Sims, Boling and others.

The body generally flexible; the toes and fingers firmly clenched. The umbilicus enlarged, hardened, and a portion of the surface to which the cord was attached looks dry and red; the cuticle is denuded in two small places, about half an inch from the umbilicus, and on the right side.

*Head.*—Membranes and substance of the brain natural. Dr. Sims thinks the most bloodless brain he ever saw.

*Spine.*—*A thick and very firm coagulum of blood found covering the posterior and lateral surfaces of the medulla spinalis, extending the whole length of the spinal column.*

Lungs, heart and abdominal viscera normal.

The reader may here draw his own comparisons, and appreciate, if any, the inconsistency of the pathological anatomy in the disease, as it occurs in the two ages. In both instances, it frequently occurs that no morbid lesion is detected in any part of the body; in both, lesions about the spinal cord are *generally* detected, which sometimes are *identically* the same, and in the other instances bear a sufficient resemblance to prove them results of the same morbid action. I may, however, allude to this subject again, when I come to speak with more especial reference to the pathology of the disease, and endeavour to reconcile with the view of their identity, that which some may contend shows a difference in their morbid anatomy—the fact that *extravasated blood* is perhaps more *generally* found about the spine in the infant than adult.

It would be well, perhaps, before leaving this subject, to remark, that changes in other organs have been observed after death, and reported as morbid states belonging to this disease—such as a turgid state of the vessels of the brain, abscesses in the substance of the brain, gastro-enteritis, &c., but which, I think, should be more properly regarded as coincidences, or complication of the primary disease, than as a part of its pathological anatomy.

The cause producing this disease seems still to form a subject for speculation, and not unfrequently for lengthened discussions. As I before remarked, I am persuaded that the disease is nothing more or less than traumatic tetanus. In support of this view we have the fact that *the wound is always present*. Next—the infant, from its great nervous development, *comparatively* much greater than that of the adult, is much more susceptible or predisposed to tetanic and other convulsive diseases.

All who are conversant with the habits and mode of life of the negro, whose offspring are more peculiarly the subjects of this disease, can readily perceive to how very many causes which are capable of producing an unhealthy condition of the umbilicus, they are exposed. As an elucidation of the cause, as well as of the fact, that the negro infant is most often the subject of this disease, I will make a few remarks upon the circumstances which generally surround it, on its being ushered into an independent existence. These remarks; however, are made with more especial reference to negroes living on large plantations. The mother generally pursues her ordinary avocation up to the hour when the warnings of nature admonish her of the near approach of the termination of pregnancy, and this, if her vocation calls her far from the quarter, (as is sometimes the case with those who are accustomed to working in the field,) does not, in some instances, allow sufficient time for her to be conveyed to her room, and occasionally parturition takes place in the field, or between there and the house. I mention this fact merely to show how ill prepared the mother is to receive and properly treat her infant—the evil is one difficult to avoid, for not knowing herself, she is not able to make her own owner sensible of the period when her confinement will probably take place. When, however, her confinement takes place in the house, (the converse, of course, forming rare exceptions,) the infant is exposed, from the condition of the house itself,

in many instances, to two or three causes, *each* one of which has been reckoned as *alone* sufficient to produce the disease, and which at least may assist in producing an unhealthy condition of the umbilicus. The houses are so constructed as to admit the greatest extremes of cold, and yet such as are not easily ventilated, whilst the roof and walls, and, in some instances, even the floor, are not such as will insure dry apartments during damp weather. Added to this the mother's bedding is often insufficient, and composed of such material as is not capable of keeping up a uniform temperature of body. Such exposure to the varying vicissitudes of weather, it must be apparent to all, but ill befit a young and tender being who has been accustomed, for the last nine months, to an uniform temperature of 98 degrees.

Surrounded by such circumstances the child is born. The accoucheur is conducted, in a majority of instances, by a negro woman—frequently one of the daily labourers on the same farm, ignorant, of course, and often old and decrepid. This woman, who is dignified with the title of midwife, has care of the infant for the time, which she washes imperfectly, and if she has no hurtful notions of first applying irritating salves for the purpose of making the cord drop off in a short time, she proceeds to dress the navel, *in her own way—rude and awkward*, of course—applying such dressings as the mother may have provided, which are often composed of coarse and rough material. When this last office is performed, the midwife's attention to the case ceases. The mother is then left, inexperienced though she may be, *to attend herself and child*. One can well imagine what kind of attention *the child receives!* The mother, with her blunted affections and filthy habits, perhaps herself suffering from bodily malaise, is not disposed, *if indeed she is able*, to perform the most trifling service for it. The consequence is, the umbilicus receives little or no attention. In contact with the coarse and irritating cloths, often for days at a time, and the same with which it was first dressed, and they perhaps saturated with urine and other acrid secretions—the cord itself either dry and irritating, or putrid and excoriating, together with the quantities of dirt and other foreign substances which may have insinuated themselves between the dressings and the skin—the umbilicus is left to take on inflammation, ulceration, or gangrene, or to gradually enclose or envelop dirt or other foreign substances within its callous cicatrix—as the case may be.

With these powerful causes, all of which are rarely absent in any one case, and one of which *might* produce the disease, with a predisposing constitution of the atmosphere, it is wonderful to me that even a greater number of infants are not the victims of tetanus nascentium. Expose the refined and elegant white lady, with all her acute and sensitive feelings of affection for her offspring, to the same physical causes, and throw the care of her child upon her own hands amidst bodily disabilities, and we will find she will much oftener be called upon to mourn the loss of her babe from the effects of this “direful malady.”

Moseley, in his work on “*Tropical Diseases*,” p. 510, in alluding to the occurrence of tetanus nascentium among the negroes of the West Indies, (which has been an extensive theatre for its ravages,) remarks, “*it is most often seen where the mothers of the children are very young, or very poor, or very worthless.*” That the remote causes, inviting an unhealthy condition of the umbilicus, and favouring the development of this form of disease, may, in some instances, be so powerful as to induce tetanus nascentium

tium, notwithstanding all proper treatment has been premised, I have no doubt; but that the number of its victims has been greatly augmented by neglect in this respect, I have just as little.

I think I am fully justified in saying, from our knowledge of their habits, that the remarks made in relation to the treatment which the umbilicus of the newly-born infant receives with the negro slaves of this country, are applicable to them *as a race*, in whatever country found. As also to the indigent portion of our own race, who are educated and accustomed to habits of indolence and filthiness, and whose offspring furnish, next to the negro, the greatest amount of subjects for this disease.

As we have seen that the *negro* infant is most frequently the subject of tetanus nascentium, so also we see the adult negro is most often the subject of traumatic tetanus—hence it has been intimated that the negro race possess, incorporated within their nature, some idiosyncrasy, which favours the development of diseases of this character. I cannot conceive of any difference in constitution which can afford a *comparative immunity* to the white race against attacks of tetanus. As we have seen the negro infant is exposed to more causes calculated to produce an unhealthy condition of the umbilicus, functional derangements, &c., so also we see (as has been remarked by others,) the adult negro is more exposed to *wounds*, which are oftener left in a neglected and unprotected state. There is no difference in their nervous development, and if there is a difference in their nervous impressibility, I cannot see why it should predominate in the negro. Subject them to the same moral and physical causes, in conjunction with the same predisponents, and I apprehend no difference will be found on the bills of mortality, from the effects of this disease upon the two races.

In some instances, tetanus nascentium supervenes when the umbilicus seems to be doing well, and not unfrequently, when the wound is partially, or even entirely cicatrized—and this by some is urged as strong grounds for the belief, that, the umbilicus exerts no agency in its production. We read in "*Evanson and Maunsell on the Diseases of Children*," p. 220, "This view" (of its probable traumatic origin) "has, however, been impugned by Dr. Labatt, who has published particulars of some post mortem examinations, in which no appearance, satisfactorily accounting for the disease, showed itself in the cord." The same grounds are taken by many others, but to my mind it is only evidence of the still greater analogy which it bears to traumatic tetanus in the adult; for, here we also see the disease supervene in many instances, when the wound appears to be doing well, and often after it has entirely healed.

Curling, in his work on "Tetanus," p. 25, remarks, "In many of the cases in the table the primary wound was *completely healed and almost forgotten*, when the symptoms of tetanus appeared."

"Mr. Morgan found on dissecting the thumb, in a case of tetanus, two pieces of splintered teak, imbedded in the radial nerve, without any appearance of inflammation in the part." (*Ibid.* p. 40.)

Dr. Rush, whose ample opportunities for observation in the Military Hospitals of the United States entitle any remark from him to the highest consideration, observed that there was "*invariably* an absence of inflammation in the wound causing the disease." *Amer. Philo. Trans.*, vol. ii.

In an article entitled "Observations on Tetanus, by M. A. McDowell, M. D., of Louisville, Ky." contained in the *New Orleans Med. and Surg. Journ.* for March 1846, the author details at length numerous cases where the wound had *completely* healed before the symptoms of tetanus were

manifested: so completely was this the case and so trifling had been the wounds in the first place, that the author, in order to satisfy himself of the fact, was compelled to institute a diligent search before the cicatrix could be found, the patient not even recollecting that he ever had received a wound on any part of his body, and yet subsequent developments proved beyond a doubt that the disease was dependent upon the little inconsiderable "*cicatrix*," detected alone by the perseverance and acuteness of the physician.

In accordance with the views of the last named author, relative to the production of tetanus in the adult, I maintain that the umbilicus in the infant, may have every seeming of a healthy condition, and yet be the *punctum saliens* of tetanus nascentium. He contends, (and his position is well fortified by many stubborn and interesting facts,) that in order to the development of tetanus in the adult, "The smallest portion of any such foreign matter fixed in the substance of, or lodged in contact with these sensitive structures, may, as well as the largest, suffice to produce the malady." Particles of metal or of metallic oxides, such as film of the turned edge of a knife, a particle of oxide of iron or of lead, a briar, a splinter of wood, or a particle of glass or *sand*, &c. &c. We have seen that the mothers of those children who are most often the subjects of tetanus, are either from habit or necessity addicted to filthy habits, and pay but little attention to the state of the umbilicus in their children, leaving it pretty much to itself, in contact with dirt or any other foreign substance which may chance to lodge about it, to get well as best it may. Under such circumstances, is it not very probable that a grain of dust, or sand, or other particle of foreign substance, however small, may be lodged in the delicate granulations of the umbilicus just after the detachment of the cord, and be included in the cicatrix, and in contact with a nervous filament, and thus become a point of irritation sufficient to produce tetanus? or, without any foreign substance being enveloped in this way, may not the cicatrix itself include and compress within its callous substance, a nervous fibrilla, which with the peculiar constitution of the atmosphere or presence of any of the remote causes favouring the development of that form of disease prove quite sufficient to produce it? When we know that the almost invisible point of a needle piercing the finger, or the sting of a bee on the nose, has given rise to the worst forms of tetanus in the adult, how inconceivably small must be the cause which is *capable* of producing it in the newly-born infant with all its tendencies and predispositions to diseases of that character?

I think we are abundantly authorized in believing, from the evidence before us, that the umbilicus, as indicated by its external appearance, is not always a sure index as to the agency which it may exert in the production of tetanus nascentium; and, the supposition most reasonable and legitimate, in connection with the other facts brought forward, is that, in the absence of inflammation, ulceration, or gangrene in the wound, (which are perhaps the most frequent exciting causes,) its production is properly attributable to the irritation produced by contact of some foreign substance with a nervous fibrilla, or pressure of the same in the callous cicatrix of the umbilicus.

In an article in the April No. of the *American Journ. Med. Sci.* for 1846, p. 363, on "*Trismus Nascentium*, by J. Marion Sims, M. D., of Montgomery, Ala." before referred to, the author, with much earnestness and great plausibility, attributes its production to imperfect ossification of the cranial bones of the fœtus, and mechanical pressure upon the os occipitis, produced by decubitis on the back; the imprudent habit which some mothers



have of keeping their children "for a length of time in the recumbent posture on a hard matrass, or a folded blanket with a little bit of hard old quilt, or a bunch of dirty clothes (as we often find amongst negroes) wadded up and stuck under the occiput," averring as its consequence an interruption to the flow of blood from the "medullæ spinal veins," and a "congestion" and ultimate extravasation "within the dura mater of the cord."

This most ingenious hypothesis, however creditable to the author for its originality and the beauty of its conception, is supported by but few facts, which will not sustain with equal or more force very opposite conclusions. I do not intend a review of this article, but, one striking and well authenticated feature in the history of the disease occurs to my mind as being so completely irreconcilable with this view of its pathology, that I cannot forbear reference to it. It is that *the post-mortem appearances are not uniform*. Cases, very many cases have been reported, which, upon dissection, so far from revealing traces of "spinal apoplexy," or "a coagulum of blood occupying the spine in its whole length," did *not even present an increased vascularity about the parts*, the cord and membranes exhibiting *a perfectly normal appearance*.

As having some bearing upon this theory, I must make a few extracts from my case-book, relative to the case of tetanus nascentium which occurred in Judge Goldthwaite's little negro; the post-mortem appearances of which have been given at another page. The statements of the mother may be taken for what they are worth. I do not vouch for their correctness, though she seemed to be an intelligent, truth-telling negro, and as I did not question her concerning what had been the position of the child, in such a manner as to bias her answers, I take it for granted her statement was correct.

*March 14th.* \* \* \* \* "The mother states that the child had an operation at 10 o'clock last night, and at that time seemed to be doing well; at 12 o'clock (two hours after) it became very fretful and refused to nurse, and occasionally was noticed to "jerk" as if it had spasms, and continued in this situation through the night. Has passed the meconium, and its last operation was yellow; 11 o'clock A. M. The mother is lying on a *soft feather bed*, and in every way comfortably situated. When questioned relative to what position she had kept the child in since its birth, she stated that it was *her habit to lie on her side*, and that she kept the child *on its side facing her*, changing its position as she changed her own from side to side, but never on its back." When the dressings were removed this morning, the cord was found among them, detached. The mother does not know when the cord came off, as the midwife who attended the case did not make her appearance on yesterday morning; consequently the dressings were not removed; the cord was detached, then, some time in the last forty-eight hours. I shall not detail the symptoms present, but simply remark it was an undoubted case of tetanus nascentium, and marked by the usual symptoms. My friends, Dr. Sims and Dr. Boling, who I invited to see the case, both concurring with me in opinion that it was a genuine, well marked case of tetanus nascentium.

Among other prescriptions left for the child, I directed that it should not be allowed to lie on its back, and that its position should be changed often, from side to side on a soft pillow. As regards the position of the bones of the head "they seemed to me to be about as regular and well formed as in infants generally of its age. Dr. Sims thinks the lateral edges of the occipital bone are *'slightly'* overlapped by the parietal. \* \* \* \* Dr. Boling, after

carefully examining the bones of the head, and especially the occipital and parietal, remarked, that if their position was remarkable for anything, it was for its very great regularity and uniformity."

In a subsequent conversation with the mistress relative to this case, she seemed to be altogether convinced that the umbilicus had been very much neglected, and appeared quite incensed with the midwife, for what she considered a remission of duty on her part in not attending to it: and this opinion was expressed by her, before she was aware of the views which I entertain on the subject.

I have detailed the particulars of this case, because I consider the facts and statements embodied in it, if considered valid, go, as far as *one* case can go, towards disproving the *premises* relative to "position" and "occipital displacement."

I shall refrain from further comment upon these views, as I deem sufficient has been said to show their inconsistency with the true state of facts connected with the history of the disease; there are, however, other and material objections to them, which present themselves to my mind. In closing my remarks upon this article, I hope I may be allowed to say, as the talented author is a fellow-townsmen, and a gentleman with whom, I am pleased to say, I am on the most amicable and friendly terms, in both a private and professional point of view, I have endeavoured, though as *briefly* as possible, to state his premises and conclusions fairly, and to treat them with that due consideration which their respectable origin demands. Regarding him as essentially discriminating, judicious and practical, and not prone to hasty conclusions, *in this* I believe him to be *in error*.

Tetanus nascentium having been made the special subject for discussion at a late meeting of the "Medico Physical Society of New Orleans," Dr. Harrison, in the course of a few brief remarks, (reported in the *New Orleans Med. and Surg. Journ.* for May 1846, p. 631,) observed, "Again, we find it scarcely existing on the main land of Iceland, but in the small island adjacent, such has been its fatality, that not a child has been raised there. These are highly valuable historic facts, which forbid the idea of its having any connection with umbilical injury." This "small island" referred to, is the island of Heimacy, on the *southern* coast of Iceland, *completely* devoid of vegetation, its surface "*entirely* covered with lava," and in the immediate neighbourhood of Mount Hecla. May not the physical face of this island, the variation of temperature occasioned by a direct exposure to a southerly wind on the one side, and an extensive volcanic eruption on the other, exert a sufficiently modifying influence upon the character of the climate, to account for the greater endemial predisposition to the disease here than on the main land? Added to this there is some difference in their habits of life as a people; this little island being entirely inhabited by fishermen, who are generally poor and destitute.

The pathology of tetanus has been and is still, a subject of peculiar interest; and yet it is one far from being settled. In order to be more forcibly impressed with the opposing theories advocated on this subject, one has but to look at the multitude of varying and incompatible modes of treatment, which at the present day claim the attention of the medical profession, relative to its cure.

I will not attempt a review of the several doctrines advocated on this point, but, could I venture an opinion upon a subject, the complexity and intricacy of which has eluded and bewildered the most distinguished minds, I would say that much has been done to elucidate the true nature of its

pathology, by the labours of *Dr. Marshall Hall*. In his work "on the nerves," he has demonstrated most conclusively, the existence of a new physiological action in the medulla oblongata and spinalis, which he terms "*the reflex function*," and which he attributes to a distinct system of nerves, denominated by him "*excito-motory*," and which "*function*," he thinks, presides over the "*whole tone of the muscular system*," (p. 40.) Stated in substance it is simply this: an irritation being made upon a particular nerve, is conveyed to the medulla, and from thence *reflected* through other nerves to any part of the body, however distant it may be from the nerve to which the stimulus was originally applied. This the author (Hall) thinks furnishes a clear and admirable solution to the phenomena observed in traumatic tetanus. To use his own language, which is forcible and comprehensive, would perhaps be doing greater justice both to himself and the subject. In speaking of the "*excito-motory*" system, p. 43, he remarks: "The influence of the stimulus is not only *reflected* upon the limbs, but it is *retrograde* in its course, passing from a nerve proceeding from the middle part of the spine *forwards* to the anterior, as well as *backwards* to the posterior extremities."

By experiments instituted upon the excito-motory system of some of the lower orders of animals, (decapitated, in order to show the independence of this action of the brain,) the author just quoted has been enabled to produce at will, simply by compressing a nerve, or irritating it in other ways, states of tonic contraction, or spasmodic rigidity of the muscular system, so intimately allied to those abnormal phenomena which are the chief characteristics of tetanus in both the adult and infant, that they cannot be rationally accounted for, except upon the same general physiological laws. These experiments not only satisfactorily explain the phenomena of tetanus, but receive themselves strong confirmation from some of its features. The clear and lucid intellect, and the absence of pain, or any unnatural or uncomfortable feeling in the brain amid the violence of tetanic convulsions demonstrate most forcibly its *ex parte* relation to the morbid action going on, except so far as it may be *occasionally* affected through its sympathetic agencies.

If the results of this author's experiments be admitted, and I believe they have met confirmation from subsequent experimenters, at least so far as the excito-motory system is concerned, his deductions seem altogether plausible, if not irresistible, and the rationale of tetanus of easy comprehension.

An individual receives a wound upon any part of the body, and, however slight, (for it has been proved by observation that an injury to the smallest nervous filament is just as apt to produce the disease as a direct injury to a large nerve,) in a few days, other circumstances favouring, tetanus is the result; the irritation at the seat of injury produced upon an excitor nerve or filament, is conveyed to the medulla oblongata and spinalis, the great center and axis of the "*excito-motory*" system, and from thence reflected through the motor nerves to any or every part of the muscular system, giving rise to the rigidity and spasmodic action observed in tetanus. This action is strikingly exemplified and the perversion of nervous influence beautifully displayed in both infant and adult labouring under tetanus, in taking fluid or any other substance in the mouth requiring the act of deglutition, whilst it also shows the great nervous impressibility of the system after the disease has once been established. The substance so taken in the mouth and attempted to be swallowed, frequently acts as an *irritant* to the excitors of the pharynx, which impression being conveyed by them to the great spinal axis is instantly returned by the respective motor branches

for that region, causing a spasmodic closure of the pharynx and the contents to be forcibly expelled through the mouth and nostrils. The support afforded by this particular phenomenon in tetanus, to the theory of the "reflex function," does not stop here, for it must be observed that though the irritation is applied only to the larynx and conveyed to the great spinal axis by the excitor branches of the pneumogastric, it is, however, *reflected* to every part of the muscular system, however remote from the point of irritation, supplying a stimulus to abnormal action.

In order to the full comprehension of the pathology of this disease, it would seem proper to reconcile the post-mortem appearances with the rationale of the symptoms observed during life. These, as we have seen, are not always uniform, yet the spine is sufficiently often the seat of morbid lesion to justify us in looking at the changes observed there, as the true pathological anatomy of the disease, and the abnormal condition of other organs sometimes superadded, as complications to the primary disease, the effect of remedies administered during its treatment, &c. The irritation produced on an excitor nerve by puncturing, dividing, lacerating, or wounding it in any way, or compressing it by ligature or cicatrix, or by its being in contact with a particle of foreign substance, or in *whatever* manner the irritation is produced, other causes equal, gives rise to the symptoms observed in tetanus. In order to produce these phenomena, this irritation must be conveyed to the spine, thus making it the great seat and center to and from which this powerful and unnatural stimulus is conveyed. Is it not most rational to conclude that the spinal column cannot long continue to be the medium and theatre through and around which these extraordinary functional aberrations are being produced without itself becoming involved in even a greater lesion? In diseases of other organs, having their commencement merely in a failure to perform their natural and healthful function, we often witness in post-obit dissections the greatest amount of organic disease. So in tetanus, the disease is functional in its commencement, and it may be throughout its course, but as *consequences*, organic changes are dependent upon it, and these are found in the spinal column, where we would most reasonably expect to see them. The irritation producing the disease being first conveyed to the medula spinalis, causes, in unison with the well known and universally recognized axiom or law in pathology, "*ubi irritatio ibi fluxus*," a flow of blood to the medulla and its investing membranes. From the symptoms, such as coolness of the extremities, &c., it is evident there is a concentric tendency of the blood; it is most reasonable to suppose that this determination is principally to the spinal region, from the nature of the action which we presume to be going on there. The vessels of the membranes of the cord continuously burdened with these undue columns of blood which are being forced through them, become weakened and inadequate to the performance of this super-amount of labour imposed upon them by morbid action, are unable to propel their contents forward. Consequently on dissection we find the vessels "*turgid with blood*," sometimes an effusion of "*reddish serum*" takes place, and occasionally the distension is so great as to *rupture* the vessels, even in the adult, and we find "*blood effused and coagulated within and around the spinal sheath.*"

I have not seen any mention made of the time of duration of the disease in those cases, in which nothing varying the natural and healthy appearance of the spine was observed, but think it probable that they were of short duration. It is a prevalent opinion, and one which I have not seen

doubted, that tetanus frequently kills by asphyxia. Indeed, we know the disease does terminate, frequently in a very short time, so short as not to allow time for organic changes to be effected, and hence, we can only account for it by the arrest of some function important to life. For instance, in the case where the servant sustained a slight injury on the hand, by the breaking of a plate, and died of tetanus in a quarter of an hour after the reception of the wound, we would not have expected to have found organic changes as the result in any part of the body. The immediate cause of death in that case must have been altogether a functional one; the man died from asphyxia, or a suspension of respiration, the result of spasmodic closure of the larynx during the violence of the clonic spasms. These facts and views explain, clearly and fully to my mind, the want of uniformity in the reports which have been made upon the post-mortem appearances in this disease.

In closing my remarks upon the pathological changes, observed in the infant and adult dying from this disease, I observed that I might refer to the subject again, and endeavour to explain that which some might contend was a difference in their morbid anatomy,—extravasated blood being *more generally* found about the spine in the infant than in the adult. All can appreciate the obvious and material difference, which must necessarily exist between the texture and power of resistance in the vascular coats of the adult, and those of the infant. There is in both instances *the same flow of blood to the spinal region*. The more firm and unyielding texture of the vascular coats of the membranes, of the medulla spinalis in the adult, together with the maturely ossified and hardened case formed by the vertebræ, affords a partial immunity to these vessels against the inordinate flux of blood,—not so however in the newly-born babe,—the same morbid phenomena in this region *we would expect* to develop very different pathological changes, differing even more widely than observation, proves to be the fact. From its greater proportionate nervous development and impressibility, it is exposed to the same, or even more intense morbid action, whilst the imperfectly organized, soft and almost gelatinous nature of its solid tissues, does not afford it that protection which more mature development gives to the adult. The vessels soon become weakened from distension, and not having that firm lateral support afforded to the adult by a more perfect ossification of the vertebræ, their own contractile powers naturally feeble, they are much easier ruptured by the columns of blood which are being forced through them, and hence “this hemorrhage of blood upon the spine,” when in the adult, perhaps, there would only be a “turgid” condition of the vessels.

There is no disease common to infancy and adult age, scarcely excepting the eruptive diseases themselves, (which under like circumstances must always appear in the same garb,) when the symptoms are less modified by time of life and difference in constitution, than in tetanus nascentium and tetanus in the adult. In both, recognized by the same symptoms, they are met with alike in the same climates, in the same classes of society, prevailing too in the same seasons of the year, and governed by the same vicissitudes of weather. Added to the essential sameness of their symptomatology and history, a farther and most conclusive proof of their identity is, that the post-mortem appearances in each exhibit no discordance in morbid lesion; which cannot be harmonized by legitimate reasoning.

I shall not attempt a lengthened discussion upon the numerous modes of treatment, which have been urged in view of the cure of tetanus nascentium. In regard to this point, suffice it to say—in practice they have all been un-

successful.—Now and then a case has been reported as cured, but so rarely has this been the case, that many would rather regard such instances as *errors in diagnosis*, than to believe the disease in any way amenable to medical treatment.

Notwithstanding I cannot doubt that cases of recovery have occurred, I am disposed to regard the *prophylactic* treatment, as almost the only one which promises a diminution in the mortality arising from it,—being far easier to avoid its dangers, than to encounter them successfully. The application of *clean, soft and unirritating* dressings to the umbilicus, I esteem as of the very first and highest importance. These should be removed and others applied at least *once* in the twenty-four hours, and the parts properly washed, with warm water, in order to clear away any acrid secretions which may have collected about them. This is more especially necessary after the cord has been detached. In conjunction with these, the proper hygienic rules prescribed in relation to cleanliness, ventilation and the avoidance of atmospheric vicissitudes, as applicable to protection against other diseases, are of equal value and importance here. A proper and strict observance of these rules, which are simple and within the reach of all, I cannot entertain a doubt will go very far towards curtailing the number of deaths arising from this malady.

And here I might, with propriety, close my remarks in relation to the treatment of this disease. I will, however, offer a few suggestions as to the treatment which occurs to my mind as being *most rational*, after symptoms of the disease have been developed. In accordance with the views expressed in relation to its cause, I have been led to believe that a proper local treatment of the umbilicus may be productive of beneficial results *in the commencement of the disease*. This consists in free incisions in the umbilicus, followed by the liberal application of nitrate of silver and bland poultices, or any application calculated to promote a healthy suppurative action in the wound. Not omitting at the same time the use of such remedies as may seem to be indicated to restore or correct deranged secretions, subdue spasmodic action, &c. In addition to a resort to free incisions in the wound, the use of the *actual cautery* would seem to promise farther aid in effecting the end desired, and as the case is one of extreme emergency, should perhaps be preferred before the nitrate of silver. The manner in which it is thought these means may promote favourable results, is by isolating any point of irritation by division of the nervous filament, deadening it by the application of nitrate of silver or the actual cautery, and effecting the removal of any “fixed irritant” by suppurative action. Dr. M'Dowell, of Louisville, Ky., in his article on tetanus in the *New Orleans Med. and Surg. Jour.* for March 1846, has reported several cases of traumatic tetanus in the adult, which he treated with perfect success on this plan. In some instances the relief afforded was instantaneous.

As having some connection with this part of my subject, and possessing within itself some points of interest, I have condensed the following notes from my case-book. On the estate of Mr. P. A. Wray, a wealthy planter, living within a few miles of this place, I visited, on the 12th January last, Bishop, a negro man about 26 years of age, labouring with a severe attack of traumatic tetanus. The disease was caused by an injury sustained about three weeks prior to my visit, by the bite of a hog on the lower and outer portion of the fleshy part of the *gastrocnemius* muscle of the right leg, causing a wound about an inch and a half long running parallel with the length of the leg. The man, instead of obeying his master's orders in

relation to the dressing of it, filled the wound with soot and other foreign substances. When I saw the case the wound looked indolent and dry, the edges separated by a dingy, hard granular substance, resembling very much in colour and appearance, beef which has lain for several days in salt brine. On the third day after I saw him, I divided the *peroneal nerve* just where it passes by the tendon of the biceps muscle; in the popliteal region, about ten or twelve inches from the wound on the leg. The division of the nerve caused considerable pain at the moment and brought on a spasm. The case proved fatal in the end, but the patient insisted, and gave me repeated assurances of the fact as long as he continued to live, that the operation mitigated materially the præcordial pain, which previously had been continuous; he afterwards only felt it during the clonic spasms. He had felt some symptoms of the disease ten days before I saw him, such as stiffness and weariness in the muscles of the jaws, but not apprehending anything serious he failed to inform his master of it until he was found with violent clonic spasms. I therefore conclude the operation did exert a modifying influence upon the disease; but at the time of its performance the disease had become so far independent of its local origin, as to limit the beneficial results which might have followed an earlier resort to it. I also observed in this case another fact which lessens the force of some objections which have been urged to dividing the nerve between the seat of injury and the spine. Some contend, that even when the proper nerve is divided, its interlacings and connections with other nerves *beyond* the wound, are sufficient to convey the irritation to the medulla spinalis. In the case under consideration, after removing the indurated granulations, I made a free application of nitrate of silver to the wound, which caused an acute burning pain. This was done two days previous to dividing the nerve; and the caustic application was repeated several times, each time causing more pain than at the previous application. *Subsequent* to the operation the caustic produced no pain, the patient expressing himself as being scarcely conscious of its application. The patient died on the 19th. From my observations in this case, and the reports of successful cases treated in this way by others, I am induced to the belief that it will be found a valuable sanative means, *if practiced in the earliest stages of the disease*, and that the cause of its failure in many instances has been too great a delay before its performance. Three incisions *above* the wound, when the anatomical arrangements are such as to admit of it, may answer the same end, which is the only practicable mode of doing it in tetanus nascentium. From the violence and rapidity with which the disease runs its course in the infant, of course it will sooner be out of reach of this means of treatment than in the adult,—hence the propriety of even greater promptness in a resort to it.

In an article on "Mustard poultices applied extensively to the surface," in the January number of "*The Western Journ. of Med. and Surg.* for 1845," I mentioned the fact of my having enveloped an infant labouring under tetanus nascentium, in a mustard poultice, and was pleased with the palliating influence which this remedy seemed to exert. I have had but the one case of the kind since, and for particular reasons did not resort to this remedy; but from my experience with it generally, I am still disposed to think it may exert considerable influence in mitigating the violence of spasmodic action in this disease.

Through a fear of rendering this article tedious, I have curtailed, as much as possible, quotations from authors—only resorting to them so far as was deemed necessary to establish the different points in relation to the

history of the disease; and, for the same reason, I have abbreviated materially my own reflections in regard to it. The subject, however, is one of considerable interest, and far from being exhausted; and one which, I am pleased to see, is attracting of late the attention of some of the most gifted members of the profession.

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ART. VI.—*Cases of Epilepsy.* By H. CAMPBELL, M. D., of Uniontown, Pa.  
(Communicated by Prof. HORNER.)

CASE I.—On the 11th July, 1839, J. Strickler, while working in the harvest field, received a wound from a pitchfork, on the left side of the frontal bone, the prong of the fork penetrating to the depth of an inch and a half, a little in front of the coronal suture, about three inches above the zygoma. The fork struck so firmly as to require some force to extract it. The direction of the wound was obliquely from before backward, the point of the fork inclining towards the centre of the cerebrum. When I saw him two hours after the accident, there were complete paralysis of the right arm and leg, and entire loss of speech. The wound was small and closed up with coagulum. I made an incision through the scalp directly across the puncture, down to the bone to allow the escape of blood through the opening in the cranium, and put him upon strict antiphlogistic treatment. The wound healed rapidly, and in five or six months he was able to walk with crutches, and to talk so as to be understood. About this time he had an attack of epilepsy. The fits recurred at intervals of eight or ten weeks for about a year, when they increased in frequency and returned pretty regularly about every four weeks, until the spring of 1841, when I proposed to him the operation of trephining as a *possible* means of relief. After some time he consented to the operation; and in the month of May, 1841, in the presence of Drs. Fuller and A. H. Campbell, I removed a portion of the bone with the trephine, embracing the puncture in the centre of the instrument. When the circular piece embraced in the trephine was entirely separated from the surrounding bone, I could not remove it because of an attachment to the dura mater. I raised it slightly and detached the dura mater with the scalpel as close as possible to the surface of the bone. On examination after removal, it was evident that the prong of the fork in entering had produced a slight crack or fissure on the internal surface of the inner plate, from the point of entrance backwards in the direction in which the fork entered, and when the fork was extracted the edges of this fissure closed, and pinched up a portion of the dura mater, which adhered firmly in that position. How far this crack and pinching up of the dura mater extended beyond the limits of the bone removed by the trephine we could not tell, but it was visible at the inner edge of the opening. The fits gradually diminished in



frequency for about a year, when they ceased altogether, and from that time to the present (July, 1846), he has had no return, and he has recovered almost perfectly his speech and the use of his arm and leg.

CASE II.—The following curious case came under my observation. S. W. had been afflicted for many years with that form of epilepsy which makes its attacks in its early stages during sleep. The fits at first occurred only after long intervals, and did not interfere with his regular attention to his business of a tanner. Gradually they became more frequent, and would seize him at all hours of the day, frequently as he walked along the street, with all the violent spasm of the worst forms of the disease. He became dull, and totally unfit for business, with a downcast and almost idiotic look.

Some three years ago a mesmeric lecturer visited our town, and W. became strongly impressed with the belief that if he were mesmerized, it would cure him. He consulted me, and simply to gratify him, I approved his determination to make trial of it. A time was appointed, and he invited me to be present. I would remark here that for three or four days, W. was under a high degree of mental excitement, produced by the hope and even firm belief that mesmerism would cure him. His step was light, quick and elastic, his countenance bright, and face slightly flushed. He walked rapidly, indeed almost ran whenever he moved. His eye sparkled, and the vacant stare was entirely changed for the look of intelligence. He constantly attended the mesmeric lectures, and took great interest in all the experiments exhibited. At the time fixed for operating, by putting him in the mesmeric sleep, I attended, but all the efforts of the mesmerizer were ineffectual except to increase his excitement. After an hour or more the experiment was discontinued, with the common excuse that "he was not a suitable subject." But a young man for his own amusement took W. into a private room, telling him that he could mesmerize him. Here he became so much excited, that the young man was frightened and left him. He was taken home and continued in that state all night. Next day I was requested to visit him. I found him somewhat calm, and lying quietly on his back in bed. On my asking him what was the matter, he told me to sit down and I would see. He began to explain to me that his legs, arms and body were drawn violently, but without pain in every direction, without his being able to control or prevent it, although perfectly conscious. Presently he observed, "Now it's coming on," and almost immediately his legs and body were violently thrown up until he stood nearly on his head. When his body and limbs came down, he appeared to be thrown convulsively to the opposite side of the bed—then back again. After a minute or two he settled down quietly, observing, "Now it's all over for a while." I asked him if he could not keep still by holding to the bed. He answered that he had tried but could not. When he gave me notice of the approach of the next spasm, I told him to hold fast with his hands and resist with all his might, while I

held one arm, and his brother held his feet. "It's coming," said he, "I can't help it," and presently he threw us both off like children, and after a few gyrations in the bed, again settled down. During this period his pulse was slightly accelerated, his countenance was calm and his conversation rational. His tongue was furred, and his bowels had not been acted on for three days. I ordered him a large dose of cal. and jalap, and 10 gr. of assafetida every four hours.

After the operation of the purge and the continuance of the assafetida for forty-eight hours, he was well, and has been free from fits ever since, now more than three years—is an active business man, and has the countenance of intelligence and health.

What was the cause of his fits, and how was he cured?

I offer the following suggestion by way of explanation. From some unknown cause the circulation through the brain, if not obstructed had become generally sluggish, and being still more retarded during sleep, brought on that state of syncope which frequently precedes actual convulsions. This functional derangement and torpor of the brain continuing and increasing, brought on the frequent and violent fits and almost idiocy. Still there was no organic lesion, and when the mind was roused and highly excited with the confident hope of cure by mesmerism, the natural circulation was restored and for a short time carried beyond the healthy standard, producing those uncontrollable muscular spasms. Finally it settled down to the proper healthy condition, and the fits ceased to recur. The tone of the brain has since been preserved by action and attention to business. Mesmerism derives no support from this case.

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ART. VII.—*Obstetrical Cases*.—By WILLIAM M. HUDSON, M. D., of Chester, Meigs Co., Ohio.

Dr. I. HAYS.—Sir: In the course of a somewhat extended medical and surgical practice of upwards of thirty years, it has been my fortune to meet with a number of unusual cases. I take the liberty of troubling you with the description of one or two cases that have occurred to me in my obstetric practice, and which were to me quite interesting at the time of their occurrence. I shall merely state facts without further remarks, all of which you are at liberty to make just such disposition as your good judgment may direct.

CASE I. *Labour, prolapsus of the bladder*.—May 28th, 1825, was called in great haste to attend a lady (Mrs. T—n), a few miles in the country, (I was then residing in Freeport, Harrison Co., O.,) who was then in her

ninth, and I believe her last labour. I found the case making good progress, pains frequent and in full force; the liquor amnii had been spontaneously evacuated just before my arrival; but proceeding immediately to my duty, I discovered a membranous tumour outside of the vulva, containing, I should judge, four or five ounces of a very limpid fluid. At first, I supposed it to be a sacculated portion of the amnion, containing its peculiar liquor, but upon a more careful investigation, found it to be the fundus of the protruded urinary bladder partially filled with urine; it had found its way through a laceration in the anterior part of the vagina, and thus was quite external to the labia majora. I confess I was somewhat alarmed, and used the utmost caution as the labour advanced, to prevent any unnecessary injury of the protruded bladder. Other than this accident, the labour terminated naturally and in reasonable time. Immediately after delivery I proceeded carefully to return the urinary cyst, with its contents, through the vaginal fissure (which was longitudinal, and I suppose extended nearly two-thirds the length of the vagina), to its natural situation. This I accomplished without any difficulty, and it seemed to retain its situation as readily as a button on its proper side of the button hole. The contents of the bladder were evacuated about two and a half hours after the lady was put to bed. When she was thus comfortably situated, (as I then supposed,) I gave her the first intimation of what I deemed her critical situation, but I found it was no new piece of intelligence to her. She informed me that this was the eighth successive labour in which the same accident had occurred, and which she attributed (with how much reason I cannot say), to mismanagement, and the "rough handling" of the practitioner who attended her in her first confinement. She apprehended no unpleasant consequences, having never experienced any excepting during parturition, and the rapidity of her puerperal convalescence, together with subsequent ability to perform all, and more than the usual duties of women of large families, in a new country, proved the justness of her expectations.

CASE II. *Labour—separation of bones of pelvis.* July 19th, 1836, was called on to attend Mrs. M——e, a woman of small stature, in her fifth accouchement. From her peculiar appearance one might suppose a plurality of fœtuses or an unusual quantity of the liquor amnii; neither however proved to be the case, though the former was intimated by some of the attendants, but the result was a single child of large size, if I recollect right, of ten or eleven pounds, with a well ossified cranium, born in a state of asphyxia, from which it was soon resuscitated, and did well. The membranes protruded and were ruptured, and the cervix uteri completely dilated before the engagement of the head, which presented with the occiput towards the left acetabulum of the mother. The pains had been severe for several hours previous to the evacuation of the liquor amnii, and shortly after, that occurrence came on with redoubled violence, during one of the most forcible of

which a sudden report was heard (proceeding manifestly from the pubic region of the mother), which almost started me from my seat. Some of the ladies present said it was as "loud as a pistol," though it had not a similar sound. The labour however progressed and was terminated in about an hour after this accident, but not without great suffering to the patient in the pubic and sacro-iliac regions, aside from the pains of expulsion, and which distress did not subside when the delivery was completed, and it was found that she was utterly unable to stand or to use the inferior extremities. Upon examination (a painful one, to be sure) the ossa pubes were found to move readily upon each other, and there was a general mobility of the bones of the pelvis, as well at the sacro-iliac junctions as at the symphysis pubis. I had her placed carefully in bed upon her *back*, in which position she generally lay for a number of weeks, for she could not turn upon her side without assistance. I had a roller about four inches wide and six or eight yards in length, passed round the pelvis, well secured and re-applied as often as it became loose or displaced. Pursued an antiphlogistic course to prevent inflammation, which however did supervene to such an extent as to render catheterisms necessary. With such a course of treatment she was able to walk a little in the course of six or seven weeks, (the pelvis being *well* bandaged,) but generally kept her bed for nearly three months (doing such light work as knitting, &c.), and found the pelvis strengthened by wearing the bandage, (which she learned very well to apply herself,) for a much longer time. Eventually the pelvic junctions obtained nearly their former strength; at any rate I attended her in her next confinement, about two years afterwards, viz., on the 17th July, 1838, at which time she had comparatively an easy parturient season, being delivered of a child of about seven and a half or eight pounds, and from which accouchement she had a very rapid convalescence.

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ART. VIII.—*Cases illustrating the use of Sanicula Marilandica in Chorea.*

By J. B. ZABRISKII, M. D., Physician to the King's Co. Almshouse.

THE publication of the following cases of chorea, which were treated successfully by the use of the *Sanicula Marilandica*, may lead to the trial of the virtues of this plant on a more extensive scale by other practitioners. The *Sanicula Marilandica* is an umbelliferous plant growing in marshes, or low and shaded ground, from two to three feet in height, and has its gashed, oblong and digitate leaves growing around the stalk; the fertile flowers are sessile, subternate, the others are barren, pedicelled and most numerous. The root is fibrous, aromatic, and possesses the active matter of the plant. Diluted alcohol extracts the active principle in some degree, but the best

form of administering it is in the powder of the dried root. It is highly tonic, and is used in popular practice as a favourite remedy in intermittent fever.

CASE I. C. V. S., a girl of eight years of age, of fair skin, light hair and eyes, lively disposition, was attacked in May, 1837, with involuntary movements of different parts of her body. Her hands were constantly in motion; she had twitchings of the face, spasmodic winking of the eyes, and agitation of the whole of her body. All these symptoms gradually increased; her walk became very awkward; the spasmodic twitchings of the muscles of the face increased in violence and frequency; the motions of her hands were irregular and appeared to be involuntary, and her whole frame was greatly agitated. The spasmodic action was greatest in the arm and leg of the left side, although it extended to the right. As she was pale, lifeless, with tumid abdomen and constipated bowels, I suspected the irritation of worms, and administered some brisk cathartics, followed by anthelmintics, but without effect. These were followed by tonics and anti-spasmodics, but she still grew worse. The root of the *cimicifuga racemosa* was then given as recommended by Dr. Young in the ninth volume of this journal, but this was also inert. I then determined to try the effect of the *sanicula*, which is extensively used in popular practice under the name of black snake-root. Having procured a quantity of this, it was administered in doses of half a drachm of the powdered root three times a day mixed with water. The effect was well marked; the progress of the disease was at once arrested, although she had been getting worse until this day. In three days a very sensible improvement was perceived and in two weeks the disease was entirely removed.

CASE II. E. B., aged ten, of delicate frame and sanguineo-nervous temperament, was afflicted with the symptoms of this distressing disease in the autumn of 1837. After using cathartics, opiates and tonics without success, she was put upon the use of the *sanicula*. Not having any of the root dried, some of the fresh root was made into a syrup by boiling in water. Under the use of this, the increase of the disease was checked, but she did not improve. After using this preparation for a week without any satisfactory benefit, the powdered root was given with immediate effect. She improved from that time, and in a short period was entirely cured. This shows that boiling water does not extract the greater part of the active principle of this plant though it may dissolve a small portion, as the syrup checked the onward progress of the disease and made it stationary, but did not relieve, while the effect from the powdered root was immediate. The active principle appears to reside in an essential oil, which was probably dissipated by the heat of the boiling water.

CASE III. M. B., aged sixteen, of dark complexion, hair and eyes, very lively in disposition and constantly cheerful, who had previously enjoyed good health, felt in March, 1840, some of the symptoms of chorea, which she found to be gradually increasing. She had spasmodic motions of the face and eyes, a great disposition to involuntary motions of the hands, although she could control them by making a strong effort of the will; her mind was easily excited, irritable, and nervous; she was very restless and in constant agitation. The menses were not disturbed. After a brisk cathartic

she was put at once upon pulv. rad. saniculæ, which immediately prevented all further progress, and in a short time effected a cure. This was evidently a case of chorea in its incipient stages, and was arrested by a timely application of the sanicula.

CASE IV.—M. S., aged eleven, had for nearly a year been subject to a severe attack of chorea. The spasmodic actions were very severe and constant, jerking of the hands very great, frequently so much so as to prevent her from putting a cup to her mouth; at times the convulsive actions of her limbs were so great that she could not walk, at other times she would be better, the symptoms remitting in violence. She had been subjected to a great variety of treatment by her family physician and several other distinguished medical gentlemen; all the most powerful means which are commonly used in this disease had been tried in vain. Being consulted in this case I gave her a quantity of the pulv. saniculæ to be administered in doses of a teaspoonful three times a day. This produced almost immediate relief, and in a short time caused all the most severe symptoms to disappear.

Some remains of this disease continued, however, for a time, although the severity was subdued, which were however gradually dissipated under a tonic plan of treatment.

FLATBUSH, July 25th, 1846.

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ART. IX.—*Intestinal Obstruction from novel causes*.—By J. F. PEEBLES, M. D., Petersburg, Va.

CASE I. Mrs. Hanbury, the wife of a small shopkeeper, having died under circumstances originating a suspicion of poison, I was requested by the coroner to make a post-mortem examination of her case. The history of her illness, so far as I could learn, was as follows. Soon after an altercation with her husband, during which a struggle ensued, and she was known to have used the most violent exertion, she complained of pain in the pelvic region, which increased and continued during the whole of her sickness. Her disease was most accurately diagnosed by the attending physician, Dr. Cox of this town,—who saw her perhaps the day succeeding her attack—as acute enteritis.

The most judicious and active treatment was employed, as local and general depletion, &c., without relief, and she sunk on the fourth or fifth day with many of the symptoms of strangulated hernia. The deceased was a finely-formed athletic woman, apparently about thirty years old. She had borne several children, and at the time of her death was but two months out of a confinement. On exposing the abdominal cavity, a quantity of pus mixed with flaky serum, was observed, floating among the intestines. Everywhere were seen traces of the highest degree of inflammatory action. The attempt, in carrying on the examination, to raise the small intestine, disclosed that it was firmly adherent, low down in the pelvis; and a further exposition of this cavity at once fully brought to light the true cause of all the mischief. The uterus, somewhat larger than natural, and firmly attached in its location, was seen protruding across the superior strait, to within an inch of the promontory of the sacrum; and between its fundus and that process two portions of the small intestine, the one entering into, and the

other returning from the pelvic cavity were firmly held, thereby inducing a complete obstruction of the canal. The greater extent of the congestion at this point, clearly indicated it as the focus of all the inflammatory action; adhesive inflammation had enveloped the intestines with plastic matter, firmly uniting the ascending and descending portions to each other. Though small, this aperture, through which a fold of the intestine had been forced, was not of itself sufficient evidently to bring on such complete strangulation, and to fix the parts, although neither adherent to the sacrum nor fundus uteri, so firmly in their location; there was obviously space enough to allow the free passage of a fold of empty intestine: the cause of this result, however, became very evident upon a dislodgment of the mass, a process which it required some force to effect. For that part of the intestine below the constricted portion, and where it was folded upon itself being free, had become disturbed partly by effusion into the canal, presenting a sac or pouch firm to the touch, and of a size amply sufficient to present a physical obstacle to the return of the gut to its natural position, thus at once accounting for the very firm manner with which the knuckle was held in situ.

Besides serum tinged with the excrement, this pouch or sac was found upon examination to contain several pieces of hardened feces. As before remarked, the two portions of the intestine were greatly congested, and after their removal from the body it was found that they had become firmly adherent for the space of three inches.

The phenomena attendant upon the history of this case admit of easy solution. It is plain that, during the struggle before mentioned, which this woman had with her husband, this knuckle or fold of small intestine was forced into its novel location. Retained there, owing to partial constriction, the sac or enlargement, ultimately preventing the return of the fold to its natural position, and which therefore rendered the result so serious, was then almost immediately formed, simply by the natural gravitation of the contents of both the ascending and descending portions of the canal, to this most pendent point; and finally the adhesion of the walls of the intestine was a most beautiful natural process, it being a step preparatory towards overcoming this obstruction in the digestive canal, by opening a communication between its two sides above the stricture.

CASE II.—The subject of this case was a negro boy, apparently thirteen or fourteen years of age, who, about a week before his death, was seized with an anginose affection, prevalent at that time, accompanied with some threatening head symptoms. He was visited by Dr. White, who treated the case actively by emetics, purgatives and local depletion. On the fourth day the symptoms all gave way; appetite returned; the boy was cheerful and free from fever, and Dr. W., having no hesitation in pronouncing him convalescent, therefore dismissed the case. A few hours afterwards, however, the patient was seized with severe tormina, followed by a return of fever and great tenderness over the umbilical region. Despite the most judicious and active treatment, tympanitis came on, and the case immediately assumed the gravest possible aspect. Thirty-six hours after the relapse I saw the patient in consultation. He was then dying with the symptoms of acute enteric inflammation, and survived but a few hours.

The sudden apparent translation of the disease, in this case, from the head to the bowels, and the rapid and altogether uncontrollable nature of the latter affection, led us to feel some curiosity in regard to it, and an examination was made after death. The abdomen was excessively tympa-

nitic, and its cavity when exposed was filled with serum tinged with pus. The intestines and their appendages were found deeply injected. The starting point of the inflammatory action was easily traced to the cæcum, near its beginning, where, upon a closer examination, a very curious malformation was discovered, which it cannot be doubted led to the fatal result. It was this: one of the sacs of the cæcum, created by the longitudinal and transverse bands, instead of presenting outwardly, was formed inside the intestinal cavity, creating a deep concave surface externally and a convex one internally. Looking at the intestine a deep pit was observed partially closed in by the two lateral or transverse bands, marking the spot outside where the deformity existed.

This malformed sac had become incarcerated completely by the firm bands bounding its four sides, and thus strictured it was blackened and sphacelated, and so much thickened as almost entirely to plug up the bowel. Some idea may be formed of the extent of this thickening, when it is asserted that after a removal of the diseased part from the body it was found impossible to push it outwardly through the constricting bands, so as to make it assume a natural position. Nothing can be easier than the explanation thus afforded to the phenomena attendant upon this case,—the sudden intervention of new and altogether different symptoms after the complete subsidence of those which were first presented. This malposed sac, as we have seen, ordinarily created a partial obstruction in the cæcal canal, and therefore was peculiarly obnoxious to disease. It cannot be doubted that the purgatives which were used had irritated this projecting surface; and irritation, once set up in such a part, it is obvious could not occur without the most serious consequences, since its slightest enlargement tended to a strangulation, by the bounding firm bands of the whole sac; which, when once strangulated, as the enlargement consequent upon that result directly tended to increase the constriction, could only progress to sloughing and, through it, to a fatal result.

ART. X.—*Case of Varicose Aneurism, successfully treated by Pressure.*  
By Wm. JOHNSTON, M. D., of White-house, N. J.

JACOB APGAR, a blacksmith aged 78, consulted me on the 4th of March last on account of an injury received in blood-letting about four weeks previously. Upon uncovering his arm I discovered an aneurismal varix about the size of a walnut, produced by a wound of the brachial artery. Its pulsation was synchronous with the artery at the wrist: There had been very extensive effusion of blood in the cellular membrane for several inches, both up and down the arm; but this had disappeared after a few days, and at the time I now examined the arm there was no discoloration of the tumour, which presented a slight irregularity over its surface. Its pulsations were so strong as to raise the fingers with a bound, when pressed firmly upon it; indeed, the artery at the wrist pulsated with unusual force. The impulse of arterial blood into the aneurismal sac immediately under the cicatrice in the vein was very striking, and left no manner of doubt as to the real nature of the case. He complained of no pain upon handling the tumour. His general health was good, though his habits had been rather intemperate. He stated to me that there had been some difficulty



in stopping the bleeding, and that when his attention was first directed to the tumour it was about the size of a hazelnut.

I was much struck in reading the relation of several cases of spontaneous aneurism successfully treated by pressure, and resolved to give the plan a trial. I had an arched tourniquet constructed so as to touch the arm, only at two opposite points. It was made of steel. At one end there were a couple of holes for screwing on a piece of board of three or four inches in diameter, which was well padded, and at the other end a long screw, which was attached to a similar board, on the back of which was a female screw. The boards were suited to the convexity of the arm. The apparatus was placed upon the most convenient part of the humerus to command the artery. By means of the screw I could regulate the pressure upon the artery so as instantly to stop the circulation in it, or merely weaken its force. The first application of the instrument, which was continued less than two hours, completely destroyed the *aneurismal bruit*; I could not detect it afterwards. The circulation through the artery was completely interrupted about one-third of the time in which the instrument was applied. *My principal object, however, was merely to weaken greatly the force of the circulation in the artery of the arm, and thus to produce a state favourable to the coagulation of blood in the aneurismal tumour, and consequent deposition of fibrine.* The instrument was applied again on the next day for the space of less than two hours. Upon the following morning I found that all pulsation had ceased in the tumour, and the most careful examination could detect it in no part thereof. It never returned. The instrument was applied on the six following days and on every alternate day until the 23d of the month, when it was laid aside, and the patient advised to keep his arm at rest in a sling. The tumour became harder; almost of a cartilaginous feel, but slowly decreased in size, and at the time of writing this, June 26, (3 mo. 22 days,) is about as large as a split pea, and in all probability at the end of a few months more there will be no vestige of disease remaining. He has the free and perfect use of his arm. The instrument was not left on the arm after the first two applications longer than one hour at a time, and in reflecting upon the case I am satisfied that it would have terminated favourably had the instrument been applied but twice. Coagulation of blood in the sac, and deposition of fibrine and interruption to the ingress of arterial blood were in all probability effected, and nothing was wanting but patience to wait the progress of absorption.

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ART. XI.—*Wound of Abdomen, Expulsion of Intestines, and Cure.* By  
J. GIBBS, M. D., Claiborne, La.

A LABOURER, 25 years of age, in a rencounter, on 9th Feb. 1846, received, with a dirk knife, a cut commencing opposite and two inches to the right of the navel, extending outwards and upwards, to the length of one inch and a half. The struggle did not cease until the jejunum, ileum and colon, together with the omentum, were expelled with serious lacerations to the latter. An incision of half an inch through the peritoneal coat of the small intestine was the only observable injury to the gut.

Two or three hours had elapsed before medical aid could be obtained, and now the patient lay prostrate and exhausted. The skin cold and

moist, pulse feeble, nausea and ineffectual efforts to vomit, on the least motion. The gut was quite cold to the hand, and the large portion had assumed a dark purplish hue, and was badly swollen.

Two small mangled portions of epiploon were removed with the scalpel: then by patient manipulation, the small gut was returned, securing it with the ulnar fingers of the left, pressing inwards with the thumb and index of the same, assisted by the right hand and thus staying it, whilst the first hold was being renewed.

The strangulation and tenderness of the colon required the incision to be enlarged some five or six lines before its reduction.

During the reduction, hemorrhage was profuse, and the patient suffered intensely, yet as no artery was discoverable, and the parts being brought in apposition, the interrupted suture, including seven or eight lines on either side, and the entire abdominal wall, save the peritoneum, with a pledget of lint, straps of plaster and bandage completed the dressing.

Quietude, horizontal position and farinaceous diet, being strictly enjoined, together with directions for giving an occasional anodyne and the mildest aperients, the patient was left quite disposed to sleep.

On the 11th the patient had lost blood occasionally for the first twenty-four hours, from the wound, on turning or exerting the abdominal muscles. Stools small and in slugs. Had slept. Skin and appetite improved. Pulse 74 and full.

Vs. to extent of  $\text{§xxv}$ .—(pulse rose to 100;) hydrarg. chlo. m. gr. iij, and repeated, constituted additional treatment.

On 25th, patient walking about, without having had scarcely an unfavourable symptom. The sutures, which now formed the only apparent injury, were removed and patient discharged.

In the above case, we have additional evidence of the passiveness of the stomach in vomiting, and the fallacy of dispensing with sutures in similar cases.

It should be remarked that the sutures used were large, and barely brought the lips in contact, allowing liberally for swelling and the exit of blood, and that, instead of aggravating, they appeared to perform the office of a salutary revulsive, obviating the wonted tumefaction in the lips of the wound, and the irritation, in the adjacent peritoneum, consequent on so much handling.

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ART. XII.—*Case of Death from Inhaling Chemical Fumes in a Sulphuric Acid Chamber.* By EDWARD A. MANS, M. D., of Baltimore, Md.

RICHARD FORD, negro, æt. 34, of good muscular development, healthy constitution and temperate habits, was at work, about two hours at noon, on Saturday, 2d inst., in a sulphuric acid chamber, at Chappell's factory. There was probably a large amount of nitrous acid gas in the chamber. While breathing it he did not complain of any ill effect, and neglected to go to the door for purer air, as often as his fellow-workmen, who were occupied in the chamber with him. In the evening he complained of feeling unwell, and had proceeded some twenty yards from home, on a visit to

his mother, about 10 o'clock, P. M., when he experienced vertigo and fell down. He was next affected with nausea, great prostration, and diaphoresis. I was sent for shortly afterwards, and found the patient in bed, with an air of sufferance, distortion of countenance, petulance of temper, jactitation; no disturbance of mind; power of deglutition perfect; tongue natural; irritation of the trachea, with a hacking cough and inclination to expectorate; dyspnœa; extensive mucous rale of both lungs; præcordial oppression; pulse full and bounding; no pain upon pressure over the stomach, nor any symptoms whatever of abdominal distress; temperature of the surface moderate; diaphoresis.

*Treatment.*—Venesection. ad  $\xi$ xii; epispastic to the chest; diluent drinks; slight alcoholic stimulus and a mixture of syrup. Tolu. et scillæ. c. gum ammoniæ.

The patient expressed himself better, and I left him, as I thought, much relieved. I received an urgent message, at 5½ A. M., to visit him, and upon arriving, was informed by his wife that he had remained comfortable until 4 o'clock, when a most decided change for the worse took place: that his mind then wandered, that he became exceedingly stupid, and indisposed to reply to any question that was put to him. The aspect of the case, at this time, was truly ominous of the result. The eyes were closed and dull, pupils contracted, although somewhat sensible to light; consciousness nearly extinct; he could be aroused to take drink, and would mutter words altogether unintelligible; temperature equable, skin perspiratory, pulse but little affected. At 7 o'clock he died. An *autopsy* was made at 4½ P. M., ten hours after death. Present Drs. Mans, Fonerdon, Theobald, W. H. Davis, C. Johnston and C. Frick. Dissection by Dr. Frick. Report by Dr. Johnston. The post-mortem appearances were as follows:—The mucous membrane of the pharynx and œsophagus normal; that lining the trachea, however, from the epiglottis downward and also through the bronchial divisions, slightly injected. The left bronchus much less injected than the other. The lungs, on opening the chest, did not collapse, and were greatly distended. There were ancient adhesions in several places to the parietes of the thorax, and the right lung was particularly attached. Upon being cut into, a great quantity of frothy serum exuded from the bronchial branches; clotted blood was readily pressed from the vessels; they appeared firmer than natural, and a feeling of elastic resistance was universal. Their colour was purplish, and although very slightly crepitant, they still floated in water. The mucous membrane of the trachea, where most injected, was raised in small strips only. The *pericardium* contained an ounce of fluid. *Heart* large, but in proportion to the great muscular development of the subject. Its *cavities* and *veins*, the *cavæ*, *aorta* and *veins* of the *neck*, more or less distended with black clotted blood; valves normal. *Liver* of a very dark hue, but otherwise healthy. *Stomach*. Mucous membrane thickened, softened, and of a notable red colour. The

cardiac portion especially very much congested in patches, interspersed with numerous bright red spots. The veins of the great *omentum* much distended with clotted blood. In the *small intestines*, the patches of Peyer and isolated follicles generally evident, but not reddened; occasional *plaques* of arborescent injection were discovered, but were not peculiar to any portion of the mucous membrane. A moderate quantity of citrine-coloured fecal matter in the tract of the lower small intestine.

There was some diversity of opinion as to the manner in which the deleterious effect of the gas proved fatal. Some referring it to the pulmonic lesions, and through them to the injurious action of an imperfectly aërated blood upon the cerebral mass, while another was disposed to think that a chemical theory could most satisfactorily explain the result, viz. the absorption of the gas through the cutaneous capillaries, the consequent changes wrought in the blood, and the noxious effects which such a change must of necessity produce. The intensity of the gastritis attracted the attention of all present, and was not satisfactorily accounted for by any of the known causes of that disease. Its presence seems to have been noted in the few cases reported upon this subject, and was adduced as an argument particularly substantiating the chemical hypothesis to which I have referred. This autopsy throughout was deeply absorbing, not merely from its novelty, but from the beautiful exhibition of post-mortem phenomena with which it presented us.

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ART. XIII.—*On a Remedy (the Ambrosia Trifida) for Mercurial Salivation.* By WM. ROBERTSON, M. D., of Harrodsburg, Ky.

ONE of the most common plants on our farms possesses, as I have discovered, more prompt and efficacious remedial powers in the cure of mercurial salivation than any article I have ever seen tried for that loathsome disease. During a practice of forty years I have seen the disease in all its forms, and various remedies employed for it, but do not recollect to have ever witnessed an obvious curative influence exercised by any of them.

The remedy I have lately adopted, in every case in which I have tried it, has proved a speedy and effective cure, relieving the patients in from six to eight hours of all the most distressing symptoms, and within twenty-four or forty-eight hours, removing every symptom of salivation. However, I would observe, that all these cases have been of a mild character or in the incipient stages. What influence this remedy would exert in those violent cases of the disease, occasionally met with in practice, attended with extensive swelling, ulceration, sloughing, and falling out of the teeth, I am unable to say, having met with no such case since my adoption of the

article, but I think it probable that such a case would call for the use of other remedies. Nevertheless the use of this remedy in the commencement of such cases, would very probably arrest their progress and prevent their attaining an aggravated form. In this view I am sustained by the result of a case that came under my notice within the last month. In this case the power and influence of this medicine to control mercurial salivation, were most strikingly exemplified. It was that of a female, aged thirty-five, in the eighth month of her pregnancy, of delicate frame and phlegmatic temperament and predisposed to hysteria. She was advised, for habitual costiveness and torpid liver, to take one or two doses of calomel, milder purgatives having procured only momentary relief. The calomel was retained about thirty hours, although followed by a large dose of castor oil in ten or twelve hours. The consequence was, a violent attack of mercurial salivation. Within twenty-four hours from the attack, some unusual symptoms having manifested themselves, the family became alarmed, and I was hastily called to visit her five miles in the country. The bowels having been evacuated by injections, I found the patient without fever, and only complaining of the salivation. The gums and mucous membrane of the mouth were inflamed, a little swelled, and had a soft puffy appearance; the whole surface was coated with thick viscid mucus, adhering with unusual firmness, and so offensive in smell and taste to the patient, that every effort to discharge it was attended with nausea and vomiting; a putrid effluvia was exhaled with every breath, along with the mercurial fetor, perceptible and offensive to the bystanders. It was this symptom that had alarmed the family; they concluded that mortification had already taken place. All perception of taste had ceased, and food and drinks were rejected with disgust. The putrid smell, perceptible in the breath, evidently proceeded from the viscid mucus adhering to the mouth and throat, acquiring a putrescent tendency from being detained there long after the secretion was thrown out from the secreting glands, &c. This was proved by an examination of the secretion; when discharged (as it was with great effort) into some vessel, the same putrid smell was present, and the mucus was about the consistence of the white of an egg.

This case of pure mercurial salivation—I say pure, because this disease is very generally accompanied by other diseased conditions of the system—afforded me the best opportunity I had seen of testing the powers of the remedy. I immediately procured from an adjoining field, a large handful of the green leaves; poured on them in a suitable vessel one quart of boiling water: as soon as it was cooled sufficiently, the patient was directed to wash the mouth and throat freely every half hour; nothing else was used except the common soda powders; they were given every three hours in an effervescent state.

I remained with the patient six hours. By that time the mouth and throat were cleared of the thick viscid mucus; the nausea and vomiting

had ceased entirely; the natural taste was nearly restored; the patient felt greatly relieved, and partook of some light food with relish. The next day she was still improving and comfortable, and on the third day, within forty-eight hours from the time of commencing the use of the remedy, every symptom of salivation was removed, and the female was engaged in her usual domestic avocations.

I will give another case which occurred within the last two weeks, because there is a fact connected with it, giving rise to an opinion that the remedy may prove beneficial to inflammation in mucous membranes, arising from other causes than mercury. A gentleman, from bathing in a river, took cold. He called on me, complaining of headache, sore throat, a stiff neck. He was bled; some active cathartic pills, containing a small quantity of calomel, were given, with directions to use them so as to keep the bowels in a solvent condition; to use a light diet, and apply vol. liniment to the throat. Three days afterwards he called on me to inform me that the pills, as used, had not been active enough, and that he was salivated; the sore throat still continued without abatement.

I gave him a handful of the fresh leaves, and directed him how to use the infusion. He afterwards informed me that twenty-four hours' use of the remedy removed every symptom of salivation, and that the sore throat had also been cured. He further informed me that at the time he received the remedy, he felt so badly about the mouth and throat, that he did not expect he would be able to preach for a week, (he is a minister of the Gospel,) but that after using the remedy he found himself as able to preach at the end of two days as ever he had felt in his life.

May not this remedy prove beneficial as a local application in leucorrhœa, prolapsus uteri, and gonorrhœa, also in various affections of the throat? I shall certainly in future extend its use to diseases of this character, and I hope that practitioners of medicine, especially those residing in districts where the plant abounds, may be induced to give it a trial, and report to the profession the result of their practice.

This plant is found in all parts of Kentucky, and is known to all our farmers under the popular names of horseweed, richweed, horsemint, and horsecane, but is an entirely different plant from that described in the appendix to the fourth edition of *Wood and Bache's Dispensatory*, at page 1137, under the title of *Collinsonia Canadensis*, and vulgarly known by names similar to those applied to the Kentucky plant.

I was induced to make a trial of this plant in mercurial salivation, from the fact that this plant, when given to a horse affected with a disease called slabbering, effects a complete cure of the disease in a few hours.

This salivation or slabbering disease in the horse, doubtless proceeds from some diseased condition of the salivary glands. About two years ago, passing a field where the plant was abundant, its effect on the salivated horse occurred to my mind, and immediately a question suggested itself—that if

this remedy can exert so speedy and such surprising effects on the salivary glands of the horse, may it not possess properties that would render it useful and beneficial in salivation in the human subject? Under this impression I resolved on a trial of its powers in the first case that should present itself. The trial convinced me that it possessed powers for relieving and curing mercurial salivation, greatly surpassing any means I had hitherto used, and subsequent experience has firmly established that conviction.

The effects produced by the local application of the infusion, in the human subject, induces me to think that the effect it produces on the horse, does not arise from the plant taken into the stomach, and reaching the diseased glands through the medium of the circulation, but that the direct application of the juice of the plant while the horse is chewing it, effects the cure. It has so happened that all the cases in which I have had occasion to use the remedy, have occurred during the spring, summer, or fall, when the plants are in a green state. I have the dried leaves, but have never used them; whether the leaves lose any of their virtues by drying, I am unable to say. I have never heard of the plant being used in any shape as a medicine until I tried it as a remedy for salivation.

[Dr. Robertson was polite enough to send us, with the above communication, some dried specimens of the above plant, which we submitted to our friend Dr. R. E. Griffith, an able botanist, from whom we have received the following note.

Dr. Hays.—Dear Sir. The plant you left with me appears to be *Ambrosia Trifida*, though from the absence of flowers or fruit, it is difficult to decide with absolute certainty; at the same time the characters of the leaves and stem are so striking as to leave little doubt on the subject.

Torrey and Gray. (*Flor. Nor. Amer.*, ii. 290) describe it as follows: "Stem tall and stout, hairy, rough; leaves scabrous and hairy, deeply three-lobed; the lobes oval, lanceolate, acuminate, serrate; the lower leaves often five-lobed; petioles narrowly winged, ciliate, racemes often paniculate; fruit (fertile involucre) turbinate-obovoid, with a short conical pointed apex, six ribbed, the ribs terminating in as many cristate tubercles.

"Low grounds and along streams, Canada to Georgia and west to Louisiana and Arkansas. Aug.—Sept. annual."

It is also noticed by Riddell (*Synop. Flor. West. States*, No. 1014), as everywhere abundant: he gives the vulgar name of bitter-weed to it. Rafinesque (*Med. Flor.*, ii. 190) speaks of it and says that it is called horse-weed, one of the names given by Dr. Robertson, and states that the species of ambrosia are antiseptic.

The *A. trifida* has not, as far as I can ascertain, been employed as a remedial agent, though some of the other species have been used with some success as febrifuges. Should the present plant, on a more extended trial, be found to be as successful in cases of mercurial salivation, as is shown by Dr. Robertson, it will be a very important addition to the materia medica.

It is to be found in abundance in the vicinity of Philadelphia. It is probable that the *A. elatior*, or ragweed so common in all our fields, would prove still more efficacious, as its sensible properties are much more developed than in the present plant.

Yours, &c., R. E. GRIFFITH, M. D.]

ART. XIV.—*Case of Cæsarian Section*. By BRODIE S. HERNDON, M. D.,  
of Fredericksburg, Va.

MRS. RUSSEL, aged about 30, moderately robust, the mother of several children, fell in labour on the evening of the 1st October, 1845. She quickened early in May, and reckoned herself at her full time. In the course of the night the pains which had been paroxysmal became permanent, and there supervened tenderness to pressure, vomiting and high vascular disturbance. The labour gave place to peritonitis. The duration of the phlegmasia was a month, during which time the fluid contents of the uterus escaped, the abdomen shrunk, and the vaginal discharge became very offensive. On the 6th of November, the os uteri admitting two fingers, I made persevering efforts to dilate it and introduce the hand; the cranial bones of the child denuded of scalp, rough and sharp, rested on the uterine aperture. These attempts failing, ergot was freely given, which occasioned unequivocal pains, and procured the expulsion of a very putrid placenta. The patient continued to waste under irritating fever; the discharge from the uterus filling the room with stench. On the 16th of November, it being now manifest that the woman must die soon without relief, I determined on the Cæsarian section. She was placed on the table and the catheter introduced: an assistant gathering up transversely a large fold of the abdominal integuments. I thrust a bistoury through them, and, cutting outwards, made at one stroke an incision of the full length required. With the scalpel, division was now cautiously made at the upper point of the wound of all the tissues, including the uterus, which was found adherent to the parietes. The finger being introduced, the incisions were extended upon it, laying open the parts from the umbilicus to the pubis. A full-sized putrid child was readily extracted. The operation lasted half an hour, and was performed in the presence of Drs. Brown, Wallace, Willford, and M'Guire. Only one artery was tied, and that was in the integuments; there were no utero-placental vessels; the uterus did not contract in the least, but remained a large flaccid sac, receiving the impression of the liver at its upper part, this last organ being somewhat enlarged. It was sponged out, and then the integuments were brought together with stitches and strips. The patient took an anodyne, and a few hours after



the operation expressed herself as feeling much more comfortable. With morphia at night, and quinine and nourishment in the day, the case did very well. The wound united by adhesions in nearly its whole extent. The woman is now—three months since the operation—quite well.

The obstetric student will find in Dr. Davis' great work, some interesting cases of gestation, in which the instinct of parturient expulsion was lost, and children were borne in utero for years, undergoing decomposition and being discharged by piecemeal. In Mrs. Russel's case, the operation was simplified, and the danger usually due to hysterotomy materially lessened by the adhesions of the uterus to the abdominal parietes. Extensive wounds of the abdomen seem often to be readily recovered from. A case occurred to Dr. Carmichael (now of Richmond), and Dr. Welford, in which a lad was ripped up and completely disemboweled by a boar, who, nevertheless, did very well; nay, even wounds involving the viscera may end favourably, as in the case of the lunatic related by Dr. Brigham, where a woman plunged a pair of scissors into her belly, tore out seventeen inches of bowel and cut it off, and yet recovered. In 1843, I performed the operation for inguinal hernia on a young man who had suffered strangulation a week. The gut was mortified, but all the symptoms were relieved. dejections were procured through the wound, and the patient recovered. There was artificial anus for a time, and, on one occasion, the bowel was prolapsed five or six inches through it, occasioning much difficulty in the reduction; but by the use of a truss, a recurrence of the accident was prevented, and defecation per anum became gradually established.

It must be admitted that these are fortunate cases, exemplifying a part of the aphorism "there are some you cannot kill," whilst we full often find in our experience abundant verification of its alternate, "there are others you may not touch."

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ART. XV.—*Case of Labour complicated with Prolapsus Uteri.* By AUGUSTUS K. GARDINER, M. D.; Memb. of Massachusetts Med. Soc.; Physician to N. York City Dispensary; District Physician to N. York Asylum of Lying-in Women.

I was called, August 11th, at 4 P. M., to Mrs. Potter, aged 35, in labour, at her full time. The water had passed off early in the morning, and the pains had commenced within an hour. This was her fourth confinement. The children by the two first, healthy girls, now living, the youngest aged two and a half years. After this confinement she had slight prolapsus uteri. A year since she aborted at the fourth month, since which period the prolapsus has been much worse. Within the last four months the

uterus has descended so much, that from two to three inches have been always external. During the last two months she has been obliged to keep her bed most of the time, from the irritation and pain proceeding therefrom. She had never made any attempts to restore the parts to their natural locality.

When I arrived, from the movements and cries of the woman, I judged that the labour was far advanced, and proceeded immediately to make an examination. Placing the woman, for that purpose, upon the back, and passing the finger down, from the os pubis, it encountered a somewhat firm mass, which at first I supposed to be the œdematous labia. This struck me with surprise, as the extremities were not at all swelled. By the side of this supposed lip, the finger entered nearly its whole length, when it arrived at the bottom of the cavity without encountering any presentation. Still moving the finger where the resistance was least, I found that it could be passed entirely round, leaving the above-mentioned mass in the centre, which, with its edges swollen and turned over, bore a strong similarity to the nose of a demijohn, especially when upon further examination an orifice was discovered in its centre, sufficiently large to admit two fingers, and here the vertex was found already arrived in the hollow of the sacrum, and presenting in the position *occipito iliâque droite postérieure*, or the third position of Baudelocque.

What was this mass which I have described projecting from the vulva, and out of the body some two or three inches, firm, corrugated, dry? Evidently it was not any form of polypus, or morbid growth, nor a prolapse of the cord. And, therefore, it must be—what? The os uteri.

At this time, not to incur the responsibility of the diagnosis and treatment of a case almost unprecedented in the annals of obstetric science, I sent for a friend, but he was engaged, and I was forced to continue alone. Till 10 P. M., I limited my services to the application of cold cloths to the parts; not that there was much indication of their need, for the parts were cool, but in reality for the purpose of doing something.

During this period the pains were strong, not alternating with repose, but continuous, with exacerbations. The suffering was ascribed to the region immediately above the pubis, and at the small of the back, corresponding to the insertion of the broad ligaments.

Seeing little if any advance of the head, and but slight dilatation of the os during six hours, I proceeded to one of the most distinguished of this city's practitioners, and recounting the case, asked his advice. He gave but little credence to my improbable story, and advised delay. WARR, Blundell very frequently says, in capitals, and I followed their joint advice.

At 4 A. M., on the 12th, the woman, debilitated by several weeks' confinement to her bed, began to show signs of exhaustion, from the continuance of the pains, which did not allow her a moment's rest. Her pulse, previously 60, had risen to 70, and various symptoms indicated convul-

sions. Delay, I considered, would be no longer justifiable, and that the time for some active interference had arrived; and though alone and without counsel, I proceeded to apply the short forceps. The vertex remained as before described. The first blade was applied with much ease, but the other with more trouble; partly owing to the irritation caused by the rubbing of the first upon the sides of the uterus, and partly from the difficulty of inserting the blade through the narrow orifice. On making some traction, applying but little force, with the conjunction of a stronger pain than any before, the head, as if dislodged from its situation, came down, but bringing with it the os, or rather stretching out its corrugated folds, its orifice remaining hard, and unyielding as for hours previous. I found greater difficulty than I had anticipated in taking off and withdrawing the forceps, as they were so strongly bound by the pressure of the head upon the walls of the uterus.

At this period the parts were in a most extraordinary state. The projecting tumour, which exteriorly the vagina, and interiorly the uterus, was of the form of a cone, eight inches in length; five inches in diameter at the base, where it united with the body; and three inches in diameter at its apex. In the apex was the opening, two and a half inches in diameter, through which the hairy scalp of the fœtus, in *rugæ* with the pressure, was slightly projecting. What was now to be done?

I felt the pulse—72, full, soft. I sent for various physicians for counsel in this difficulty. After the lapse of three-quarters of an hour, I was happy to see Dr. Cheeseman enter, a gentleman well known to the profession by his great attainments, and to the public by his extensive practice. To him I explained the case, and proposed to make an incision through the neck in order to liberate the stricture. After some deliberation he acceded to my plan. Some little time was lost in obtaining bistouries. Passing my finger into the os and gliding the bistoury upon it, I transixed the part and made a cut of one and a half inches in length. This, however, not proving sufficient, with a probe-pointed bistoury I enlarged the incision an inch. Soon after, a renewal of the pain advanced the head, tearing up the incision an inch and a half, and a healthy girl soon looked us in the face, greeting us with more noisy exclamations than ever freed prisoner from the Bastille hailed his liberator.

The after-birth attached to the fundus was distinctly apparent above the pubis, and was delivered without difficulty or hemorrhage, after which the uterus remained external, but contracted to one-half its previous dimensions. The hemorrhage from the incision was now very slight, arising principally from its congested state. The os along its whole border, for the width of an inch, was indurated, and the integument thickened, and on its inner surface was changed in its character, so that it resembled cartilage—similar to the semi-cartilaginous patches sometimes seen upon the spleen in certain diseases.

At 6 A.M., I left the woman cleansed and comfortable. Skin moist; pulse 68, full, soft.

Since this period up to the present time, August 31st, the woman has continued to improve, though but slowly. The pulse has not risen above 84, always soft, and easily compressible. Appetite good. She has nursed her child regularly, and has been kept constantly upon the bed, rising only for the operations of nature. The uterus has been kept cool and moist with wet cloths, and with no other application has diminished in size; the cartilaginous portions around the border had ulcerated and sloughed off, and the incision granulated, till on the 24th, being about an inch in length, I attempted and easily succeeded in reducing the uterus—carrying it back to nearly its natural situation. On my next visit I found that upon rising to micturate it had again prolapsed. Since that period I have not been able to attempt the use of pessaries, which is my design as soon as the wound is entirely cicatrized.

The disease being now only a prolapse of the uterus, I have not thought it necessary to defer the publication of the case. Whenever the uterus prolapses, the woman reduces it herself; and using an injection of a decoction of white oak bark and alum, will, I hope, with a nutritious diet, so strengthen the parts, that nature may assist in performing a complete cure.

Before I had searched the various works on Midwifery, and the Diseases of Females; I supposed that this case was unique, and the treatment adopted on the occasion was that which my judgment indicated on the spot, unbiassed by precedent. Subsequent research has brought to my knowledge two cases which have great similarity to my own, and also has discovered two others (in one where the woman died, in the other no account given of the delivery), bearing upon it, so far as they prove the fact that a woman may become pregnant and carry her child to the full time, notwithstanding a prolapsus uteri to the extent mentioned. (*Diseases of Females. Prolaps. Uteri*, by Colombat and Ashwell.) But these are so slightly substantiated by facts that they would have little weight, were they not corroborated by the others.

The first of the two principal cases, though given upon the testimony of Marignés, a physician of Versailles, and reported by Chopart, (*Traité des Maladies de la Vessie*, voi. ii. p. 73,) and repeated by Richerand and Capuron, has the following endorsement by Prof. C. D. Meigs, of Philadelphia. "Such relations as the above require a stronger confirmation before they should be deemed credible. They are necessarily hypothetical as to the important steps of the doctrine." This case is to be found in Colombat's, Ashwell's, and Churchill's *Diseases of Females*.

The second, from the *American Journal of Med. Sciences*, is as follows: "Dr. Grhun, of Reppen, relates the case of a woman, æt. 28, who, in the fourth month of pregnancy, in consequence of a violent effort, had a pro-

lapse of the uterus; gestation, nevertheless, went on without any accident to the full time. When Dr. G. saw her, thirty-six hours had elapsed since labour had set in, and twenty-four since the waters had been discharged. The uterus hung between the patient's thighs. The vertex of the child presented, and the neck of the uterus was dilated to the size of a two franc piece. Not being able to obtain a greater dilatation, Dr. G. made an incision, one inch in length, in one side of the neck of the uterus, and a dead, but well-developed child was extracted. The delivery of the placenta was attended with very profuse hemorrhage, which was arrested by injections of cold water. Afterwards the uterus was reduced, and everything went on well."

Cases of prolapse during labour are sufficiently numerous, but the uterus being in its natural state, the os dilatable and not suffering from previous irritation, little if any additional difficulty is caused by this accident.

In all the cases reported before mine, where the os was indurated, *the child was born dead*; these could not, therefore, be called successful cases, and any line of practice deduced from them, must have been, however correct, on supposition. No mention is made in these reports of the time allowed to pass away in awaiting dilatation. Cazeaux, my former excellent instructor, on the strength of Chopart's case, in his valuable *Traité des Accouchemens*, p. 569, thus gives the following rules:—"During the labour, all attempts at reduction would be hazardous. We must be content in hastening, as much as possible, the dilatation of the neck and to prevent by proper incisions the ruptures of which it is in danger, in case it should be indurated.

"The extraction of the placenta requires great circumspection; it is easy to perceive that we should not trust to its expulsion by the operation of nature, and still less should one make traction by the cord, in the customary manner; it is, therefore, necessary to peel off the placenta artificially. Immediately after, the uterus contracts, and the reduction is oftentimes an easy matter."

In addition to this, I would urge the necessity of bringing the labour to a close as soon as possible, and not suffer the child to perish, as I fear was done in the two reported cases, by allowing the head to remain long in the lower strait, undergoing great pressure in that impacted situation. If the membranes are intact, they should be early ruptured, and the presenting portion brought down by the application of instruments, if necessary. This, most assuredly, should be done before making any incisions in the neck, waiting *a reasonable time* for dilatation.

Blundell (*Diseases of Women*, p. 43), says, "If the woman is at the end of pregnancy, or if the womb came into sight through the external parts, I suppose it would be your duty to dilate the os uteri with the fingers, and in this way accelerate the birth of the child as much as possible; but if it was down a little way merely, I should not meddle with it, but leave

the woman to her own resources. But if, in the latter months, the womb were lying externally and between the limbs, and it could not be put back, I should recommend the bringing on of delivery, by puncturing the membranes; and then, when parturition came on, I should as before assist in dilating the os uteri. In Hervey's case, it was proposed to extirpate the uterus; but I certainly prefer the induction of parturition before extirpation."

These remarks are evidently those of a theorist, and are applied to a case which he had never seen. If he had, what possible reason could he give for such practice? Would it not only destroy the child, but also the woman? Would not the hemorrhage be immense? Hervey's case I do not find, but I cannot conceive one which would require such treatment. This surely must be a vision of his younger days, which now, in the face of this instance, when the labour was allowed to go on to its full time, he must renounce for truth born in actual, not imaginary labour, and strengthened by success.

## REVIEWS.

ART. XVI.—*On Disorders of the Cerebral Circulation; and on the Connection between Affections of the Brain and Diseases of the Heart.*  
By GEORGE BURROWS, M. D., &c. &c. 8vo. pp. 220. London, 1846.

THIS volume forms an interesting and very valuable contribution to pathology. Without laying any pretension to the character of a complete treatise on the subjects discussed, the author has, in the first three of the sections into which the work is divided, presented views in relation to the peculiarities of the circulation within the cranium, and to the effects of alterations of vascular pressure within that cavity on the functions of the brain, which are calculated to throw much light upon cerebral affections generally; while, in his application of these views to the elucidation of the proximate cause of apoplexy and other comatose affections, he has done much towards removing the errors into which many of the most distinguished of our modern pathologists and therapeutists have fallen in regard to the nature and treatment of an all-important class of diseases.

In the four remaining sections Dr. Burrows directs attention to the frequent connection which exists between affections of the brain and nervous system and structural diseases of the heart. In this portion of the work numerous facts and considerations are adduced calculated to improve our acquaintance with the true pathological character and proper management of many of these nervous affections, the actual cause of which has heretofore been involved in obscurity, while their treatment has, in consequence, been empirical and often highly injudicious.

The mass of the materials of which the volume before us is composed, formed the substance of the Lumleian Lectures, delivered by the author in the months of February and March, 1843 and 1844. The first series of which lectures were published in the London Medical Gazette, for May, 1843. The present work is not, however, a mere transcript of these lectures. A considerable amount of new matter of a very interesting character has been introduced, as well as several cases bearing upon the pathological doctrines maintained in the different sections. The observations, also, on the treatment of apoplexy and hemiplegia, as well as those upon the treatment of acute affections of the heart accompanied with symptoms of nervous irritation have been extended considerably beyond the limits to which they were confined in the lectures delivered before the college of physicians.

To the volume is appended six coloured drawings, exhibiting the relative degrees of congestion of the vessels of the head, both external and internal, in animals destroyed by hemorrhage and by strangulation; by prussic acid, and then suspended, the one by the ears, the other by the hind legs; and by strangulation, the one being immediately suspended by the ears, and the other laid upon its side.

The first section treats of the cerebral circulation—in reference more especially to the modifications the circulation in the brain is capable of undergoing in health and disease; and as to how far the central organ of the circulation, the heart, when its circulating powers are increased or dimin-

ished, is capable of disturbing the functions of the brain, and in what manner these changes in the circulation affect the brain.

Our readers are all aware that many modern physicians of very high standing in the profession have, from experiments, and from reasonings founded on the mechanical construction of the cranium, arrived at the conclusion, that the absolute quantity of blood within the cranium is at all times very nearly the same.

The adoption of this opinion by most of the medical writers of Great Britain is, in a great measure, owing we suspect to the authority of Dr. Abercrombie, in whose work on the Diseases of the Brain and Spinal Marrow, it is inculcated as being founded upon a series of experiments performed by Dr. Kellie, as well as upon the peculiar construction of the cranium.

Dr. Burrows presents an analysis of some of the experiments of Dr. Kellie, and shows how very little evidence they afford in support of the general inferences he has drawn from them,—namely,

“1. That a state of bloodlessness is not discovered in the brains of animals which have died by hemorrhage; but on the contrary, very commonly a state of venous cerebral congestion.

“2. That the quantity of blood in the cerebral vessels is not affected by gravitation, or posture of the head.

“3. That the congestion of the cerebral vessels is not found in those instances where it might be most expected; as in persons who die by hanging, strangulation, suffocation, &c.

“4. That if there be repletion or depletion of one set of vessels (arteries, or veins), in the cranium, there will be an opposite condition of the other set of vessels.”

After pointing out the direct opposition which exists between the conclusions to be drawn from the experiments of Dr. Kellie, according to his own admissions, and the assertion made by him in a subsequent communication to the Medico-Chirurgical Society of Edinburgh, “that in the ordinary state of the parts, we cannot lessen to any considerable extent, the quantity of blood within the cranium by arteriotomy or venesection;” whereas, if the skull of an animal be trephined, then hemorrhage will leave very little blood in the brain; Dr. Burrows proceeds to detail a series of experiments performed by himself by bleeding animals to death, and comparing the state of the cerebral blood-vessels in them and in animals which had died from other causes.

“On the 11th of January, 1843, I killed,” remarks Dr. B., “two well-grown rabbits. The one (A, Plate I.) by opening the jugular vein and carotid artery on one side of the throat, the other (B, Plate II.) was strangled. Each animal died violently convulsed. A ligature was drawn tightly round the throat of the rabbit A immediately it expired, to prevent any further escape of blood from the vessels of the head. The rabbits were allowed to remain twenty-four hours on a table, resting on their sides. While the blood was flowing from the rabbit A, the conjunctiva was observed to become pallid, and the eyeballs to shrink within the sockets. Upon the examination of the head of this rabbit, the integuments and muscles appeared blanched and exsanguined. Upon removing the upper portions of the cranium, the membranes of the brain were found pallid, and scarcely the trace of a blood-vessel was to be detected on the surface of the brain. The longitudinal and lateral sinuses were nearly empty of blood, and their course was not denoted by any colour of blood. Upon making sections of the brain, the interior appeared equally exsanguined.

“Soon after the cord was drawn tight round the throat of the rabbit B, the conjunctival vessels became congested, the eyeballs turgid, prominent, and even projecting beyond the margin of their sockets. The integuments and muscles of the



head were found full of blood. Upon opening the cranium, the superficial vessels of the membranes, as well as the sinuses, were full of dark liquid blood. The whole substance of this brain, and its membranes, appeared of a dark reddish hue, as if stained by extravasated blood. The contrast between the two brains in point of vascularity, both on the surface and in the interior, was most striking. In the one scarcely the trace of a blood-vessel was to be seen; in the other every vessel was turgid with blood."

These experiments were repeated with similar results.

Dr. Burrows remarks that he has found the brains of sheep, slaughtered by butchers, much less depleted than the brains of rabbits which have died of hemorrhage. "But," he remarks, "these sheep did not die from simple loss of blood; but partly from division of the pneumonic nerves and cervical portion of the spinal cord, which lesions no doubt influenced the appearances."

In a tabular view of the principal pathological appearances met with in 72 insane patients examined at Bethlem Hospital by Mr. Lawrence, it appears that in 53 the vessels of the brain were found congested; sometimes turgid, at others extremely turgid,—and upon one occasion, Mr. Lawrence reported that he had never seen the blood-vessels of the brain and its membranes more injected with blood.

"Among the whole number, the cerebral vessels were found exsanguined—'unusually empty'—only once, and in this case death was caused by hemorrhage from the bursting of a femoral aneurism three days prior to the fatal event. When we learn," remarks Dr. B., "that in the autopsies of 72 insane patients, the cerebral vessels were found unusually empty, only once, and that this patient bled to death from the bursting of an aneurism, can we give credence to the statement, that the quantity of blood in the cerebral vessels is always nearly the same, or, that artificial abstraction of blood does not diminish the actual quantity in the cerebral vessels? Hence," remarks Dr. B., "it is not a fallacy, as some suppose, to assert that bleeding diminishes the actual quantity of blood in the cerebral vessels. By abstraction of blood we not only diminish the momentum of blood in the cerebral arteries, and the quantity supplied to the brain in a given time, but we actually diminish the quantity of blood in the vessels."

In order to ascertain, so far as such an experiment can do, the effect of the gravitation of the blood upon the vessels of the brain, Dr. B. destroyed by prussic acid two full-grown rabbits—

"And while their hearts were still pulsating, the one (C, Plate III.) was suspended by the ears, the other (D, Plate IV.) by the hind legs. They were left suspended for twenty-four hours; and before they were taken down for examination, a tight ligature was placed around the throat of each rabbit, to prevent as effectually as was possible, any further flow of blood to or from the head, after they were removed from their respective positions.

"In the rabbit C, the whole of the external parts of the head, the ears, the eyeballs, &c., were pallid and flaccid,—the muscles of the scalp and bones of the cranium were also remarkably exsanguined. Upon opening the cranium, the membranes and substance of the brain were pallid, the sinuses and other vessels were exsanguined; anemic beyond expectation. In the rabbit D, the external parts of the head, the ears, eyeballs, &c., were turgid, livid, and congested. The muscles and bones of the cranium were of a dark hue, and gorged with blood, which at some parts appeared extravasated. Upon opening the cranium, the membranes and vessels were dark and turgid with liquid blood; the superficial veins were prominent, the longitudinal and lateral sinuses were gorged with dark blood, and there was staining of the tissues, if not extravasation of blood into the membranes. The substance of the brain was uniformly dark, and congested to a remarkable extent.

"From the foregoing experiments it would appear, that the principle of the

subsidence of fluids after death, operates on the parts contained within the cranium, as well as upon those situated in the thorax or abdomen.

"The discovery," remarks Dr. B., "of the operation of this force on the blood within the cranium after death, suggests a precaution very essential to be followed, when it is desired to ascertain the precise amount of congestion of the cerebral vessels at the time of death. In such cases a ligature should be placed around the throat of the corpse, and drawn sufficiently tight to compress the cervical vessels, and arrest all flow of blood through them. This precaution will be most required in the examination of bodies, where from the kind of death, the blood may be suspected to remain fluid in the heart and great blood-vessels. The depending or elevated position of the head during the examination of the body will not then induce deceptive appearances, which mislead us in our conclusions as to the previous amount of congestion of the cerebral vessels."

In the brains of those who have died from hanging, Dr. B. admits, that frequently the vessels will be found neither congested nor overloaded with blood, and he attempts to account for this fact from the circumstance of the pressure of the rope not always being such as to completely obstruct the return of blood through the external jugular vein on the one side, although it has effectually stopped the current on the other—and from the subsidence of the *fluid* blood after death, while the body is yet suspended, through the cervical vessels which are not completely obliterated by the pressure of the cord, and through the vertebral sinuses, and spinal plexus of nerves which are scarcely if at all affected by the compression of the rope.

"The true state of the cerebral vessels in the bodies of those who have died by hanging, is often," remarks Dr. B., "incorrectly estimated, from the anxiety to examine the lesions produced by the compression of the rope on the larynx and trachea, as well as the condition of the heart and lungs. In making such examinations all the great vessels of the neck are usually cut across, and the thoracic organs removed from the body before the head is examined. While the head is elevated during the operation of removing the skull cap and examining the brain, the *fluid* blood gravitates from the cranium, and pours from the divided cervical vessels into the chest, and then, to the surprise of the by-standers, 'the sinuses of the dura mater, and the larger veins on the surfaces, are found but moderately filled,' or 'do not appear unnaturally full.'"

In relation to the position, that when there is repletion or depletion of one set of vessels (arteries or veins) in the cranium, there will always be an opposite condition of the other set of vessels, while the total amount of blood within the cranium will be unaltered; Dr. B., while he admits the probability of an occasional disturbance in the equilibrium between the quantities of blood in the arterial and venous systems of the brain, and that such disturbance, is productive of many serious cerebral symptoms, still presumes that, even in these cases, there is also a change in the absolute quantity of blood within the cranium, and not a mere change in the relative quantities in the two systems of vessels.

Dr. B. regards the extravascular serum which exists in the ventricles, membranes, and substance of the brain as an important element of the contents of the cranium—and he admits that the whole contents of the cranium, that is, the brain, the blood, and this serum together, must be at all times nearly a constant quantity.

"But variations in the quantity of serum within the cranium are quite compatible with health; and in morbid states of the brain we know that, at one time, the cerebral substance, its membranes and ventricles will be nearly devoid of serum, while at another time, these parts abound with serous effusion. In accordance with the variations in the quantity of extravascular serum, there must be fluctuations in the quantity of intravascular fluid, the blood. From this consid-

ration alone, it seems that the blood may be increased or diminished in the cranium. The increase or decrease may affect the two systems of vessels, arterial and venous, equally, or the equilibrium may be disturbed; there may be excess in either arteries or veins, without any necessary diminution in the quantity of blood contained in the other set of vessels."

Dr. B. does not believe that the contents of the cranium are entirely removed from the influence of atmospheric pressure.

"If there were not always," he asks, "an equilibrium of pressure on the parts within and without the cranium, very serious consequences would arise at the various foramina of the skull."

"Atmospheric pressure is undoubtedly exerted on the blood in the vessels entering the cranium. This pressure, by a well-ascertained law in hydrostatics, must be transmitted in all directions through the fluid blood, and hence to the blood and other contents within the cranium. "If," adds Dr. B., "in the natural state of the parts, the brain is defended from atmospheric pressure, should we not expect to find the functions of that organ disturbed in some way when part of the walls of this sphere is wanting. But in children with open fontanelles, and in adults who have lost part of the bones of the cranium, we observe no peculiar disturbance of the functions of the brain from this gap in the walls of the imaginary sphere. But, lastly, the effects of gravitation on the fluid contents of the cranium, and the effects of cupping-glasses, which will often draw blood from the vessels of the dura mater, causing ecchymosis there, assure us that the cranium is not a perfect sphere in the sense in which it has been supposed."

Dr. B. next refers to the recorded experience of those most distinguished for their cultivation of morbid anatomy, who state that they have frequently discovered the vessels of the brain and its membranes "full of blood," "loaded," "turgid," "gorged," "congested"—in a state of hyperæmia. The leading authorities referred to are, Morgagni, Portal, Bright, Andral, and Constadt.

In the second section of the work Dr. Burrows treats on "Vascular pressure within the cranium, and its influence on the functions of the Brain." He thinks it probable that the functions of the brain cannot be maintained in a healthy state without a certain amount of pressure upon the cerebral substance.

"Under the ordinary conditions of health, the cerebral substance is defended by its osseous case from the influence of all variations of external mechanical pressure, and is only subjected to causes of pressure acting within the cranium. A principal and constant cause of pressure from within, is the momentum of the blood distending the arteries and veins ramifying in the membranes and substance of the brain."

Dr. B. proceeds to consider the objections to this position. 1st, that the cerebral substance is principally composed of inelastic fluids which are incompressible; and 2d, that the brain is incompressible by any such force as can be conveyed to it from the heart through the carotid and vertebral arteries. He shows, that the contents of the cranium, although very incompressible, are, nevertheless, highly elastic, and that a force is constantly operating upon the cerebral substance, through the momentum of the blood, derived partly from the contractile power of the left ventricle, and partly from the reflux of the venous blood during expiration.

The dilatation of the blood-vessels, which is the result of a distending force, must be a cause of outward pressure on the surrounding tissues in all parts of the body, but, as Dr. B. remarks, there is no organ which will so completely sustain this pressure as the brain.

"We observe that the tissues of other organs expand and swell in proportion

to the momentum of blood in their vessels; but the substance of the brain cannot expand because it is confined within the limits of its osseous case. But when a portion of the cranium has been destroyed by accident or disease, or when a piece of the skull has been artificially removed in a living animal, and the dura mater, has thus been exposed to view, phenomena are remarked which indicate pressure on the cerebral substance by a force acting from within. With every systole of the ventricles of the heart, the surface of the dura mater rises a little; the brain appears to swell for a moment, and has a tendency to transgress the level of the bones of the cranium. The surface of the dura-mater subsides, and the brain shrinks during the period of their diastole. That such movements of the brain, result from the arterial pulse, or momentum of the blood distending the arteries, may be proved by making pressure upon, and still more decidedly by putting ligatures around the arteries going to the brain."

In proof of this Dr. B. refers to the observations and experiments of Cheilus, Richerand, Bichat and Ecker of Stuttgart. It has also been remarked, he adds, that when syncope occurs, this alternating motion of the brain ceases, and again returns when the heart's action is renewed.

The respiratory movements of the brain are next alluded to; these are generally considered as partly if not wholly attributable to the reflux of the blood in the veins during expiration, though Ecker attributes them in great part to the ascent of the cerebro-spinal fluid during expiration.

"The existence," says our author, "of a considerable force within the cranium, arising from vascular distension, and producing outward pressure, must, I think, be conceded. If the walls of the cranium were soft and yielding like those of the abdomen, the cranium would expand, and its capacity be increased in proportion to the increased volume of the brain at each expiration, during which that organ is the seat of an increased afflux of blood; again, when the brain was diminished in bulk during inspiration, the walls of the cranium would follow this subsidence, and its capacity would be proportionably lessened. But as the walls of the skull are rigid and inexpandable, when the brain has this recurring tendency to increase in volume, the walls around oppose the expansion, and sustain this centrifugal, or excentric pressure, which is reflected back upon the substance of the organ."

This pressure which the brain constantly sustains is equalized under the various circumstances which tend to augment or diminish it by the ample development of the venous system in the cranium and spinal canal, which affords such ready exit for redundant blood, and, very materially, according to Dr. B., by the movements of the cephalo-rachidian or cerebro-spinal fluid.

After pointing out the facts which prove the ready escape of this fluid from the lateral ventricles, through the intervention of the third and fourth ventricles, into the spinal canal, and vice versa, and of the serum of the arachnoid of the brain into the vertebral canal, Dr. B. notices the conclusions of Ecker as to the flow of the cerebro-spinal fluid to and from the brain during expiration and inspiration, and then proceeds to inquire into the true functions of this fluid. He remarks:

"This extravascular serum appears to me to be supplemental to the other contents of the cranium: it is removable by pressure or absorption, at one time giving place to an increased quantity of blood in the cranium; at another, making up for a deficiency of blood in the vessels of the head. This extravascular serum not merely acts as supplemental to the varying quantity of blood, but also to the variable quantity of nervous matter in the brain. Its quantity is in the inverse proportion to the quantity of this nervous matter. Thus in hypertrophy of the brain there is a most remarkable deficiency of serum within the cranium; the brain, its ventricles, and membranes, are so devoid of this fluid that they are almost dry: on the contrary, in atrophy of the organ, the ventricles and membranes are distended with fluid. This extravascular fluid may probably perform another office:

perhaps through this cerebro-spinal fluid a more equable pressure is diffused over the whole mass of the brain and cord; and for the reception of this regulator of pressure may be the contrivance of those cavities called ventricles, which dip into the central parts of the brain. The sac of the arachnoid may serve the same purpose."

After pointing out the manner in which disturbance of the functions of the brain is prevented when congestion of the brain is suddenly induced, or an abstraction of blood from the cranium is in any manner effected, by the escape from or flow into the brain of a portion of the cerebro-spinal fluid, Dr. B. remarks—

"When obstruction to the return of blood from the brain takes place, so that the blood becomes almost stagnant in the sinuses, that part of the force of the left ventricle, which, in the normal state of the cerebral circulation, is expended partly in propelling the blood onward through the capillaries towards the right auricle, and partly in distending the vessels through the cranium—is, under such circumstances, expended upon the interior surface of the cerebral blood-vessels. This pressure is partly sustained by the resistance of the vascular tissues, and the remainder is borne by the surrounding cerebral substance; whatever this force may be, it becomes a source of increased pressure upon the cerebral substance; and the more so, according to the pre-existing morbid states of the encephalon and its vessels. When the circulation is thus excited or obstructed, an obvious state of congestion of the integuments of the head and face is produced, and from the experiments I have detailed, it may, I think, be inferred, that a simultaneous congestion of the internal vessels of the cranium is formed.

"In previously healthy conditions of the cranium, when it contains nothing but the brain and normal quantity of serous fluid, the cerebral substance may readily accommodate itself to a temporary increase of blood in its vessels, arterial or venous, or in both, and to the consequent pressure, by expulsion of a certain amount of serum; but when the cranium contains abnormal and unremovable substances, then the brain cannot bear these accessions of vascular fullness, and consequent pressure. In those pathological states of the encephalon where there is an increase in the quantity of solid matter within the cranium and a diminution of the quantity of extravascular serum, as in hypertrophy of the brain, tumours and cysts in that organ, and in large extravasations of blood on the surface, every cause which is capable of exciting the heart's action produces a notable increased disturbance of the functions of the brain. The variable character of the symptoms of cerebral disturbance in these permanent lesions within the cranium are thus probably accounted for, by the variable vascular pressure. It seems to me probable that many permanent structural lesions within the cranium do not affect the functions of the brain by pressure, except when there is some cause in operation capable of inducing vascular congestion, or when the lesion is of a mechanical nature, or is gradually increasing.

"If the force of the contractions of the heart be diminished in these morbid states of the encephalon, there is usually a corresponding relief to the cerebral symptoms; but should the same diminution in the force of the circulation be produced in healthy states of the brain, the functions of the organ are suddenly annihilated, from insufficient vascular pressure: syncope is the result. Syncope is occasioned by insufficient vascular pressure on the brain, and not from the inadequate quantity of blood supplied to the brain and its vessels, as is commonly supposed."

The accuracy of this latter position is shown by a reference to the particular causes and circumstances by and under which syncope is the most readily induced. Thus every means by which vascular pressure on the brain is diminished to any great extent, as depressing mental emotions, sudden loss of blood, the erect posture of the body in debilitated persons, and all contrivances which diminish the momentum of the blood flowing towards the brain, will almost certainly induce syncope; which, according

to Dr. B., differs from apoplexy in every respect but this one, namely, that in both there is total temporary abolition of the functions of the brain.

Dr. B. refers the more prominent of the nervous symptoms which take place in anæmia rather to insufficient vascular pressure than to an insufficient quantity of blood in the substance of the brain.

"Simple anæmia," he remarks, "certainly does not produce that train of symptoms which is usually ascribed to a want of due supply of blood to the brain. Probably there is no condition of the brain, not even that induced by repeated hemorrhage; in which the substance of the organ is so completely anæmic as in genuine hypertrophy. In this rare cerebral affection the membranes and substance of the brain are found dry, and devoid of blood or serum; the medullary matter is as white and firm as blanc-mange, or hard-boiled white of egg. The cranium is already so completely filled by hypertrophy of the cerebral substance, that the blood is unable to make its way through the vessels; and with all this bloodlessness of the organ, we witness none of the nervous symptoms of general anæmia. On the contrary, the symptoms are rather those commonly ascribed to the effects of inordinate supply of blood to the brain. In these cases of hypertrophy of the brain the force of the heart is unimpaired; the blood is propelled with its normal force into the cerebral arteries, but it cannot make its way through the capillaries: and thus the static force of the heart is expended on the surrounding cerebral substance: so that, in anæmia of the brain from this cause, there are none of the symptoms present which accompany anæmia of the organ produced by hemorrhage, in which condition there is insufficient power of the heart to produce the amount of vascular pressure essential to the functions of the brain. Hence, we infer that the simple condition of anæmia of the brain, independent of diminished vascular pressure, is insufficient to produce the peculiar train of symptoms so often observed in general anæmia.

"It is in general anæmia, caused by profuse hemorrhage, that we witness the fearful catalogue of symptoms indicating disturbance of the nervous centres in their most aggravated form. In such a condition of the body, we remark, that whatever tends temporarily to assist the heart, or stimulate it to propel the blood more forcibly towards the brain, alleviates the nervous symptoms; on the contrary, whatever places the heart at greater disadvantage in propelling the blood to the brain, aggravates these nervous symptoms. The loss of consciousness in syncope, the convulsions after hemorrhage, are often immediately terminated by the horizontal posture so favourable to the momentum of the blood in the carotid arteries.

"From a number of curious and interesting cases illustrating the state of the nervous centres, when the system is reduced to a state of general anæmia, either by loss of blood, or other debilitating causes, the work of Dr. Marshall Hall 'On the Loss of Blood, &c.,' may be consulted with advantage. In one and all of these cases of disturbed functions of the brain, an immediate but temporary relief to the symptoms is afforded by horizontal posture, stimulants, and other means which favour the momentum of the blood through the carotid and vertebral arteries. The senses of sight and hearing are often impaired or lost during states of general anæmia, but posture alone will sometimes restore these functions of the brain."

The author adduces various examples to show the influence exercised by posture over the cerebral functions, and points out finally the effects of diminished and unequal vascular pressure upon the brain as strikingly manifested when one or both common carotids are temporarily compressed, or are suddenly and permanently closed by ligature or other means. The effects thus resulting have induced a resort to ligature of the common carotid in the treatment of some cerebral disorders, such as epilepsy, which are almost incurable by other means. After referring to the experience of Mr. Preston, Dr. Caleb Parry and Dr. Norman Chevers in relation to the success of this procedure, Dr. B. remarks, that although ligature of the common carotid is attended with risk to life in perhaps one-fourth of the cases in

which it is employed, still experience has shown that where proper precautions have been taken, it is not so dangerous an operation as many suppose, and that, consequently, when in violent and hopeless cases of epilepsy and some kindred affections characterized by extreme cerebral congestion other remedies fail us, this operation may be resorted to.

This closes the first two sections of the work; both of which are replete with interest. While the facts set forth in them remove many of the errors in regard to the intercranial circulation into which most recent pathologists have been led by mistaken views, sanctioned it is true by high and imposing authority, but nevertheless clearly disproved by experiments and the facts revealed by pathological anatomy, they present also clearer and more consistent views of the nature and causes of many of the disorders of the cerebral circulation than those generally entertained. Though our extracts from this portion of Dr. B's treatise have been frequent and extended, we have scarcely done justice to the good sense and candour exhibited by the author in the prosecution of his inquiries—the clearness of his reasoning, and the aptness of his illustrations.

The third section is devoted to a consideration of apoplectic coma—or that state which consists in the suspension of the functions of the brain while those of the spinal cord remain more or less intact.

The causes of coma are analogous to those which we experimentally find are adequate to destroy the functions of the cerebro-spinal nerves in any part of their course, namely: pressure on the nervous fibres; division of the nervous substance; disorganization of the nervous matter; interrupted supply, or deficient momentum of blood in the nervous substance; the action of narcotics. Each of these causes will, Dr. B. believes, be found adequate to explain the fatal event in different cases of apoplexy; and that when we cannot discover a physical cause of pressure in fatal cases of those diseases, we shall generally be able to detect the presence, or previous operation of one of the other causes just enumerated.

Dr. B. notices the cases upon record of fatal cases of apoplexy in which, after death, no morbid appearance in the brain is discoverable. In some of these cases the absence of any unusual turgescence of the vessels of the brain, Dr. B. conceives, may be readily accounted, for from the effects of the active depletion, cold applications to the scalp, elevated position of the head, and abstraction of all stimulants practised in the treatment of the case.

It appears to him that the true explanation of the cause of the coma in the cases of so-called simple apoplexy, is to be found in the previous existence of a state of congestion of the vessels within the cranium, brought on either by determination of blood to the head, or detention of blood in that part. The fatal event in these cases he ascribes to the gradual saturation of the substance of the brain with undecarbonized blood, when the pressure has been adequate to suspend consciousness for a time, and the respiration has become altogether involuntary, slow and stertorous.

“The apoplectic person remains in a condition analogous to that of one whose rima glottidis is constricted, or who has been suffering from apnoea for some time. The apoplectic patient then dies, not simply from pressure or lesion of the brain, but from the effects of imperfect respiration.”—“Other cases of so-called simple apoplexy are probably fatal from the circulation of poisoned or narcotized blood through the nervous centres—the vital fluid being contaminated by urea, spirits, opium, or some other deleterious principle.”

Serous apoplexies Dr. B. refers to congestion of the cerebral blood-vessels. After noticing the views of Portal and Abercrombie, both of whom

deny that the serum in what has been termed serous apoplexy is the cause of the coma, our author remarks that,—

“From the concurrence of all the best authorities, it would seem that the presence of serum in the brain renders that organ more susceptible, and liable to suffer from any congestion of its blood-vessels and the consequent vascular pressure, but the effused serum itself cannot be regarded as a cause of cerebral pressure adequate to the production of apoplectic coma.”

Dr. B. denies, also, that, in those cases of apoplexy attended with extravasation of blood, the latter is to be viewed as, in every instance, the cause of the apoplectic coma, although it undoubtedly is of the concomitant paralysis. The coma does not always commence until some time after the extravasation has taken place, and in some cases goes off while the pressure from the extravasated blood remains, while, in other cases, an effusion of blood in much smaller quantity takes place either simultaneously with or subsequent to the occurrence of coma. Hence, if the pressure of the extravasated blood is supposed to be the immediate cause of the apoplectic coma, then, remarks Dr. B., we must account for the paradox of a small extravasation producing a coma which terminates fatally, and a large effusion of blood having no such effect.

Dr. B. details two cases of fatal apoplexy in order to show that copious extravasation of blood into the brain cannot by itself be regarded as the cause of apoplectic coma; although it is of the paralysis that is concomitant with the coma, and which remains after the coma has disappeared.

“Unless blood be effused towards the medulla oblongata, and thus interrupt the continuity of the nervous substance between the brain and the upper part of the spinal cord, there appears to be something beyond effusion of blood required to produce fatal apoplectic coma.”

“When cerebral hemorrhage has stopped, I suspect the blood ceases to be a real source of general pressure, although as it increases the quantity of extravascular matter in the cranium, it also offers additional resistance to the entrance of the normal quantity of blood into that cavity; hence, healthy vascular distension becomes excessive, and the symptoms of general cerebral pressure are easily developed. Thus, the compression produced by extravasation will depend more on the rapidity and situation of the effusion than on the amount. If the effusion be slow, and not near to the base, although the amount be considerable, the effects will be slight in comparison.

“I therefore infer, that in the larger number of cases of apoplexy accompanied with extravasation of blood, the coma is to be attributed to antecedent or co-existing cerebral congestion; while the paralysis, which is more durable, is dependent upon the limited local mischief produced by the extravasation. The close affinity between apoplexy succeeded by hemiplegia, and sudden hemiplegia unpreceded by apoplexy, has long been recognized. Each of these affections is usually connected with extravasation of blood within the cranium. In the former case, the hemorrhage is associated with vascular congestion; in the latter, the congestion is probably not present.

“From the foregoing inquiry into the different lesions discovered in fatal cases of apoplexy, it would appear that all the symptoms of apoplectic coma may exist without any cerebral effusion; and that effusions of serum or blood may exist to a very large extent within the cranium, and yet there shall be no apoplectic coma. Again, a person may be in a state of apoplectic coma for many hours, entirely recover from the coma, but shortly afterwards die, when effusion of serum or blood will be discovered in the brain.” “Hence, it appears, that apoplectic coma may exist without effusion, and cerebral effusions to a great extent may exist without coma. This supports the opinion already expressed, that in a vast majority of cases of apoplexy, the coma is attributable to the pressure induced by vascular congestion. It is not meant to assert that other causes may not annihilate the functions of the brain, and cause coma, as, for example, the circulation of



venous blood through the organ, but the foregoing remarks apply to the coma of apoplexy."

Dr. B. next inquires why that apoplectic coma is not produced by those frequently recurring causes of determination of blood to the brain, such as violent exercise, strong mental emotions, and intemperance. He refers the protection of the brain from pressure under ordinary circumstances of increased determination to its vessels, to the course of the arteries through tortuous canals of bone, and the anatomical arrangement of the sinuses of the dura mater. Any sudden increase in the momentum of the blood flowing to the cranium being diminished by the first, while the second give a free exit to the blood from the brain without being liable to have their areas either increased or diminished in any considerable degree,—and in addition to these means of protection, Dr. B. considers that the existence of the extravascular serum within the cranium, by its capability of flowing from the cavity of the skull into the spinal canal, affords an additional protection to the brain against the effects of any sudden congestion of its blood-vessels.

The views of Dr. B. in relation to the immediate cause of apoplectic coma are of a highly interesting character; they are, it is true, in direct opposition to those advocated by some of the most distinguished of our modern medical authorities,—but a careful examination of the facts and arguments upon which they are based, will, we are persuaded, be sufficient to convince every one of their correctness. We believe that the errors into which modern pathologists have fallen in regard to the true character of apoplexy, have resulted from misconceptions as to the peculiarities of the circulation within the cranium, on the one hand, and their assuming, on the other, as correct, the results deduced from experiments undertaken to prove that under no circumstances, so long as the bones of the skull remained entire, no material change can take place in the absolute quantity of blood circulating in the vessels of the brain. Dr. B. has, however, very clearly shown that the amount of blood in the intercranial vessels, both arteries and veins, may be augmented or diminished by a variety of causes, and that this augmentation or diminution, when carried to a certain extent, is invariably productive of a disturbance of the functions of the brain—the pressure resulting from over distension giving rise to a state of coma—and the diminution of this pressure, from a deficiency of vascular distension within the cranium, producing a sense of exhaustion or complete syncope. He has likewise, to make use of the words of Dr. Watson, (*Lectures on the Practice of Physic*), "most convincingly shown that the conclusions of Dr. Kellie and others (drawn from experiments performed by Dr. K.) were erroneous. The theory which is thus demolished," continues Dr. Watson, "involved probably more than one erroneous assumption. By this refutation of a prevalent error, not unlikely to warp or mislead our practice in some cerebral disorders, the science of medicine has derived an essential service."

In the fourth section Dr. B. considers the connection of apoplexy and hemiplegia with diseases of the heart:—the influence of lesions of the heart on the cerebral circulation, and the relative frequency of different cardiac lesions in apoplexy and hemiplegia.

Dr. B. believes that there is a much greater dependence of structural and functional disorders of the brain upon diseases of the heart, both acute and chronic, than is commonly supposed;—that hypertrophy, dilatation, and valvular diseases of the central organ of the circulation, which are

such constant causes of congestion in other parts of the body, will be frequently found to co-exist with similar conditions of the vessels within the cranium; and that in cases of effusion of serum, or extravasation of blood within the cranium, we shall find, not unfrequently, those same lesions of the heart which are so repeatedly remarked to be coincident with dropsies and hemorrhages in other parts of the body.

From various sources Dr. B. has formed the following table, which gives an analysis of 132 cases of apoplexy and sudden hemiplegia with reference to the co-existence of cardiac disease.

Authors.	Cases.	Diseased Heart.	Per cent
Andral	25	15	60
Clendinning	28	15	53.5
Hope	39	27	69.4
Burrows (case books)	34	23	67.6
Guillemin	6	4	66.6
Total	132	84	63.6

"The inference from the foregoing calculation," remarks our author, "is that in any given number of cases of apoplexy and sudden hemiplegia, no less than three-fifths will present unequivocal signs of cardiac disease, either hypertrophy, dilatation, valvular disease, or some combination of these lesions. This proportion proves the frequency is much greater than is commonly supposed, even by those who admit the occasional influence of cardiac disease in the production of apoplexy and hemiplegia. On the other hand, it is fair to state, that M. Rochoux has come to a different conclusion. After citing the opinions of Corvisart, Briche-teau, Ravier, and Lallemand, in favour of the connection between apoplexy and hypertrophy of the heart, M. Rochoux states, that out of forty-two cases of apoplexy, which he had examined prior to 1818, three only presented hypertrophy of the heart. In reply to this calculation, I would state, that upon referring to the accounts of thirty cases of apoplexy observed by M. Rochoux, and detailed in his work, it appears that the state of the heart was examined into only fourteen times; and that in four of these serious changes in the muscular or valvular structure of that organ were discovered. Thus, if the simple statement of M. Rochoux be taken, it would appear that in cases of apoplexy the heart was found hypertrophied in the proportion of 3 : 42, or 1 to 14; whereas, if the more correct calculation be made, the proportion of diseased hearts in cases of apoplexy examined by M. Rochoux, ascends to 4 : 14. Although that average, obtained from the analysis of the cases reported by M. Rochoux, is less than that given in the foregoing table, still it must be borne in mind, that lesions of the heart were not so easily appreciated prior to 1818, as at the present time. It is necessary, also, to remark, that this author restricts the term apoplexy to those cases where there is rupture of the proper tissue of the brain followed by hemorrhage; and that he excludes from this category those cases described by Abercrombie and others, under the title of 'simple congestive apoplexy.' To such varieties of the disorder he applies the term 'coup de sang,' and admits that the state of the heart exerts a very great influence in the production of congestion of the blood-vessels of the brain, although very little upon genuine apoplexy—i. e. cerebral hemorrhage. If this admission be taken into the calculation, it is probable that the average obtained from M. Rochoux's cases would not have materially differed from that given in the table."

Dr. B. examines next, the question as to which of the lesions of the heart is most likely to give rise to apoplexies and sudden hemiplegia; and from the result of his observations, concludes that hypertrophy with valvular lesions, or these affections of the heart combined with disease of the cerebral arteries, are, one or the other, most frequently found in fatal cases of apoplexy. To show the relative frequency of these several cardiac lesions in cases of apoplexy and sudden hemiplegia, Dr. B. gives in the following

table the analysis of 25 cases recorded by Andral, and 34 cases taken from his own case books.

	No. of Cases.	Heart Diseased.	Hypertrophy with Valvular disease.	Hypertrophy, (simple.)	Valvular Disease.
Andral	25	15	9	4	2
Burrows	34	23*	10	6	6
Total	59	38	19	10	8

In regard to the period of life most prone to apoplexy and hemiplegia, Dr. B., after deducing various calculations drawn from different sources, and presented in a tabular form, remarks that

"The first general inference from these tables is, that the relative frequency of apoplexy steadily increases from 20 to 80 years of age; and the second, which is more remarkable, is, that the actual number of apoplectic cases increases in each successive decennial period upwards, from 20 to 70 years of age, while the numbers living gradually diminish. The original statistical researches of Dr. Clendenning upon the heart and other viscera, forming the Croonian lectures for 1838, will afford some interesting points for further comparison with the results of the previous table. This physician ascertained, by a series of extended observations, that the force of nutrition of the heart increases as life advances. This is proved by the increasing average weight of the heart, so that in estimating hypertrophy of the heart, some allowance must always be made for the age of the individual. From this writer we learn that hypertrophy of the heart is a change concomitant to that period of life when apoplexy is most prevalent.

"Hence, then, the frequency of apoplexy in advanced periods of life, the usual co-existence of hypertrophy of the heart at the same period, and the ascertained tendency of cardiac disease to produce hypertrophy of the brain, are facts which, when taken in conjunction, confirm the conclusions arrived at in the previous part of the section, namely, that the coincidence between apoplexy and diseases of the heart is something more than accidental."

In the fifth section Dr. B. presents some observations on the treatment of apoplexy and hemiplegia. He points out the importance of posture in emptying the vessels of the head both external and internal, and the necessity of attention to the state of the heart, before the question of large abstraction of blood is decided upon.

When no cardiac disease is discoverable, or only simple hypertrophy without notable valvular disease, depletion both general or local, may, according to Dr. B., be carried on so far as the cerebral symptoms appear to call for that kind of relief; but if, upon examination, the existence of valvular disease to the extent of obstructing the circulation through the cavities of the heart be detected, the pulse will be a most deceptive guide as to the propriety or impropriety of abstraction of blood. If it be the mitral valve which is principally implicated, allowing of regurgitation from the left ventricle, the small and irregular pulse would probably dissuade from that free abstraction of blood which the cerebral symptoms might require; if the aortic valves be found diseased to the extent of not only obstructing the onward current of blood, but also of allowing regurgitation into the ventricles, during its diastole, there will probably be associated with this lesion hypertrophy of the left ventricle.

"Here will be observed a full and vibrating or thrilling pulse; but a pulse of increased action without real power, and hence a deceptive pulse; and one which if it be regarded without reference to the structural changes of the heart, would invite to a more copious abstraction of blood than was called for by the general

\* In one case there was simple dilatation of the cavities.

symptoms. In each of these last-mentioned cases greater relief to the symptoms will be obtained by a free local abstraction of blood from the vicinity of the heart (either by cupping from beneath the left mamma, or between the left scapula and spine), than by a much larger depletion by venesection."

When with the indications of serious valvular disease of the heart there is well-grounded suspicion that an osseous deposit has occurred about the valves of the left ventricle, in the coats of the ascending aorta, and, in all probability, in the tunics of the arteries within the cranium, in such cases an accidental congestion may have been followed by extravasation of blood, and thus have arisen the most common symptoms of apoplexy. Here the copious abstraction of blood should be abstained from, notwithstanding the fullness and hardness of the radial pulse.

In cases of apoplexy and hemiplegia complicated with dilatation of the cavities of the heart and extensive emphysema of the lungs, the appearances of extreme congestion and dyspnœa might tempt a resort to large depletion, and thus the very conditions of the heart which have induced the cerebral congestion and apoplectic symptoms would be aggravated. In these cases Dr. B. suggests cupping glasses to the nape of the neck or between the scapulæ, with the internal administration of stimulating diuretics, diffusible stimulants, and the application of rubefacients to the sternum.

"In cases of apoplexy, about two or three days after emergence from the coma, and recovery from the depletion practised during the fits, we often observe the patient's face to become flushed, the scalp hot, a frowning or knitting of the brows, slight strabismus, and complaint of pain on one side the head. This pain is usually referred to the temporal, parietal, or occipital region opposite to the paralyzed limbs,—and if the patient be deprived of the power of expressing his sensations, his uneasiness is often indicated by the occasional movement of the sound hand to the forehead." "At the same time, the circulation, which had been much enfeebled by the remedies employed during the fit, becomes more active,—the patient is thirsty, and is sometimes troubled with an oppressive heat of the surface of the body."

"These symptoms, which are indicative of inflammatory action commencing around the clot of blood in the brain, may generally," according to our author, "be controlled in a most striking manner by small local depletion from the temple or mastoid process on the side opposite to the paralysis,—by the application of cold to the head, and by the administration of purgatives, by restricted diet, and by extreme quiet in the sick-room."

When the heat of the head is diminished, he has found a blister applied near the occiput to afford great relief to the oppressive headache. When the patient is not of a very advanced age, nor extremely exhausted by depletion, great benefit, Dr. B. observes, will be derived from the administration of one grain of calomel every six hours; allowing it to act as a purgative, or to slightly affect the gums.

At the same stage of the disease the paralyzed limbs are not uncommonly affected with involuntary movements which usually consist of spasmodic contractions, either tonic or clonic. The patient will now most probably complain of severe pains in the palsied limbs, and of burning heat in them, so that he will long to plunge them into cold water; the integuments of these limbs often feel hot and are red and swollen.

"These wearing pains," remarks Dr. B., "are not confined to the integuments, but appear to pervade the deeper seated parts, so that the periosteum of the bone of a palsied limb will become swollen and painful. In some cases these pains have appeared to be attributable to the continuance of irritation from the clot upon the surrounding cerebral substance; in other cases there has been no evidence of cerebral excitement; but this painful state of the limbs seemed to de-

pend upon returning functions in the nerves, and partly upon the capillary circulation in the tissues of the limb not being duly regulated by the nervous system. These pains in palsied limbs are very analogous to those which are experienced in a part when the circulation and animal temperature are returning to it after it has been benumbed by cold."

In this condition of the limbs the remedies which Dr. B. has found most serviceable, are slight local depletions from the head, when there has been any remaining symptom of cerebral irritation, combined with the application of evaporating lotions, either tepid or cold, to the affected limbs. In a few cases, unattended with headache, flushing, heat of scalp, and other similar symptoms, he has found marked relief to follow the application of leeches, and evaporating lotions or poultices to the painful limb. In some cases the redness and heat subside, but the neuralgia remains, and after continuing weeks or months gradually disappears.

"A change in the condition of the soft parts of the paralyzed limbs now succeeds; they shrivel and waste away,—the limb becomes more or less contracted in different cases; any attempt at extension causes much pain; the limb loses its temperature readily, if exposed uncovered to the atmosphere, and when confined in bed it is often found bedewed with a warm, clammy perspiration. There is very little hope of any restoration of muscular power when such changes have taken place,—but much may be done to prevent such limbs getting worse."

Dr. B. strongly cautions against allowing patients, recovering from an attack of apoplexy, to rise too soon from bed, or to take too much freedom in exercise, mental occupation, and diet. By a neglect of this precaution a fresh extravasation of blood in the brain may be induced, productive of irreparable mischief—the head symptoms may be renewed with subsequent disorganization of the cerebral substance, or the supervention of inflammation of the brain and its membranes may be caused.

Three or four cases are detailed illustrative of the bad effects of premature exertions after apoplexy.

"When," Dr. B. remarks, "sufficient time has elapsed from the fit to allow of a restoration of the injured cerebral substance to its healthy condition, it becomes of importance to excite the suspended functions of the nerves in the paralyzed limbs. To effect this object, the application of blisters or frictions with stimulating liniments over the affected limbs, particularly in the course of the great nervous trunks, are sometimes attended with good results. Such measures, if not directly beneficial to the palsied limbs, sustain the confidence and hopes of the patient during his tedious convalescence."

From the effects of electro-galvanism Dr. B. has not witnessed any decided good effects even when used under the most favourable circumstances. His opinion of strychnia is even less favourable than that which he has expressed respecting electricity. In some cases, he states, it does much harm by aggravating the wearing pains in the affected limbs to a much more acute suffering. Regular frictions over the surface of the affected limbs and well-devised exercise of the muscles he recommends as the best means of exciting a more active circulation in the wasted muscles and nerves of the limbs, and of renovating and invigorating the proper functions of their different tissues.

In cases of paralysis, in which there has been some idiopathic affection of the encephalon, a seton or issue at the nape of the neck may, he conceives, retard the unfavourable progress of the disease; but if, on the other hand—

"The cerebral congestion is attributable to some of those serious lesions of the heart alluded to in the foregoing sections," then he considers "counter-irritation at

such a distance from the affected organ not only useless, but sometimes positively injurious."

Section the sixth treats of the influence of diseases of the heart in exciting functional disturbance of the brain. Dr. B. presents the detail of a number of cases of severe epistaxis occurring in chronic cardiac diseases; of epistaxis and serous effusion alternating in cardiac diseases; and cases of epistaxis and apoplexy in cardiac diseases; these cases strongly corroborate the necessity of examining into the state of the heart whenever epistaxis occurs unexpectedly in adults. This hemorrhage, Dr. B. believes, may often be considered as strictly pathognomonic of an obstructed circulation through the heart, as hemoptysis is symptomatic of tuberculated lungs, or intestinal hemorrhage of an indurated liver.

Dr. B. notices next a class of cases marked by a variety of cerebral symptoms, of which the most prominent is *headache*, and where the primary disease, or cause of these symptoms, is hypertrophy of the heart. He refers also to the fact that mental disturbance is often produced by chronic cardiac disease.

The whole section is replete with useful practical hints calculated to lead to the better understanding of the pathology, and the more successful treatment of a very common class of obscure or intractable head affections.

The seventh and last section is devoted to a consideration of affections of the brain and spinal cord depending on acute diseases of the heart. This subject is illustrated by the details of sixteen cases of acute cardiac disease; some of which were marked with all the usual symptoms of inflammation of the brain and its membranes; others simulating mania and dementia; others characterized by apoplectic and epileptic symptoms; others with well-marked symptoms of tetanus and trismus, and others, again, accompanied by symptoms of aggravated chorea and hysteria.

After an analysis of these cases, Dr. B. remarks—

"From the foregoing analysis it is evident, that an early diagnosis of the cardiac affection is a very important element to ensure a successful termination to the case. It also appears that the plan of treatment, which alone was attended with success, consisted of a combination of general and local depletion, with the application of blisters to the cardiac region, and the free use of calomel combined with opium."

Dr. B. is strongly in favour of depletion from the region of the heart by cupping glasses applied between the base of the left scapula and the spine, or by leeches applied near the left mamma, in cases of acute rheumatic pericarditis; after which, there is no remedy upon which he places more reliance than a blister over the cardiac region. In all cases of pericarditis, excepting in patients affected with tubercular disease of the lungs, or in the very anæmic or cachectic, he employs mercurial preparations as freely as in acute inflammations of other parts. He administers a few large doses of calomel, varying from five to ten grains, combined with a grain of opium, and then continues half the dose of the same combination at moderate intervals, until the urgent symptoms abate, or the mouth becomes distinctly affected by the mercury. He sometimes, although rarely, orders mercurial inunction, together with the internal administration of the calomel.

We have met with few works which have interested us more than the one before us, and we strongly recommend it to our readers as one replete with hints calculated to lead to still further advances in the pathology, diagnosis, and treatment of diseases of the brain, as well as of a large class of affections vaguely described as nervous or spinal.

ART. XVII.—*Outlines of Naval Surgery.* By JOHN WILSON, (D.) Surgeon R. N.; Late Surgeon H. M. S. Vanguard. 12mo. pp. 134: Edinburgh, 1846.

SURGEON JOHN WILSON in this small book has little to say on the subject of surgery as the word is commonly understood; under this term he includes nautical medicine. Strictly speaking, there is so little peculiar in the practice of surgery on ship-board, that the principles of military surgery are as applicable to the navy as to the army. Perhaps the naval surgeon exercises his profession with fewer advantages than the medical officer of the army, and is more frequently driven to expedients for the accommodation and comfort of his patients than the military practitioner on shore.

The volume is divided into four parts: the first treats on the examination of seamen and on medical surveys; the second, on preparation for battle; the third, on prophylaxis; and the fourth on fevers. The work is badly written, and the value of its contents, which are really practical, scarcely redeems it from entire condemnation. But as this journal is widely circulated in the navy, and as the subjects may be novel to very many readers, it is thought expedient to bestow on the volume a much more extensive notice than its merits can claim for it.

The ultimate object of all arrangements on board of ships of war is to do battle successfully. As space is limited, it is important that every part should be turned to the best account, and none but able and efficient individuals should be permitted on board. Besides the secondary duty of managing the sails, &c., seamen are required to perform the more essential service of working the heavy guns, and of using the musket and cutlass as occasion may require. The number of men allotted to ships of war is based on the weight of metal, caliber and number of guns which constitute her armament; and has no direct reference to the dimensions of her spars or sails. Therefore, *cæteris paribus*, the warlike efficiency of the ship is in direct proportion to the corporeal strength or health of the individuals composing the ship's company. To manage efficiently a long forty-two pounder gun, the weight of which is about three tons, requires a crew of sixteen able-bodied men and one boy; and a long thirty-two pounder, fourteen men and a boy.\* As the guns on both sides of a ship are not usually fought at the same time, the number of men is estimated for one-half the guns only; so that a ship armed with one hundred long forty-two pounders would require eight hundred men and fifty boys to man her batteries. When the guns of both sides are fought at the same time, it becomes necessary for the men, reduced one half in number at each gun, to unite their force "to run out the gun" at the port-hole. Therefore, when a ship is compelled to fire from both sides at the same time, her crew fights at considerable disadvantage.

It must be perceived, that it is very important that each gun's crew should be composed of able-bodied men; if feeble, or sickly, the efficiency of the gun is proportionately impaired.

For reasons of this kind, although not stated by him, our author very properly recommends that every surgeon, on joining a ship, should carefully inspect the crew, and cause those he may find unfit for service to be

exchanged for healthy men. "From rendezvous establishments, guard-ships, and such like, come the bulk of chronic cases, of constitutions shattered or undone, or subjects who are unfit, who in their examination have been only glanced at, or glossed over by those who were little interested, or were in this matter in a manner wholly unconcerned." Surgeon Wilson does not dwell on the subject of inspecting recruits, nor does he refer to the excellent volumes of Henry Marshall, Deputy Inspector of Army Hospitals, "On the enlistment, discharging, and pensioning of soldiers," and his "Military Miscellany; comprising a history of recruiting in the army," works which should be carefully studied by medical officers both of the army and navy. Exposed to disease from vicious habits and poverty, as those men are, who form the classes from which men-of-war's crews are obtained, it requires some judgment and considerable knowledge to select from the mass only those who are efficient. The surgeon of a rendezvous has a most important, responsible, and often difficult duty to discharge; for carelessness on his part may not only affect the honour of his country's flag in the hour of trial, but involve his government in the expense of feeding and paying men unable to make a proper return in labour for what they receive.

"From the general constitutional condition of the men, will the character of the ship be formed, or according to the state and nature of the one, will be that of the other;—she will be happy, healthy and effective, or sickly and disunited,—the character of the compound whole, being the necessary consequence of that of the ingredients acting.

"Life at all times is uncertain, but particularly at sea. Sailors soon put on the symbols of age. Manhood, or the meridian of life with them, is soon over. For action or sea duty, youth is the period. The serviceable or really working period of a sea-going seaman's life is about twenty years—from twenty to about forty.

"Under twenty, sailors are soft and unseasoned; beyond that, they become more firm and more seasoned; but at, or about forty, begin to break down. Complaints with them become much more frequent, and much more unmanageable; they become rheumatic, asthmatic, stiff in their joints, sullen, and very often discontented,—the consequences of privation, broken rest, exposure, and a life hard spent.

"The race of sailors is by no means tall,—they rarely reach six feet,—the majority of them being of the middle stature, from five feet four to eight. Sailors in their nature are thoughtless, indifferent about the present and equally so about the future, loose in their attachments; witty, shrewd, regardless of danger, fond of change.

"The duties of a ship at sea are active. For this, men are required who are young,—duties which to the feeble, or those in years, are unsuited.

"The congregate body of an efficient sea-going crew, should be in manhood—two-thirds between twenty and thirty, and one-third above. Among seamen newly raised, the inspection or medical examination must tend, not only to the constitutional condition of the individual, but to age and stature. A man may be sound, but still, from the smallness of his frame, or natural inability, useless in a large ship. In large numbers, a few individuals of this description would probably be but little felt, but in a small crew most seriously, or if numerous, even in a large one. With the rate of the ship, or the weight of the sails and guns, the strength of the crew must at all times correspond; large ships require large robust men, small ships those more light.

"With every care, men damaged in character or constitution will creep into all ships, arising from the natural obscurity of chronic disease or concealment."

The author is probably correct in his estimate of the moral qualities of sailors; but these qualities are not necessarily derived from their vocation: they carry these attributes with them from the land to the sea, which is most generally chosen by the thoughtless and by those who are "regardless of



danger." A very large proportion of the seamen in our navy first see the ocean after they are of a legal age, when the moral character is already fully formed. There is nothing connected with life at sea which necessarily leads those who pursue it to be immoral, reckless, drunken, or heedless. Unfortunately for the class of sailors, it is too common to regard the navy as a proper receptacle for the turbulent and dissolute; and, through the mistaken benevolence of law-officers, our ships of war often contain men whom the laws of society condemn to penitentiaries.

The examination of recruits, for the purpose of ascertaining their physical ability, devolves on the surgeon, and should be conducted in private. The recruit should be entirely naked, that the surgeon may have a full opportunity to inspect his form and its defects, if any exist. With a view to insure uniformity in the decisions of the surgeon in his examinations, they should be conducted according to some rule; for instance, let him look to the general figure; then the head and the senses; chest, abdomen, groins, genitals, and anus; and lastly, the extremities and joints. By observing some method of the kind, the surgeon will be less liable to pass men labouring under serious defects, such as hernia, fistula, defective vision or hearing, &c.

From crowding and bad ventilation in ships, from broken rest and exposure to the vicissitudes of weather and change of climate, the proportion of sick in a ship's company may be stated in round numbers to average five or six per cent. According to the "Statistical Reports of the Health of the British Navy," every man is on the sick list once in every eight and a-half months, and the annual decrease in the mean strength of a squadron from invaliding and deaths, is at the rate of about fifty per thousand, or five per cent. on all stations. About the same proportions may be stated for the navy of the United States. In this view of the subject, it is very important that all members of a ship's company should be in vigorous health at the time of embarkation for distant service, that the expenses incident to sending invalids home from remote stations may be spared, and that the reduction of the ship's force may be avoided as much as possible.

If the inspection be carefully conducted at the rendezvous, on board receiving vessels, and again after the men join the vessel in which they are to serve, cases for invaliding will be rare. The inspection of recruits is surrounded with difficulties: the recruit is always anxious to pass inspection, and, if really unfit, will resort to every means to deceive the surgeon, and may conceal defects from him. A moment's reflection will satisfy any intelligent member of the profession, that an external examination of the body, unassisted by information from the individual being examined, will not enable us to determine certainly, the physical ability of a man: it is even more difficult to determine that a man is sound, from simple personal inspection, than to decide on the soundness and qualities of a horse without trial. Therefore, to protect the interests of the government, by excluding unfit men from the naval service, and from improper claims for pension, it becomes necessary to examine a recruit a second time on board of a receiving vessel by another medical officer before the enlistment is complete. Instances occur, in spite of this care, where men are passed by two medical officers, with hernia, fistula, and old strictures of the urethra, and other affections which escape detection. Such recruits, when called upon to labour, speedily complain to the surgeon of the vessel, and make known their infirmities. Such instances are not alluded to here as proof of carelessness or incapacity on the part of inspecting officers, but to show the

difficulties of inspecting recruits for the navy. When men are scarce, it becomes a question, With what degree of disease or imperfection may a recruit be accepted? how far may the list of disqualifying causes be reduced, in order to supply a pressing demand for men, especially in time of war?

“Cases of disease for survey are always occurring in ships of war, but in every one varying, and both as to number and kind. The frequency of such will vary with the youth and stamina of the crew, with the care taken in their first selection, the one being generally the inverse of the other, and with the period of the commission, the season, and climate.”

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“Assimilation to the habits and living of seamen is, at all times, gradual, and at all times, more or less, in progress, during the first year partially affected, during the second more fully, and during the third and fourth more completely.

“Among a body of men who are young, robust, and bred up from infancy to a sea-life, cases of disease for survey, from assimilating causes, will, in all probability, seldom occur, and not more the first than the third year. On the contrary, in crews that are young, and unused to a sea-life, or in those who are old, or worn out, cases of disease for survey will undoubtedly be much more frequent; in the first, from excitement or non-assimilation, in the last from frailty.”

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“The medical examination at first, and the survey after, are naturally counterparts or parts of the same duty, but controlling each other. By the one, all those who are unfit, will be, or ought to be, rejected or objected to, and by the other, those will be got rid of who may have escaped the first. The more closely, therefore, the first or the examination is conducted, the less necessary and frequent will be the second or the survey. In both the object in view is the same, to select the serviceable, and reject those who are unfit; only that, of the two modes of procedure, the first is the more simple, the second the more complicated.

“Subjects more immediately calling for a medical survey are chronic cases of disease, cases that may be lingering, and such as may be incurable or doubtful. Diseases are curable at a proximate or distant period or they are not. Acute disease is not generally embraced within the limits of a medical survey, such terminating too speedily in health or dissolution.

“In a sea-going ship, the duties necessarily incurred are active, urgent, and, under any circumstances, must be performed. In all ships there must and will be sick, cases of disease, both acute and chronic. The strength of the ship, and the length of the sick-list are naturally inverse, or the longer the latter, the weaker and more crippled must be the former.

“In acute cases of disease, there is always some prospect of a termination, either in the return of a patient to health or dissolution. In chronic cases it is otherwise. In them the termination may be distant or doubtful, or there may be no prospect whatever.

“In a sea-going ship the duties are active, important, and must be performed, and either by the parties themselves whose duty it is to perform them, or by some others, when necessary, in their stead. In all cases, the length of time for absence from duty must have a limit, and will necessarily vary with the nature of the disease, the probability or improbability of a cure, and the nature of the duties required.

“Some cases brought forward for survey, as phthisis, paralysis, or dropsy, are clear, or at once made out, and no delay necessary. Others, as rheumatic, hepatic, dyspeptic cases, are less obvious or more obscure or doubtful, and more time or consideration required. In sea-going ships, however, to absence from duty there must be a limit. Here all duty is important, imperative, and, by some one or other, must be performed. If a case, whatever that may be, should still be lingering and doubtful, and, notwithstanding all the remedies that may have been employed, at the end of six weeks or two months, and particularly in an individual known to be of a thorough bad character or constitution, it may be considered then a fair one for survey.

"Officers are more wanted, and more important than men—their duties more urgent and more responsible; therefore can be less spared—and an earlier decision, therefore, in their case must be come to, but resting always on the nature of the case and duty; for instance, farther trial of what may be done, or a longer probationary period, may be granted to a marine officer or chaplain, whose duties are not so urgent or onerous, provided the case is doubtful or not incurable, than could be reasonably allotted to an executive officer, purser, or surgeon, whose duties are more urgent, more personal, and must be performed."

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"A medical survey may be plain or it may be exceedingly difficult. In phthisis, paralysis, hernia and such like, there is commonly no difficulty, but rheumatic, hepatic, chronic cases, are often difficult to unravel or tangibly detect.

"In a clear case, where the disease is obvious, the officers surveying may at once, from the appearances before them, decide, but in a doubtful chronic neuralgic one, the business in general is much more difficult and complicated. Symptoms then are not so prominent, striking, or characteristic. To unravel or make out tangibly, the character of the case, or the man, circumstances or symptoms, in all their bearings, must then be considered.

"If the man, the object of survey, is damaged in his character, and on this much will, in all cases, depend, if dissipated, quarrelsome, or, whenever he can, a skulker, or if the evidences of disease are not so tangibly developed as to make the case clear, and all should depend on his own statement, the case then may be considered doubtful or suspicious, and may be prudently, if it is desirable, deferred; but on the worth of the man and his previous character, much will depend.

"If the man should be useless, discreditable, or old, his discharge from the ship may be desirable, but if young, of a previous good character, and an acquisition, the disease being at the same time doubtful, more time may be given. Something more of the nature of the case or man may be drawn out, or the disease yield.

"Disease may be real or it may be feigned. Cases of both occur among seamen, but which time and observation together will in most solve.

"In naval medical surveys there are always one of two objects in view,—entire removal from the service, or a change of climate, and to both surveys abroad refer. On foreign stations cases of disease for survey are of two kinds, the one considered curable by change of climate, the other incurable. In the first are included all cases of functional disease, in the other, those involving morbid deposit or change of structure.

"On some constitutions climate acts severely. In young, sensitive, and unseasoned subjects, or new comers, through the influence of climate, excitement in the latent constitutional germ is induced; and with that morbid action. In men of moderate habits, and used to a sea life, the influence of climate is always least felt, or by them best resisted."

When it is probable in any case, in the opinion of the surgeon, that an officer or man cannot be restored to a condition for duty within a reasonable period; or that change of climate and removal to better accommodations than the ship affords, are essential to restore health or save life, it is his duty to request "a survey," or medical council. The case is then examined by the officers of this board, and the captain of the vessel or commander of the squadron disposes of the individual in conformity to the recommendation of the surgeons. It sometimes happens in the British navy that "surveys" are resorted to for the purpose of getting rid of inefficient, or worthless, or querulous officers and men, even when the health of the individual is of the smallest consideration. Superior officers are sometimes detached in this way, unnecessarily as far as health is considered, to make way for the promotion of favourites in a squadron. In the navy of the United States this may happen, but we have heard of no instance of the kind.

Surveying officers should carefully consider the interests of the government, of the ship or service, and of the individual. But they must be wary that an individual, and especially if an officer, whose private interest requires him to return home, or who is discontented or love-sick, does not impose upon them his moral pains as bodily infirmities.\* They must consider too, the harmony and well-being of the ship, and rigidly weigh how far it may be expedient to yield to the desires of the peevish and useless members of the ship's company, which may be more efficient when unwilling labourers are withdrawn. Setting aside individual interests, it should be constantly borne in mind that the object of a survey is to preserve the efficiency of the ship, by enabling the commander-in-chief to remove the infirm and substitute the robust in their places; his authority for this purpose depends very much on the opinion expressed by the surveying officers.

Naval surgeons of the United States are frequently assembled to survey officers and men to determine in what degree a permanent disability entitles them to a pension for hurts received while acting in the line of duty. The surgeons are called upon to determine whether the disability be permanent, and whether a man is totally incapable of contributing to his support by labour, and consequently entitled to a full pension, or whether he be able in part to support himself, and, therefore, entitled to three-fourths, one-half, or one-fourth pension. Where an individual has totally lost his sight, or lost both arms, the case is plain; but it is not always easy to determine what amount of pension, if any, should be granted, for the loss of a finger or toe, or for an injury impairing in some degree, without destroying, the use of an extremity. In the cases of common seamen and marines who may claim pension for wounds, it is believed the interests of the government as well as of humanity will be best served, by acting on the most liberal view the case permits; but in the case of commissioned officers in receipt of pay, the most rigid construction of the pension laws should be closely adhered to, particularly for small injuries. There is an instance on record of a commissioned officer receiving, during more than twenty years, an annual pension of three hundred dollars, on account of injury received in battle, which in a small degree impaired the use of a thumb, although, for a greater part of the time, he received the full pay of his grade. As far as the duties of his office were concerned, the injury occasioned, in fact, no disability whatever; had he been a common seaman, however, the disability would have been serious, perhaps sufficient to prevent him from discharging the duties of his vocation. When a seaman claims a pension for any disability sufficiently great to preclude his admission into the naval service, or from exercising his profession on board of a merchant vessel, his case is entitled to the kindest consideration by the surveying surgeons: in such a case they would be justified in awarding a full pension, (six dollars a month,) although they might justly deserve censure, should they

\* Examples of officers of rank feigning illness are not wanting, and they should not be forgotten. Charles, Duke of Bourbon, Constable of France, wishing to desert the emperor, "feigned sickness in order to have a pretence for staying behind;" Hotspur's father, old Northumberland, lay crafty sick to avoid the battle of Shrewsbury. Essex, the favourite of Elizabeth, is said to have feigned a violent disease to move her compassion; and Raleigh pretended madness, sickness, and a variety of diseases, to protract his examination and procure his escape. Gustavus Adolphus IV., of Sweden, was mean enough to simulate a wound of the leg."—See *Gavin, on Feigned Diseases*.

grant any pension whatever to a commissioned officer on full pay, labouring under precisely the same degree of corporeal disability.

It may be proper to go behind the letter of the law, and recur to the policy or motives of its enactment, to enable surgeons to determine the degrees of disability in the variety of cases presented for their examination. A pension should not be regarded exactly as a substantial evidence of the government's sympathy, for the bodily suffering or inconvenience sustained by an individual in its service, but as a charitable provision, made for those who are unable to obtain the necessaries of life without assistance, in consequence of disability sustained for the sake of the country, which disability they probably would have escaped had they not exposed themselves in the employment of the government.

It is to be presumed that, in the United States at least, the system of pensions for those hurt in the public service was, in a measure, designed to encourage men to expose themselves to the chances of war, by securing subsistence to them if maimed, or to their widows, if killed in the line of duty. The object was to secure them against want, and as far as possible remove all pecuniary anxiety in event of mishap. It was presumed that few men would expose themselves to the hazards of battle, while it was sure, if they should leave the navy disabled, they must depend for sustenance on begging in the streets, or become messmates with paupers in some almshouse.

Under this view of the case, unless we accept Dr. Samuel Johnson's definition of a pension, viz., "an allowance made to any one without an equivalent. In England it is generally understood to mean pay given to a state hireling for treason to his country:"—Under this view of the subject, it is questionable whether commissioned officers while receiving a salary are entitled to any pension. It may be urged, however, that notwithstanding the officer receives his salary without performing duty, he ought to receive some compensation for the permanent inconvenience he suffers in consequence of a wound or hurt, especially if the wound be of such a nature as to probably shorten his life. The same argument is equally applicable to the common sailor or marine; and on this ground it might be argued, that the pay of the sailor should be continued, as in the case of officers, and that a pension should also be granted as a compensation for personal suffering and inconvenience.

Under whatever aspect the subject may be viewed, it is very certain, under the same circumstances, the common sailor or marine has greater need for a pension than the commissioned officer. In determining the degree of disability in the case of a sailor, surgeons should satisfactorily answer the question: To what degree is the claimant incapable of obtaining a livelihood as a sailor? They have no just plea for deciding that although incapable of getting subsistence as a sailor, he may maintain himself in some other pursuit, or to resort to an extreme case for illustration, that although disabled as a sailor, he may thrive by street-begging, and is, therefore, not entitled to a pension. The surveying-surgeons are no more warranted in deciding that a seaman shall engage in a new vocation, than they would be in the case of a commissioned officer.

Surveying-surgeons should bear in mind that in all cases, whether for pension or invaliding on a foreign station, the individuals surveyed naturally endeavour to exaggerate their disability, although at a rendezvous the same individuals would strive to make light of any slight disability which might be observed by the inspecting-surgeon. They are bound to keep the

interests of the service constantly in view, and never sacrifice them to the selfishness or particular benefit of individuals, whether the case be one for enlistment, invaliding on a foreign station, or for pension. It is their duty to protect the treasury, consistently with justice to individuals, and to the service, from all unnecessary expenditure.

The second part of Surgeon Wilson's volume is devoted to "Preparation for Battle."

"Battle, however disagreeable or painful to contemplate, is the aim or object of a ship of war,—her duty when called for. It is the test which will infallibly try her. It will try her character, her crew, her discipline; it will try the skill of the most skillful, the valour of the most brave. For such an event all must be prepared, and at all hours and seasons, for come it will without doubt, in some shape, and at some time, while nations and human nature continue."

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"During battle, the cockpit, or place allotted for one, is the receptacle for all who are unable to go to quarters, or hurt or wounded. \* \* \* For a surgical operation, the cockpit, or place allotted for one, is not the most eligible; but, under all circumstances, at the time the best. For the reception and relief of wounded men, much, both in action and previous preparation, becomes necessary. Under any circumstances, and even the best regulated and prepared, duties of the description here referred to are always onerous and trying.

"In war, where all is uncertain and sudden, nothing should ever be left to chance. Suddenly, or unexpectedly, or at night some conflict may take place, but which it is the duty of every person to be prepared for. To a lubberly, careless, or unprepared ship, the first salutation from the enemy may be a broadside, and the cockpit become rapidly filled with wounded men. For emergencies of this nature, everything and every one, in a ship of war, ought to be prepared,—there ought to be nothing at such a moment ever wanting or to be looked for.

"The importance, onerous nature, or responsibility of medical duty during battle, can never, by any one, it is imagined, be questioned, embracing, as it does, death and wounds in every shape, limbs lacerated or torn off—wounds from musketry or splinters—bayonet or cutlass wounds, everything, in short, appalling or horrible, and all, probably, in a few minutes, in the midst of bustle, the noise of guns, a close atmosphere, and what renders all still more embarrassing, candle-light, and through heat and closeness in tropical climates, still more grievously augmented. In obviating inconveniences about the cockpit, much, however, by a previous, medical, methodical arrangement, may be effected, and both for the comfort and convenience of those wounded, and that of the medical attendants."

Of the importance of preparatory arrangements for battle, a notion may be formed from the rapidity of the events which cause officers and men to require prompt medical assistance. On the 1st of June, 1794, the Queen Charlotte, a three-decked ship, under command of Admiral Lord Howe, engaged the Montagne, a three-decked ship, commanded by the French Admiral Villaret, and in an action of twenty minutes, 100 were killed and 200 wounded on board of the latter ship.

In the battle of the Nile, August 1st, 1798, there were killed and wounded on board the French ship of the line Guerriere, from 350 to 400 men, in three hours.

About midnight, March 1st, 1799, the British frigate Sybille engaged the French frigate Forte, and after an action of one hour and forty-five minutes, of 370 on board the Forte, 65 were killed and 80 wounded; and of 370 on board of the Sybille, 5 were killed and 17 wounded.

On the 1st June, 1813, the British frigate Shannon engaged the United States frigate Chesapeake, and after an action of fifteen minutes, of the 330 men on board of the Shannon, 24 were killed and 59 wounded; and of 386 on board the Chesapeake 47 were killed and 99 wounded.

In an action of half an hour between the U. S. frigate Constitution and British frigate Guerriere, 14 were killed and wounded on board the former, and 79 on board the latter.

In the action between the U. S. frigate United States and British frigate Macedonian, of little more than an hour, on board of the former there were killed 5, and 7 wounded; and on board of the latter, of 300 men, 36 were killed and 68 wounded.

The action between the U. S. frigate Constitution and British frigate Java continued about two hours. On board the former 9 were killed, and 25 wounded, and on board of the latter 22 were killed and 102 wounded.

In an action of fifteen minutes between the U. S. sloop Hornet and British sloop Peacock, there were 4 killed and 33 wounded on board the latter, and on board of the American ship 1 killed and 2 wounded.

In an action of 43 minutes between the U. S. sloop Wasp, and British sloop Frolic, there were killed and wounded on board of the latter between 90 and 100, and on board of the former 10.

These examples are sufficient to show how arduous must be the labours of naval surgeons in battle, and how important it is they should be fully prepared at all points.

The arrangements of the cockpit, or place used for the same purpose, must be anticipated as far as possible before the ship leaves port. In ships of the line and frigates, an *operating table* should form part of the furniture of the cockpit. It may be so contrived as to serve for the accommodation of the cockpit-mess in ordinary times, and of the medical officers during battle. A table ten or twelve feet long by six or seven broad, having eight drawers arranged in two rows on each side, to contain the surgeon's armament should be provided. The drawers should be distinctly labelled, so that their contents may be known at a glance. For instance, a drawer, labeled 1, surgeon's instruments; 2, senior assistant surgeon's instruments; 3, junior assistant surgeon's instruments; 4, tourniquets; 5, muslin or calico, flannel, lint, &c.; 6, rollers, tape, &c.; 7, sponges, adhesive plaster, &c.; 8, sundries. A contrivance of the kind indicated would embrace, in a small space, everything necessary for rendering immediate assistance to wounded men.

As the cockpit is totally dark at all times without artificial light, lanterns should be provided and kept constantly in their places, ready to *light up* at any moment: of course proper hooks for their suspension must be fixed in the cockpit before going to sea.

That there may never be a deficiency of *water* for the wounded in time of need, two or more casks should be kept in the cockpit, always full, ready for action; the casks should be furnished with proper cocks, and be distinctly marked with the word "Surgeon," or other suitable means taken to prevent the chance of want of water for the recently wounded, or for operations.

*Buckets* and *swabs* should form part of the cockpit furniture; and *sand* should be at hand for sprinkling and drying the deck. In a sea-way it renders the footing more firm, and assists the medical attendants in their movements and work.

Before leaving the dock yard, the *sick cots* should be inspected to be certain that the frames or stretchers fit, that none of the parts are wanting, and that they be of proper size,—that is, not too large, as economy of space must always be studied in all matters connected with a ship of war.

When men are wounded aloft, they become helpless themselves, and a hindrance to those about them. *Topslings* of canvas, or of rope, large

enough to fully envelop a man's body and sufficiently strong to bear the weight, should be provided, one for each top, so that all who may be disabled aloft may be at once sent down to the cockpit.

Means should be provided also to relieve the cockpit of the presence of those whose wounds have been dressed. *Platforms* arranged in the *tiers* of ships of the line would answer; but hooks or battons for slinging cots in the same part of the vessel would serve the purpose equally well.

When the ship clears for action, the *Sick-Bay*—that part of the vessel where the sick are usually accommodated,—like other parts, must also be cleared. The bottle-frame should be closed up at least, if not removed to guard against the chance of fragments of glass, and corrosive or inflammable liquids from being driven about by the enemy's shot. For this reason the bottle-frame should be movable.

When an engagement is about to take place, the surgeon should furnish the captain of each top, and of each gun, three or four *field tourniquets*; it is presumed they have been previously instructed in their use by the surgeon on occasions of "exercise at general quarters." The cockpit is to be lighted, and the drawers of the operating table should be examined to ascertain that everything which may be required, is present and ready for use. Much will depend on the care the surgeon has taken in procuring his supplies before putting to sea, so that, in fact, preparation for battle forms an important part of the surgeon's duty from the hour he joins the ship. He should not forget that boat expeditions are not unfrequent, and that these must be provided for. "A boat expedition is always in its nature uncertain both as to the distance to be gone, the success and return, all depending on winds, weather, locality, and on the skill and character of the officer commanding." A portable box, covered with canvas to protect its contents from damp, should be prepared, containing in a small space instruments and dressings: a shoulder strap, to facilitate its transportation in case of landing, should be attached.

In the bustle of battle, attention to slight injuries, such as involve no danger, may be deferred in favour of those cases which are serious. "It would be unreasonable or unjust to trifle away time at such a moment with cases of no importance in the face of others more urgent or pressing."

In cases of fatal injury, something may be done to palliate the condition of the dying man, by placing him in an easy position, or the exhibition of an opiate or stimulus.

Hemorrhage is undoubtedly the most alarming and urgent symptom presented to the surgeon during battle. Where the bleeding is serious, everything must be set aside till the vessel be secured. "A general or superficial oozing of blood from a wounded surface may not be, among a number of others more urgent, immediately pressing; but bleeding from a wounded artery will at once demand aid." In some cases immediate operation is indispensable; in others it may be delayed.

"Nervous concussion, or derangement, marked by sinking of the pulse, cold sweats, and syncope, is often simultaneous with, or immediately consequent to, severe or extensive gun-shot injuries. If such is nervous only, and does not depend on hemorrhage, in all probability it will be transient and pass off. If from the loss of blood, the case then will be different: and probably lasting and fatal."

Preparation for battle does not consist solely in the arrangements of the cockpit, nor in providing instruments, dressings, &c., for the wounded. Naval surgeons begin to prepare for battle in the medical schools; for, unless they be professionally competent, well instructed in the principles



of medicine and surgery, instruments and appliances will be of little avail to the wounded. During a naval engagement, there is nothing to substitute in the cockpit of a frigate for coolness and sound professional knowledge. Ignorance at such a time, in spite of the best intentions, must prove fatal to many whom surgery might save from death. 'On such an occasion a skilful surgeon is a blessing and a hope; but an ignorant one is a curse, an ally to the enemy.

It is important not only that the surgeon should be well qualified, but the assistant surgeons and subordinate attendants should be well trained, docile and obedient, and be, as it were, imbued with a common motive of action. It sometimes happens that assistant surgeons, although well educated and in spite of passing a rigid examination with distinction, are, in fact, very inefficient or even useless, from an over, or misappreciation of their personal dignity. There are some who leave the schools and carry into the naval or military service, a notion that it is derogatory to do anything which may soil their hands; and consequently, they frequently direct subordinate attendants to do what should be done by their own hands.

Speaking of army hospitals, the late Dr. William Ferguson (Inspector-General of Military Hospitals), says, in his "Notes and Recollections of a Professional Life"—

"The position of the young physicians, meanwhile, was both pitiable and ridiculous. Divested of the pride they had imbibed at the universities, they might have made excellent hospital assistants; and, from their high general education, would have been deserving of every promotion the rules of military service—which in due course of time they would have come to grace and elevate—could permit. Their station in society, too, proclaimed them to be a class far superior to what the army had commonly received; but as physicians, setting aside their utter ignorance of diseases at so early an age, more especially military ones, they were *far too fine for common use*. To one of them I was attached in the first campaign. He could read Hippocrates in the original Greek, but he did not know the grain scales and weights when he saw them; and to have touched a bleeding wound, even while the sound of the cannon was booming in our ears, would have been to lose caste. He was my superior by at least four degrees of military rank, but I had to teach him what I myself was taught in the early days of my apprenticeship. With an apothecary, an assistant, a nurse, and a clerk in his train, he might have made a routine book prescription, in classical Latin, (for what young inexperienced physician ever makes any other, unless by chance,) out of the military medicine chest; but had the ingredients of his own prescription been put in his hands, he would have known as much about them as if they had been sent from Timbuctoo. He had worn a cap and gown at Cambridge, but it is not to be supposed that he had ever entered an apothecary's shop, or contaminated his hands with drugs either in bulk or detail. In fact, no medical officer should be tolerated in an army who cannot, like the man of Ross, 'prescribe, attend, the physic make and give.' He must be a man of all work to have any business there; but to have placed such men over the heads of all who were experienced in military medicine and diseases, while he was not fit for any work, was as stupid and gross an abuse as could have been imposed on an army. The assumption, the affront to every principle of service, was monstrous, and shows to what extent university and corporate pride will proceed, when unchecked by wise regulation and military rule."

The information of medical officers of the navy should not be limited to the science of medicine and surgery, as taught in the schools; but their hands should be practically educated in the application of dressings, and in putting up prescriptions in a neat and artist-like manner. They should also possess a knowledge of accounts, so that they may never be at loss in

computing the expenses of their charge; or experience any difficulty in ascertaining the correctness of bills they may be called on to approve for payment. This is essential to the efficient discharge of a surgeon's duty in the navy of the United States, especially when the surgeon has charge of a large hospital establishment.

The third part of our author's volume is devoted to prophylaxis, or the means of preventing disease on board ships of war, in which ordinarily there are many causes of disease always active. Cleanliness of the ship and of the persons composing the crew; clean and well-aired bedding; clothing suitable to the climate; dryness and proper ventilation of the vessel; food and drink of proper quality and in sufficient quantity, and temperance in the use of alcoholic liquors are essential to a healthy ship. On all these points the surgeon should be capable of giving proper advice.

*“Temperance.*—Few vices are more ancient, more extensive or ruinous, than drunkenness, the root of evil, misery and disease; and to ruin or a premature grave has consigned thousands. Among the seamen of every nation the propensity to intoxication is strong, the bane of the profession, the rock on which thousands split. In a profession, girded or beset with so many perils, the danger of drunkenness to all must be manifest. Intemperance among a ship's company is very soon manifested, by their appearance, by that of the ship, by the absence of cleanliness, by that of comfort, and by the general prevalence of crime. It is manifested also by the amount of punishment, and the length of the sick list. A drunken or dirty ship, it is here maintained, never was, nor ever will be healthy. And what is more, never will be efficient. The amount of punishment, and the length and strength of the sick list will, in most, tell, or form an index, not far from the truth, of the state of the ship, of the state of discipline, of that of organization, of the character of the officers, or that of the crew. On seamen drunkenness acts twofold—directly, by falls in board, by falls overboard; by the commission of crime or the general omission of duty; indirectly, by inducing morbid action, fever, hepatitis, dropsy.”

Of the general truth of these remarks there is no question; and Congress could in no wise contribute more to the health and efficiency of our public ships than by abolishing the spirit ration in the navy. The opinions of a majority of the best officers in the service are in accordance with this view of the subject. Mutiny, crime and punishment, in nine cases of ten, have their origin in the grog-tub.

It is a practice with some of our commanders to restrict the crews of their ships too much in the allowance of fresh water; a practice exceedingly baneful to health, leading, as it very frequently does, to scurvy, which, under ordinary circumstances, may always be avoided in our ships. We are aware of no instance of scurvy attacking seamen who have a full allowance of wholesome water. Too much attention can hardly be given to this subject; we are not sure that, where scurvy makes its appearance in a public ship, the commander and surgeon ought not to be brought before a court of inquiry, to show that the disease had its origin in circumstances entirely beyond their control. Scurvy is the result of defective nutrition, and, when it occurs, may be generally traced to a deficiency of water and bad quality of the articles composing the ration. As a general rule, the commander may be charged with neglect of duty, when he permits bad provisions, or a short allowance of water to be served to the crew.

It should be borne in mind that salt does not absolutely prevent meats from spoiling, but merely retards the progress of decomposition for a period. For this reason, the more recently meats have been “put up,” the more nutritious and the more wholesome they are. It sometimes happens that

our ships leave port with provisions which have been already one or two years in salt. In the course of time, old salted beef undergoes a change, becomes hard, and almost as destitute of nutritive qualities as mahogany, and hence the vulgar name of salt-junk (anglice, old rope salted,) given it by seamen.

“By salt, climate, and keeping, the nutritive principles, or the gluten and albumen of meat and bread become decomposed; new combinations and new bodies forming, solid, fluid, and gaseous, with a diminution in the volume, as well as the quality of the article. Beef loses most, pork much less, by keeping the nutritive or gelatinous, albuminous, fibrinous principles of the meat being decomposed, forming with salt and with each other, compounds soluble or insoluble, leaving in meat, long kept or long salted, little remaining, save a carneous mass, indigestible, innutritious, and dry.

Under the influence of heat and dampness, bread soon deteriorates on board ship, becoming light, friable, honey-combed, crumbling to pieces under the fingers, and abounding in weevils and maggots. Various plans have been adopted for the preservation of this essential article. Under the direction of a chief of the Bureau of Provisions in the navy department, attempts were made to preserve bread by packing it in barrels with small bags of pepper, and of camphor; but at the expiration of a few months, as might have been anticipated, the bread was found to be camphorated, or peppered so as to be uneatable, and in other respects not better preserved than that which had not been drugged or spiced.

“For the accumulation and decomposition of matter, few places are more favourable or fitted than a ship's interior, divided, or partitioned off, as it is, and necessarily must be, into numberless apartments, hidden, shut and stagnant. In a temperate and cold climate, the want of cleanliness, discipline, or organization, may be for a time, as it often is, harmless, or without anything morbid immediately resulting; but send a ship of this description to a warm one, the East Indies, or coast of Africa, for instance, and then observe her,—matter heretofore quiescent, would then, through heat and moisture, begin to act, or to assume, in some form, life, and new bodies, with new properties, be evolved, with disease.”

Our author urges the importance of keeping the interior of ships clean, dry and thoroughly ventilated, in order to guard against disease. On this point we will permit him to speak for himself; these extracts from his work will enable our readers to perceive that, although somewhat observing, Surgeon Wilson's style is far from being creditable to him, and is scarcely redeemed by the matter he communicates.

“In investigating the source or cause of disease among seamen, it is not to the upper parts or works of the ship we are to look, which are necessarily exposed, and through exposure harmless, but to the deeper and darker, or those parts which are shut up, damp and stagnant holds, store-rooms, magazines, or all under hatches.

“From the outer aspect of a ship, much may be learned of the interior, or what is within—or from what is seen, of what is not. A slovenly, neglected, dirty ship or crew, will nine times in ten be the same within. A ship, however, may be clean, painted, and polished without, or about her upper works, but still ill-conditioned, or ill-ventilated below—a more common, a more deceiving, therefore a more dangerous case. In every search after the cause or source of disease among seamen, we must look, therefore, not to the upper, or exposed parts of the ship, but to the interior, or to all those under hatches.

“The accumulation of matter, the source of decomposition, germination and disease, may take place in the holds or limbers, or in both. The holds may be cleared, washed, white-washed and ventilated; yet, if the limbers be not opened and cleaned, nothing may be gained.—The limbers of a ship it may be necessary

to say, are two in number, one on each side of the keelson, and unless the ship be bottle tight, (a rare phenomenon,) moist.\* Over, or on these limber boards, rest the whole contents of the hold—water—stores provisions. Into these hollow ways or limbers, then, water, and however introduced, from all parts of the ship's bottom trickles, finding its way by descent at length to the pump-well. Water, unmixed with other matters, may in the limbers, be harmless; and, undoubtedly in a cold climate often is so,—but, mixed up with debris, animal or vegetable, in a hot one, and particularly in a close bottomed ship, often the source of disease.

“Ships are sweet or foul, in proportion to their leakage—open, leaky ones, being generally most sweet, and those that are close, or tight-bottomed, foul or fetid. The state of the limbers, or inferior interior parts, may be in most in a great measure, gleaned from that of the water pumped up, being copious and clear in the former, and fetid or foul in the latter. In hot climates, and close-bottomed ships, water, in the limbers and pump-well, termed bilge water—through seclusion, stagnation, and matter, very often becomes fetid, and very soon offensive. On the state of these parts (holds, limbers and pump-wells), in all ships, and all climates, much, and not easy to say how much, the health and comfort of the crew, or all will depend, and much more than is commonly imagined or thought of.

“Example is better than precept—more forcible and more lasting. Some years ago, it is believed somewhere about the year 1826, the *Pyramus*, a frigate, was employed on the West India station. After being for some twelve, or perhaps more months there, the crew, after having been all previously healthy, became suddenly, and, without any obvious cause, attacked with fever. From the very first onset, the disease betrayed itself as violent, phlogistic, and intractable, and very soon alarming; and what was the worst feature of the case, no reason could be given. There were some points, however, which, although material, appear to have been overlooked in this case, bearing immediately on its nature and cause. The disease, for instance, was limited to this ship; no other in the squadron had it at the time, therefore it could not have been an epidemic. It was uninfluenced in its violence or virulence, by change of place, or the disease was the same at sea as in harbour—therefore the cause, whatever it was, must have been in her, or carried about with her, or endemic. A great number of the crew both officers and men, were attacked, and died, and, without any distinction as to rank or age, or whether they had lived and slept on board or on shore, indicating that the cause, whatever it was, was indigenous, or in the ship. In this disease were exhibited all the symptoms of the most virulent tropical endemic. In a short time, half, or perhaps two-thirds of the crew were attacked, disabled, and laid up.

“This ship, like many others, was clean in her decks or upper works, clean where it is not of much consequence; but, as it proved afterwards, unwholesome and pestilent below—a fallacious—a too common—a very dangerous case. After various speculations as to the nature and cause, and antidote to the disease, and, after the loss of much time, and both officers and men not a few, it was then resolved, as it ought to have been at the first, to clear and clean the ship.

“With this view the frigate was taken to Antigua. At first, in clearing out the holds, nothing was discernible calculated in the least to elucidate this subject, all seemed as it should be, and had the search gone no deeper than the holds and pump-well, and that was possible, the cause would probably have not been ascertained. The limber-boards, however, were taken up, and then the cause of the disease came into view. In and through the limber spaces, was found accu-

\* The angular spaces, bounded by the keelson and bottom of the ship, and intersected by the floor timbers and ribs placed at right angles with the keel, are called the limbers. There is one on each side of the keelson, running its whole length. In order that water may freely flow fore and aft, holes are cut through the timbers, near the keelson; these are termed limber-holes. The plank which covers the timbers on the inside of the vessel; and lies nearest to the keelson is termed the “limber board;” it is not usually nailed. In some vessels a chain or a rope (called limber rope) is kept passed through all the limber-holes from stem to stern, for the purpose of keeping these holes free from obstruction, by accumulations of solid matter. Limber chains or ropes are now obsolete.

mulated, and that to a very considerable amount, matter, black, blue and putrid, both animal and vegetable, and of all colours and stages of decomposition. From all parts of the limber spaces issued, and with noxious ebullition, gases sulphuretted, hydroguretted, or virulent. Of those persons who were employed in this duty—all, or the greater number of them, were attacked with fever,—some died, and some with very great difficulty recovered.”

About the year 1827, in the month of June or July, the U. S. sloop John Adams was at Matanzas. Fever attacked the crew, although there were no cases of fever at the time on shore, or on board of the U. S. sloop Hornet, then lying in the harbour. The John Adams went to Pensacola, and the origin of the disease, in the opinion of the medical officer, was traced to the foul condition of the hold. After the lower parts of the vessel were thoroughly cleansed, no new cases of fever occurred among the old crew, but about twenty out of forty men transferred from another ship were attacked.

The U. S. frigate Macedonian was fitted out at Boston in the winter season and sailed on the 2d of April, 1822, and arrived at Havana on the 28th of the same month. Ten days afterwards a case of malignant fever occurred on board and terminated fatally on the third day. The ship went to sea on the 4th of June, having lost seven of her crew. It appears there was no fever prevailing in the town or on board of other vessels in the harbor at the time. Captain James Biddle says, in his report, believing the disease had its origin in the climate, “It was of course a just expectation, that on quitting the port the condition of the crew, in respect to health, would have become ameliorated. Yet, to my great distress, this was not the case; and although the weather at sea was unusually fine, the people were taken down with fever in the most surprising manner.”

It appears that of the 376 persons, officers and men on board, 101 died of fever between the 11th of May and 10th of October, a period of five months. The surgeon and the assistant surgeon were ill at the same time, and the former died, and thirty-five men perished without any medical advice, owing to the illness of the medical officers.

Captain Biddle, as well as the surgeon of the ship, attributed the fever to the foul condition of the lower hold. He stated that when the Macedonian “was last commissioned, a part of the ballast was taken out and landed; and for the purpose of ascertaining if the keelson was cased, the ballast immediately on each side of it was moved. All the ballast was completely covered with black mud and dirt, and so hardened together, that it was a work of much labour to separate the different pieces of iron. The quantity of mud taken from the ballast that was moved, was so great, that Midshipman Gerry informs me he saw two cartloads of it carried from the wharf. Mr. Doggett, the gunner, acquainted me that the water casks of the ground tier were immersed in mud to about a fifth of their height; and this extraordinary accumulation of mud is to be explained by the extraordinary fact, that on the return of this ship from the Pacific, the hold was not broken out; and that as respects the hold she was laid up in ordinary nearly in the condition in which she arrived from sea. Midshipman Turner acquaints me that he was stationed within the navy yard last winter; that the firewood he used was taken from out of this ship, and that when ignited it became so offensive that he was unable to remain in the room. Thomas Pewmont, a seaman on board, informs me that he was employed in the Boston Navy Yard last summer; that in September last, some gang casks were taken from this ship for the United States’ brig

Spark; that this ship's hold was at that time extremely offensive, and that there were in it several casks of salt provisions in a rotten and putrid condition. *Pewmont* also states, that this ship had not been pumped out for upwards of two months; that it then took half a day to pump her free; and that the water discharged from the ship was horridly offensive.

"The sickness and mortality on board have been greatest among the persons employed in the hold, and among the carpenter's crew, who, by working the pumps, were most exposed to the offensive bilge-water discharged from the ship.

"With all the means in our power, such as ventilation, white washing, fumigation, fires, and letting water into the ship, we have been able but partially to correct the impure and offensive air emitted from the hold."

Captain (now Commodore) *Biddle's* report, charging neglect on those officers who fitted the ship for sea, caused a court or board of officers to be assembled to inquire into the truth of the allegations. The statement by Captain *Biddle* was substantially corroborated by the testimony adduced: although no blame was attached to the officers under whose direction the ship was prepared for sea, the evidence shows that notwithstanding the usual steps were taken, a very considerable part of the hold had escaped cleaning for more than three years. The severity of the weather was probably unfavourable to this kind of work; and everything being frozen, it is probable there were no odours indicating a foul condition of the hold. It appears before the court of inquiry that, at *Havana*, the temperature was usually about 86° F. on the gun-deck, but probably ranged somewhat higher on the birth-deck and inferior parts of the vessel. On one occasion, under these circumstances, Captain *Biddle* compelled the whole crew to sleep on the birth-deck, with the air-ports closed, as a means of detecting some one of the crew who had committed the wicked and treasonable act of cutting the breeching of one of the guns. The punishment was most severe, but, in a military point of view, possibly not too great for the offence. The crew were forced to exert themselves daily in practising the manœuvres or exercise of the great guns, even after the appearance of the malignant fever on board. Notwithstanding, it was clearly shown to the court that Captain *Biddle* was always solicitous for the health of his crew, and resorted to various means to secure this important object.

Let us, however, extract from our author some other facts bearing on the point under consideration.

"Of accumulations within the limbers, the *Pyramus* was a sample. The following, of that without:—In the year 1817, and for about a year before that, the *Pique*, a frigate, was employed on the *Jamaica* station, and had generally during that time been healthy. Catarrhal and synochial cases, from solar exposure, or atmospheric changes, or some indiscretion, as in most ships had occasionally occurred, but nothing diffused or malignant. About July, of that year, the ship was ordered to *Vera Cruz*, and up to her arrival there, it must be remarked, was in her ordinary state of health. At this place, cochineal, a very decomposing substance when wet, was taken on board, in considerable quantity, in raw or untanned hides, in open boats, and in close rainy weather, and wet or soaked, and softened as it was, stowed below in the after hold and spirit room. The temperature at this time ranged from 80° to about 100°. Except on duty, none of the crew ever landed, and only during the day, and all, except a very few of the officers, always slept on board. Eight or ten days after leaving this place, and while at sea, fever suddenly and unexpectedly, and without any apparent cause, broke out, and soon become general. Men in good health, and all ages, began to be affected, were suddenly and unaccountably attacked with headache, fever and delirium. At all hours, to our surprise, applications for assistance began to be made, and in a few days the main-deck of the frigate, usually clear, became,

through increasing numbers, everywhere crowded with hammocks, cots, and sick men, and the sick list averaging in general from about eight or ten, mounted rapidly to forty, fifty and sixty.

"What then was the cause? became the first and important question. Discipline, system, temperature, were all the same they ever had been; therefore it could not reasonably be imputed to them. No other ship in the squadron was attacked in the same way; therefore the disease was not an epidemic.

"On reaching Port Royal harbour, the holds were cleared, but not on the score of health, to get at the cochineal and other matters. From all parts, fumes, fetid and offensive, issued, and which for some time had been felt. The bales of cochineal at the bottom, which had been wet at first, through heat and confinement, it appeared, had become putrid and decomposed. The holds were cleaned, washed, whitewashed and ventilated, and the disease ceased.

"The following, also, is one of accumulation and morbid formation without the limbers, of a nature different from the preceding:—The Salisbury, a two-decked fifty gun ship, during 1817, and for a year or two before that, bore the admiral's flag at Jamaica; she had always been looked on as a clean and healthy ship, Febrile cases, from irregularity, exposure to the sun's influence, as in all ships, had occasionally occurred, but nothing general or diffused. About the middle of 1817, she was ordered to the Spanish Main. In the neighbourhood of Portobello, a flat marshy tract, she watered and wooded, taking on board a large quantity of green, soft, rank and vegetating, cut from the forest around, and either during, or soon after the rainy season. Two or three weeks from this, being then in Port Royal harbour, cases of fever began to appear, spread, and in a very short time became general, and particularly among such as slept below, in cabins, tiers or cockpits.

"During the progress of this malady, a very great proportion, probably half the crew, became affected. Of these many died, and many with difficulty recovered, and without any respect as to age or habits. Among the gun-room officers, or midshipmen, who slept in the cockpit, it spread, and most severely; some died and some recovered with difficulty; but it must be particularly remarked, and worthy of notice, no case occurred with the captain and wardroom officers, who all, at that time, slept on the main-deck. This was no temporary attack,—it lasted two, three, or more months, and one, two and even three dying daily. What was the cause? became again naturally the question. The disease existed in no other ship at the time; therefore it could not be an epidemic. Did it proceed from locality, as in the Pheasant and Phoenix? Was the germ its cause, inhaled at Portobello and evolved at Port Royal? It certainly would appear not, because those who had been on shore, and those who had not, became indiscriminately the victims. Did it originate in the ship? To this several circumstances concur. In the first place, it was general only in this ship, and particularly among such as slept or lived in or about the tiers, cockpits or lower parts. In the second place, cases of fever continued to occur, not only among such as had been on the Spanish Main, but among new men, (as it happened with Mr. Saunders, the pilot, in the case of the Eclair, who had embarked at Plymouth,) who had volunteered, or were drafted afterwards from other ships. Was this infectious? Here, again, it appears not; no restriction was ever imposed or thought of in this instance, and no case was ever transmitted out of the ship, or to any of the medical or sick attendants.

"What then was the cause? is still the question. It was clearly, it would appear, something in the ship, and something placed there, either previous to quitting Port Royal harbour, or after. For the first, no reason whatever could be supposed, as the ship, up to the time of her leaving, had always been healthy. It was not until she had quitted; or was about to quit, the Spanish Main, and had taken on board green, rank and vegetating wood, that she became unhealthy. It is particularly to be remarked, that this wood was not dry, inert or deadened, as it ought to be, to fit it for a ship's hold, particularly in a tropical climate, but full of sap, capable, when subjected to stagnation and a high heat, of life and germination, or the evolution of noxious gas."

These examples are sufficient to show the very great importance of  
No. XXIV.—Oct., 1846.

keeping the limbers and holds of ships perfectly clean, and free from all substances liable to easy decomposition by moisture, heat and confinement. A great deal of filth often collects in the spaces of the ribs, between the outside planking and internal skin or ceiling of the vessel. Lately an improvement has been made in our ships of war, by leaving out a large part of the ceiling, so that the timber-spaces can be readily reached and thoroughly cleaned. It would be far more beneficial to bestow attention and labour in keeping the hidden parts of vessels pure, than to devote so much time in polishing the iron and brass about the decks. "Bright work," however agreeable it may be to the eye, is not always a sure indication of a perfectly clean ship; belaying pins, and rails may shine, and the upper decks may be dazzlingly white, while the limbers, pump-well, and holds are filled with pestilence and death.

On board of ships of war it is too common a practice, or rather it was the practice, to deluge the decks in water daily, without regard to weather or other circumstances. We are informed that the birth-deck of one of our sloops of war was daily wet, although the water froze as it fell, and scrapers were necessary to remove the ice; and at the same time, the deck was crowded with the hammocks and cots of men labouring under pneumonia and pleuritis. The surgeon protested in vain; the first-lieutenant insisted on the importance of scrubbing the deck daily in spite of every argument. The fourth part of Surgeon Wilson's volume is devoted to the consideration of tropical fevers. He is a non-contagionist, and contends that the African marsh fever is neither infectious nor contagious. Alluding to the case of the *Eclair* steam-vessel, on the coast of Africa, in which 65 died of 145 persons on board, he expresses an opinion that the disease had its origin in the vessel, and was not brought on board by a single person as asserted in the newspapers. However, it is not our purpose to pursue the subject.

Our object has been to present such an outline of the cares and duties which devolve on medical officers in the navy, as would indicate how valuable the science of medicine and surgery may be in the service of the government; and show the necessity of bestowing the greatest care in the selection of medical men for the navy. Surgeon Wilson truly remarks:

"It must be obvious to the most superficial observer, to the merest tyro, to every one at least, acquainted in the slightest degree with the nature and duties of a ship of war—*health is power*; without it, no ship and no fleet can be efficient; and what are ships, what are fleets, it may be here asked, in sober reason, without efficiency? What are ships, built after the most approved models, guns cast on the most improved calibre, without efficient men, (not with men laid up with fever or scurvy, as in former days they used to be,) without men in health, without men able to work and fight them? Let him who can answer, tell. It is the bravery of her seamen, not the model of her ships—for they seem to have sailed as fast formerly as now—that has made Great Britain what she now is. And what, or where is bravery,—it is here repeated, without health? they are co-existent and inseparable; no man can be brave who is laid on a sick bed,—hence the importance of the subject at present under discussion. It is the bravery, and, consequently, the health of her seamen, it is here contended—and let him deny it who can,—that has made Great Britain what she now is, that has made her flag brave, for a thousand years, the battle and the breeze, and makes her now ride Queen among the nations."

To prevent feeble and sickly men from being imposed on the government, for pay and subsistence, the surgeon must exercise his professional discrimination at the rendezvous. To maintain the strength and efficiency



of the ship's company, the surgeon should be ready, not only to prescribe for those who may be sick or hurt, but to point out the best means of preserving health under all circumstances on board. The surgeon must be ever ready to provide for the wounded in the hour of battle, and to succor all, even when panic-struck by an epidemic, in the time of need; and to him the government looks to guard the treasury from improper claims for pensions, or respite from active duty.

Surely the duties of a naval surgeon demand for their proper and efficient discharge, sound intellect, varied intelligence and a high degree of professional acquirement. To secure men possessing all these qualifications is not less the interest of the officers and men in the navy than of the government; but disguise it as we may, the fact is that the importance of a highly educated medical corps is not universally appreciated in the navy of the United States. The temptation of high pay is not offered; even a defined position relatively to others in the service is denied, and until very recently, assistant surgeons were berthed and messed, and classed, with "school-boy midshipmen" of fourteen years of age and upwards. Still they seek a defined relative rank, that the military community of which they are essential members, may know how to respect their rights and privileges as educated gentlemen.\* The surgeon in the navy of the United States is called upon to be content with a moderate pay, and the silent approbation of his own conscience, while he often labours hard, and shares the perils and privations of naval life, without any prospect of the honors, rank, promotion or reputation which inure to officers of the line. On occasions of ceremony, he may find himself preceded by a midshipman; and last, though not least, as indicative of the appreciation in which his professional services are held, the law makes him share prize-money with the boatswain, carpenter and sailmaker. Truly, we should be humble, and set less value on the profession which wise legislators have determined is deserving of no better reward for exposure in battle than the craft of a sailmaker or carpenter. The members of the profession generally should take the matter into consideration and use their influence to place their professional brothers in the military service of the country on a proper footing.

W. S. W. R.

\* Since this article was in type, the Hon. George Bancroft, late Secretary of the navy, has issued a "General Order," assigning a relative rank to medical officers.

## BIBLIOGRAPHICAL NOTICES.

ART. XVIII.—*Animal Chemistry, with reference to the Physiology and Pathology of Man.* By Dr. J. FRANZ SIMON, Fellow of the Society for the advancement of Physiological Chemistry, &c. Translated and Edited by GEORGE E. DAY, M. A., and L. M., Cantab., &c. Philadelphia, Lea & Blanchard, 1846: pp. 295-717: 8vo.

IN a former number we noticed the first part of this work, and an attentive perusal of the present and larger portion of the work, confirms the opinion then expressed of its great value in the elucidation of many intricate questions connected with physiology and pathology. Animal chemistry is still in its infancy, and although much has been done in it, within the last few years, a vast field is yet open to the observer in this department of scientific inquiry. Enough, however, has been accomplished to enable the practitioner to perceive that the various changes in the composition of the animal solids and fluids; some arising from normal and some from pathological causes, are to be accounted for, in a measure, by the aid of chemistry. It will, it must be admitted, to use the words of the author, "require an immense number of analyses to ascertain these modifications, to express them in definite terms, to connect them duly with functional disturbances in the organism, or with other symptomatic phenomena, and finally, as far as possible, to endeavour to discover their origin."

The present work is of much importance in facilitating these researches, as it not only is a record of what has hitherto been done, but points out the best modes of carrying on the investigations, and the probable connection between various diseases and certain modifications in the composition of the different fluids and solids.

This part opens with a consideration of the secretions of the chylipoietic viscera and the theory of digestion, noticing the chemical composition of the saliva, bile, gastric and pancreatic juices, both in a normal and diseased condition, and their action on aliments, followed by a consideration of the process of digestion; this latter the author is of opinion, is mainly attributable to the action of pepsin, a free acid, and a suitable temperature, as from recent experiments it has been shown that alimentary substances are dissolved as rapidly in an artificial digestive fluid consisting of pepsin and a properly diluted muriatic acid, as in the gastric juice itself.

He next takes up the subject of milk, and after alluding to its composition, shows that it is much affected by temperament, nutrition and disease, so as to act in an injurious manner on the child, even where the nature of the change cannot be determined. The subsequent chapter on secretions of mucous membranes is of much interest in a practical point of view, as it points out the best methods of distinguishing pus or purulent mucus from pure mucus, in the expectoration of persons suffering from diseases of the respiratory organs. Passing over the chapters on sweat, fat, &c., we come to the most important portion of the work, that on the urine. From its great length and the copiousness of its details, it is impossible to present even an analysis of it in a notice of the character of the present, but it will amply repay an attentive study, and proves most clearly that the profession has neglected this source of diagnosis, and suffered it to fall into the hands of ignorant empirics. Although it will require a long series of observations to perfect what has been so ably begun by Dr. Simon, he has fully shown that there is scarcely an abnormal change that occurs in the fluids of the body, that is not followed by some characteristic change in the urine. As respects the kiesteine in the urine of pregnant women, after noticing at some length the experiments of Dr. Kane published in this Journal in 1842, he shows that the formation of this pellicle, although generally indicative of pregnancy, is not always so, as it has taken place in the urine of women who were not impregnated, is not uncommon in amenorrhœa, and even occurs in the urine of males.

The chapters on feces and vomiting are of much less interest, as is also the case with those on the bones, cartilage, muscle, &c., but that on concretions is highly valuable, as it points out the composition of vesical and renal calculi, the best methods of ascertaining their character, even by persons little skilled in chemical manipulation. The work concludes with a short view of the fluid products of disease, and an appendix containing further analyses of many substances previously mentioned.

Such, in a few words, are the subjects treated on by the author, and it will be perceived that he has occupied a wide field of observation, and it is much to be regretted that his untimely death prevented us from profiting by the extension of his researches so ably commenced. His observations, as now presented, are calculated to lead to very important results, and throw much light on many of the most obscure departments of physiology, and we feel satisfied that they will tend to encourage a taste for a science, from whose cultivation and development the medical art will derive the utmost benefit.

R. E. G.

ART. XIX.—*Fifth Annual Report of the Registrar-General, of Births, Deaths, and Marriages, in England.*

IN a bibliographical notice of the above-named work, formerly presented in this Journal,\* we promised to enter into some examination of the letter of Mr. Farr, the able statistician, which accompanies, and greatly enhances the interest of the Registrar-General's valuable publication.

In the appendix to the first report, noticed in this Journal for October, 1842, (vol. iv. p. 426,) the proportion of deaths in towns and the open country, was a subject of investigation, and the much higher rate of mortality in towns referred to. The inquiry at that time necessarily left some uncertainty in the results, since it was founded upon the returns of half a year, and the population could only be estimated on an assumed rate of increase. The census taken subsequently furnishes a mass of facts which afford more ample means of determining the influence of the density of population on the causes of death. For the sake of comparison with the appendix of the first report, Mr. Farr takes the same districts and pursues the same order.

He informs us that the country population in 1841, of the districts in the counties of Essex, Norfolk, (except Norwich,) Suffolk, Gloucestershire, (except Bristol and Clifton,) Herefordshire, and Westmoreland, an area of about 9,352 square miles, was 1,700,484. The deaths registered in the four years, (1838-41,) were 132,116.

The town population of Birmingham, Aston, Bristol, Clifton, Manchester, Salford, Liverpool, West Derby, Cambridge, Carlisle, Derby, Dudley, Exeter, Leeds, Leicester, Maidstone, Newcastle-on-Tyne, Sunderland, Northampton, Sheffield, Stoke-on-Trent, Wolstanton, and Wolverhampton, covering 666 square miles, was 1,883,693; and the deaths in four years 205,966.

The population of the town being greater than that of the country districts, as a correction of this excess, Mr. Farr strikes off 20,000 from the deaths in the former districts, and makes the mortality in the same time and out of the same population 132,116 in the country districts, and 185,966 in the towns, showing an excess in the latter of no less than 53,850 deaths.

In another statement he includes the mortality of London, inhabited, in 1841, by the enormous population of 1,875,493. The deaths in the metropolis are compared with those in the South-Western Division, comprising, in Wiltshire, Dorsetshire, Devonshire, Cornwall, Somersetshire, a population of 1,740,017. The deaths were 189,927 in the metropolis, and 130,298 in the South-Western Division; and after the same kind of correction as before, the result is of the same character, the deaths being in the same time, (4 years,) out of the same number of inhabitants, 130,298, in the rural, and 176,210 in the metropolitan districts.

The excess in the London mortality amounts to 45,912. Taking the same po-

\* See Amer. Journ. of Med. Sciences, for Oct., 1845, p. 390.

pulation, a greater number died in 3 years in the metropolitan districts than died in 4 years in the south-western districts. If the mortality were reduced to the same rate, there would be 13,700 less funerals in the metropolis every year.

By combining the two series of observations just noticed the following aggregate numbers are obtained:—

Deaths in the same time (4 years) out of the same population 262,414, in the country districts, and 362,176 in the town districts. Difference, 99,762.

During the 10 years which embrace the 4 years included in this calculation, the annual increase of population was 2 per cent., in the town, and only 8 in the country districts. One in 52 died in the country, 1 in 37 in the town districts. The mortality in the dense districts was to that in the less dense districts as 140 to 100.

Mr. Farr presents tables constructed to show the causes of death in the country and town districts. From these it appears that the deaths by diseases of the zymotic class were 86,835 in towns, 45,114 in the counties; and the annual mortality was 6013 and 3422 in a *million* living. "By small-pox the mortality was twice as great in the town as in the country districts (1045 and 507); 8348 persons died by this disease in the '25 towns,' (so they may be called for shortness,) and 6739 in the metropolis, 3844 in the south-western division, 2834 in other counties. The mortality (in a *million*) by *measles* was 914 in the town districts, 364 in the country; by scarlatina (including putrid sore throat) 988 and 478; by hooping-cough 829 and 415. Thus 8465 (children chiefly) died of *measles* in the '25 towns,' 2774 in the south-western division, 4729 in the metropolis, and 2021 in the 'eastern, &c., counties;' 7627 died of scarlatina in the '25 towns,' and 2499 in the south-western division, 6640 in the metropolis, and 3801 in the eastern and other counties. *Hooping-cough* was fatal to 11,975 in the metropolis and the '25 towns,' to 5468 in the south-western division and the other counties. *Croup* was much more fatal in the '25 towns' than in the metropolis, while it was the reverse with 'thrush;' and neither of these maladies was much more fatal in the town than in the country districts. The mortality of diarrhœa, dysentery, and cholera together, was 385 in the towns, 196 in the country. Typhus (comprising the cases returned as *fever*) was fatal to 18,111 in the town, 13,159 in the country districts: the mortality (to a *million* living is understood throughout) was 1254 and 997. The mortality from erysipelas was 133 and 53; from syphilis (as returned) 18 and 10 in a million. *Twenty-eight* died of *hydrophobia* in the towns, 10 in the country districts.

"The diseases of 'uncertain or variable seat' were nearly equally fatal in the two classes of districts. More deaths were referred to scrofula and carcinoma in the country than in the town districts; 13,194 persons died of dropsies in the town, 12,040 in the country districts.

"The mortality by cephalitis and hydrocephalus together was 1143 in the town, 445 (to a *million*) in the country districts. Hydrocephalus destroyed 4409 children in the country, 12,656 in the town districts. The mortality by convulsions was 852 in the country, 2090 (to a *million*) in towns, where tetanus and chorea were also most fatal. The mortality by apoplexy and paralysis, as well as the 'sudden deaths,' in which inquests were held, was not much greater in towns than in the country. By phthisis the mortality was 4463 (in a *million*) in the town, 3660 in the country districts: or 64,449 persons died by this disease in the town, 48,252 in the country districts. The mortality by phthisis was raised only 22 per cent. in the town districts; but in this destructive disease the increase produced 16,197 deaths, while the *total* deaths from typhus in towns was but 18,111, and the excess over the deaths from that disease in the country 4952. Sufficient attention has perhaps not been paid to the great excess in the mortality of consumption, caused by the insalubrity of towns. The deaths by bronchitis, pneumonia, asthma, and other diseases of the chest, were raised from 21,966 in the country to 50,609 in the town districts; the mortality was 1667 and 3504 in a million.

"The annual mortality by diseases of the *digestive organs* was 1972 (to a *million*) in the town, 1042 in the country districts; but of this mortality 616 and 120 were referred to *teething*, a disproportion which, vague as the term is, serves to indicate the greater degree of irritation and danger of dentition in towns. Enteritis and

peritonitis were nearly twice as fatal in the town as in the country districts. *Tubercles mesenterica* was, like consumption, increased only about one-fifth in the town districts. By liver disease and hepatitis the mortality was 171 in the country, 233 in the town districts; by jaundice 55 and 57. The mortality by diseases of the urinary organs was 101 in the country, 117 in the town districts. The mortality by diabetes was the same (13); by stone and gravel 12 in the country, 16 in the towns. The excess was perhaps caused by the resort of patients to the hospitals in towns.

"Of childbirth, 3195 women died in the town, 1806 in the country districts; the excess in towns was more than 1000 lives; the mortality was 221 and 137

"The mortality by rheumatism was greater in the towns than in the country (72 and 48); so was the mortality by diseases of the joints.

"The mortality ascribed to intemperance was 19 in the town, 7 in the country districts; to starvation, 15 in the town, 8 in the country districts.

"I have pointed out only some of the more important results in the table, which should be carefully studied in all its details.

"The essential character by which we have been guided, in classifying districts under the head of 'town' or 'country,' is the density of the population, which can be expressed numerically by the 'population to a given area,' or the 'area to each person.' Whether the population possesses any privileges, is incorporated, or is under any peculiar jurisdiction; whether the place is technically a 'city,' or a 'town,' or neither, has not been considered; if the district have a considerable population, living in close proximity, it is considered a 'town' district. The Registration Districts are generally single parishes, or parishes united for the relief of the poor, and were formed by the Poor Law Commissioners with this object exclusively in view; hence the 25 town districts comprise entire parishes, which have all the character of country districts, and the municipal boundaries themselves frequently take in open suburbs and spaces. The width of the streets, and the relative area covered with dwellings, also differ in the same city.

"Upon the other hand, the 'country' districts in the table comprise all the towns in Wiltshire, Devonshire, Dorsetshire, Cornwall, Somersetshire, Norfolk (except Norwich), Gloucestershire (except Bristol and Clifton), Herefordshire, and Westmoreland. The terms 'town' and 'country' districts must consequently be understood in this inquiry to designate prevailing and not exclusive characters. The comparison is instituted between denser and less dense, not between the densest and most scattered populations in the kingdom. Assuming that the mortality is increased as the population grows denser, the mortality of the class of 'town districts' is less, that of 'country districts' greater, than it would be if the population were exclusively of the kind by which the class is characterized. The effect of the agents is understated by which the disease and mortality of towns are caused. It is as if the specific gravities of two masses of metal were compared, the one containing eight, the other two parts in ten of gold; the ratio of the specific gravities of the two masses would be less than the ratio of the specific gravities of the pure metals; but a comparison would prove incontestably that gold was by far the heaviest.

"Without any difficult analysis this broad irrecusable result is then obtained from an immense number of facts, that certain diseases are much more fatal, and that the mortality is much greater in towns than in the open country."

Mr. Farr proceeds next to inquire into the agencies leading to the production of diseases which prove so much more destructive in towns than in the country. He traces the disease of towns to groups of causes, and of these gives a partial analysis.

"Experience," he observes, "has shown that there are certain things which may be called necessities of life: they are the produce of labour, and possess a variable value; a portion therefore of every population, savage or civilized, cannot procure them, and is subject to privation in different degrees. These necessities are—(1.) Water (beer, wine, tea, coffee); (2.) Food (meat, bread, fruits, vegetables); (3.) Physic; (4.) Clothing; (5.) Firing (light); (6.) Lodging; (7.) Cleansing, (washing, sewerage.) The relative value of these articles is represented by their price, which varies at different times and places, but the price does not express the relative facility of procuring them, that is, of procuring,

in the wide sense of the word, subsistence. The facility is expressed by the ratio of the earnings of a family to the cost of its subsistence; for if the seven necessaries of life cost 100*l.* in one place and 120*l.* in another, and the earnings are also 100*l.* and 120*l.* respectively, each family would have a competency. But competency, comforts, and the *aisance* of the French, significant as they are, cannot be expressed numerically; they may be understood differently; in a sanitary inquiry, we may, therefore, substitute for them the value of this ratio, which can be determined and expressed in terms to which all would attach the same signification. Let it be assumed, for a moment, that a sufficient supply of the necessaries of life can be procured by a family for 100*l.* a-year, what would be the effect of reducing the earnings of the family to 50*l.* a-year? We know, by observation, that it does not reduce the duration of life in that ratio; and the reason is, that by the substitution of a coarser and cheaper food hunger can be appeased, and the body supplied with sufficient nutritive matter, or matter which may serve as a substitute for those more grateful combinations, which the appetites and experience seem to point out as the best food for mankind. The animal food in the case supposed, would be replaced by bread or potatoes; the beer, tea, and coffee, would be weaker; in illness medical advice would be less frequently obtained; the clothing would be coarser and scantier; the fire smaller; the lodging less spacious; cleanliness would be less attended to. I take here the average results, which would vary in different circumstances with the desires of the people and the products of the place. A reduction from abundance to a rude subsistence would probably be attended by a reduction in the mean duration of life, but to a comparatively small extent. A low standard might, however, be fixed upon, any fall below which would be accompanied by a certain reduction of the mean life of the people."

Among other influences affecting life and health, Mr. Farr refers to those of soil and atmosphere, climate and seasons, winds, temperature, hygrometricity and electricity.

In relation to another class of agents, namely, atmospheric impurities, organic matter undergoing decomposition, and the contagious principles of zymotic diseases, our author makes the following observations:

"The atmosphere, besides oxygen and nitrogen, contains carbonic acid and aqueous vapour. The mean proportion of carbonic acid is 49 volumes in 100,000 volumes of air, according to the younger Saussure; who also states that it varies from 37 to 62 volumes. Mr. Coathoupe has estimated the quantity of air which passes through the lungs of a man of ordinary size in 24 hours at 267 cubic feet, of which nearly 8 per cent. by volume, or 21 feet, are exchanged for carbonic acid; the bulk would be equivalent to a cube of 6.4 feet. If, for a mere illustration, we assume that on an average 16 cubic feet of the gas are thrown off from the skin and lungs of each person, 30 million cubic feet will be exhaled daily by the population of the metropolis, distributed over an area of about 1951 million square feet. Add the amount of the same gas formed by animals of every kind, —fires, lamps,—and multiply the sum by 100, inasmuch as respiration for several hours in air which contains 1 or 2 per cent. of carbonic acid has been found to produce alarming effects (Broughton), and it will be seen that the amount of air vitiated in the metropolis, by this element alone, is by no means inconsiderable.

"Is the excessive mortality, then, in towns, to be ascribed to the accumulation of carbonic acid, or of any other similar gas, which is so rare, as to be innocuous in open districts? It was natural, when it had been discovered that carbonic acid mixed in air destroyed animals, and after the accidents in mines and close chambers had been traced to this agent, to ascribe the excessive mortality of towns to the same cause. Further investigation must show, I think, that it has but a small share in raising the mortality of towns, the provision for its dispersion is so complete."

Connected with this topic Mr. Farr has communicated some facts and observations relating to the diffusion of gases in the atmosphere, of such exceeding interest that we cannot refrain from laying some of them before our readers.

"Dalton discovered that carbonic acid entered the space occupied by hydrogen

in the same proportion as if no hydrogen had been present. He inferred that gases do not, like liquids, exclude each other, and this is now admitted. So that if an air-tight chamber full of carbonic acid communicate with the external air, the same quantity of air will find its way into the chamber as if no carbonic acid gas were present; and if water were introduced, the same amount of aqueous vapour would occupy the space as if neither gas were present. The elasticity and density of the atmosphere of the chamber would be the sum of the densities and elasticities of all the gases and vapours present. It has been assumed here, to simplify the statement, that while the atmospheric air entered the carbonic acid gas remained; but it would in fact go out, for the same reason that the air entered in order to set the gases without and within in equilibrium."

A great deal of light has been thrown upon the subject of the rates at which gases are diffused through small apertures and porous substances, by Professor Graham, in his Elements of Chemistry. To form an idea of the dispersion of the carbonic acid gas generated in towns, according to the pneumatic law, "assume," says Mr. Farr, "that 1000 cubic feet are formed per second; it will be equal to a cube of 10 feet. Now if this volume of carbonic acid were in the centre of a vacuum, it would disperse in every direction at a velocity of 1049 feet a second. It is nearly the velocity of sound. A particle would fly a mile in 5 seconds, 12 miles in a minute. The velocity of a 'high wind' is 50 feet a second, 'a hurricane that tears up trees' 147 feet a second—one-seventh of the velocity with which carbonic acid rushes into a vacuum. If the gaseous film evolved every second over the area of the metropolis were pure air, it would only move slowly away, by the impulse with which it was thrown off, and because it was lighter than the atmosphere; but as it is carbonic acid, the surrounding atmosphere is a vacuum, into which its rush is opposed only by the small quantity of carbonic acid gas existing, and the sluggishness of the aerial particles. The rapid removal of this gas from cities is effected by a force much greater and altogether independent of the winds. It is carried rapidly through the air, until it is fixed again by vegetation and exchanged for oxygen, which flows into the atmosphere of cities, according to the same law, to replace the oxygen consumed.

"These results are confirmed by chemical analysis of the air. The differences in the quantity of carbonic acid in winter and summer, night and day, are considered by Dumas due to more of the gas being absorbed, retained, and brought down by rain in cold than in warm weather. They are meteorological changes extending over all the atmosphere. Chemists have hitherto failed to detect any excess of carbonic acid gas in cities. A commission is now sitting in Paris, engaged in the analysis of the atmosphere by Dumas' method, which is held to yield the most accurate results. I am not aware that the air of any place in England has been analyzed by the new method; but the observations in other countries show no diminution of oxygen in the city air. Thus the oxygen was to the nitrogen in the air of Paris as 230.0 to 770.0 (by weight); and on Faulhorn, in Switzerland, 8767 feet above the level of the sea, as 229.7 oxygen to 770.3 nitrogen.

"Carburetted hydrogen and sulphuretted hydrogen arising from graves are less dense, and are dispersed more rapidly than carbonic acid: scarcely a trace of them can be detected.

"Carbonic acid and other noxious gases can, as is well known, be confined for a time in well closed apartments, and oxygen can be excluded; but the dispersive force is so great that chemists have seldom succeeded in detecting any difference in the proportions of the gases, even in the air of crowded hospitals. If any difference exist it must be small, and might have a slight effect on health; but, as the experience of our collieries proves, would not raise the mortality to anything beyond a fraction of 40 per cent.; besides, the country is exposed as well as the town population to the influence of deleterious gases in the close chambers of small cottages.

"It is, then, to matters suspended in the atmosphere of cities that the excessive mortality must be referred. Smoke is heated gas, carrying with it unburnt particles in suspension; the carbonic acid is scattered immediately by its diffusive velocity, and the particles of solid matter, carried up by the heated air into the sky, disperse, become invisible, and fall around insensibly, in a clear atmosphere, or

at a distance when there is any wind. If the air contain watery vesicles also in suspension, the column of smoke ascends but a little, the carbonic acid is absorbed, the carbon imbibes water and air, it mixes with the watery cloud, and all the phenomena of a London fog are produced. These fogs are caused, apparently when the temperature of the Thames is higher than the temperature of the air, which is calm (or if there be any wind it is nearly saturated), the fogs generally disappearing as the temperature of the air is raised by the sun.

"That the smoke is irritating to the air-passages, injurious to the health, and one of the causes of death, to which the inhabitants of towns are more exposed than the inhabitants of the country, is exceedingly probable; but if the effect were very considerable it would be most evident in the dense fogs, when the atmosphere is loaded with smoke, and breathed for several consecutive hours by the population—men, women, and children. Now we have never observed any connection between the increase of the mortality and the London fogs. The diseases, again, caused by smoke must be of a mechanical nature; and affect the lungs and air-passages; it may increase the pulmonary diseases; but will assuredly not produce scarlatina, measles, typhus; and other diseases which prevail in towns.

"There is another class of agents. In a school-room, say there are 100 children: a child is introduced for a few hours, in a state of scarlatina. The children have not had the disease before: 10 of them are affected. If 10 children with scarlatina were introduced, and the room were ill ventilated, 30 or 40 of the children might be affected. If instead of scarlatina, the sick children had small-pox, measles, or hooping-cough, these diseases would be communicated. If dysentery, cholera, typhus, and plague patients are frequently introduced into barracks or workhouses, a certain proportion of the inmates are affected. The numbers who are attacked by an infectious disease depend upon—1st. The susceptibility of the person exposed (if the children in the school-room had previously had small-pox, or been vaccinated, a very small proportion of them would be infected by breathing the small-pox atmosphere); 2dly, on the strength of the zymotic matter, which varies in the stages and forms of the several diseases; and 3dly, on the density and ventilation of the room. If 100 healthy were placed in a room in immediate contact with 10 sick persons, if the room were small, the doors and windows closed, the greatest number possible would be infected; and if they went through the disease in the same circumstances, the mortality would also be the greatest possible. So if public buildings, in which crowds of people assemble, were well supplied with pure air, it would be quite safe to resort to them; but as opera houses, theatres, concert rooms, lecture rooms, Exeter-hall, chapels, churches, and large workshops, are not yet provided with the proper mechanical means of ventilation, and the air is not withdrawn, the very walls reek, when filled with the breathed atmosphere; and if any epidemic, such as influenza, be rife, several persons affected with the complaint are present, and great numbers are infected; the headache and oppression which come on are the first and often not the last symptoms. This is literally 'taking poison;' but it is generally called 'taking cold,' through the common prejudice of ascribing all our maladies to sensible causes. It is a vulgar error to suppose that rooms are healthy when they are not hot; but the heat which is generated may increase the effect of the zymotic matter.

"Certain substances, then, taken from the bodies of the sick, produce, when introduced into other bodies, a series of specific phenomena, developed according to a determined type: *Varioline* (small-pox matter), for instance, produces small-pox. These substances have the same relation to diseases as ferments have to well-known chemical processes. Several of them will float in the air, and form a morbid atmosphere, the density of which will be in proportion to the proximity of the bodies by which it is given off, and to the greater or less facility for escape. The 267 feet of air passing through the lungs daily, if charged with these particles, will bring them into contact with the blood.

"What are the physical properties and chemical nature of these morbid particles? Chemistry has left us much in the dark; but, until English chemists enter seriously upon the investigation of this subject (which is of inconceivable importance), we may accept the well-supported hypothesis of Professor Graham:—



“Of the odoriferous principles of plants, the miasmata of marshes, and other matters of contagion, the presence, although sufficiently obvious to the sense of smell, or by their effects upon the human constitution, cannot be detected by chemical tests. But it may be remarked, in regard to them, that few or none of the compound volatile bodies we perceive entering the atmosphere could long escape destruction from oxidation. The atmosphere contains, indeed, within itself the means of its own purification, and slowly but certainly converts all organic substances exposed to it in simpler forms of matter, such as water, carbonic acid, nitric acid, and ammonia. Although the occasional presence of matters of contagion in the atmosphere is not to be disputed, still it is an assumption without evidence that these substances are volatile or truly vaporous. Other matters of infection, with which we can compare them, such as the matter of cow-pox, may be dried in the air, and are not in the least degree volatile. Indeed, volatility of a body implies a certain simplicity of constitution and limit to the number of atoms in its integrant particle, which true organic bodies appear not to possess. It is more probable that matters of contagion are highly-organized particles of fixed matter, which may find its way into the atmosphere, notwithstanding, like the pollen of flowers, and remain for a time suspended in it; a condition which is consistent with the admitted difficulty of reaching and destroying those bodies by gaseous chlorine, and with the washing of walls and floors as an ordinary disinfecting practice.”

“It is quite certain that animal matter is exhaled from the pulmonary and cutaneous surface. The particles are small and rare; but, according to Graham, they are inelastic; they are without that diffusive force inherent in gases and vapours, and will therefore only spread through the air like vesicular water, or fine dust over smooth water. When the breath is expired in winter, it passes from the lung with a certain force; but it instantly separates into two portions; 1st, the carbonic acid, which would fall to the ground if it were not dispersed in all directions by the diffusive force; and 2dly, the aqueous vapour which ascends with the heated air and particles of animal matter; the air to mix in air, the water to be condensed, fall, or be dissolved, and the animal matter to fall or be decomposed in the air. In a crowded room; theatre, or church, the carbonic acid is dispersed through every aperture at a retarding velocity. The gas exhaled from the body does not ascend to the roof, as is sometimes supposed, when the temperature of the room is low, for the specific gravity of carbonic acid is 1.524; and, as the density is inversely as the volume, and gases expand 1.493d part (Rudberg) for every degree of Fahrenheit, it only becomes lighter than air when 258 degrees hotter. The carbonic acid emitted from the lungs at a temperature of 100° is more than a third heavier than air at 40°: its density is 1.359, air being 1.000. The carbonic acid emitted by the candles or gas of a room is more than as light again as air; but it soon cools and grows heavier, and, if it were not for the diffusive force, would fall to the ground. Gases cannot remain permanently at the upper or lower parts of any apartment or edifice in other proportions than they exist in the atmosphere; it would be easier for a torrent to stand still on a mountain declivity. They disperse, and can only accumulate around their source when generated rapidly. But particles in suspension—inelastic, smaller, lighter than motes in the sunbeam—stagnate in air, and can only fall to the ground, or be carried away with the fluid in which they float. Smoke and organic matters are removed from a room in the same way—by replacing all its gaseous contents; and particles of both are left adherent to the exposed surfaces, but in quantities smaller in proportion to the velocity of displacement.

“Every population throws off insensibly an atmosphere of organic matter, excessively rare in country and town, but less rare in dense than in open districts; and this atmosphere hangs over cities like a light cloud, slowly spreading—driven about—falling—dispersed by the winds—washed down by showers. It is not *vitalis halitus*, except by origin, but matter which *has lived*, is dead, has left the body, and is undergoing by oxidation decomposition into simpler than organic elements. The exhalations from sewers, churchyards, vaults, slaughter-houses, cesspools, commingle in this atmosphere, as polluted waters enter the Thames; and notwithstanding the wonderful provisions of nature for the speedy oxidation of organic matter in water and air, accumulate, and the density of the poison (for

in the transition of decay it is a poison) is sufficient to impress its destructive action on the living—to receive and impart the processes of zymotic principles—to connect by a subtle, sickly, deadly medium, the people agglomerated in narrow streets and courts, down which the wind does not blow, and upon which the sun seldom shines.

“A small quantity of organic matter can only escape with the carbon and aqueous vapour, (37½ ounces daily, according to Dalton,) from the skin and lungs. The presence of a putrid atmosphere is too evident in parts of all towns; and Liebig, by operating on large masses of the atmosphere, has obtained ammonia, which is a product of the putrefaction of animal matter. The existence in the atmosphere of organic matter is, therefore, incontestable; and as it must be most dense in the densest districts, where it is produced in greatest quantities, and the facilities for decomposing it in the sunshine and sweeping it away by currents of wind are the least, its effects—disease and death—will be most evident in towns, and in the most crowded districts of towns.

“It is to this cause, it appears to me, that the mortality of towns is to be ascribed; the people live in an atmosphere charged with decomposing matter, of vegetable and animal origin; in the open country it is diluted, scattered by the winds, oxidized in the sun; the vegetable world—the great organizer—incorporates its elements, so that, though it were formed, proportionally to the population, in greater quantities than in towns, it would have less effect comparatively. The means of removing impurities in towns exist partially, and have produced admirable effects; but the most casual observation must convince any one that our streets were built by persons entirely ignorant of the nature of the atmosphere, and of the mortality which has been proved to exist, and is referable to causes which, though invisible, are sufficiently evident. Every one feels the difference of the polluted and the pure atmospheres described by Milton:—

“As one who long in populous city pent,  
Where houses thick and sewers annoy the air,  
Forth issuing on a summer's morn, to breathe  
Among the pleasant villages and farms  
Adjoin'd, from each thing met conceives delight.”

“I shall proceed to show, not only that the mortality is greater in the town than in country districts, but that the mortality of town districts has a certain relation to their density. The relation exists strictly between the density of the organic particles suspended in the atmosphere and the mortality; but the density of the matter in the air cannot be determined directly, for obvious reasons; and with the same number of persons on a square mile, the number of particles in the atmosphere will vary in different districts, according as the means of removing the refuse matter, by sewerage and other means, are more or less efficient. Still by taking districts, which, if the circumstances are not quite the same, and the populations are not entirely homogeneous, will yet have atmospheres which bear a certain specific relation to the numbers living on the same area, it will not be difficult to obtain interesting results.

“It is proved beyond doubt that, if the population be the same in other respects, an increase of density implies an increase of mortality; and that the ratio of increase in the mortality is as certain roots of the density. If a further and more extended inquiry, into which I have not time now to enter, should confirm the principle that the ratio of mortality in towns is as the 6th roots of the density of the population, it will be time enough to ask why this should be the particular ratio. But the chemists must first discover means of determining the density of the atmosphere of organic matter, which may be called the *zymotic atmosphere*, in different districts. The density of population is no strict measure of the density of the zymotic atmosphere; nor, admitting that the matter is a poison, does the relative density of the population express the relative doses inhaled in a given time; if it did, it is improbable, and contrary to all analogy, that the mortality should increase in the simple ratio of the dose. The exact effect of increasing doses of poison has not been accurately determined; but it is well known that small doses of all poison are taken with impunity, and that the dose of arsenic, opium, or prussic acid, may be increased up to a given point, at which the dis-

case produced is severe or fatal. Four drops of prussic acid, diluted, may be taken with safety, when four drops more would be fatal to a certain number of persons. How large, or, rather, how small, the dose of matter may be which will produce a zymotic disease it is impossible to say; but if a minute diluted charge of *vaccinæ* (vaccine lymph) produce cow-pox, say one time in 1000, it would be an interesting problem to determine, by doubling the quantity, in what ratio the proportions infected increased."

It is interesting to find—as we do by the following quotations—that the labours of those who have recently devoted so much attention to vital statistics and the laws of hygiene, have been put to profit and already furnished important results. "We hope," says Mr. Farr, "to be able to determine more exactly, at some future time, the effects of the different external agents on health and the duration of life; in the meantime the results already obtained suggest two or three practical inferences, which I respectfully submit to those authorities who have the means of carrying sanitary measures into effect, not as new, but as resting on a more extended series of observations and calculations than have before been at our disposal."

"The Appendix to the First Report of the Registrar-General had the following statement, which is borne out by the experience of the four subsequent years:—

"The mortality of cities in England and Wales is high, but it may be immeasurably reduced. A good, general system of sewers, the intersection of the dense, crowded districts of the metropolis by a few spacious streets, and a park in the east end of London, would probably diminish the annual deaths by several thousands, prevent many years of sickness, and add several years to the lives of the entire population."

"This passage, with others in the interesting sanitary reports of Drs. Arnott, Kay, and Southwood Smith, with the facts disclosed by registration, were brought by the inhabitants of the Tower Hamlets under the notice of her majesty's government, who, in a very liberal and kindly spirit, introduced a bill into Parliament, and carried it, for founding the Victoria Park, 'in the east end of the metropolis.' Wider streets have already been carried through the densest districts, and others are projected. The results which have been arrived at in this paper justify and confirm in the fullest manner the utility of these measures, and will, I hope, lead to their extension, not only in the metropolis, but in the other cities of the kingdom."

"All 'improvements' disturb property, and injure individual interests; they are, therefore, not only attended by expense, but open to positive objections, over which it can only be shown that the advantages preponderate. The 'improvements' which followed the fire of London were obtained at the expense not only of much loss of property and inconvenience, but of suffering and death; still if the result was the annihilation of plague, which destroyed not the houses but the lives of the great mass of the population every 10 or 20 years, drove the court and parliament from London, spread through the kingdom, paralyzed trade, and was attended by a host of less dreaded maladies, which cut short and embittered life, the improvements were cheaply purchased. It has been stated that the narrow streets and overhanging houses, which are so common on the continent, and of which too many traces remain in England, were erected in close contiguity, among other reasons, for the sake of the shade which they afforded in summer and the shelter in winter; it is now known that this form of building, by obstructing the sunshine and atmospheric currents, is the cause of innumerable diseases; and notwithstanding the cost of any extensive alterations, there can be no doubt that it would be repaid by the amelioration of the health of present and future generations. To take down and rebuild whole districts at once is however impracticable; the best that can be done in the circumstances is to cut open spacious streets, which will carry and produce streams of air through the densest parts, drawing lateral currents from the adjacent narrow streets. Upon visiting some of the unhealthiest districts of the metropolis, I was greatly struck with the number of courts, or streets shut up at the end, particularly in the city of London without the walls; as the wind cannot pass through them, it is evident that the inelastic matter so abundantly generated must be slowly replaced: why should not all these courts be opened at once? Compared with other 'improvements,' the expense would be inconsiderable."

“The inhabitants of the ‘rookeries,’ which have been recently taken down; are dispersed. It would be interesting to know in what places they are now located. The objection to these measures is that ‘you take down the dwellings of the poor, build houses in their places for which the middle classes only can pay, and thus by diminishing the amount of cheap house accommodation increase the rents, and aggravate the evil which you attempt to cure.’ It is undoubtedly much easier to displace than to regenerate such a population—the sediment of vast cities which sinks into these obscure receptacles. If the displacement be gradual, however, the inconveniences of removal are diminished: and on a small scale it can have no influence on rents; the people dispersed, if they obtain money obtain houses; or they return, like the Irish, home. The working classes, also, in cities, get lodged in the larger houses of the tradespeople and merchants, who from the facilities afforded by omnibuses and railways, live every year in greater numbers out of town.”

As a branch of this important subject, Mr. Farr has thrown out many useful suggestions relative to the means to be employed for the diminution of the quantity of animal and vegetable matter introduced into the atmosphere of towns. These suggestions result from most extensive observation, and are contained in a volume which does not admit of republication in this country. Hence the importance of condensing as much of the most valuable matter as possible, in the pages of this Journal, where it will be read with interest and remain for future reference.

The chief city impurities are the halitus, from the breath and perspiration of living animals, the gases from privies, burial-grounds, slaughter-houses, streets and sewers. As the halitus cannot be diminished in quantity, the only thing that remains to be done is to prevent its undue accumulation in particular places. “While,” he observes, “the collection of persons in Robert Owen’s parallelograms, and in public institutions, barracks, large schools, prisons, workhouses, must necessarily lead to the concentration of the respiratory excretion, particularly when, as is too frequently the case, the sleeping apartments are crowded, and no mechanical means are employed to facilitate ventilation. A public institution in the periodical epidemics, is like a town without party walls in a fire; nothing but a natural immunity can prevent the propagation of the zymotic action through the imprisoned atmosphere from person to person. According to the common estimate 4 in 100 persons are sick, and as in the metropolis there are 75 persons in 10 houses, one person in every fourth house must be ill, while one in every eighth house will be afflicted with a disease of considerable severity. When the house is small and perhaps dirty, it appears a great act of charity, to remove a poor man suffering from fever for instance to a hospital, where he is provided not only with skillful medical attendance and with physic, but with the proper diet, warmth, and nursing. But have the benevolent founders of hospitals, and the medical officers who generally give their services gratuitously, sufficiently reflected on the probable consequences of bringing 50, 100, 300, or 400 sick bodies under the same roof, and into a few wards, which the sickly breath saturates? Have they ever compared the results of cases treated in hospitals, and in the poorest homes? When hospitals are crowded the increase of mortality soon becomes striking. In the Hôtel Dieu at one time 1 in every 4 persons who entered the pestilential walls died.\* The mortality in the large metropolitan hospitals is twice as great as in the smaller country hospitals. This, cannot be ascribed to defect of medical skill in the metropolis. Erysipelas and gangrene are still not infrequent in hospitals. The patient is fortunate who escapes phlebitis, or purulent deposits, after any serious operation in a hospital. It is the adventitious disease, and not the knife of the surgeon that is fatal. Are not the effects of general hospitals of the same nature as those that have more than once led to the evacuation of lying-in hospitals? I have rarely seen any statement of the mortality of cases of fever, small-pox, or any other disease, in which it did not appear that a person was twice as likely to die in a hospital as a person suffering from the same disease out of doors. It is generally said, ‘Yes, we admit that our mortality is high,

\* Il périssait (18th century) le quart de ce qui y entraît et la moitié du reste n’en sortait qu’après avoir échangé une maladie en elle-même de peu de durée contre une langueur sans remède.—*Eloges Historiques par Cuvier—Tenon.*

but the worst cases are sent in the last stage of disease to the hospital.' Is this certain? Will it account for all the difference? I doubt it very much. And I express the doubt in the hope that the question may be strictly, honestly, and conscientiously investigated by some qualified person who has time to devote to the subject. Until this be done no attempt should be made to extend the system of assembling the sick in the same buildings.

"The expense of a patient in a hospital is from 7s. to 14s. a-week. What would be the effect of allowing a married man in a fever the same nutriment at home, supplying him with an occasional nurse, and seeing that his house was well cleaned and kept comfortable? He would be surrounded by those who from natural affection loved him, instead of the dying and dead in the wards of a hospital. Would not this be an excellent season, too, for religion, charity, and science, to instil just principles and habits into the families of the poor—to ameliorate their homes—so that the sick man may not, as when he returns from a hospital alive and convalescent, be exposed to the same agencies as produced the previous attack?

"The space allotted to the sleeping-rooms of many public institutions in towns is too small. It should in no case be less than 8 feet cube (=512 cubic feet) to each person, with proper apertures for the removal of the breath. If the air were removed twice as fast, a room of 500 cubic feet would be equal to a room of 1000 cubic feet; but it is a difficult matter to remove air from a room with a double velocity—more difficult and expensive than to make the rooms, at least on land, of sufficient extent in the first instance. The influence of a too limited space, if carefully investigated, would no doubt show a certain inverse relation between the mortality and the space, a death marking every degree of concentration of the expired atmosphere. The families of many artizans who get good wages lodge in a single small room, the rent of which is equal to that of a cottage in the country. This is a miscalculation on their part; on coming from the country they get in town higher wages, and could afford to pay for more expensive lodgings, but finding they can live in one room, do so, expending the surplus wages on dress, beer, and better food. To forego these would be to curb a natural appetite; they are only reminded of the want of room and pure air by a slight present uneasiness, and discomfort. As they do not trace to their causes the deaths of their children, and disabling, dangerous attacks of sickness, they are led to look upon these events as inevitable. With the limited income at his command, a working man in a town is compelled to practice self-denial; now calculation shows that clean spacious lodging is one of the most necessary things to the maintenance of health, and that it is safer to refrain from other things, such as beer, which, though nutritious, is not indispensable, while spirits are more frequently injurious than beneficial.

"Cuvier's definition of life, or of a living thing, in its utmost generality, is so strictly applicable to a city, that 'London' may be substituted for '*la vie*' in the following sentence:—

"'*La vie est donc un tourbillon plus ou moins rapide, plus ou moins compliqué, dont la direction est constante, et qui entraîne toujours des molécules de mêmes sortes, mais où les molécules individuelles entrent et d'où elles sortent continuellement, de manière que la forme du corps vivant lui est plus essentielle que sa matière.*'<sup>2</sup>

"It would be easy to draw the parallel. But I wish merely to remark, that if it is of vital importance to procure the flow of a constant stream of sustenance into a city, it is equally important that the used matter, animal or vegetable, when it has entered and passed through the 'tourbillon,' should be restored back to its source, or be at least removed. In effecting this, art and labour are required, as well to return as to bring in the organized matter, yet it could not be expected while people remained ignorant of the fact that the accumulation of effete matter is as fatal to a population as famine, that they should be ready to incur any great expense for its removal.

"The present investigation fortifies the frequent recommendations which have been made in these reports, and in all works on public health, with regard to sewerage. I shall not refer to the subject further at the present time, as it has occu-

pied the special attention of the 'Health of Towns Commission,' comprising among its members distinguished persons largely interested in property, as well as gentlemen of great sagacity and scientific knowledge, from whom may be expected such practical suggestions on the subject as shall serve for the basis of an act of Parliament, regulating, by some simple provisions, the entire architectural structure of towns.

"I have stated, in previous papers, that we should not rest satisfied with throwing the refuse of towns into the rivers, as this refuse matter, which in certain circumstances is a poison to man, is the nutriment of vegetation, and constitutes, with water, the difference between the barren desert and populous kingdoms. The more organic matter there is, the more subsistence—the cheaper food will be. If Mr. Martin's grand and magnificent conceptions cannot be carried out, the ingenious suggestion of Dr. Arnott deserves to be seriously considered. 'Engineers who pump from the Thames many miles above London, to supply pure water to the inhabitants, could as easily, by pumping away to any desired distance the fluid from the drains, supply the most valuable manure yet known—fluid town manure—to the horticulture and agriculture of the district; and the purity and beauty of the Thames, where it passes through London, would be preserved. Fluid manure, by sinking at once into the earth, is much less offensive to the neighbourhood, and affects less the purity of the atmosphere, than an equal quantity of solid manure, spread, as it usually is, on the surface of the earth.'\*

"Whether the remark in the last sentence is correct, we are perhaps not quite prepared to say; but no matter of the kind appears to be sensibly injurious to animals when it is in contact with vigorous vegetation, and is not present in excess. If the irrigation, in an instance referred to by Dr. Alison, convert the meadows around Edinburgh into 'putrid marshes,' it would be in the teeth of all analogy to infer that the practice is innocuous; but if it render the soil more fertile, and is not in excess as manure, it must be inoffensive to the health of Edinburgh. Upon the same principle drainage, and all the improvements of agriculture, which tend to increase and invigorate vegetation, to the same extent absorb, impress vitality on decaying matter, and promote the health of the population.

"Much of the putrefying atmosphere of English towns would be got rid of, by removing the cattle markets, slaughter-houses, and manufactories of the animal remains to convenient, distant localities. At present the greatest amount of putrid matter is accumulated in districts like Whitechapel; and the city of London without the walls, where the population is most dense, or the sewerage is most imperfect."

One of the most effective means of introducing sanitary measures calculated to lessen the evils existing in crowded communities, Dr. Farr thinks would be the diffusion of such knowledge as would satisfy the public of the intimate connection between the causes of insalubrity and the excessive number of deaths in towns. "The middle classes," he says, "are now very easily put in possession of the facts relating to health by the popular literature of the day, and will probably be, ere long, sufficiently convinced to induce them to incur the expense necessary for the improvement of the districts in which they live. That conviction has not yet reached this point in many towns is to be greatly regretted. After all the public measures have been introduced, to which the poor have no means of contributing, much will remain for them to achieve by their own exertions. It is important, therefore, that they should be made aware of the few simple facts and principles which demonstrate the effects on their families of cleanliness, ventilation, and the observation of the rules of health. Their minds would thus be carried along with the improvements, and would second instead of thwarting them. Few men like to be compelled to do what they are told—but have not the means of knowing—is for their own interest; and this feeling is quite as strong in the worst as in the best parts of London. Much will be effected by example. The higher classes in this country were, not many years ago, intemperate; they became aware that it was injurious to health; and, from this and other motives, are now as temperate, on the whole, as can for the 'health's sake' be desired. The poor, in the worst districts, will no doubt ultimately follow the example, and expend

\* Dr. Arnott's Sanitary Reports; *Local Sanitary Reports*, Scotland, p. 12.

the money on lodging and food which they now waste on spirits. I have before adverted to the influence of medical advice on the habits of the poor; information may also reach them through newspapers which circulate in their districts. Tracts on health may perhaps be distributed with advantage, or persons may be employed to lecture to and converse with them on the subject. In the worst districts the poor reason; for I cannot agree with an eloquent passage in Dr. Southwood Smith's valuable evidence, to the effect that, in towns, 'physical wretchedness annihilates the mental faculties;' although it is admitted that it dwarfs and obscures many of 'the faculties distinctive of the human being.'

"Until latterly," he continues, "the government of this country has done little directly to promote the public health; for one obvious reason, that the causes of insalubrity were not well known, and it was not clear that anything useful could be done; such distinguished writers as Dr. Price, who were well aware of the excessive mortality of towns, ascribing it, among other causes, to 'luxury.' Now that the legislature and the government, having become acquainted with the evil, have evinced the best disposition to investigate its nature and to introduce measures for its removal, care must be taken to discriminate between what can be done by legislation for the people, and what can only be accomplished by themselves individually, and swayed by the slow progress of opinion. Over the supply of water—the sewerage—the burial places—the width of streets—the removal of public nuisances—the poor can have no command; it is only by choosing and paying better rents for well-constructed houses that they can exercise any influence on the landlords; and it is precisely upon those points that the government can interfere with most advantage. The legislature would enact the removal of known sources of disease, and, if necessary, trench upon the liberty of the subject and the privileges of property, upon the same principle that it arrests and removes murderers, who, if left unmolested, would probably only destroy lives by hundreds, while the physical causes, which have been adverted to in this paper, destroy thousands—hundreds of thousands of lives. The movement for the sanitary improvements of towns must originate, it is true, and be carried out in a great measure by the educated and more intelligent classes,—by the statesman,—because the causes in question are not palpable, cannot be seen, and are only discovered by extended observation, calculation, and abstract reasoning. Upon such subjects there can be no excitement. Still much may be done with the people, as well as for them. Health is as dear to the poor as to the rich. The most abject part of the population—creatures who belong to no class, but are the reprobates, unfortunate, and fallen of all classes—can understand its value; and, as we know, are capable of making sacrifices for the good of others; what may not then be expected from the great mass of the labouring population, from the intelligent artisans of towns, who are so apt in acquiring their difficult arts, and are certainly not surpassed by other classes in the facility with which they grasp and carry out a scientific principle clearly announced. To leave many things to the people themselves will be to proceed slowly, because knowledge and new principles on such subjects can only be communicated slowly, but it will be to proceed surely—and the improvement will not die away or be superficial, for it will be the act of the mind, penetrate the inmost recesses of home, and be imparted to future generations."

G. E.

\* Dr. Smith quotes a remark by the medical officer of the West Derby Union:—"Amidst the greatest destitution and want of domestic comfort, I have never heard, during the course of 12 years practice, a complaint of inconvenient accommodation." Upon which Dr. Smith makes this eloquent comment:—"Now this want of complaint, under such circumstances, appears to me to constitute a very melancholy part of this condition; it shows that physical wretchedness has done its worst on the human sufferer, for it has destroyed his mind. The wretchedness being greater than humanity can bear, annihilates the mental faculties,—the faculties distinctive of the human being. There is a kind of satisfaction in the thought, for it sets a limit to the capacity of suffering, which would otherwise be without bound."

ART. XX.—*Experimental Researches on the Post-mortem Contractility of the Muscles, with observations on the Reflex Theory.* By BENNET DOWLER, M. D. (Reprinted from the *New York Journal of Medicine*, for May.) 8vo. pp. 39. New York, 1846.

THIS essay, or monograph, as the author is pleased to term it, though written in not very good taste, nor with much attention to accuracy of arrangement, is, nevertheless, the production evidently of a mind deeply imbued with the spirit of inquiry, but one better adapted to observe and record facts than to weave the conclusions to which these facts lead into a consistent theory.

The essay is replete with matter of very deep interest, and which, by directing anew the attention of the physiologists to the once popular, and perhaps correct, doctrine of a *vis insita* inherent in the muscular fibre, and constituting its *vita propria*, may be the means of introducing more correct views of the functions of the nervous system, and a modification of at least some of the pathological theories of the day.

The main object of the publication before us, is to prove the continuance of muscular contractility for many hours after death, and that it may be then called into action by a blow inflicted upon the muscle. The author adduces in proof of this fact, the summary of forty-three cases, in which the muscles, when struck with a shingle, cane, the flat side of a hatchet, or the operator's hand, contracted forcibly, fifteen to thirty minutes, and even one, two, three, five, ten or more hours after death, and the limbs were, in consequence, caused to perform slow and regular contractions, relaxations, pronations and supinations, which simulated the living voluntary movements. The contractions were, in many cases, so forcible as to cause a heavy weight to be lifted by the limb in which they occurred. For the production of these post-mortem contractions, the integrity of the corpse is not at all necessary; they were excited by Dr. Dowler, hours after the dissection of the body, and after the limbs were severed from the trunk.

The bodies operated upon were of individuals of both sexes, and of different ages, varying from 20 to 58 years. The majority of the subjects, however, were young; thus of 39 cases in which the age at which the subject died is given, 31 were below 33 years, and 18 below 26.

The author, without attempting to give all the details in relation to post-mortem contractility, presents thus a few of its leading features.

"Béclard asserts that the most remarkable and best established fact in the muscular action is, the shortening of the muscle during contraction; that its swelling is caused by its shortening, each compensating the other mutually.—*Gen. Anat.*

"It is natural to every muscle, says Haller, to shorten itself by retracting its extremities towards its belly or middle; when in action, becoming shorter and thicker. Whether this doctrine be true of galvanic muscular contraction, I do not know, but I am certain that it does not apply to post-mortem contraction, because it often happens that both the forearm and elbow joint are stiff and immovable, while the biceps may be powerfully contractile, its belly swelling up into a hard lump, relaxing and contracting repeatedly, and unavailingly, owing to the rigidity at and below its insertion in the arm. Here it is evident that the indurated swelling is not owing to the approximation of the two extremities of the muscle, for they are fixed. The increase of volume seems to be owing to an expansion, with rigidity, among its elementary fibres; a zigzag oscillatory motion, upon the summits of the contracting masses, is visible to the naked eye."

The author adduces three cases of post-mortem muscular contraction, without shortening, in yellow fever subjects.

Six cases (five of them yellow fever subjects), are given to illustrate the increase, declination, and subsequent resuscitation of post-mortem contractility. In one, "from four to five hours after death, and two hours after dissection and the removal of the entire viscera, the *cadaver* being warm and rigid, great force was found necessary to extend the arm to a right angle with the body; extensions, flexions, and frictions were used, after which, a blow caused the forearm to rise



until the hand pointed to the zenith—the motion was slow and equable; a second blow caused a slight motion without elevating the arm."

In another case, in a few minutes after death but feeble contractions were presented, which ceased, for a time, in half an hour.

"But soon after, the contractile function returned to the same arm with much force, but after repeated blows it was exhausted a second time. Again, after a similar interval, it returned with a like force, a third time. In three hours the rigidity, beginning in the neck, extended itself to most parts of the *cadaver*. The blows were found to have caused well-marked contusions, cellular ecchymoses, &c."

In a third case, observed from five minutes to an hour after death—

"The contractility was found to be active, but after appending a weight of two to three lbs. to the hand, the arm was unable to raise the weight more than twice; a third blow caused the biceps to be convulsed; it gathered up in a knotty heap, which, by forcible extensions and frictions, was, at length, removed temporarily, but in half an hour it returned, and was unchanged as long as observed, being to the touch like a bony tumor."

In a fourth case, observed from fifteen minutes to two hours after death:

"The contractility, though strong, was extinguished, in the right arm, by five or six blows. The left arm, about two hours after, was somewhat rigid, but frictions and extensions caused the contractile function to revive again."

In a fifth body, dead two and a half hours—

"A single blow with the flat side of a hatchet produced flexion, but exhausted the contractile force for half an hour, when it slightly returned. The supinators and pronators acted for an hour longer than the flexors of the arm; the experiments ending five hours after death."

According to Dr. Dowler, the destruction of the contractile function in one arm does not affect the other.

"If several blows," he remarks, "on the same spot, follow each other rapidly, there is but one contraction, but they exhaust the contractile function more than a single blow. If the force be greatly augmented, the contractility may be killed, almost immediately, in the muscle struck, without impairing the action of any other part.

"This force has no resemblance to that purely physical property of bodies called elasticity. A man may stand on the hand of the *cadaver* while the blow is being made on the biceps; let him then step off the hand, and the contraction will follow as usual. If the power be feeble, the relaxation will be quick in most cases; the arm will fall back upon the plane from all points short of the perpendicular, each elevation being less than the preceding one, until the force is expended. Should the hand pass its meridian, gravitation will, of course, aid the contraction in bringing it to the trunk.

"The blood has no appreciable influence upon post-mortem contractility, because when the limb is severed from the trunk and drained of its blood, its action is not thereby diminished."

"The continuance of, or rather the degree in which post-mortem heat is evolved bears no proportion to the intensity of post-mortem contraction." "I find, however," remarks Dr. D., "on examination of the original papers, not yet published in detail, that for the most part, when the heat had declined, the contractility was exhausted, but that the presence of great heat, ranging as high as 113°, did not by any means imply the presence of contractility, nor the absence of rigidity."

"Post-mortem contractility, in the human *cadaver*, Dr. D. found to have no connection with, or dependence upon, the spinal marrow. This may be received," he remarks, "as an axiom, though directly opposed to the reflex theory, and is easily proved by amputating the shoulder in a proper manner. In performing this operation, it is best to leave a few portions of the skin undivided, so as to tie the arm down to the trunk, that the former during action may not turn quite over."

"Post-mortem contractility, when excited by percussion, is, according to our author, the best fundamental type of the periodicity of the contractile function, better, certainly, than that originating in galvanic electricity; voluntary action may be much more perfect, electrical action necessarily more rapid. The contraction operates for several seconds or minutes. Relaxation, by which the muscle is

made to assume its normal figure, presents another distinct interval. The counter motion, though less prolonged, is equally distinct. Then comes the period of repose (analogous to that following the fatigue, in living muscles) the forerunner of renewed efforts; efforts, however, which must speedily exhaust the quantum of contractile force forever. The period of exhaustion may be hastened or deferred, in many cases, according to the manner of procedure. The contractility may be *killed* by a severe blow, especially if at the same time a weight be fastened in the palm. It is worthy of remark, that by a proper application of blows all the elemental motions of the arm may be produced more or less perfectly."

Dr. D. has attempted to show that muscular contraction occurs spontaneously after death. We have ourselves seen it repeatedly after death from cholera, and the author thinks or rather hints that the changes in the position of the limbs said to be observed after death, and which have given rise to frightful tales of persons having been buried alive, are to be explained by the occurrence of spontaneous post-mortem muscular contraction.

The results of Dr. Dowler's experiments are, we confess, as unexpected as they are important. That the muscles were capable of being excited to contract for some time after death was well known to physiologists, but it was admitted that the period this capacity to contract existed was very short, and that it could be excited into action only by some powerful agent, as electricity or galvanism, but that genuine muscular contractions could be produced for many hours after death by a simple blow of the hand, was a fact no one suspected until the appearance of Dr. D.'s original paper.

Our author believes that post-mortem contractility is altogether independent of nervous agency. Now, although it is more than probable that the capacity to contract does exist in the muscular fibre as an inherent property unconnected with any special nervous influence, and may be excited into action by stimuli applied directly to the muscle, without the agency of motor nerves, still we cannot admit that the experiments before us prove this conclusively; they certainly, if no error shall hereafter be detected in them, prove that regular muscular contractions may take place independently of the brain and spinal marrow, but still we are to recollect, that the muscles of the limbs, as of other parts, even after separation of these from the trunk, contain a certain amount, more or less, of nervous matter, by impressions made upon which it is possible, though the idea is ridiculed by the author, their contractions may be produced for a limited period. We are still too little acquainted with the true physiology of the nervous matter to assert positively that a portion of a nerve disconnected with the nervous centers is incapable of calling forth action when stimulated.

The remarks of the author upon the doctrine of the reflex action of the nerves of the spine are destitute of that philosophical spirit which would render it a very easy or pleasing task to enter upon their examination. His experiments prove, what we think was very evident before they were undertaken, that muscular contraction is not necessarily or invariably a reflex action; and in this they are so far important, but they do not prove that during life, in the state of health and disease, muscular contraction takes place independently of the brain and spinal marrow. The division of certain of the nerves going to a part suspends, we know; its muscular action under all the ordinary stimuli applied to it, while, on the other hand, violent contractions or rapid contractions and relaxations in the muscles are frequently excited by irritations seated in the brain or spinal marrow, or in some organ remote from the muscles which are thrown into action. There are, in truth, many phenomena connected with the muscular system, which go to show incontestably the dependence of their action upon the nerves distributed to them, and many which can with difficulty be explained unless we admit the truth of the reflex theory.

We recommend to Dr. Dowler the further prosecution of the inquiry he has so successfully begun. We at the same time suggest that the adoption of a more dignified style than that of the essay before us, with more systematic arrangement and less of sarcasm, would have a tendency to recommend the author's views to a more favourable notice by the profession, both at home and abroad; ridicule is not argument, and, by arousing partizan feelings, often prevents the perception and adoption of truth.

ART. XXI.—*Das Blei und seine Wirkungen auf den Thierischen Körper. Ein Beitrag Zu dessen Charakteristik für Aerzte, Chemiker und Techniker.* Von Dr. FERDINAND RUMPELT. 8vo. pp. 268. Dresden und Leipzig: 1845.

*On Lead and its Action on the Animal Body; a contribution towards determining this action for the use of Physicians, Chemists, and Artists.* By FERDINAND RUMPELT, M. D.

To the physician the most interesting portion of the present treatise is that which refers to the character of the morbid effects produced by the action of lead upon the human body, the parts of the body on which the poison acts either primarily or secondarily, and the manner in which it exerts its deleterious influence. The leading conclusions of the author in relation to the pathological effects resulting from the different preparations of lead, are drawn from cases in which these were administered to cows and other animals—and notwithstanding he has compared, and apparently with much care, the phenomena which resulted from the action of the lead in these experiments, and the morbid phenomena exhibited by the human subject, when accidentally exposed to its influence, we have less confidence in the accuracy of those conclusions than we would have had, were they based throughout upon an extended and cautious series of strictly clinical and pathologico-anatomical observations. The work, nevertheless, presents a number of very interesting and valuable facts.

It is divided into twenty-nine parts. The first comprises a general account of lead as a metal, and in its several chemical combinations. In this the author advances the general proposition, that, in no form, can it be without danger introduced into the animal body, and only under certain conditions externally. In the second part is presented a view of the deleterious influence of lead upon those who are engaged in procuring its ore, as well as upon those artizans who make use of its oxydes and salts—with some remarks upon the means of avoiding its poisonous action.

In the third part, the course of the disease produced by the action of the carbonate of lead is shown by the detail of seven cases, which fell under the care of the author. The general summary of the symptoms is given in a succeeding chapter.

The fourth part treats of the direct, indirect, and remote effects of the lead; compares the phenomena resulting from its action upon beasts and men; investigates the mode in which it gains admittance into the circulating fluids; and notices the primary and secondary diseases produced by it.

From this we translate the following account of the morbid phenomena resulting from the poisoning by lead which occurred in seven workmen in a white lead factory, and which phenomena were designated as those of colica pictonum.

“As in all other diseases, the lead colic is destitute of premonitory symptoms: the very first symptoms which present themselves being the commencement of the pathogenetic reaction. Hence, the first indications of disease are a sense of languor and lassitude with a disinclination to rise from bed. The digestion is disturbed; the taste is diminished; sweetish—astrigent—or abnormal and bitter. The taste for food becomes entirely lost, and all food is alike disgusting. There is intense thirst. The patient becomes disgusted with everything; is agitated at intervals; commences to gag, without discharging anything from the stomach. The bowels become costive, and no evacuation occurs for many days, and is only obtained by the use of purgatives, when the forces are discharged in the form of hard balls. The discharges are, however, sometimes of a pulpy or fluid consistence. As soon as the purgatives are suspended costiveness again recurs. At first the patient passes but little urine, not more than a tablespoonful at a time, which is of a straw colour, and only passed with much straining and pressure upon the bladder—subsequently scarcely any urine is discharged, and finally none. The face is pale, as if dusted over with flour. The countenance becomes, as it were, broken up, distressed, and of a grayish-yellow colour. There is a

sense of oppression, heaviness, and numbness of the head, with pain and vertigo. A sense of weight and burning in the stomach or umbilicus; then a painful tension extending from the stomach to the scrotum; and in the rectum, bladder, and back, a dragging pain. The stomach is drawn inwards, and the whole abdomen flattened. The patient is unable to lay still for any time, but throws himself about upon the bed. The distressing sense of uneasiness extending throughout the body renders sitting and lying equally painful. In consequence of the pain he is unable to lie upon the back or either side. The pulse is either weak or somewhat firm, full or small. It is either slow, oppressed, or jerking. It is synchronous with the motions of the heart. The respiration is deep. During the paroxysms of pain it is somewhat increased. The parietes of the chest cease to act. The patient is affected with chilliness, often running into a complete rigor. His eyes have a staring expression. The gums have a morbid dead-like appearance, and their edges are of a livid or whitish-blue colour. The legs and hands tremble, and the knees in walking give way. The chills begin to alternate with increased heat; with this commences the second stadium of the disease.

“As the pain increases or diminishes, and as the organs of the abdomen or chest become affected, the pulse increases in frequency; in a single day rising from 65 to 100, from 100 to 120, or even to 126 strokes—it is, at the same time, either hard—hard and full—or soft and small. The breathing becomes short, quick and superficial—often impeded by pain of the chest, and the patient experiences a distressing sense of oppression.

“The patient discharges a portion of mucus without cough. A tetanic contraction of the extremities comes on—a pain extends down the arms to the hand, and causes the limb to be kept extended, unless when the hand is spasmodically directed towards the epigastrium, and the fingers bent. Severe pains extend from the stomach over the left ribs towards the back, shoulder-blade and breast. At the same time a burning pain extends from the spine into the rectum, causing frequent ineffectual efforts at stool, and into the testicles. The severe pains of the extremities cause them to become contracted and distorted. The pains of the thighs are often so great as to cause the patient to scream out. The parietes of the abdomen at the hypogastrium lie upon the spine—they are rather contracted and tense than drawn inwards. The bowels continue obstinately costive and sometimes for fourteen days no urine is passed, or only in a very small quantity at a time. The pain in the back, rectum and testicles, destroys the patient's strength and renders him unable to move. There is a rigidity extending from the forearm over the whole body. The eyes are open and fixed; the pupils do not contract. The pain extends towards the eyes, the motions of which cause a stinging sensation.

“The third stadium is properly only the remote effects of the poison. The pulse is now scarcely to be counted; it is tremulous, irregular and intermitting. The head is oppressed, with a tendency to determination of blood to its vessels. There is pain at the top of the head, and the brain feels as if compressed. The pain is sometimes so severe as to cause the patient to scream out. The patient becomes speechless; by the motions of his head he gives intimation that he hears and understands the questions put to him, but finally loses the power to do this. There is a distressing sense of restlessness throughout the whole body—so that the patient can neither sit nor lie, and when he attempts to stand his knees sink under him. He feels a painful sense of oppression behind the sternum and in the left breast.

“If the disease does not terminate in palsy, and the remedies employed during the second and third stadii have their desired effects, the bowels become relaxed, the stools of a more natural appearance; the pain in the abdomen disappears, and the functions of the digestive organs begin to assume their normal activity. The skin becomes moist, and the urine is regularly discharged. The sense of oppression at the chest disappears; the pains diminish in intensity, and the breathing becomes free and regular. The oppression of the head and the vertigo cease; the intellect becomes clearer, and the countenance assumes a more sprightly expression. The nights are more quiet, and the patient sleeps more naturally. The digestion becomes regular. The feeling of every part of the body becomes improved, and the strength gradually increases. The complexion, however, continues, for

some time, pale, and the livid, the deadened gums remain separated for a length of time from the teeth.”

This section concludes with some remarks on the different groups of symptoms which result from the poisonous effects of the different preparations of lead according as they are introduced into the lungs, or stomach, or are applied upon the skin. Colic, it is very generally admitted, is one of the most common forms of disease resulting from the lead in whatever manner introduced: and, accordingly as the lead affects the motor nerves, the lungs, or the brain, succeeding or super-added to the enteralgic symptoms, there occur arthralgia, diminished action and rigor of the muscles of the extremities ending in paralysis, diminished action of the heart, encephalopatia and various nervous symptoms, disturbed respiration, oppression of the chest, &c.

In the ensuing chapter the author presents more fully his views of the mode in which the lead, when introduced into the system, produces its deleterious effects. When acetate of lead is introduced in large or in repeated small doses into the alimentary canal, he describes it as at first uniting with and changing the properties of the mucus contained in that cavity, while, at the same time, it excites gradually an irritation of the mucous membrane, indicated by increased thirst and loss of appetite—which becomes more or less intense, according as the acetate of lead is more or less changed by its combination with the mucus. A portion of the lead also enters into the orifices of the veins of the stomach and intestines, and thus mixing with the blood changes the properties of the latter—producing small dark coagula. The irritation produced by the action of the poison upon the stomach being conveyed through the nerves to the spinal cord, is thence reflected upon the other nerves of sensation as well as upon those of motion. The disturbance of the respiratory function, the author believes, to be in part owing also to the direct action upon the pulmonary blood-vessels and their nerves of the lead conveyed to them through the blood. The irritation of the stomach is, in time, extended to the brain, and thence reflected upon the different cerebral nerves; but the brain may also be immediately affected by the lead contained in the blood circulating through its vessels.

It is not necessary to follow the author in his exposition of the views thus briefly announced, and his application of them to explain the nature and succession of the symptoms produced by the action of lead upon the animal body.

To show the action of the lead (white lead) upon the coats of the stomach and bowels, the morbid appearances presented by the different organs in animals destroyed by the acetate of lead, are compared with the pathological condition of the same organs in the human subject after death from the effects of lead, as given by Tanquerel.

#### RUMPELT.

“1. The upper layer and epithelium of the stomach formed a slimy, 1-2 lines thick, homogeneous mass.

“2. A thick, glutinous, ropy, semi-transparent mucus, took the place of the epithelium of the stomach.

“3. The mucous membrane of the stomach was at different parts changed to a whitish appearance, dry and friable.

“4. Through these whitened spots, the vessels, distended with blood, of the inner coat of the stomach could be distinctly perceived, as red lines.

“5. The entire length of the small

#### TANQUEREL.

“1. The mucous membrane of the alimentary canal, from one foot below the stomach to the rectum, was covered with a thick, glutinous, yellow mucus, which was with difficulty scraped off with the knife.

“2. A thick, coagulated mucus covered the mucous membrane of the intestines.

“3. The small and large intestines were somewhat contracted, and covered with a thick adherent layer of a glutinous matter. They could be readily distended by inflating them with air, but tore upon the slightest handling.

“4. Brownish lines indicated the course of the gastric vessels.

“5. The stomach and small intestines

intestines were in parts decidedly reddened.

"6. There occurred numerous small ecchymoses on the whitened portions of the mucous membrane of the small intestines."

In the same manner the author exhibits the effects of lead upon the brain.

## RUMPELT.

"In a portion of brain of a young man examined after forty-eight hours,

"1. The gray and white substances were not to be readily distinguished from each other, both being of a grayish-white appearance.

"2. By a slight traction some portions were easily separated, which presented an evident fibrous appearance.

"3. In the brain of an adult forty-eight hours after death, both substances were easily distinguished from each other, notwithstanding the gray had assumed an appearance similar to the white.

"4. The substance of the brain was of a dense consistence and was easily broken.

"5. The pia mater was thickened and easily separated from the brain."

"A portion of the *spinal cord* of adults examined twenty-four and forty-eight hours after death presented the following appearances:

"The pia mater very much contracted and thicker than in its normal state, so that the cut end of the medulla spinalis projected considerably beyond the divided edge of the former, in the form of a button, in which the fibres of the cord were very readily detected."

were slightly reddened—and the rest of the canal in parts of a rose red, and, as it were, injected.

"6. Ecchymoses and blackish stripes appeared upon the mucous membrane of the small intestines."

## TANQUEREL.

"Tanquerel describes the following as the pathological appearances presented by a number of brains.

"1. *a.* The substance of the brain was externally of a dirty yellow, and internally of a grayish colour, and the gray substance of an ash-gray yellow. *b.* The substance of the brain was of a dirty yellow.

"2. Dr. Glück notices a number of canals through many parts of the white substance of the brain in a person destroyed by the effects of lead, as a morbid appearance.

"3. *a.* The substance of the brain was of a yellowish colour. *b.* The white substance was of a dirty yellow, ash-gray, and dull, and almost transparent; the gray substance, instead of a rose-red, appeared of a yellowish-green.

"4. *a.* The parenchyma of the brain was hard. *b.* It was as readily broken as the substance of the liver. *c.* The convolutions of the brain were flattened so that the depressions between them had disappeared. *d.* The texture of the brain was hardened.

"5. *a.* The meninges were dry. *b. ibid.* *c.* The meninges in a patient who had died of encephalopathy from the poison of lead were dry. *d.* The meninges with their distended blood-vessels were dry."

Tanquerel notices a softening of the substance of the cord; an injected and yellowish appearance of the cerebellum and medulla oblongata; a softening of the medulla lumbaris; a number of coagula of blood in the dura mater spinalis; from 3–4 ounces of fluid in the spinal canal, the veins being at the same time gorged with blood. The pia mater decidedly reddened by over-distended ramifications of its blood-vessels, and the substance of the medulla spinalis presenting numerous red points. Other accurate observers notice dryness of the membranes of the cord and increased density of its substance.

A portion of the *ischiatric nerve* three inches long, presented, twenty-four or forty-eight hours after death, the following appearances.

"The sheath of the nerve was thickened and contracted, so that the contained nerves, with their neurilemma, separated from each other, projected beyond the divided end of the sheath in the form of a button, and of a firm consistence, opaque and of a yellowish-white colour. The nervous sheath was at its edges somewhat transparent, and appeared of a dead grayish-white colour. The individual nervous filaments appeared separated, and distinct from each other."

Tanquerel notices the following as the morbid appearances observed by him in the nerves.

"The substance of the nerves in a patient who died amaurotic and comatose, was firm.

"The ganglions in a patient who had suffered from encephalopathy had a grayish-yellow appearance."

In the tenth part the author treats of the entrance of the oxide of lead into the blood. While he admits that the presence of lead in the blood has not been detected by chemical analysis, the fact, he believes, may be established by the change in the physical appearance of the blood, and in its physiological relations.

In the thirteenth part, the author presents the results of experiments made to test the presence of lead in the blood of different portions of a rabbit, destroyed by the administration of acetate of lead.

A portion of the lungs being subjected to the action of a solution of sulphuretted hydrogen, there occurred immediately a black precipitate. Sulphuric acid, added to the blood from the lungs, caused, after some time, a slight cloudiness.

A portion of the heart subjected to the action of a solution of sulphuretted hydrogen, became immediately of a brown colour, and after twelve hours a slight dark-coloured precipitate was thrown down. Sulphuric acid, added to the blood from the heart, caused an almost imperceptible cloudiness.

Blood from the thoracic aorta and vena cava, subjected to the action of sulphuretted hydrogen, became of a brown colour, and after some time a slight precipitate took place;—subjected to the action of sulphuric acid, after a long time it became slightly cloudy.

The small intestine, from which the yellowish mucous coating was removed, being submitted to the action of sulphuretted hydrogen, there occurred, after twelve hours, a little blackish precipitate, but under the action of sulphuric acid presented no perceptible change.

When the duodenum, after removal of the coating of mucus, was subjected to the action of sulphuretted hydrogen and sulphuric acid, the same results occurred. The kidney gave under the action of sulphuretted hydrogen a black precipitate, and under the action of sulphuric acid, a slight precipitate. Results were obtained, when the brain was submitted to the same reagents, similar to those obtained from the heart. No traces of lead could be detected in the medulla spinalis.

In the remaining portions of the work, the author explains the pathology of the several forms of disease produced by the poisonous effects of lead, by an application of the general proposition which he has attempted to establish, namely, that the lead produces a direct irritation upon the tissues with which it comes first in contact—which irritation is extended through the nerves, the spinal marrow and brain, to neighbouring and remote organs; the lead also causes, by its entrance into the blood and the changes it occasions in it, an irritation in the deep seated organs, to which it is carried in the course of the circulation, so as to come immediately in contact with their tissues. The whole of the morbid phenomena produced by the poison of lead, are referred to a sub-acute inflammation of the vascular and nervous systems.

Thus, in what the author terms the five forms of the lead disease, we have

1st. Symptoms referable to the abdominal cavity, commencing with colicky pains, (*colica saturnina*), and terminating with all the symptoms of enteritic inflammation.

2d. Symptoms referable to the thoracic cavity—oppression and tightness with shortness of breathing, and symptoms similar to those of pneumonia and carditis.

3d. Symptoms referable to the cranial cavity—as severe pain and oppression, and other symptoms indicative of an inflammatory disturbance of the brain.

4th. Symptoms of painful irritation or inflammation of the medulla spinalis—giving rise to tonic and clonic spasms of the muscles, terminating in anæsthesia or paralysis.

5th. In the subcutaneous cellular membrane, and in the synovial membranes, more particularly of the lower extremities—violent arthralgia, with swelling of the joints, and other symptoms of inflammation.

The treatise of Dr. Rumpelt is, upon the whole, calculated to improve our knowledge of the pathology of the diseased condition produced by the action of lead upon the animal body. Some of the positions assumed by the author appear to us to want a more extended series of observations to support them—while his pathological reasoning does not strike us as being always very accurate. Many of the data from which the author's conclusions are drawn are derived from the effects, direct and remote, which resulted when large or repeated doses of one or other of the salts of lead were given to animals—now, whether the pathological effects of these salts, when thus administered, are identical with those caused by the lead introduced gradually into the system—as in the case of the various workmen who are brought daily in contact with one or other of its preparations—is a question which may admit of some doubt. It is true, that the author has endeavoured to remove this doubt by comparing the symptoms during life, and the state of the organs after death as presented in animals poisoned by lead, and in patients labouring under disease produced by exposure to its influence: still there is a very great difficulty in making the comparison so as not to be led into error.

D. F. C.

ART. XXII.—*Ueber eine Neue Geburtszange zur Extraction des im Beckeingange Stehenden Kindskopfes. Mit abildungen.* Von Dr. TH. HERMANN, arzt und wundarzt erster classe in Bern. 4to. pp. 50. Bern, 1844.

*Description of a new Obstetrical Forceps for abstracting the child's head when engaged in the upper strait of the Pelvis—with Plates.* By T. HERMANN, M.D. Physician and Surgeon of the first class in Bern.

THE author does not confine himself in the present work to a mere description of the new instrument which he has invented, but enters into a very full and interesting examination of the structure and form of the female pelvis, and its different diameters, in reference to the mechanism of labour; of the several means and instruments that have been proposed for facilitating labour; of the form and qualities demanded in a forceps adapted for application upon the head when in the upper strait; and of the mechanism of instrumental labour in general.

There is a difficulty in rendering the views and explanations of the author intelligible without the diagrams by which they are accompanied, or we should have been inclined to have made numerous extracts from the introductory portion of the work; and though we shall give a description of the forceps invented by him, we fear that no very accurate idea will be formed of it without a drawing.

“1. This forceps has, in fact, the principal attributes of all the other obstetrical forceps; it differs from them, however, in the following particulars.

“A. The blades have no greater length than those of the long forceps generally; on the contrary, they appear somewhat shorter, which, however, is a deception resulting from their peculiar curvatures. Their length measures eight inches nine lines (*ped de roi*). Their blades are, in the same manner as Siebold's, fenestrated; at their greatest breadth they are one inch and a half, and, as well as the whole instrument, are strongly made.

“1. The curvature to receive the head is similar to that of Siebold's, but less decided; the greatest distance between the blades measuring barely two inches four lines; the distance between the points four lines.

“2. They differ in their lateral (pelvic) curvature from the common forceps, in



this being made very nearly to correspond with the median line of the pelvis. The curvature is the most decided near the middle of the blades; towards the points their direction approaches more to that of a straight line, without being so entirely; towards the handles the curvature is the same as towards the points, until it passes over into the perineal curvature.

"3. The bold perineal curvature (which is in the opposite direction to the first) belongs only in part to the blades—the lock being placed within it, upon that part which lies without the vulva near the centre of the curve. This curvature marks the prolongation of the pelvic curvature, and forms almost a semicircle, the cord of which is from fifteen to eighteen lines. The curvature rises boldly over the perineum, reaches the lock, and then descends and passes onwards to the straight portion of the handles:

"4. Nearly in the centre of their length, namely, four inches three lines above the lock, and in the middle of their breadth, which here is about one inch four lines, each blade is pierced with a round hole two and a half to three lines in diameter.

"B. The handle of each blade of the forceps passes from the lock downwards in a bold curve which completes the second portion of the pelvic curvature; on arriving at the horizontal plane, it makes a short curve in the opposite direction and then continues in a straight line. The handle is precisely similar to that of Siebold's forceps, and is covered with wood on its outer surface, as far up as the commencement of the perineal curvature. To the lock, the handle is seven and a half inches in length.

"C. The lock is a combination of the original one of Dubois or Nägele, and that of Siebold. The button is similar to that of the first, but somewhat higher. It is rounded upon its upper surface, in the centre of which there is a funnel-shaped depression four lines deep and not quite two and a half lines in diameter. Its edges are milled, and on opposite sides there arise two small projections, which enable it to be more readily turned. The mode in which the blades are secured by the lock is the same as in Siebold's.

"To facilitate the attachment and separation of the blades, in the opposite direction of the knobs a groove can be made upon the upper surface of the button about a line in depth, into which a key may be inserted.

"The smaller or supplementary forceps are intended to fulfil a double object, namely: first, to effect a pressure upon the larger forceps from above, in order to facilitate the passage of the fetal head into the pelvis; and, secondly, to effect a traction upon those forceps downwards, in order to facilitate the passage of the head through the pelvis. This instrument is composed of three parts.

"A. A perfectly smoothly polished and very accurately formed globe of the diameter of five and a quarter lines. It has on its sides, two projections which stand in such a direction with each other, that a line drawn from the basis of each shall cross at a right angle in the centre of the ball. These projections consist of,

"1st. A somewhat conical pin five lines long, which is so formed as to pass readily into the hollow upon the upper surface of the button of the forceps lock. The end of the pin must be so formed as to pass easily in and out of this hollow.

"2d. A forceps apophysis of about the same caliber as the pin. It commences to rise from the surface of the ball of a cylindrical form; which form it preserves one line of its length, when it suddenly assumes a greater diameter, so that a cylindrical ring shall project around it to the extent of one line, being flat on its upper and lower surfaces, and one inch in thickness; after this the apophysis assumes a cubic form, the dimensions of this portion being about two cubic lines. From the upper surface of the cube a cylinder proceeds provided with a screw worm. This cylinder is about two and a half or three lines long and one and a half lines in diameter to the edge of the projecting worm. Upon this cylinder a nut passes, which is made similar to that of Siebold's forceps. It should not be too smoothly polished nor rise too high. When the nut is screwed tight it should stand transversely with the instrument.

"B. A spring bow two inches three lines long, of nearly an oblong horse-shoe shape, and formed out of a strong steel spring. At the center of the bent portion there is a square opening which fits exactly upon the cube of the forceps apophysis, so that by the screwing on of the nut the bow can be firmly attached to the

ball. The sides or arms of the bow run parallel with each other, and not so far apart as the blades of the large forceps in the neighbourhood of the round openings, when closed. From each of their free extremities, and projecting inwards, there arises a smooth cylindrical pin, of such a caliber as shall pass readily into the round openings in the blades of the large forceps, and of such a length that they shall not project beyond their inner surface.

"C. The handle of the supplementary forceps is composed of two exactly similar lateral halves. Each half is formed of a strong metal plate covered on its outer surface with wood. The end which attaches to the steel bow by means of the polished ball, is so curved as to form with the opposite half a kind of steel fork, the strong prongs of which have on their inner surfaces a spherical depression, and stand so far from each other as to receive between them the ball of the forceps, a small segment of which passes into the spherical depressions, and when the two parts of the handle are attached, hold it fast so that it can escape on no side while it moves readily in every direction. The prongs of the forked extremity are about eight lines long. The opposite end of the handle is bent latterly so as to form when the two halves are attached, a transverse arm three inches long.

"From the thick forked extremity the metallic part of the handle becomes gradually thinner and broader, and from a short distance below the fork is covered with wood. As near as possible to the forked extremity the two sides of the handle are fastened firmly together by means of three strong screws. Another screw passes through the entire thickness of both parts of the handle near the cross piece at the opposite end. On one end of the cross piece is a screw key for opening and closing the lock of the larger forceps, in the form of a beveled projection four or five lines long, and one line high.

"When the whole instrument is united, it measures seven and a half inches in length. When it is to be used to make a downward pressure upon the forceps, the pin upon the side of the ball is to be placed in the depression upon the upper surface of the lock button, the bowed portion being passed in the direction of the handles of the long forceps. When used as an extractor, the pins which project from the ends of the bows are to be passed into the round openings in the blades of the long forceps."

The peculiar advantages of the forceps invented by Dr. Hermann, and the exact mechanical operation of the supplementary portion, as well as the precise circumstances under which this latter is to be resorted to, cannot certainly be perfectly understood without a reference to the remarks of the author upon the axes and curvatures of the pelvis and the mechanism of labour generally, and these could not be rendered very intelligible without the diagrams by which they are illustrated. Even the description of the instruments which we have given will with difficulty be understood without a drawing. We add, to render, if possible, the description more plain, a tabular view of the dimensions of the forceps as laid down by the author.

#### *Long forceps.*

Length sixteen inches three lines *piéd de roi*.

" of the handle to the lock, seven inches six lines.

" of the part of the handles covered with wood, five inches six lines.

" of the handles to the commencement of the perineal curve, six inches.

" of the blades, from their points to the lock, eight inches nine lines.

" from the lock to the round openings, four inches three lines.

" from the points to the round openings, four inches six lines.

Greatest breadth of the blades, one inch six lines.

Greatest distance of the closed blades from each other, two inches four lines.

Distance between the points of the closed points, four lines.

Pelvic curvature—height of the points of the blades from the level plain on which the instrument lies, five lines.

Perineal curvature—height of the apex above the horizontal level, one inch five lines.

Lock button—breadth, nine lines—thickness, three and a half lines.

Depth of the depression on the upper surface of the button, four lines.

Diameter of the depression, two and a quarter lines.

*Supplemental forceps.*

Length, eight inches.

“ of the handle, five inches seven lines.

“ of the wooden layer, four inches three lines.

Distance between the ends of the pins upon the free extremities of the bow, four and a quarter lines.

Length of the transverse arm at the end of the handle, three inches six lines.

D. F. C.

ART. XXIII.—*Theoretisch en Praktisch Handboek der Mechanische oogheelkunde, het zĳn, het middle waardoor wij zien, den optisch-Physiologischen werkkring der oogen, de eigenschappen en gebreken des hulpglazen en de wijze van derzelver aanwending bij de Mechanische oogzichten; door. J. A. HESS, geotroijeerd gezigt en oogheelkundige te Middelburg. 8vo. pp. 273. Middelburg, 1842.*

*Theoretical and Practical Manual of Mechanical Ophthalmology, containing an account of the Organs, Physiology, and Mechanism of Vision, and of the proper construction of the glasses adapted to remedy the various forms of defective vision. By J. A. HESS, Optician in Middleburg.*

THE first part, which constitutes the greater portion of the present volume, is taken up with an inquiry into the optico-physiological functions of the eye; the second chapter, occupying sixty-nine closely printed pages, being composed of extensive quotations from the works of Rénatus Descartes on Optics, of G. Adams on Sight, and from that portion of the physiology of Müller which treats of the theory of vision. The succeeding six chapters contain an exposition of the author's own views of the physiology and mechanism of vision. These views are interesting, and may be studied with profit by those who desire to become acquainted with the true character of the various defects of vision, in reference, especially, to the mechanical means best adapted for their relief.

The second part treats of the mechanical and organic defects of vision and the means of remedying them. In the several chapters of this part an account is given of the cause of myopia, presby-myopia; presbyopia, and photophobia, and of the properties of the concave, convex, flat and coloured lenses, and the cases to which they are respectively adapted.

It is certain that the true character of those several defects of vision which are capable of being benefited by appropriate lenses, and the proper form of those demanded in each case, cannot be understood without an intimate acquaintance with the optico-physiological construction of the eye and the theory of vision generally. As an introduction to the leading facts in relation to these subjects; the volume before us is not destitute of considerable merit. The extent of that portion devoted to the subject of vision and the operation of the eye as an optical instrument, is certainly disproportionate to that devoted to the consideration of the defects of vision, their nature and the mechanical means best adapted to remedy them. This latter, short as it is, presents, however, a series of very useful rules and suggestions, all important to direct to a proper selection of glasses for the use of those who have from birth defective vision, or whose vision has become impaired by organic changes, the result of disease or of the spontaneous changes which take place in the progress of life, in one or other portion of the optic globe or its contents.

The author has not entered into a consideration of all the forms of defective vision—but has confined his remarks to those alone in which artificial lenses can be employed with advantage; he has consequently not occupied the whole of the ground properly embraced under the denomination of mechanical ophthalmology.

D. F. C.

ART. XXIV.—*The Writings of Hippocrates and Galen. Epitomized from the original Latin Translations.* By JOHN REDMAN COXE, M.D. *Multa renascentur.* 8vo. pp. 681. Philadelphia, 1846. Lindsay & Blakiston.

WE return our thanks to Dr. Coxe for having undertaken the somewhat laborious task of preparing the present edition of the writings of Hippocrates and Galen. We are not, it is true, in favour of compendiums and epitomes, generally, but in the present case, are convinced that it is the only means by which a large proportion of the profession in this country can obtain an introduction to the Fathers of medicine, however illy adapted it may be to render them acquainted with all that their writings contain. Dr. Coxe has therefore in our estimation conferred a very important favour upon a numerous body of American physicians who, deprived of the means of consulting in the original, or even in an English translation, the works of "these pioneers of medicine," are shut out from all acquaintance with them, save the fact that their names are in some manner or other connected with the history of the healing art. There are few who could be found willing to undertake the labour of epitomizing the writings of Hippocrates and Galen better qualified for the task than the editor of the volume before us. There are none amongst us who have studied these writings with more care, or who have more fully prepared themselves for forming a correct estimate of their true value. The digest Dr. C. has given to us, though not quite so extended as we could have desired, is nevertheless a highly interesting one, and upon the whole ably executed. It will no doubt be read with avidity by thousands who until now were unaware of what "the old man of Cos," and his industrious commentator and generous defender knew and taught concerning the institutes and practice of medicine.

We are not, certainly, of the opinion that the most profound study of the writings of Hippocrates and Galen would be sufficient to indoctrinate any one into the principles of the healing art, or to fit him for its successful practice, or that a good practitioner may not be formed without the slightest acquaintance with these writings. We still regret that the physicians of this country should know so little of what they contain. As a mere subject of commendable curiosity we should expect the members of "a learned and liberal profession" would be desirous of acquainting themselves with the opinions and practice of the ancient sages of medicine. But the writings of Hippocrates and more especially of Galen, recommend themselves to our notice not merely as a means of gratifying curiosity, but as containing many a precept the result of close and accurate observation, which will still be found of value,—even if it only serves to confirm the accuracy of modern doctrines. While we shall find in them, likewise, the first sketch, if not the finished model of many of what are esteemed almost contemporaneous discoveries, and of medical doctrines, the enunciation of which by living authors, or their immediate predecessors, as novel views deduced from recent observations, has been the means of placing their names high upon the roll of fame.

Neglect of the vast body of facts recorded in the writings of those who have gone before us and the constant desire for novelties in doctrine and in practice, which prevails, less, it is true, now than formerly, have been the means of retarding the progress of medicine, and rendering it amenable to the charge of moving in a circle,—teaching, after the lapse of years, for truths in pathology and therapeutics, what had in times gone by been denounced as essentially erroneous, and proving facts by a laborious series of observations, which had been already in former times established by similar evidence, and faithfully recorded by writers worthy of all credit.

D. F. C.

- ART. XXV.—1. *Sixth Annual Report of the Superintendent of the Insane Hospital, (Augusta, Maine,) 1845.*  
 2. *The Annual Report of the Eastern Asylum, in the city of Williamsburg, Virginia, for 1844.*  
 3. *Report of the Committee of Regents, of the Physician, and of the Superintendent of the Lunatic Asylum of South Carolina, for 1842.*  
 4. *Report of the Committee on the Lunatic Asylum (of South Carolina), including the reports of the Regents, Superintendent and Physician, 1845.*  
 5. *The Twenty-Second Annual Report of the Officers of the Retreat for the Insane, at Hartford, Connecticut, May 1846.*  
 6. *Report of the Maryland Hospital (for the Insane,) for the year 1845.*  
 7. *Report of the Trustees and Superintendent of the New Hampshire Asylum for the Insane, June 1846.*

1. The Report of the Hospital for the Insane at Augusta, for the past year, was prepared by Dr. James Bates, the successor of Dr. Ray, as Medical Superintendent of that Institution. It manifests ability, and an interest in the investigation of Insanity. The Statistics for the year are as follows, viz.

	Males.	Females.	Total.
Patients in Hospital, Nov. 30, 1844	50	26	76
Admitted during the year	50	49	99
Whole number for the year	100	75	175
Discharged (including deaths)	45	45	90
Remaining Nov. 30, 1845	55	30	85
Of those discharged there were cured	21	17	38
“ “ “ died	5	2	7

*Causes of Death.*—Old age 1, phthisis pulmonalis 2, intestinal hemorrhage 1, suicide 1, suicidal wound inflicted before admission 1, not stated 1.

	Males.	Females.	Total.
Whole number of patients since opening of Hospital Oct. 14, 1840	276	210	486
Discharged (including deaths)	213	162	375
Of whom there were cured	-	-	171
“ “ died	18	8	26
<i>Civil Condition</i>			
Married	122	117	239
Single	137	76	213
Widowed	17	17	34

The average time spent in the Hospital by the 171 patients who were discharged cured, was 140 days. Fifty-six cases believed to be curable, were prematurely removed from the institution. "This," says Dr. B., "is one of the evils which it has been thought most prudent not to remedy, by stipulating a period, before which patients should not be removed uncured, until the house should be filled.

"Our present situation will not only justify, but I think imperatively demands the correction of this evil.

"Before closing this branch of the subject, it may be proper to observe, that from no source does a well-regulated asylum for the insane suffer so much in its reputation, as from the false representations of patients prematurely removed.

"It is peculiarly interesting and gratifying to observe the kind feelings and gratitude expressed by those who leave the hospital perfectly cured, towards those who have had the charge of them; whereas those who go away still insane, remember with bitterness every restraint and denial they have experienced.

"All this would be harmless, but for the morbid sympathy which induces some persons to place more reliance on the evil reports of an insane man, than the truth from the best citizen in the community."

"Of 486 admissions, 211 are represented by their friends as having insane ancestry, or blood relatives in that condition.

"When we add to these, foreigners and others concerning whom little informa-

tion can be obtained, there can be little doubt that more than one-half are hereditarily predisposed to that state of brain of which insanity is a symptom."

*Ages at which insanity commenced.* Ten to twenty years 85, twenty to thirty 168, thirty to forty 110, forty to fifty 72, fifty to sixty 35, sixty to seventy 11, seventy to eighty 5.

Taking these items, and the data afforded by the last census of the state of Maine as the elements of a calculation, Dr. B. arrives at the conclusion, that the greatest proportion of men attacked with insanity is between the ages of 30 and 40 years, and that of women between 20 and 30. He also infers that "males under 20 furnish a fearful proportion," and that in both early and advanced life men are more subject to the disease than women.

The people of Maine have been slow to take advantage of the means afforded by this hospital for the cure of their insane. From the records of the past year, as embodied in this report, it is evident that there is an increasing appreciation of the value of the institution, and the time is not far distant when all its apartments will be constantly occupied.

2. During the year 1844, the Eastern Asylum of Virginia was enlarged by the construction of another wing. This addition, while it increases the extent of accommodations, affords very satisfactory facilities for the classification of the patients. The report before us opens with a very pretty coloured lithographic view of the asylum.

	Males.	Females.	Total.
Number of patients Jan. 1, 1844	62	47	109
Admitted during the year	17	24	41
Whole number	79	71	150
Discharged during the year	7	5	12
Died	3	3	6
Remaining Dec. 31, 1844	69	63	132

The condition of the patients discharged is not mentioned in the report.

The Eastern Asylum was the first establishment in the country, devoted exclusively to the treatment of the insane. It went into operation prior to the American Revolution. During the whole period of its existence, to the time of the preparation of this report, the number of patients admitted was 851, of whom 539 were males, and 312 females. In respect to seasons, the largest number of admissions was in summer, the next in autumn, and the smallest in spring.

The report of Dr. Galt contains a large number of valuable and interesting facts in relation to insanity, exhibiting great industry on the part of its author, and an extensive acquaintance with the divers authorities upon the disease.

3. It is but recently that we have been enabled to lay our hand upon any document relating to the asylum in Columbia, South Carolina. The pamphlet now before us includes the several reports of the managers and officers, together with the laws for the government of the institution.

The report of the managers is a well-written essay upon the condition of asylums in general, the curability of the insane, and the necessities of the particular institution under their care. It however contains nothing important in a medical point of view, which has not heretofore been published in this journal. The report of the physician, Dr. Frerevant, is chiefly devoted to the difficulties under which the asylum has laboured, and to its wants in order to place it upon an equality with some of the other institutions in the country.

The following remarks are well worthy of notice. "When a lunatic is kept at home, and subjected for five or six months to the care of the family physician, when everything that his skill (with a perfect knowledge of the constitution, habits, &c. of the patient) could direct has been done, then to bring him to the asylum, and expect a cure to be immediately effected, is asking more than justice or reason can demand. But is it right that the patient should be placed under the care of those who are unaccustomed to the attendance on lunatics? How often is the case aggravated, either by the foolish indulgence of friends, the restraints of gross ignorance, or the injudicious treatment of the medical adviser! I trust I shall not be deemed disrespectful to my brother physicians when I say, that in no set of diseases are they more at fault than in those of the mind. It falls to the lot

of but few to be called to attend the insane, and when such a case occurs, their own experience offers no guide, and they must follow the directions of others. How various and conflicting are the directions which they there meet! Talk of madness, and you have the lancet, drastic cathartics, emetics, &c. &c., instantly presented, and lamentable are the effects constantly produced by their injudicious use. There is not a year that I do not see constitutions shattered, and the recuperative energies of the brain entirely destroyed. Case after case of imbecility is the result of injudicious bleeding.

"Lunatics generally do not require and will not bear very active treatment; and he who prescribes merely because he has a lunatic under his care, is more apt to go wrong than right, if his attention has not been fully directed to that particular species of disease."

"When I took charge of this institution," says Dr. T., "there were 53 inmates; in the last seven years we have received 153, making in all 206; of this number, 83 have been cured and removed much improved, and 54 have died. It must be borne in mind that very many of the fatal cases were in a hopeless situation when they entered; four died in 48 hours after they came; several never rose from the beds in which they were placed, and many sunk from chronic disease of the lungs and bowels, engendered by exposure during their wanderings, or the want of attention when confined in the jails and workhouses; 20 were epileptics, 10 eloped, 6 committed suicide."

4. The report for 1844-5 of the asylum at Columbia is very brief. The statistics for the year are as follows:

Number of patients Nov. 5, 1844	-	-	-	-	72
Admitted during the year	-	-	-	-	23
Whole number during the year	-	-	-	-	95
Discharged, 6 deaths included	-	-	-	-	24
Remaining, Nov. 5, 1845	-	-	-	-	71
Of those discharged, there were cured	-	-	-	-	13

*Causes of death.* Typhus fever 1, epilepsy 1, apoplexy 1, consumption 1, found dead, supposed apoplexy, 1, not stated 1.

The legislature of the state having granted permission for "religious exercises to be carried on and conducted at the institution," a chaplain was employed during the year, and Dr. Trezevant expresses himself "gratified with the result."

5. During the past two years very extensive additions and improvements have been made at the Hartford Retreat. Among these, are two new wings, each 120 feet long by 36 wide, together with "connecting wings," by which the former are attached to the old building. These "have effected such a change," says Dr. BUTLER, "both in the external appearance and internal arrangements and accommodations of the Retreat, that it would be hardly recognized by those who have not watched, from step to step, their progress. They in fact constitute a new era in the history of the institution. Its capacity for the accommodation of patients has been doubled, and the conveniences and appliances for successful treatment, as well as the arrangements for their more comfortable and pleasant classification, have increased in no less a ratio.

"Our means of classification have been greatly improved. We are enabled to make eight distinct classes, and, if necessary, subdivide even these, so that the social affinities of all may be consulted, and such influences associated as will have the most happy action and reaction upon each other."

	Males.	Females.	Total.
Patients in the Retreat, April 1, 1845	53	50	103
" admitted during the year	56	72	128
Whole number during the year	109	122	231
Discharged, deaths included	55	60	115
Remaining April 1, 1846	53	63	116
Of those discharged, there were cured	24	31	55
Died	5	11	16

"The premature removal of patients from the Retreat, continues to be to us a fruitful source of anxiety and regret, and to them fearful and cruel in its results. Of

those discharged 'improved,' and 'much improved' several were rapidly convalescing. Contrary to our advice and earnest expostulation they were removed, and the results in most cases have justified our worst apprehensions; several remaining in a weak, nervous condition, and others relapsing into insanity. A recital of a few cases which have been recommitted to our care may prove instructive, and lead others to avoid an error always serious and often fatal in its consequences.

"CASE I.—Admitted in a most wretched state of melancholia, unwilling to speak, move or make any effort, and negligent of the calls of nature—under treatment became better, would talk, laugh, sing and walk about, and was rapidly recovering his health. On his being removed, contrary to our wishes and advice, he immediately relapsed, and was, the last we heard of him, a hopeless maniac, caged up like a wild beast.

"CASE II.—Admitted a violent maniac, noisy, wilful and dangerous, and from the nature of the cause almost hopeless—became much better, and just as we were rejoicing over the unexpected prospect of recovery, was removed on suspicion that pecuniary considerations influenced our advice. In a few months was returned to us more wretched, noisy and ungovernable than ever. This case may admit of alleviation, but recovery is hopeless."

Two other similar cases are related in the report.

The whole number of patients admitted since the retreat was opened, April 1st 1824, is 1560. Of these 846 recovered and 120 died.

The causes of death of 51 patients, being all that have died during the past five years, are as follows: viz., epilepsy 1, apoplexy 5, phthisis 4, old age 4, injury 1, fever 3, psoas abscess 1; disease of the brain 5, internal hemorrhage 1, marasmus 2, fracture of thigh 1, general debility 4, general paralysis 4, exhaustion 7, inflammation of bowels 2, disease of lungs 3, suicide 3.

The report contains many valuable tables, illustrative of particular points relating to insanity. In discussing the causes of mental disorder in the United States, Dr. Butler makes the following very sensible remarks:

"As a community we have too little recreation, too little regard for the refreshing and invigorating influences of social intercourse; and far too little time is given to the enjoyment of literary and scientific pursuits. Our devotion to business of every kind, is too long continued and too absorbing. We rise early and set up late, and eat the bread of carefulness, and eat it hastily, that we may carry out those plans of advancement which are so engrossing. The deep traces of care and anxious thought are written on the brow, and their corroding influences consume not only the elasticity of our frame, but, in too many instances, it is to be feared, the better feelings of the heart. These influences pervade society in this country more than any other on the globe. They draw within their vortex many who would gladly escape them, and, increasing with the flood-tide of national prosperity, threaten to engulf the better feelings and sympathies of the nation in the maelstrom of ambition and gain. Remonstrating one day with an intelligent young merchant on the folly and danger of devoting himself so exclusively to his business, to the neglect of exercise and the enjoyments of the family circle, he confessed that it was wrong, but pleaded that the eager competition that existed around him required the devotion of all his time and energies to sustain his character as a business man. 'I never allow myself more than five minutes for any meal,' said another, who in one month was a raving maniac."

6. The Maryland Hospital (for the Insane), as appears from the report of Dr. Fisher, is in a flourishing condition, the number of patients during 1845 having exceeded by 33 that of any former year. Exclusive of cases of mania-a-potu, which, according to Dr. F., are always omitted in his reports, the statistics are as follows, viz:

	Males.	Females.	Total.
Number of patients Jan. 1, 1845	52	35	87
" admitted during the year	54	37	91
Whole number	106	72	178
Discharged inclusive of deaths	49	20	69
Remaining Dec. 31, 1845	57	52	109
Of those discharged there were cured	27	13	40
Died	8	2	10



*Causes of death.*—Apoplexy 2, paralysis 2, chronic diarrhœa 1, pulmonary consumption 1, marasmus 1, epilepsy 1, old age 1, exhaustion from long-continued excitement 1.

The whole number of patients during the eight years, from 1838 to 1845, inclusive, is 644. Of these, 305 were cured and 53 died.

Dr. Fisher, like the physicians of most of the American asylums, in reference to their respective institutions, complains of the premature removal of patients. In the hope of assisting to overcome this evil, we quote his remarks upon the subject, as we have already quoted the remarks of others.

“The premature removal from the institution of convalescents is an evil which we, not unfrequently, have to deplore. In some instances they are, unfortunately, removed in consequence of the inability of the friends to support them any longer in the hospital; but in too many other instances, from the friends acting upon their own judgment of the condition of the patient, in opposition to the judgment and advice of the physician. They, perhaps, pay a short visit to the patient, during which they find him free from illusion, composed, cheerful, rational; they learn that he has been for some time employing himself steadily and usefully, and they immediately conclude, as they are unprepared to distinguish between convalescence and perfect recovery, that he is well enough to go home, and frequently persist in removing him. But it often happens that a few days or weeks only are sufficient to convince them of their error. The company and exciting conversation of old friends, and the revival of old associations, prove too much for the mind in its weakened state, and soon the derangement returns, perhaps with more intensity than ever. ‘If,’ says Dr. Ray, ‘we are entitled to any confidence at all, we claim it in regard to our opinions on this point. If the friends are better judges than we are as to the proper time for removing a patient, then are they also more competent to direct the management of the case in its previous stages. Since, however, the latter are immediately entrusted to our management, what can be more inconsistent or absurd than the idea that in the latter stage of the disease, no further dependence need be placed on our judgment. The fact is, that the convalescent stage is that in which there is most liability to mistake, and in nothing is the judgment of the physician more closely exercised, than in determining where convalescence ends and perfect recovery is established. On a point so important and delicate as this, we conceive that our ampler means of information render our opinion worthy of some consideration. If incorrect, it can, at the worst, merely occasion a few days or weeks unnecessary detention, and every one must know how much less an evil it is to keep a patient too long than to remove him too soon.’”

After a connection with the Maryland Hospital during a term of ten years, a decennium of untiring industry in his vocation, and of ardent devotion to his duties, Dr. Fisher, in this report, resigns his situation. He has already left the institution, and been succeeded by Professor Fonerden, a gentleman well known for his attainments, not only in medical, but in general science.

7. Dr. CHANDLER having retired from the New Hampshire Asylum for the Insane, his place is supplied by Dr. Andrew McFarland, whose name is appended to this, the fourth annual report of the institution.

	Males.	Females.	Total.
Number of patients in asylum May 31, 1845	39	37	76
“ “ admitted during the year	51	47	98
Whole number during the year	90	84	174
Discharged, including deaths - - -	40	36	76
Remaining May 31, 1846 - - -	56	42	98
Of the patients discharged there were cured	16	10	26
Died - - - - -	6	5	11

“The average number of patients has been about twenty per cent. greater than last year; while the number discharged has been nearly equal. The number of admissions has exceeded that of last year. These results have been owing to the greater readiness of the friends of the suffering insane to grant an extended trial of the benefits of the asylum, as well as the more confirmed public sentiment of its utility.”

The asylum was opened in Oct. 1842. Since that time 376 patients have been admitted and 112 cured.

Dr. McFarland gives the following account of the appearance and progress of variola in the institution. "On the 9th of January, with a household, including officers, patients, and attendants, of 125 persons, a case of small-pox made its appearance in the gallery occupied by the better class of patients. Fortunately, its character was detected on the first appearance of the eruption, and an opportunity afforded for immediate precautionary measures. The affected person was removed to the new building, and an express messenger dispatched to the health office, Boston, who returned the same day with a supply of pure vaccine virus. Thorough vaccination, with the isolation of the affected cases, prevented the disastrous consequences which must otherwise have ensued from a disease of so much malignity, under circumstances so favourable to its extension.

"The individual first afflicted died on the twelfth day of the disease. His case was of the confluent kind, and no hope of his recovery could be entertained from the first inception of the malady.

"Seven cases followed, as the result of the one just mentioned. Two of these proved fatal, which would probably have terminated otherwise but for the previous existence of exhausting bodily infirmity.

"After being free from all appearance of the disease in the asylum for more than two months, it again made its appearance in an infirm old woman of 70, in whom it terminated fatally. How the disease could have, originally introduced itself is a subject of some mystery. The first individual afflicted had visited the village two weeks prior to the attack, or a period just sufficient for incubation to take place, which renders it highly probable that the disease was contracted at that time, though no well authenticated case then existed in Concord.

"It speaks well for the character of those employed as assistants in the asylum, that in a season eminently calculated to spread consternation among those of the stoutest heart, none abandoned their post of duty; and several who were absent when the disease made its appearance, returned and resumed their places, when all were filled with gloomy forebodings.

"Exertions, such as nothing but the existence of such a calamity could have called for, were not without ill consequences upon the health of our household; and the month of February closed upon us with the assistant physician and five of the attendants wholly or in part disabled by sickness."

Much of the remainder of the report is occupied with judicious remarks upon insanity, its causes and treatment; but as similar views of the subject have heretofore been presented, it is unnecessary to exhibit them at this time.

P. E.

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ART. XXVI.—*Human Physiology; with three hundred and sixty-eight illustrations.* By ROBLEY DUNGLISON, M. D., &c. &c. Sixth Edition, greatly improved. 2 vols. 8vo. pp. 651-694. Philadelphia: Lea & Blanchard, 1846.

It is not always that a new edition of even a scientific work can with propriety be termed an improved edition. Not unfrequently each successive edition is printed from the same stereotype plates, the only change being in the title-page and perhaps the preface. Such is not, however, the case with the work before us; each edition has been carefully revised, and everything of importance that had been recorded since the appearance of the preceding edition embodied in it, so that the last will be found invariably to be on a level with the then existing state of physiological science.

As a text book for students the work of Dr. Dunglison will always maintain its place among the numerous very excellent manuals of physiology, that have recently appeared; and for those out of the profession who desire to become acquainted with the organization and functions of the human body, it is perhaps better adapted as a guide than many of its cotemporaries. But the work is too well known to require any extended notice of its peculiar merits on the present occasion—we need only remark that the present edition exhibits the same care in

its preparation that was bestowed upon the last, and may be received as a very full and fair exposition of the present advanced state of the science of which it treats.

We observe that to the present edition a large number of new illustrations have been added, and that many of the former cuts have been replaced by others.

D. F. C.

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ART. XXVII.—*Small Books on Great Subjects. The connection between Physiology and Intellectual Science* From 2d London Edition. Lea and Blanchard, 1846: pp. 85.  
*On Man's Power over himself to Prevent or Control Insanity.* By Rev. John Barlow, M. A. Lea and Blanchard, 1846: pp. 54.  
*An Introduction to Practical Organic Chemistry, with references to the works of Davy, Brande, Læbig, &c.* Lea and Blanchard, 1846: pp. 66.

THE above works, occupying the places, number 2, 3, and 4, in the series of "*Small Books on Great Subjects, Edited by a few Well-Wishers to Knowledge*,"—a series which we are told has met with much success in London—are, in their subject-matter, more or less connected with medical science, and deserve a passing notice in a medical journal. As a general principle we are strongly inclined to doubt the policy of making science popular. We believe that there is no royal road to learning, and that he who would taste of knowledge or would become familiar with great truths, must toil as others have done before him, though his predecessors may have made the path somewhat less tedious and rugged. There is a class of men, however, in every community, who are so educated that they derive pleasure, useful instruction, and may have their minds directed to higher contemplations by perusing such summaries of science as those under consideration. To such a class do we conceive that these works—the medical ones, for they alone have been examined by us,—are especially addressed and peculiarly adapted.

Two of them, indeed, written by the Rev. John Barlow, contain "each the substance of a communication to the members of the Royal Institution at one of their Friday Evening meetings," meetings devoted to the consideration of scientific subjects. The first, on the "*Connection between Physiology and Intellectual Science*," gives a clear and simple account of the "machinery by which man is a living, a sentient and an intelligent animal, in other words, of the nervous system, the sympathetic nerve, the spinal marrow and its nerves, and the organ within the encephalon." It spreads before the reader an account of the functions of these different parts based upon the experiments and statements of the most recent and standard authorities; and then shows that man presents other functions than those which are observed in other animals, that he is also an intellectual being, possessed of consciousness and memory, and of an intelligent will. That it is in these last we must look for the "unknown potent cause" of the difference from other animals in his actions: in this independent "persevering and remembered will," which may indeed cause an impairment of the bodily organs through which it acts from their incapacity to respond to its urgent, incessant demands, for it is never contented, "still feels dissatisfied, and requires nothing less than infinity for its contemplation—immortality for itself."

An application of this great fact is made in the next work, "*On Man's Power over himself to Prevent or Control Insanity*," of which the object is to show "that man has in the resources of his own nature, the antagonist power which, if properly used, can set at naught the evils, ay, and the so-called irresistible propensities of the bodily organism. So nicely balanced, indeed, is the medium that a grain may turn it to either side, but it is in the power of the will to cast that grain." We will not attempt to follow the author through the train of argument by which he proceeds, but will simply point out in his own words the result.

"The result then of the whole inquiry appears to be that man being a compound of two natures, mental derangement is of two kinds. In the one kind, structural disease deadens or distorts the perceptions, and if this extends itself to the organs of all the faculties, the intellectual force having no longer the means of external action, the individual remains to all appearance a helpless machine. But as such

extensive structural disease is hardly compatible with life, so it is of very rare occurrence, and if any part of the organ remain perfect, then there is good reason to hope, that a mind thoroughly well-trained in early years will still contrive to make the little that is left available to conduct, if not to the higher intellectual functions; as we see the loss of the right hand replaced in some degree by the increased activity of the left. But in the other case, no structural disease exists in the first instance, and the inefficiency or misdirection of the intellectual force is the sole cause of derangement; sometimes by the violence of the excitement producing disease; sometimes, as I have already noticed, continuing to the last without affecting the bodily organs." p. 39.

What then is the remedy? It is in education, in the improvement and cultivation of the faculties and powers in which man resembles his Creator. That this power of self-control exists even among the most hopelessly insane, is evident to all who are familiar with the benefit and happy results of the modern moral treatment of insanity. May it not therefore be fairly assumed that the same governing and controlling power, properly trained and exercised, may be usefully and happily employed in preventing the occurrence or the manifestations of mental derangement?

The last work named above is a compilation from the best sources, and presents an useful, simple, yet comprehensive exposition of the state of organic chemistry at the present moment. The application of the results of chemical inquiry to agriculture, manufactures, and the arts—generally less thought of, but fully as worthy of study—of preserving health, has been extensively made within a few years past. And the author of this work desires only to place within more general reach the benefits conferred by science, and in so doing he deems that he is fulfilling to the best of his ability the talent confided to him by his great Master. Indeed, there is in all these publications a tone of deep religious feeling, of pure benevolence, and of disinterested philanthropy, which is highly commendable and attractive. They are evidently written by men deeply imbued with a love of their race, and with a desire to magnify their Creator by showing the wonderful skill and perfect wisdom with which he has adapted all things to his ends. These are indeed worthy of dissemination and of perusal.

C. R. K.

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ART. XXVIII.—*The United States Dissector, or Lessons in Practical Anatomy.* By WM. F. HORNER, M. D. Professor of Anatomy in the University of Pennsylvania. Fourth edition, with numerous illustrations. Edited by Henry A. Smith, M. D., F. C. P. P., &c. Philadelphia, Lea & Blanchard, 1846: 12mo. pp. 666.

THE reputation of the author of this work as a skillful practical anatomist is so extensive and well deserved, that any work on the subject from his pen cannot fail to be received with favour.

The present volume was originally published twenty-three years ago under the title of *Lessons in Practical Anatomy*, since which time it has passed through several editions. The present one has been greatly modified and improved, made to conform with the recent progress of anatomical science, and is enriched with numerous wood-cuts. To the numerous pupils of the professor, it will be a most useful guide in their studies, and by pursuing the course indicated by it, they may hope to reach the same eminence which has been attained by their distinguished teacher.

QUARTERLY SUMMARY  
 OF THE  
 IMPROVEMENTS AND DISCOVERIES  
 IN THE  
 MEDICAL SCIENCES.

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ANATOMY AND PHYSIOLOGY.

1. *General Physiology of the Spinal Cord.*—The general tendency of the investigations of the last year has been to prove that the spinal cord is neither a mere collection of tracts of nerve-fibres nor a single nervous centre, but (if I may use the most popular language of the day) a collection or series of *central stations*, each of which has its own lines of nerve-fibres terminating in it, and serves to receive, and to transmit on numerous lines and in various directions, the impressions which are conveyed by the centripetal nerves abutting on it. The chief evidence for this, which, though not a new view, has hitherto been a very doubtful one, is as follows:

1st. VOLKMANN\* has submitted the question whether the nerve-fibres of the spinal nerves remain and end in the cord, or go on to the brain, to the test of a kind of measurement. He weighed four pieces of a horse's spinal cord, each seven centimeters long, and taken respectively from below the 2d, the 8th, the 19th, and the 30th pairs of nerves. Their weights (in the order above named) were 219, 293, 163, and 281 grains; the areas of the transverse sections of the gray matter in them (in the same order) were 13, 28, 11, and 25 square lines; and those of the white matter 109, 142, 89, and 121 square lines. Thus, the quantity of white matter of the cord is absolutely less at the cervical than at the lowest part of the lumbar portion, and much less in the lower than in the upper cervical portion. The contrast was more marked in a comparison of the sum of the areas of transverse sections of all the spinal nerves of a serpent (*Crotalus mutus*) with that of a section of the upper part of the spinal cord. The former (purposely estimated below the truth) might be reckoned at .0636 of a square inch; the latter only .0058. The total size of the nerves, therefore, is at least eleven times greater than that of the cord—a difference which cannot be explained on the supposition that the nerve-fibres, when they pass into the cord, become smaller.

2d. The almost necessary deduction from these facts is that many or all the nerve-fibres terminate in or very near those regions of the cord into which they penetrate; and this is strongly confirmed by the observations of Hanover and Kölliker, already often referred to, both of whom have demonstrated the fine nerve-fibres as prolongations of the processes of some of the nerve-corpuscles of the gray matter of the cord.

3d. A step further is made by the remarkable observations of Volkmann, which have determined at least two examples of small portions of the cord having absolute and uninfluenced control over the movements of parts, to which parts they are the true and sole nervous centres. I refer here to the governance of the rhythmical movements of the lymphatic hearts by the two definite portions of the cord, of which an account has been already given. The evidence is complete

\* Wagner's Handwörterbuch der Physiologie, art. Nervenphysiologie.

that these portions of the cord are as truly the nervous centres for the two hearts as the portion of the medulla oblongata is for the respiratory movements.

4th. Something of the same kind as this influence of the cord on the lymphatic hearts is indicated by an observation of Budge.\* If a piece of the cord of a frog scarcely two lines wide, be removed from the place at which the great brachial nerve goes off, it constantly occurs that the pulse of the heart decreases in frequency within two hours after the operation, and this does not happen when all the rest of the cord below this portion is removed.

But if it be thus proved that there are in the spinal cord many central stations, the question still remains, how an impression is conveyed from one to the other, or from any of them to the brain?† It is evident, that there are other modes of conveyance besides that through the continuous course of the fibres first impressed; it is not certain that any fibres pass uninterruptedly from the periphery to the brain, yet the impressions are precisely conveyed both to and from the brain; and there is no support in all these facts for the erroneous experiments of Van Deen,‡ which would have made it appear that not only the nerve-fibres, but the impressions also, stop short in or near the part of the cord on which they fall. Some of his more correct experiments show that even a small length of the gray matter left in the cord, when all-around it is cut away, is sufficient for the conveyance of impressions up the cord of the frog, but how the conveyance is effected is as yet a question.—PAGET's Report in *B. & F. Med. Rev.*, July, 1846.

2. *Theory of Cell-Development.*—A very lucid exposition of this theory, and of the principal facts concerning the history and nature of the nucleated cell in the structures of animals, has been published by Kölliker.§ The subject has also been thoroughly discussed by Reichert|| in his "Report on the Progress of Microscopic Anatomy in 1843," his observations being included in an examination of essays by Karsten,¶ Kölliker,\*\* and Nägeli.†† The general tendency of the whole is to show that we are yet very far from the knowledge of the true mode of development of the nucleated cell in animals. There is indeed in all these essays, as well as in the personal knowledge of most anatomists, an abundance of facts bearing upon the subject; but many, perhaps the majority, of these facts cannot be brought within the expressions of Schwann's theory of cell-development; neither can there be yet traced in them any single, uniform, and constant mode of development of the nucleated cell. From the very nature of the case, it seems most probable that one law and one mode must be always observed in the development of the cell and its parts; if it be so, the one mode is unknown; if it be not so, then, in the place of the fair and comprehensive system of Schwann we have a crowd of unconnected facts such as no memory can contain, and of which it would be useless, even if it were possible, to give a brief report.

The observations of Reichert, as well as those in the other works just referred to, relate only, or principally, to the genesis of the nucleated cell and its several parts; and he implies that there is much less room for doubt concerning the metamorphoses of the cell itself, by which, of it or through it, all the more highly organized animal tissues are supposed to be formed. It appears to me, however, that we can be as little sure of many of the changes which nucleated cells are said to pass through in the formation of other tissues, as we are of the process by which the cells themselves are formed. The development of all the fibrous tissues appears especially doubtful. For the investigations of every year show the

\* *Oesterr. Medic. Wochenschr.*, 10 Jan., 1846; from Froriep's *Noitzen*, 1845, No. 783.

† Hypotheses have been suggested in the year just passed by Drs. Todd and Volkmann, (l. c.), but they both seem to me insufficient for the facts.

‡ See last Report, p. 50, and Report for 1842-3, p. 20.

§ In Schleiden and Nägeli's *Zeitschrift*, Heft ii., 1845; another part is announced for publication, but I have not been able to obtain it.

|| In Müller's *Archiv.*, 1844, No. vi. *Jahresbericht*, pp. 148-172.

¶ *De Cella Vitali*; Berol., 1843. See last Report.

\*\* *Entwicklungsgeschichte der Cephalopoden*; Zurich, 1844.

†† *Zur Entwicklungsgeschichte des Pollens*; Zurich, 1842; and in Schleiden and Nägeli's *Zeitschrift für Botanik*, 1844, Heft i.

great difficulty or impossibility of confirming the observations by which Schwann explained the development of these and some other tissues, and the equal facility of finding appearances which cannot be reconciled with his theory, or any other single theory yet proposed concerning it.\*

I have found ample reason for expressing these doubts of the sufficiency of the accepted theories of development in recent examinations of tumours and other morbid growths. Their structure seems peculiarly adapted for testing a theory of cell-development; for they are, doubtless, obedient to the same general laws of formation as the healthy structures are, and, in the unequal and often rapid growth of their several parts, it could hardly happen but that in many specimens all the phases would be seen through which their structures pass towards their fully developed state. But in very numerous examinations I have not found a single example in which a cell has appeared to be forming or formed around a pre-existing nucleus; or one in which fibres have appeared to be formed out of nucleated cells; or one in which nucleated cells have appeared to constitute a stage towards any form of higher development. On the contrary, I have found many instances of rapidly growing structures composed of large collections of fibres without a nucleated cell among or near them: others with abundant nucleated cells, but scarcely any free nuclei or granules, and nothing like a cell incompletely developed round its nucleus; and, again, others (and these of especially rapid growth) with no cells at all, but composed almost entirely of corpuscles like nuclei or cytoblasts.

From these and other observations I am disposed to think that the ordinary (and not the exceptional) mode of development of fibres is, not through nucleated cells, but from a structureless or dimly granular substance which is first *marked*, and then broken up, into fibres. There is good evidence that the cytoblasts which are usually or always imbedded in this substance, influence the development of the fibres; and though I cannot tell how they do so, yet it is certainly not by conversion of themselves into fibres; they shrivel and disappear as the fibres increase and become more perfectly formed.

I think it will be found that, in morbid growths, the nucleated cell is always a terminal, not a transitional, form; for in certain tumours in which the best formed nucleated cells are found, e. g. the epithelial tumours and some examples of medullary cancers, there are no higher forms found, not even imperfect fibro-cellular tissue, except in small quantity about the blood-vessels. Corpuscles having the characters of nuclei or cytoblasts (to adopt still the usual names) appear to be the really energetic bodies in the growth and determination of these morbid structures; they are found in some tumours so abundantly, and so unmixed with nucleated cells, that their power of multiplying and assimilating cannot be doubted; and it is in some of these tumours also that, apparently under the influence of the cytoblasts, the most perfect fibro-cellular tissue is ultimately formed. What I have seen also of the development of these cytoblasts, leads me to agree with that view of the development of nuclei generally, according to which they are described as formed, not on a pre-existent nucleolus, but out of granules collected in a dark or dim mass of the proper size and shape, which then clears up by the formation of a membranous wall and transparent fluid contents with, in some cases, one or more persistent granules holding the position of nucleoli.—PAGER'S *Report in B. and F. Med. Rev.*, July, 1846.

3. *Digestive Powers of the Stomach.*—An account of the condition of the internal surface of the stomach under various circumstances, derived from examining this organ by means of M. Blondlot's method, which consisted in maintaining for a considerable time an artificial opening into the stomach of a dog, has been published by M. BERNARD.† This account is not much more than a repetition of the results obtained and made known by M. Blondlot,‡ most of which, as well as the facts

\* A good evidence of this is in the fact that the most original observers, when they speak of the development of the tissues, almost always cease for the time to be original, and copy both the words and drawings of Schwann or Valentin.

† *Archiv. d'Anat. Gén. et de Physiol.* Janvier, 1846, p. 3.

‡ *Traité Anal. de la Digestion*, 8vo., Paris, 1844.

strychnia and ferro-cyanide of potassium; in half an hour symptoms of narcotism ensued, and the animal died shortly after in a state of tetanus. Ferro-cyanide of potassium was detected in the urine, in the clear watery lymph obtained from the lymphatics between the wound and the aorta, but not in the blood. This experiment proves that ferro-cyanide of potassium, even though contained in a solution with strychnia, may be absorbed by the lymphatics, conveyed by them into the blood, and eliminated by the kidneys; that the strychnia itself must also have been absorbed by the lymphatics, unless we can suppose it to have got into the blood through some collateral circulation after tying the aorta: and that a substance may be found in the urine, although it cannot be detected in the blood.—*Ibid.*

5. *Minute Anatomy of the Kidneys.*—The minute anatomy of the kidney has received considerable attention during the last few months, and the result is the appearance of several interesting papers on the subject. The most important of these is one by Dr. BRÜDER,\* of Dorpat. Having examined the kidneys of tritons, (which animals are especially well adapted to the investigation, inasmuch as from the natural form of the kidney they can be examined at once by the microscope, without undergoing any artificial preparation, which generally destroys the natural arrangement of parts,) he has obtained results which on the whole confirm the accuracy of Mr. Bowman's observations, although they differ in one important particular. According to Mr. Bowman's account the tuft of vessels composing each Malpighian body lies free and uncovered within the cavity of the dilated extremity of the urinary tubule, which forms a kind of capsule to it; Bidder, however, states that the tuft of vessels is in reality quite external to the cavity, being separated from it by the tunica propria of the dilated extremity of the urinary tubule; this membrane, therefore, according to him, is not perforated by the artery and vein supplying the tuft, as stated by Mr. Bowman, but by its external surface reflected over the tuft which projects into the cavity of the tubule, carrying the membrane before it, just as an organ (say the heart), covered on its external surface by serous membrane, projects into the cavity of the serous sac (or pericardium): The Malpighian tuft, thus invested, occupies sometimes half, sometimes much less, of the cavity of the dilated portion of the tubule, and at first sight appears exactly as if lying free and uncovered within it, but that it is really external to the cavity (at least in tritons) Bidder quite convinced himself, and he states that he has sometimes succeeded in separating the vascular tuft from the extremity of the urinary tubule, yet without the cavity of the latter being opened, or any of its fluid granular contents escaping. He says that one-third or one-half of the internal surface, as well as the neck of the capsule, bears ciliary epithelium (as noticed by Bowman in the kidneys of frogs), and that the remainder of the cavity is lined by a layer of fine tessellated epithelium, the cells of which have a tolerably regular polygonal form. Although commonly each tubule is in relation with its own tuft, yet sometimes one tuft is common to two tubules, or, what Bidder seems to think more probable, the dilated part where the tuft comes into relation with the tubules, is not the joint extremities of two, but a pouch-like dilatation at the side of one tubule; he thus, to a certain extent, confirms the observations of Gerlach† on this point.

Ludwig‡ having also examined the minute anatomy of the kidney in many mammalia and amphibia, observes that the mode of termination of the urinary tubules described by Bowman may be very distinctly seen in the kidney of the coluber, and that in the other amphibia examined by him, and in mammalia, such an arrangement of the terminal extremities of the tubules appeared probable, though he was not able clearly to make it out; whilst in some few it did not seem to exist.

The subject has also received a valuable contribution from Kölliker,§ who details the results of his examinations of the kidneys of foetal lizards. He confirms Bowman's account so far as concerns the structure of the urinary tubules, and the

\* Müller's Archiv, Heft v., 1845.

† Last Report, p. 308.

‡ Wagner's Handwörterbuch der Physiologie, art. "Nieren," p. 630.

§ Müller's Archiv, No. 5, 1845.



Heller has since continued his researches on the subject, and has extended them to an examination of the bile itself for copper and other metallic substances.\* He repeatedly examined the bile both of children and adults, who died of various diseases, and he states that in the bile of adults he has many times detected copper, but never hitherto in that of children. The mode of examination which he adopts is the same as that pursued in the case of biliary calculi. He observes, that after combustion of the bile, and before he applied the tests for copper, he could always tell if this metal were present, by the fused residue, when cold, assuming a light blue or bluish-green colour; when this change of colour did not take place, no bile was discovered by the employment of tests. To detect the presence of copper he found it necessary to employ the bile not merely of one but of several gall-bladders, and only that which after combustion underwent the change of colour above mentioned. He never detected so much copper in the bile, however, as he found in the dark biliary calculi.

Heller also mentions having examined the urine and blood of a young man, a house-painter by trade, who was severely afflicted with lead colic; he was not able to detect a trace of lead in either of these fluids, but after the patient's death he examined the bile, and found there both lead and copper. He observes that copper and arsenic having been already most clearly found in the liver in cases of poisoning with these substances, it seems probable that those metallic salts which are foreign to the composition of the blood, and useless to the system, are, even when present in small quantities, gradually separated from the blood by the liver, and so removed from the system. This supposition will account for the immunity from the ill effects of lead, so long enjoyed by those persons who make free use of this metal in their trades, the poison which in these cases is gradually taken into the system being as gradually removed by the liver. The same applies also to copper; and, as Heller observes, it seems highly probable that we are from time to time taking in minute quantities of the metal in our food, and that this becomes separated from the blood by the liver, and so is found in the bile. The fact that this metal is not always found present in the bile is in favour of this supposition, [and a proof that copper is not a necessary ingredient in this fluid, as most probably it is not also in any other of the fluids or tissues of the healthy body; the same may also be said of lead.] This fact, that metallic salts which are foreign to the system are deposited in the bile, is of considerable therapeutic importance; worthy of regard also is the circumstance that the yellowish-brown, the brown, and the black gall-stones, always contain copper; and that the amount of this metal is proportionate to the dark colour of the calculi. Those white cholesterine gall-stones, which possess merely an external white crust, and have their centre or nucleus of a darker colour, contain copper only in the latter part, which is tinged brown by the bile-colouring matter: the outer white layer containing not a trace. Almost all the white gall-stones possess this central brown nucleus. Moreover, in the dark-coloured gall-stones copper is always contained, though not in the bile of the same subject; a circumstance which renders it probable that the copper has some influence in the formation of the biliary concretions.

Heller suggests the importance of instituting a series of experiments on animals to consist in administering to them in their food minute quantities of lead, copper, and other metallic salts, with the object of determining whether these salts are separated from the blood by the liver, and if so, which of them are so especially. —*Heller's Archiv.* 5 and 6, 1845. †

8. *Analysis of the Urine in Dropsy after Small-pox.* By Dr. HELLER.—The patient was a young girl, who after the subsidence of a severe attack of variola, became the subject of general anasarca. The urine was of a pale fawn-colour, turbid, had an acrid reaction, and deposited an abundant coarse-grained sediment; possessing therefore most of the general characters of the urine in Bright's disease. A microscopic examination of the sediment detected heaps of urate of ammonia,

\* L. c. p. 321, et seq.

† In a more recent number of the *Archiv.* (Hest 1, 1845), Heller's statement of the presence of copper in human bile is confirmed by Dr. Gorup-Besanez, who observes, that he has frequently detected it there.

intermixed with which were numerous colourless crystals of uric acid: cylindrical tubules of epithelium from the canals of Bellini, to which also adhered crystals of uric acid; also pus-cells, blood-corpuscles, and pavement epithelium. The urine contained also a quantity of uroxanthin. Its acid reaction remained even after it had been kept four days in a warm room, at the end of which time numerous crystals of uric acid were found deposited on the sides of the vessel. Its specific gravity was 1012. On the addition of nitric acid its colour was changed to violet, and a quantity of albumen was thrown down in the form of a violet-coloured precipitate. On chemical analysis 1000 parts of the urine were found to consist of water 961.80, solid constituents 38.20. Of the solid constituents 10.53 parts consisted of urea, 0.55 of uric acid, 6.80 of albumen mixed with colouring matter of the urine, and with a small quantity of hæmatin, 17.76 of extractive matters, and 2.56 of fixed salts. Among the salts there were only traces of the chlorides found. As the patient recovered, the urine gradually resumed its normal characters; the specific gravity rose, whilst urea and the salts, especially the chlorides, gradually increased in quantity; though long after the urine contained a large amount of albumen. The chemical and microscopic characters of the first specimen of urine examined in this case were very similar to those presented by urine in Bright's disease, with the exception of urea being in about its normal proportion,—which, according to M. Becquerel, is 10.366 in 1000 parts of the urine of women.—*Heller's Archiv.*

## MATERIA MEDICA AND PHARMACY.

9. *New method of making Iodide of Iron, in a state of Purity.* By M. Kor.—It is known that the proto-iodide of iron, prepared in the usual way, cannot be obtained solid in a state of purity. The limpid and colourless solution first formed, acquires a portion of oxygen from the air, and a part of the iron is thus oxidized, while, at the same time, a portion of biniodide of iron is formed, so that the product consists of a mixture of iodide and biniodide of iron with peroxide of iron. If this mixture be dissolved in water, the solution will be of a yellowish-red colour, more or less deep, according to the quantity of biniodide, or even of free iodine present; the solution will also be turbid on account of the peroxide of iron held in suspension.

The author recommends the following method of preparing pure iodide of iron:—Triturate four parts of iodine with two parts of water in a large dish; then add, at once, one part of iron filings in a state of fine division, and continue the trituration. In a few moments there will be manifested a considerable elevation of temperature, together with the disengagement of the vapour of iodine. Sometimes, especially if the temperature of the atmosphere be low, the heat developed from the mixture is insufficient to cause the disengagement of iodine vapour; but in this case, it is only necessary slightly to heat the mixture, immediately after the addition of the iron filings. The mixture is at first liquid, but it soon becomes solid.

There exists, in the proto-iodide of iron thus prepared, a small quantity of iron, which may be easily separated by a filter, when the salt is used in solution, and the filtered liquor will then be colourless, and free from any mixture of biniodide, or of free iodine.

This preparation may be easily administered in the form of pills, made with any proper excipient.\*

The occasional effect of this medicine on the teeth is singular. Righini relates the case of a lady affected with a strumous affection, who was taking with advantage the proto-iodide of iron in the form of pills. After having gradually augmented the dose, she all at once perceived that her teeth, which were previously very white, had become quite blue. Alarmed at this result, she applied to the medical man, who soon assured her that the unexpected effect was purely accidental, arising from the liberation of a little iodine, which was exhaled in the

\* Schmidt's Jahrbucher and Journ. de Chimie Médicale.

state of vapour. The blue colour was immediately removed by using a solution of carbonate of soda.—DAY'S Report in *Ranking's Abstract*, vol. iii.—*Journal de Chimie Médicale*.

10. *Sulphate of Iron for Excessive Perspiration*, prescribed in the following manner, is highly recommended by Professor LIPPICH:

R.—Gray cinchona, 30 grammes, or nearly  $\overline{5j}$ .  
Water 300 “ “  $\overline{5x}$ .

Boil for eight or ten minutes, filter and add

Sulphate of iron, 40 centigrammes, or about gr. vi.

Simple syrup 30 grammes “ “  $\overline{5j}$ .

Mix, to form a drink, of which a half coffee-cupful is to be taken every two hours.—*Ibid*.

11. *Tubercle*.—The following is HASSE'S\* description of the tubercle: The chief organic constituents are fibrine, caseine, and fat, with a small proportion of albumen; the inorganic compounds are chloride of sodium and phosphate of soda, phosphate and carbonate of lime, oxide of iron, &c. The microscopic character of tubercle varies with the different phases of its development. At first tubercular matter closely resembles recent inflammatory exudation, but it differs from the latter chiefly in being insoluble in acetic acid. More mature tubercle consists of a distinctly granular matter, assuming different shapes, but generally the rounded or oval form. The cells are filled with a finer granular matter; about  $\frac{1}{2000}$ th of a line in diameter. No change is produced upon them either by acetic acid or the neutral salts, but they are quickly destroyed by the mineral acids, and liq. potassæ. The chalky and transformed tubercle is composed mainly of this granular matter with crystals of cholesterine and remnants of the tissue in which they are deposited. The remainder of this author's observations apply more particularly to tubercle of the lung, and will therefore be more properly given under the head of special pathology.

The much disputed question of the inflammatory origin of tubercle is negatived by the researches of Mr. Rainey,† who has observed that the comparatively healthy appearance of the capillaries surrounding a mass of tubercle, in reference to the tortuous and unequally dilated state of vessels going to parts in which fibrine is deposited as the result of inflammation, are subversive of the doctrine which attributes the deposit of tubercle to that process.—*Ranking's Abstract*.

## MEDICAL PATHOLOGY AND THERAPEUTICS AND PRACTICAL MEDICINE.

12. *On the Capacity of the Lungs, and on the Respiratory Movements with the view of establishing a precise and easy Method of detecting Disease by the Spirometer*. By JOHN HUTCHINSON. (*Proceedings of Med. Chirurg. Soc.*, Ap. 28.)—After giving an introduction to the subject by a history of respiration, from the earliest period to the present, with an account of what researches have hitherto been instituted upon this inquiry, the author commenced by dividing the different portions of respired air under certain heads, without which the subject of respiration becomes very complicated and difficult of expression. He remarks—“The latitude of movement performed by the walls and floor of the chest, to maintain a constant current of air through the air cells of the lungs, admits of three common degrees of expression:—1. Extreme expansion, or enlargement; 2. Extreme contraction, or diminution; 3. An intermediate or quiescent state.

“These divisions necessarily superinduce a difference in the quantity of air respectively drawn in or thrown out of the lungs.” These movements or portions of air he described, for perspicuity, under five heads:—

1. Residual air; 2. Reserve air; 3. Breathing air; 4. Complemental air; 5. Vital capacity.

This division was clearly illustrated by different diagrams, and may be expressed best in the author's own language:—

\* *Pathol. Anat.*, p. 326. † *Med. Chirur. Trans.*, Sec. Series, vol. x., p. 594.

"It is well known that the lungs are not capable of being emptied by the most violent muscular effort; therefore, at all times, as long as the lungs maintain their natural structure during life or death, a certain quantity of air remains in these organs, which is termed 'residual air,' and over which we have no control.

"'Reserve air.'—The gentle respiratory movement regulating the ordinary breathing, is an intermediate effort between extreme voluntary thoracic contraction and dilatation, and hence it is that a portion of air always remains in the lungs after the gentle expiration, which may be thrown out if required; to it I have applied the name 'reserve air.'

"3. 'Breathing air.'—That portion required to perform the ordinary gentle inspiration and expiration, which we term, 'breathing air.'

"4. 'Complemental air.'—That portion which can at will be drawn into the lungs by a violent exertion, beyond the moderate effort of ordinary breathing, which constitutes the deepest possible inspiration, is only occasionally added if required; to this we apply the term 'complemental air.'

"5. Vital capacity in all these three latter divisions combined, being the greatest voluntary expiration, following the deepest inspiration, to which I apply the term 'vital capacity.'

"These divisions differ in their character. The residual air is independent of the will, and always in the chest. The reserve air, to use a simile, is a 'tenant at will.' The breathing air constantly passing out and in the chest. The complemental air seldom in the chest, and when it is, only so for a brief period." The connection of these with diagnosis in disease here followed, and the whole subject of the paper, resolved itself under the following heads:—1. The quantity of air expelled from the lungs in connection with other physical observations on the human frame. 2. The absolute capacity of the thorax, cubic and superficial measurements. 3. The respiratory movements and mobility of the chest. 4. The inspiratory and expiratory muscular power. 5. The elastic power of the ribs, and the voluntary muscular respiratory power. 6. The function of the intercostal muscles. 7. General and practical deductions in reference to detecting disease in man.

Each of these subjects was fully treated, and much valuable information, both for the physician and the physiologist, will be there found, but which we must here pass over, and simply add, that Mr. Hutchinson has examined upwards of 2000 cases with reference to respiration, and that the quantity of air exhaled from the lungs is affected by four circumstances—height, weight, age, and disease. By height, in the arithmetic progressions of eight cubic inches for every inch of height. By weight, nearly in the ratio of one cubic inch for every pound to eleven stone, and then decrease nearly in the same rotation up to fourteen stone. By age (from thirty-five to sixty-five), a decrease of rather more than one cubic inch per year.

Under the second head, much curious and new matter appeared in relation to the actual capacity of the thorax and the vital capacity, and the relative size of the chest to the body. Under the third head, the respiratory movements of the sexes, and relative movements in health and disease, as measured by the spirometer. Under the fourth head, the muscular power in relation to health. Under the fifth head, entirely new considerations of the combined forces of elastic or involuntary power, and muscular or voluntary respiratory power. Under the sixth head, the intercostal muscles being true expiratory muscles. Lastly, general and practical deductions.

Here Mr. Hutchison gave many interesting cases of the spirometer detecting phthisis pulmonalis, before the ordinary means now in vogue, and many other subjects relative to the application of his instrument to disease generally in the human frame, which he demonstrates as very broadly marked by twenty-five or thirty per cent. Mr. Hutchinson's spirometer was exhibited before the Society, and its methods of application explained.

Dr. TAYLOR considered the Society much indebted to Mr. Hutchinson for the laborious investigations which he had undertaken, and now submitted to them with such ample illustrations. From the fact that an abstract only of this paper could be read, and from the novelty and the variety of the views submitted to them, it could not be expected that the members could appreciate at once the true value of the observations of the author. The importance of the results arrived at could

not be doubted, if they should turn out to be true; but the results of an investigation so entirely original ought to be received with due caution, until they have been confirmed by other observers. Some very curious results arrived at by Mr. Hutchinson, might be adduced, to show the great importance, in a science like medicine, of our being guided by observation alone; for although they are not inconsistent with any known principles, they are very different from the conclusions at which we should have arrived by *a priori* speculation; such, for instance, is the law that the quantity of air which can be expired bears but little relation to the girth of the thorax, but is influenced mainly by the *height* of the individual.

Perhaps the most important fact discovered is this—viz., that if the height of the individual be known, the number of cubic inches of air which he is able to expire in the healthy state can be calculated with tolerable accuracy. The principle of this calculation is founded upon a great number of observations, and we have just had the fairest and most convincing proof of its correctness. The gentleman who has just now offered himself to be experimented upon, has actually expired within a cubic inch of the quantity of air calculated beforehand.

The application of this principle in the diagnosis of disease is most important. Mr. Hutchinson has found that persons who are the subjects of phthisis expire a quantity of air very much less than they ought to do if healthy. In one interesting case (that of Freeman), the quantity of air expired when he was in health was actually measured, and the experiment being repeated some time after, this man was found to expire ninety cubic inches less than on the first occasion; at this time no disease in the lungs had been detected by two experienced auscultators; the man, however, was not in good health, and some month afterwards he died of phthisis, as proved by post-mortem inspection. I myself examined a number of consumptive patients by Mr. Hutchinson's apparatus; I have found them to expire much less air than, by calculation, they ought to do if in health. Another singular and very important result is this, that the quantity of air which a man can expire is not necessarily proportioned to the absolute size of his lungs or the number of cubic inches of space in his chest. We have seen, in the casts exhibited, that the lungs of one man who expired a large quantity of air were actually much smaller than the lungs of another man who expired a far less quantity. Mr. Hutchinson believes that the quantity expired depends less upon the size of the chest or lungs than upon the mobility of the thoracic walls. The diminished quantity of air expired in phthisis seems to result less from the space occupied by the tubercular deposit than from the restriction which is in some way imposed upon the thoracic movements. The bulk of the tubercular deposit in one case was only equal to one cubic inch, whilst the diminution in the quantity of expired air was forty-seven cubic inches. Hence, by this apparatus, it may turn out to be possible for us to distinguish phthisis at an earlier period than by any other means; and this has been actually proved in one of the instances adduced.

The same principle, however, complicates very much the application of Mr. Hutchinson's results in the differential diagnosis of disease. Many diseases, without the chest as well as within it, may limit the respiratory movements, and so lessen the quantity of air expired. So that, for the present, a great diminution of the expired air can only be considered to indicate the existence of disease somewhere: the seat and nature of this disease must be determined by other means, unless further investigation shall render the indications of the spirometer more precise than they are at present. As much has been accomplished in this direction as we could look for from one individual, and in a limited time. Mr. Hutchinson has done enough to make it incumbent upon physicians generally to prosecute the investigation, so as to lead to the confirmation or invalidation of his views, as well as to their extension and limitation in particular cases. In the hospital to which I am attached, one of the spirometers has been purchased, and a certain number of observations have been made, which, as far as they go, quite confirm those of Mr. Hutchinson.

A great advantage in the use of the spirometer in disease of the chest is this; that comparatively little education is required for it. The practice of auscultation requires a long education and constant application; the spirometer may be used, for the purposes to which it is applicable, with very little previous study. Hence

the importance of the instrument, as pointed out by the author, to army and navy surgeons, to insurance companies, and in private practice, generally.

After the paper had been read, Mr. Hutchinson illustrated the method of making an examination. Taking himself as an example, he observed, that though his chest was small, (thirty-three inches in circumference,) yet, according to his mere height, his vital capacity was 260 cubic inches; noting, at the same time, that the person being examined should hold himself as erect as possible, the least inclination of the head causing a difference in the quantity of air expired. A Fellow of the Society having complied with the request to come forward and submit himself to experiment, his height was found to be five feet five inches, and accordingly the quantity of air he could expel should, by the tables, be 206 cubic inches. The gentleman then breathed into the spirometer, and actually produced 205 cubic inches, which, making allowance for temperature, made it 206 cubic inches. This was so satisfactory, as to leave no doubt on the mind that these tables must have been calculated with care, and upon an extensive number of cases. Many questions were put to the author upon the different subjects of which the voluminous paper treated.

Dr. CHOWNE requested to know whether any explanation could be given for the following curious phenomenon, which was strikingly apposite, touching the relation of the chest to the respiratory power,—Why the mere height bore so strict a relation to vital capacity, and not to the size of the chest.

Mr. HUTCHINSON, in reply, observed, that he had found this hitherto inexplicable. It was a fact, that the taller a man, the more air he could expel from his lungs: and he considered that this quantity was directly dependent upon the extent of thoracic and abdominal mobility, and not upon the actual capacity of the cavity of the thorax. He referred to a series of casts taken from the chest—the heart and lungs having been first removed, and their space filled up with plaster of Paris. He exhibited one cast, taken under most favourable circumstances; the individual while living, and apparently in health, he had measured; his vital capacity, making correction for temperature, is 251 cubic inches; while the actual cubic space in his chest for the organs of inspiration was only 248 cubic inches, being three inches less than the actual quantity breathed; therefore this marked a respiratory mobility exceeding the actual capacity of the thorax. Therefore, while the spirometer measures the vital capacity, it becomes at the same time a measurer of the mobility of the parts concerned in the breathing act. Now to measure the mobility is to determine the respiratory movement, which he illustrated by drawings, as extending over the whole trunk of the body down to below the knees; therefore whatever interfered in these parts to prevent the range of movement, would, at the same time, be indicated upon the spirometer, and collateral observation would determine more minutely the locality affected. He could not here omit mentioning another circumstance which militated against an opinion prevalent at this day. It was commonly believed that adhesion of the pleura prevented the free movements of the chest; but this he believed was not the case, for he had found that in the instance just mentioned, where the mobility exceeded the actual thoracic cavity, there was no part of the pleura pulmonalis but was firmly adherent to the pleura costalis. So much was this the case, that the heart and lungs could not be weighed separately, their structure being totally destroyed in removing them. Here was a mobility exceeding by three cubic inches the actual space allotted for the heart and lungs, with the parietes and floor of the chest firmly adhering to the lungs. He believed that the latent movement of the ribs was so little, or the latent movement between the contents of the chest and its boundaries so limited, that no adhesion interfered with the respiratory actions. This was worthy of attention, the contrary opinion appearing to prevail at present.

Mr. Hutchinson pointed to a cast of a chest measuring 457 cubic inches, while 202 cubic inches of air only could be exhaled; or not one half of the space allotted for the organs. This man was corpulent, and only 5 feet 8 inches in height; therefore his mobility was mechanically impeded. Why this mobility increased with height, he felt as much at a loss to account for as he did the first day he commenced the investigation. But he was confident it was so, as upwards of 2000 cases went to prove it.

Mr. Hutchinson then pointed to a curiously interesting table, where all the

heights, increasing in the arithmetical rotation of inch by inch, between five and six feet, were drawn by scale, and upon these the relative depth and breadth of the chest, so that the whole proportion of the frame was exhibited. The thoracic cavity was seen not to increase in any visible relation with the increasing height: the taller men here exhibiting smaller chests, in all dimensions than the shorter. Moreover, it was remarkable to observe how shallow the chest was, the average depth, from the apex of the lungs to the diaphragm, being only from seven and a half to eight and a half inches; and that the shortest men, (five feet four), who could blow least, had the deepest chests of all the other heights. The absolute length of the back also appeared to have nothing to do with the question, for he had taken the sitting height and the standing height, which averaged three feet, whatever was the standing height; and that men whose standing height was many inches shorter than others, who sit taller, yet blow less out of the lungs. Here Mr. Hutchinson said he must quit this part of the subject, "for years have I studied to determine this question, but in vain."

Mr. Hutchinson was asked upon what grounds he imagined that the chest in deep expiration did not enlarge by the diaphragm descending; this being the present and general opinion of physiologists. He replied, that he had invariably found, in health, that when a man stood and made a deep inspiration, the movements were as described by the diagram already referred to—viz. that the chest, in this violent effort, enlarged its dimensions almost entirely by the ribs; the sternum advanced, the ribs and shoulders were elevated; the abdomen receded, compressing the abdominal viscera; the lateral enlargement of the chest was very small, chiefly increasing in the antero-posterior diameter. Now, he considered himself at a loss to conceive how the diaphragm could descend and the abdominal muscles recede; the point of the recti muscles, in deep inspiration, would be found as posterior to the point of these muscles in ordinary breathing; therefore the diaphragm must be pushed up, and it was perfectly possible to conceive the diaphragm to enlarge its circumference without descending; for the convolution of this muscle was such that its area was nearly quadruple that of the sectional area of the chest. The ordinary breathing was entirely abdominal or diaphragmatic in men, but the contrary in deep breathing. The chest appeared to have two ways of enlarging,—one by its diaphragm, and the other by its ribs; but these did not act in concert, either by one or the other separately. This observation must not be made in the recumbent position, for then the back of the body becomes fixed, and thus medical men might be led into an error in examining the breathing movement of a patient in bed, when the very position altered their motions. From a knowledge of this, he was inclined to believe that this fixing of the back in the recumbent position caused patients labouring under dyspnœa to prefer the erect position for breathing.

The breathing in women differed from men only in one respect: their ordinary breathing being chiefly costal, and not abdominal. Whether this was to allow for gestation or not, he could not say; he thought there was some doubt of its being caused by the peculiarities of their costume.

Mr. Hutchinson was then asked by Mr. Ayston whether there was any relation between the respiratory power and the respiratory quantity in vital capacity? To which he replied, there was a certain indirect relation. It was seen that the vital capacity increased with mere height; but by another instrument exhibited before the society, that the inspiratory power was greatest at the height of five feet eight inches, and decreases in regular progression as the height increases; and that the men of five feet eight inches could elevate by the true inspiratory muscles a column of three inches of mercury, equal, in some cases, depending upon the size of the chest, to many hundreds of pounds' resistance to their muscles, whereas the six-foot men could only do two inches and a half, and that the expiratory power was one-third higher, but not from the muscular power, but the elastic power, of the ribs being here introduced, which is not so in the inspiratory act. This inspiratory power, he considered, indicated the *vis vita* of the individual more than the expiratory power, which was affected by vocation. An elaborate series of calculations were contained in the paper upon the elastic power, and the muscular contractile power alternating in every inspiration and expiration. But this question cannot, in justice to the author, be abridged; therefore the paper must be con-

sulted, as this is a question for the closet. This elastic power has hitherto been very slightly treated by physiologists, and the circumstances under which Mr. Hutchinson determined his views are so rare and conclusive, and we may say impossible of being obtained again, that they demand the full inspection and consideration of the physiologist. But this much may be added, that Mr. Hutchinson considers the inspiratory act as demanding a muscular power to resist an elastic power of many hundreds of pounds; that is, a direct resistance to vital energy, with no counterbalance power.

Mr. Hutchinson was asked whether he had examined any men of remarkably small stature, whose height, as they sit, is not so remarkable as their standing height.

He replied, that he had, in the course of his observations, sought out the giant and the dwarf. He then handed a spirited sketch of a human being, whose height was under thirty inches—Don Francisco, aged forty-two. This little gentleman had a vital capacity of fifty-six cubic inches. He then exhibited the shade of the hand of Mr. Randall, seven feet high, the span of whose hand was nearly thirteen inches, and the length of his foot thirteen inches and a half. He had a vital capacity of 464 cubic inches, at 60°, which, if calculated at 98°, the temperature of the body, must bring it nearly to 500 cubic inches!

13. *Influence of the Quality of the Milk of the Nurse on the Health of the Child.*—In our number for January last, p. 215, we gave an account of some interesting researches by M. Donné on this subject. The following cases related by M. GIRARD in the *Journal de Pharmacie*, (Oct. 1845,) are confirmatory of the conclusions of M. D.

A young lady fifteen days after her delivery, found it necessary to procure a nurse for her child, and she succeeded in getting one whose milk was only fifteen days older than her own, and which presented all the physical characters of good milk. The nurse was besides young, and in apparently good health. After a few days the child began to sleep ill; it was attacked with a troublesome diarrhœa, and sensibly fell off; shortly after this it rejected all its food, and the mouth got foul with aphthæ. As the ordinary medical treatment failed to relieve the child, the nurse's milk was examined, and was found to present the following characters:—The milk was neuter, of an ordinary consistence, and became slightly viscous by ammonia. The globules were very voluminous, and instead of appearing under the microscope of a brilliant pearly lustre, were of a dull white opal colour. The whole field of the microscope was scattered with rounded particles, of which the form was comparable to that of a flattened raspberry, and presented the characters usually assigned to the corpuscles of colostrum. A new nurse was therefore, instantly procured, whose milk presented the microscopic properties of healthy milk. Within forty hours after the child had sucked this nurse the vomiting and diarrhœa had ceased, the aphthæ were greatly diminished, and a few days sufficed to restore the child to strength.

In another case where the mother was the nurse, the child, two months old, suffered from constant vomiting, was falling off, and had occasional comatose attacks. The milk of the mother, when examined by the microscope, was found to contain a considerable quantity of mucus. After a while the child vomited less, and the milk being anew examined, was found to contain fewer mucous globules. Soon, however, the vomitings and other disagreeable symptoms became worse, and to save the child it was absolutely necessary to change the nurse. Eight nurses were examined, but the milk of seven was rejected on account of containing globules of mucus, or granular bodies, or from having undergone some other change. The eighth nurse having apparently healthy milk, was chosen, and within two days the vomiting and other symptoms had disappeared, and did not again return.

14. *New researches on the Œdema of New-born Infants.* By M. HENRY ROGER.—In an interesting paper on scleroma or the œdematous hardening of the cellular tissue of new-born children, M. Roger points out the important fact, that in proportion to the extent and severity of that disease, is the lowering of the temperature of the child. This lowering of the temperature is not confined to the



affected parts, but pervades the whole system; and seems to be essentially connected with the pathological states which cause the disease. M. Roger gives a table of the temperatures of 29 cases of this disease, in all of which it was observed that the more severe the affection the lower was the temperature. Thus in the cases in which the œdema was slight the temperature was normal, *i. e.* 98° Fahrenheit, or one or two degrees below it; but in the severe cases, even at the outset of the disease, the temperature was found so low as 75°. This was the lowest temperature noticed in any of the fatal cases; and it is remarkable that it is the same as that which was found to prove fatal to animals which were starved in the experiments of Chossat on inanition.

In commenting on the other pathological appearances met with in the bodies of these infants, M. Roger directs especial attention to those of the lungs which are in their nature identical with those observed following the pneumonia of infants. He, therefore, considers that authors are much mistaken in calling that disease inflammatory, or those changes products of inflammatory action which are only observed in infants whose temperature is lower than natural, and whose whole functions indicate torpor and congestion, but not inflammation.—*Ed. Med. and Surg. Journ.*, July, 1846, from *Archives Gén.*, May, 1845.

15. *Ulceration of the Gums in Children occurring in an Epidemic form.*—Dr. JAMES DUNCAN describes, in the *Dublin Journal of Med. Sci.*, (Sept. 1845,) a severe and fatal ulceration of the gums and mouth of young children, attended with high fever, and apparently of an epidemic origin. Diarrhœa was the first symptom of illness observed, but there was no unusual pain of the bowels. The evacuations were unhealthy, watery, but not generally deficient in bile. In some cases, however, they were whitish, and very offensive. Blood was discharged in almost every case, either in a fluid state or mixed with jelly-like mucus. After these symptoms had continued a week or ten days the soreness of the mouth attracted attention. The gums were then found ulcerated, the fangs of the teeth exposed, and covered with a yellowish-white sordes. As the disease advanced the gums lost their pale flesh colour, and became red, swollen, and spongy, and bled both spontaneously and on being touched. The breath gradually became offensive, and the secretion of saliva became excessive. Partly from the fever, partly from the diseased state of the gums, the children could not take food, but their thirst was often excessive. The fatal termination seemed rather due to the violence of the attending fever, or the severity of the diarrhœa, than to the diseased state of the mouth. Though the diseased condition of the mouth closely resembled that produced from the administration of mercury, this drug excited rather a beneficial influence when exhibited, and tended to modify the disease.

In commenting on the nature of the affection, Dr. Duncan endeavours to show that the impaired state of the constitution is its true cause, and that in many points it presents an analogy to *purpura hemorrhagica*. One very interesting case is related of one of the members of a family being attacked with ulceration of the gums, while another was attacked with measles and purpura.

It was found that local applications, as borax and honey, muriatic acid, and sulphate of copper, had no appreciable influence on the disease; nor did the ordinary astringents, as chalk mixture, catechu, acetate of lead, or even opium itself, succeed in checking the diarrhœa. The best internal medicine was an acidulated decoction of bark, or an infusion of columba with nitric acid. These medicines controlled in a remarkable degree the diarrhœa, which resisted other remedies. The most valuable remedial agent, however, was counter-irritation produced on the abdominal surface. This was most easily effected by a mustard cataplasm kept on for an hour, by which time it produced free vesication, which was never followed by the unpleasant effects often observed after a cantharides blister. Wine was required in every case.

In another paper in the same journal by Dr. F. Battersby "on some unusual complications and sequelæ of measles," the same diarrhœa and peculiar ulcerated state of the mouth were very commonly remarked. In many of his cases, however, this state was accompanied by asthenic inflammation of the pharynx, larynx, and bronchii, which often proved fatal after obstinately continuing for one or more months.

first be reduced to ashes in a platinum crucible, and then the mineral salts which remain dissolved by a little dilute nitric acid; after this the chloride of silver is left behind, and may be weighed and estimated; it is completely soluble in ammonia.

Heller found that when he mixed nitrate of silver with the gastric fluid, chloride of silver was at once precipitated, for the gastric fluid contains a large quantity of the chlorides of sodium, potassium, and calcium; when ten grains of the nitrate was added to an ounce of the gastric fluid, the whole was precipitated in the form of chloride. From this fact Heller concludes, that so soon as nitrate of silver is taken into the stomach it is precipitated as chloride, which being an insoluble substance passes through the entire length of the intestinal canal, and appears as such in the feces; the results of examination of the blood and urine also show that none of the silver enters into the blood. This renders easily intelligible the circumstance that such large doses of nitrate of silver may be taken without any observable effect on the system resulting, yet according to Heller, leaves unexplained the statements of some writers, who describe the skin of patients who have employed the nitrate of silver for some time, as becoming brown or even black; he seems inclined to doubt the truth of these statements, inasmuch as they are opposed to the results of his experiments, and to the fact of this decoloration never occurring in any of the cases which he noticed.—*Lond. Med. Gaz.*, July 24, from *Heller's Archiv. für Phys.*, vol. i.

17. *Means of Preventing Bed Sores* (*Dublin Med. Press*, July 22d, 1846).—Dr. T. PUREFOY has employed partially inflated bladders with success as a support to parts long and unduly subjected to pressure. The bladder is to be previously moistened in tepid water, afterwards oiled and subsequently partially inflated. He has found this means to promote the comfort of the patient and to obviate sloughing of the integuments.

18. *On the Diagnosis of Bright's Disease and its Relations to Albuminous Urine*. By G. OWEN REES, M.D.—In this interesting paper the author first directs his attention to those symptoms which lead to the detection of the morbus Brightii, when the more common effects produced by the disease are either not prominently marked, or altogether absent; and secondly, to notice certain albuminous conditions of urine, which must be distinguished from that form more especially connected with a deranged condition of the kidney.

In relation to the first part of my subject, I may perhaps be excused in remarking upon an error which has materially interfered with the progress of medical truths in all ages, and which still flourishes unfortunately as an effect either of imperfect education or natural obliquity of understanding, to the great detriment of our art. I allude to the notion that we can arrive at a satisfactory conclusion on any part of a subject by reflecting upon what we may have observed previously, and at a period when our attention was not especially directed to such point of inquiry. It is true that we can in some cases at once and safely contradict assertions involving sweeping generalizations, referring to well-known and frequent symptoms; but these are rarely the instances we are called to decide upon, and we must be extremely careful how we pride ourselves on powers of judgment derived from retrospective thought, when considering the truth of observations relating to diseases of organs like the kidneys, and which refer to derangements in the healthy character of a secretion, which, like the urine, is even now very imperfectly examined by practitioners.

Notwithstanding the obvious truth of this, the discovery of Dr. Bright is occasionally repudiated, on the ground that the long experience of an individual has not presented him with cases bearing the stamp of the disease in question: nor is it easy to persuade a particular class of persons of the great differences obtained as the results of observation, according as the mind may be prepared, or, on the other hand, unacquainted with the history of the subject for investigation.

The attention of Dr. Bright was first directed to the subject of his discovery in relation to the presence of anasarca, and few practitioners are now likely to overlook the condition of the urine, as to the presence or absence of albumen, when this symptom is present in any marked degree. The kidney, however, is some-

times diseased, being either in a congested or granular condition without the occurrence of anasarca. Albuminous urine may then be passed, and other certain characteristic symptoms of the morbus Brightii be present, which may, or may not, afterwards become associated with dropsy.

It is important that these symptoms, which, independent of anasarca, point to a diseased condition of the kidney, should be familiar to the practitioner, in order that the disease may be early detected, and subjected to appropriate methods of treatment.

Now it not unfrequently happens that patients present themselves for relief at the hospital, in whom the leucophlegmatic or anæmic aspect is strongly marked. They complain, perhaps of having taken cold, and feel pain and dizziness in the head, with shooting pains in the loins and down the limbs. To these general symptoms, so often met with in ordinary colds or in influenza, we do not find superadded the excitement of pulse or heat of skin noticed in such affections; on the contrary, the skin is often cool, though dry, and this circumstance, if it be present in connection with *nausea* or *vomiting* as a prominent symptom, should be sufficient to direct attention to the kidney, and may probably lead us to elicit, on further inquiry, a list of symptoms more or less confirmatory of the suspicion that the organ is in a deranged condition.

The additional evidence we meet with, should the case be as we may have suspected, consists principally in the presence of the following symptoms: puffiness of the eyelids, either sufficiently persistent to be observed at the period of examination, or which may have been noticed by the patient during the first hour or two after rising from bed, but which disappears as the day advances, as an effect of gravitation resulting from the maintenance of the erect or semi-erect position. The urine will often be described as clear, but passed at short intervals, and the patient will remember having been frequently obliged to rise from bed to micturate.

This latter set of symptoms (to a knowledge of which we may have attained by acting on an early suspicion principally derived from the state of the stomach complained of by the patient), should at once lead us to examine the urine, and we shall generally find, in such cases, that albumen may be detected in considerable quantity, and the progress of the case afterwards prove that the morbus Brightii is present, possibly in an aggravated form.

I have here described what may be considered as an intermediate case; at least so far as the difficulty of diagnosis is concerned. We often meet with patients who do not answer fully to the description I have given above; while, on the other hand, the less probable symptoms which I have described as leading to our suspicions, may lead to our discovering all the confirmatory indications of the morbus Brightii. Thus the endocardium may be seriously involved, such lesion being obvious enough on auscultation, notwithstanding that it may not yet have proceeded so far as to produce urgent symptoms.

Again inquiry may afford us the history of a long and painful salivation, for which no very laudatory mention is made of the skill and humanity of a former attendant, or patients may even present themselves to us while under mercurial influence, and our inquiries lead to no very sufficient reason for so powerful and sudden an effect having been produced by the drug.

The excessive action of small doses of mercury thus often becomes a valuable guide for the detection of albuminuria, though a painful and injurious result for the patient, and one which no practitioner familiar with the disease would willingly bring about.

There is a severe form of cerebral disease which occurs in connection with the excretion of albumen with the urine, showing some peculiarities of a very marked character, and to which the attention of practitioners was especially called by Dr. Addison, in a paper published in the *Guy's Hospital Reports*, for April, 1839.

This form of brain affection is characterized especially by the anæmic aspect of the patient, by a quiet pulse, and frequently only partial coma. If there be any stertor, it is of a peculiar kind, and characterized by a low hissing sound.

These seizures are frequently attended with convulsive attacks occurring at intervals. Paralysis is not necessarily present, but still may occasionally exist, and anasarca is generally wanting.

This kind of case has been much overlooked; at least, as connected with disease of the kidney; and I will therefore describe the history of a man lately admitted into Guy's Hospital, under the care of Dr. Babington, as affording an instance of a form of kidney disease which might not have been detected by a less accomplished practitioner.

John O'Connel, admitted into Lazarus ward on the 6th of May, 1846. He is in a semi-comatose state. When roused he answers questions with difficulty, and unsatisfactorily. He lies on his back, and vomits frequently. As far as can be ascertained from his answers, the epigastrium and head are the seats of pain. Motion and sensation of the left side of the body impaired; the paralyzed side is the colder to the touch; tongue loaded with thick dark-brown fur; eyes starting and vacant. No remarkable dilatation or contraction of either pupil; the right somewhat the more dilated; pulse 76 per minute; respiration appears normal; chest healthily resonant; except perhaps over the pericardial region; heart's impulse rather excessive, somewhat irregular; liver enlarged: bowels constipated; body emaciated; no anasarca. (This condition, notwithstanding the absence of anasarca, at once led to a suspicion that the kidneys were in a diseased state, and further examination showed this to be the case.) Urine scanty, high-coloured, acid, strongly coagulated by heat, and also by nitric acid: specific gravity 1010.

After some days' treatment this man recovered sufficiently to give an account of himself, and related a history such as is constantly received from persons affected with the morbus Brightii.

These further particulars were as follows:—

He is 34 years of age, married, no children; a weaver by trade: had lived very intemperately till twelve or fifteen months ago: used to drink both beer and spirits: until this attack commenced he had always had good health. Is of a healthy family. Six months ago caught cold by sitting at work eight hours in damp clothes: had then severe cough for two months, and swellings of the legs, headache and vomiting, from which symptoms he has suffered more or less until the present time.

Cases such as the above are not of very unfrequent occurrence, and though the symptoms are severe, they are such as to direct attention so exclusively to the brain that the condition of the kidney but too often escapes detection.

I will now proceed to the second part of my subject, and describe various forms of urine containing albumen, and especially notice that which is characteristic of the morbus Brightii, as it too frequently happens that errors occur in the diagnosis and treatment of renal cases from a want of familiarity with the appearance and reactions of the urine.

In my description here, I shall assume that the presence of albumen in the urine has in every case been satisfactorily established, and shall not enter upon a consideration of the various sources of fallacy connected with each test used by the practitioner, but assume him capable of the task of accurately examining the urine in relation to this important point.

Urine may be excreted in the albuminous condition, owing to the presence of pus, blood, serum of blood, and (to a small extent) the presence of semen in the fluid.

The presence of pus, which is best proved microscopically, may be owing to an excretion either from the bladder, ureters, or kidneys, and therefore that urine which is rendered albuminous by its presence is in no way necessarily characteristic of kidney disease. It is not uncommon to find urine highly albuminous in cases of diseased bladder during the period that pus is pouring from the mucous membrane, and to observe its gradual disappearance in proportion as the deposit of pus decreases, and ultimately to find the urine free from albumen. This occurs, of course, only when the bladder alone is affected; for if the kidney be the subject of congestive or granular disease, we shall observe the urine persistently albuminous even after the deposit of pus has ceased to appear: and it is by attention to this point that we are often able to determine that kidney disease exists in addition to the more obvious and more aggravated disease of the bladder. I do not here allude to sources of fallacy connected with the presence of pus in the chamber-vessel, which is derived from the uterine or vaginal discharges of females: it is well to mention, however, that such sources of fallacy may interfere with our dis-

covering the truth, unless proper precautions are had recourse to; the best possible among which is to use the catheter in order to obtain our specimen for examination.

Urine often contains albumen, owing to the presence of blood. It is then either of a bright red or dark porter colour. Both these kinds of bloody urine are occasionally observed in the morbus Brightii, and chiefly in its early stages; but they are not especially or necessarily characteristic of it, which may be pretty certainly said of true serous urine.

Serous urine is not to be confounded with that which contains blood; it is free from hæmatosine, at least so far as the unassisted eye can determine; and under the microscope the true blood-corpuscles are seen only in very small number. The albumen it contains is derived from the serum of the blood, which passes from the kidney to the almost total exclusion of the blood-corpuscles and fibrin.

To come to a determined opinion on the existence of the morbus Brightii, it is necessary that serous urine should be passed for a length of time, and that this should be the great leading feature of the disease, and that if any other form of urine be passed containing albumen, owing to blood or pus, that such circumstances should not be regarded as necessary to the complete history of the case, and moreover to be looked upon as a condition which must be eliminated as a source of fallacy, so far as concerns the production of albumen in the urine, before we can feel ourselves justified in concluding that the kidney is prone to the granular degeneration.

When serous urine has been passed for any length of time, say for weeks or months, and both time and treatment fail to remove its quality of coagulability, we may conclude on the presence of kidney disease.

The probability of a cure being effected in any such case, or, on the other hand, the inevitable fatal termination of the disease, is a subject on which considerable difference of opinion will exist; a discrepancy principally owing to the fact, that we are not yet able to decide upon the exact stage of the disease—to ascertain, in fact, whether or not the congestive condition has yet given place to the deposit of adventitious matter in the substance of the organ. No one would be inclined to deny that the kidneys, which in health are engaged in excreting from the blood a large quantity of effete material, could have their action interfered with for any great length of time by organic lesion, without the destruction of the patient resulting as a consequence; and it is as clearly proved from clinical and pathological observation that the congestive stage of the morbus Brightii, accompanied by serous urine, may exist for some time without being followed by the mottled kidney, and the patient be restored to health under appropriate treatment.—*Lond. Med. Gaz.*, July, 1846.

19. *On the Treatment of Bright's Disease of the Kidney.*—The *Northern Journal of Medicine* contains an elaborate essay on Bright's disease of the kidney, by Dr. ALEXANDER WOOD, from which we extract the following interesting remarks relative to the therapeutics of that affection.

1. *Therapeutics of Acute Variety.* The indications for treatment in this variety of the disease are—

- 1st. Remove the congestion of the kidneys.
- 2d. Restore the function of the skin.
- 3d. Remove the dropsical effusion.

Almost all the authors who have written on this disease are agreed as to the propriety of *blood-letting*, and assign it the highest place as a therapeutic agent. The quantity of blood to be drawn must be decided by a consideration of the strength of the patient, the intensity and period of the disease, and the appearance of the blood drawn.

“In addition to the usual means of judging upon these points,” observes Dr. Christison, “the practitioner will find great advantage in taking also into account the composition of the blood. On every occasion where blood-letting is resorted to by him for the first time, it is advisable to examine the state of the blood, more especially with a view to discover the proportion of its colouring matter or hæmatosin, and so to determine the exact progress of the structural derangement of the kidneys. For, if it should thence appear that the disease is not in its early, but in its advanced stage, and that the acute symptoms are conse-

quently not primary, but superinduced, depletory measures must be applied with much greater caution and reserve."—(pp. 133, 134.)

It is generally recommended to follow up the venesection by *local depletion*, either by cupping or leeches. I have seldom found it necessary, at least in the variety occurring secondarily to scarlatina, the one most commonly met with, to have recourse to this, venesection alone accomplishing all that is necessary.

Should the patient, however, complain much of pain in the region of the kidneys, there can be no doubt of the propriety of having recourse to cupping or leeches, more especially if percussion can detect engorgement of one or both kidneys.

These vigorous means, besides fulfilling the first indication, will in a great measure have prepared the way for successfully accomplishing the second, the antiphlogistic treatment proving in this, as in most inflammatory affections, the best preparative at least for the diaphoretic.

The temperature of the patient's apartment should be elevated and kept at an equable standard, and the action of diaphoretic medicines, aided by the occasional employment of the *warm or vapour bath*.

The use of Dover's powder, as a diaphoretic, is highly extolled by Professor Christison, and its operation may be aided by the occasional employment of the warm or vapour bath.

Tartar emetic is much recommended by Dr. Barlow.

After stating that the recovery of patients treated by purgatives, diuretics, occasional bleedings, and diaphoretics, is for the most part tedious, and sometimes incomplete, and that depletion is in general not well borne, he continues:—"I was accordingly induced to seek for some other remedy, and regarding the affection to be essentially of an inflammatory character, as evinced by the increased frequency of the pulse, and the state of the blood when drawn; and considering, moreover, the probably injurious effects of medicines, which act as direct stimulants to the kidneys, in the recent stages of this form of disease, and the importance, consequently, of promoting, if possible, the secretion from the skin, I was led to make trial of tartarized antimony, which I found fully to answer my expectations."—(*Guy's Hosp. Rep.*, vol. v. p. 168.) And again,—“It (tartar emetic) is a remedy suggested by the nature of the affection, and calculated to fulfil the most obvious and important indications, namely, equalizing the circulation, subduing the inflammatory action, and restoring the functions of the skin.”—(*Ibid.*, p. 178.)

I confess that the trials I have been enabled to make of this medicine, have seldom fulfilled the expectations which such statements encourage; and that I have more than once seen all the symptoms advance with alarming rapidity under the active employment of tartar emetic.

For the purpose of removing the dropsical effusion, two classes of remedies recommend themselves—diuretics and purgatives.

With regard to the employment of the first, considerable difference of opinion prevails. Diuretics, in order to act, cause determination of blood to the kidneys in this disease. These glands are already in a state of congestion, and therefore, by such remedies, that congestion must be increased.

Plausible as such reasoning may appear, I question if, in actual practice, the use of diuretics is ever really injurious; it has certainly been condemned both by Dr. Bright and Dr. Osborne; but, on the other side, we have the testimony of Dr. Christison, who “has very seldom witnessed decided diminution of the dropsy, unless where diuresis or purging was either artificially induced at the time, or arose spontaneously,” and has “even repeatedly seen the albumen disappear under diuretics.”

I have found great benefit from the use of nitre in this disease, which has certainly a remarkable effect in removing the uneasiness of the loins, so often complained of; its diuretic effect may be amply secured by combination with digitalis and cream of tartar. To secure the full benefit of this remedy, however, it must be given in very large doses, which, if the salt be largely diluted, may very safely be done.\*

\* In the Posological Table of Duncan's Dispensatory, the dose of this salt is set down as from gr. x to ℥ss. In Dr. Christison's as gr. x to ℥j as a sedative refriger-

In some cases I have observed very copious perspirations to accompany its employment, and it is singular that in these it seemed the least beneficial. Given in these large doses, it has a marked effect in lowering the pulse, without at the same time affecting the tone of the system so much as tartar emetic. Where it induces vomiting or purging, the addition of a few drops of laudanum to each dose will check these troublesome symptoms. It seems speedily to enter the urine, and very soon after it has been swallowed, it may be detected in that secretion.

There can be no doubt as to the advantage of free purgation in the acute form of the disease, and we should naturally select those purgatives which produce serous evacuations. Gamboge, elaterium, and the compound powder of jalap, are all-powerful remedies of this kind.

The patient must of course be carefully preserved from cold and damp, and protected from all exposure during convalescence.

The following treatment is confidently recommended by Dr. Golding Bird:—

"I may remark, that as a prophylactic remedy, the warm bath is invaluable; I scarcely recollect, even in a large experience, a case of dropsy after scarlet fever, occurring when the warm bath was daily used, as soon as the skin began to exfoliate, and continued until a perspiring healthy surface was obtained. When anasarca has occurred, strict confinement to bed, or at least to a warm room, must be enjoined, the warm bath used twice a week, and a free action on the skin encouraged. The bowels should be kept acting by the pulvis jalapæ compositus, and the antimonii potassio-tartras administered in doses varying from one-twelfth to one-eighth of a grain, four or five times in the 24 hours, according to the age and strength of the patient. A bland and nearly fluid, but moderately nutritious diet should be enjoined.

"The plan must be continued until all anasarca has vanished, a supple and perspiring surface obtained, and urine free from albumen.

"The remedies may then be gradually left off, and more nutritious diet allowed, and the ammonio-citrate of iron administered thrice daily, in doses of three to five grains, to remove the anemiated state of the patient.

"On leaving the bed-room, a flannel waistcoat extending to the loins should be worn for some time. This treatment has been almost invariably successful in every case I have employed it; and I may remark, that I have never in these cases witnessed the extensive prostration said by some to be the almost necessary result of the employment of antimony in the diseases of children."—(*Urinary Deposits, &c.*, p. 235.)

2. *Treatment of the Chronic Form.*—However simple in its indications and usually satisfactory in its results the treatment of the acute form may be, it is far otherwise with the chronic. In it, one chief aim must be to ward off as long as possible those complications from which the suffering and danger of the patient chiefly arise.

Should there be any evidence of febrile movement in the system, depletion may be practised as in the acute form, or if active congestion of the kidneys should exist or supervene, leeches or cupping may be applied to the loins.

The functions of the skin should be particularly attended to; it should be preserved in a perspirable condition by warm clothing, and the occasional use of diaphoretics, and the warm or vapour bath.

We do not seem to possess any remedies which have the power of altering the morbid state of the kidneys. Mercurials, from their power of subduing inflammation, and removing glandular obstructions, might appear worthy of trial, but experience has decided as to their inutility; and in this disease not only is salivation easily excited, but is also liable to become violent and unmanageable.

Dr. Bright has occasionally seen the character of the urine change, and the dropsical effusion disappear under the use of tincture of cantharides, given in

ant, and ℞ii to ʒj as a diuretic. It may, however, be safely given in much larger doses. Dr. Broklesby gave ʒii dissolved in a quart of gruel, three, four, or five times a-day, in acute rheumatism; and in the same disease, Martin-Solon has given from ʒss to ʒxv in divided doses, in the 24 hours.—(*Bull. de Thérap.*, 1843.)

doses of from four to twelve drops, in some emulsion. Dr. Wells, in his paper already referred to, narrates five cases of dropsy with coagulable urine, treated with this remedy, in doses of from thirty to sixty drops in the twenty-four hours. In three of these the improvement was very decided; in one the albumen disappeared from the urine; in the remaining two no benefit seemed to result from its use.

M. Monneret has since strongly recommended the same medicine. (*Gazette des Hopitaux*, Oct. 13, 1842.)

M. Gutbrød tried the ioduret of iron in two well-marked cases of the disease with success. (*Ibid.*, Sept. 7, 1843.)

M. Alken has found benefit result from the employment of hydriodate of potass, and iodine ointment. (*Oesterreich. Med. Wochens.*, Jan. 28, 1843.) M. Rayer has found no benefit to result from the external application of mercurial or iodine ointment.

Dr. Rées following out his idea of the pathology of the disease, of which a summary has already been given, recommends the plan of treatment usually resorted to in anæmic diseases, viz., chalybeate tonics, saline purgatives, and nutritious diet, and strongly condemns depletion. (*Med. Gazette*, Aug. 16, 1844, p. 654.)

Dr. Copland has given equal quantities of the tincture of cantharides, and the tincture of sesqui-chloride of iron with marked benefit in a few cases.

In the more advanced states of the disease, the same author has seen more advantage from the preparations of iron judiciously chosen and combined with other remedies, than from any other class of medicines.

3. *Treatment of the Complications.*—A. *Dropsy.*—We need scarcely recapitulate what has been already said of the means of removing this symptom. The diaphoretic plan, so strongly recommended by Dr. Osborne, is not much esteemed by Dr. Christison or Rayer, while the latter author agrees with Dr. Bright in thinking that the importance of diuretics has been overrated by the former.

Dr. Christison is disposed to place much reliance on the combined use of cream of tartar and digitalis.

From a drachm to a drachm and a half of the latter substance is given three times a-day, the patient taking at the same time a pill, containing one or two grains of the powder of digitalis, or 20 drops of the tincture may be given in a little cinnamon water. The diuretic effect of these medicines may be increased by the exhibition of a blue pill every night, until four or five have been taken. Occasionally diuresis may be established by the administration of an emetic of ipecacuanha and tartar emetic, and sometimes even by the action of a hydragogue cathartic.

Should these fail, we are recommended to have recourse to squills, infusion of broom tops, or spirit of nitric ether, or Hollands with water, or carbonate, nitrate, or acetate of potass.

Many of these are condemned by Dr. Copland, as liable to disorder the stomach, and thereby farther impair digestion and assimilation, and thus accelerate the unfavourable progress of the disease.

M. Rayer prefers the decoction of the wild horseradish to all other diuretics.

Dr. Christison draws an admirable distinction between the mistaken employment of diuretics as a supposed cure for the disease, and their judicious use as a means of combating this particular symptom. He is not inclined to attach any value to the theoretical arguments of those who have supposed that diuretics may act injuriously; he condemns them in the early stage, because, during the state of general reaction, they do not generally excite their proper action, and the urine is restored to its healthy amount with much greater certainty by subduing the reaction; that is, by antiphlogistic treatment, and also because there is no special indication for their employment, unless when dropsy is present.

There can be no question as to the efficacy of purgatives in removing dropsical effusion; and perhaps they are the best means we possess for the purpose, if we bear in mind the tendency to annoying diarrhœa as a complication of this disease. Little more need be added to what has been already said under the treatment of the acute disease.



Rayer has often seen a remarkable diminution, and often a total disappearance of the effusion under the use of Seidlitz or Pullna water. He also recommends cream of tartar, in half ounce doses. Dr. Christison thinks favourably of gamboge, in doses of five, seven, or rarely nine grains, given once every two days. To prevent it griping, he recommends its minute division to be secured by trituration with the bitartrate of potash.

When all other remedies have failed, puncture of the limbs may be resorted to; but this operation, performed in the mildest way (acupuncture), is at all times dangerous, where the vital powers are enfeebled. Dr. Christison relates two cases where it proved fatal.

**B. Head Affection.**—This very dangerous complication requires energetic treatment; blood-letting occasionally, and very active purging invariably are demanded. Rayer recommends purgative doses of calomel, cupping over the mastoid processes, and blisters to the nape of the neck.

“Diuretics, too,” says Dr. Christison, “have always appeared to me of undoubted service.”

**C. Affections of the Chest.**—The inflammatory affections arising in the course of this disease require the same energetic treatment as when they occur idiopathically. Pleuritis, especially, is so exceedingly apt to end in sudden and copious effusion, that its earliest commencement must be watched and met with promptitude and vigour.

**D. Affections of the Digestive Organs.**—These are often the most annoying and tedious complications which we are called on to combat, and more may generally be effected by attention to diet and regimen than by medicine.

Acidity and flatulence are usually complained of, and are to be treated by a combination of alkalies and bitters. Dr. Sims' remedy for the heartburn of pregnancy is often successful. R.—Magnes. ust. ʒj; aq. ammon. pur. ʒj; aq. cinnamomi ʒiii; aq. purā ʒvss. M. Of this two or three teaspoonfuls are to be taken when these symptoms are troublesome.

Where these symptoms are associated with constipation, the carbonate and sulphate of magnesia may be given in bitter infusion, with hydrocyanic acid. R.—Magnesiæ sulph. ʒss; magnesiæ carbon. pond. ʒii; acidi prussici med. gtt. x; infusi gentian comp. ʒxi; tr. hyoscyami ʒi. M. Of this two tablespoonfuls may be taken three times a-day.

Or lime water and infusion of gentian may be prescribed in equal quantities, and the bowels solicited by gentle doses of the compound rhubarb pill.

When the violence of the symptoms are subdued, an attempt should be made to restore the tone of the stomach by the preparations of iron, or zinc, with or without the vegetable tonics.

Where chronic vomiting is present, remedies of the same kind, with or without opiates, will sometimes prove successful. R.—Sodæ carbonatis ʒii; tr. valerian. ammon.; tr. cardamom. comp. āā ʒss; acidi prussici med. gtt. viii; syrapi zingiberis ʒss; aq. lauri cinnamomi ʒviss. M. Of this an ounce may be given when the sickness is felt.

Creasote is strongly recommended by Dr. Christison, who administers it twice, thrice, or oftener, in the course of 24 hours, dissolved in an ounce of some aromatic water. The taste is, however, so repugnant to some, that I have preferred prescribing it in the form of a pill; one drop of creasote, two grains of rhubarb, and one grain of extract of gentian, readily form a convenient pill.

Opium is also sometimes of service, especially the sedative solution. One case which had resisted all other treatment, yielded to the use of a pill containing half a grain of nitrate of silver, and the same quantity of extract of opium.

The operation of all these remedies will be much aided by the application of sinapisms, turpentine epithems, or a cantharides blister externally; in the latter case the blistered surface may be sprinkled with the muriate of morphia.

Diarrhœa may be checked by chalk, astringents, and opiates; in severe cases the acetate of lead, with opium, may be given; and if all these fail, strychnine, in combination with opium, will sometimes succeed.

The other complications are to be treated precisely as we would do in cases where they occurred unassociated with kidney disease.

20. *On the Minute Structure and Chemical Composition of Tubercular Deposits.*—Dr. J. HUGHES BENNETT, in an interesting paper on this subject in the *Northern Journal of Medicine*, for April and May, 1846, observes, that with a view of establishing a correct pathology, our first efforts must be directed to determine what tubercle really is; how it can be accurately separated from the ordinary products of inflammation on the one hand, and from malignant or other morbid growths on the other. To arrive at these points we must inquire into the minute structure and chemical constitution of this substance.

*Minute structure of tubercle.*—If from the tubercular lung of an individual who has died of phthisis pulmonalis, we choose a small mass of tolerably firm consistence, make a thin section, and examine it with a power of 250 diameters linear, we observe the net-work of the lung crowded with a dense granular-looking matter. This is composed of corpuscles and granules. The corpuscles are of an irregular form, more or less angular, varying in their longest diameter from  $\frac{1}{50}$  to  $\frac{1}{100}$  of a millimetre, composed of a distinct cell wall, containing three or more granules; without any distinct nucleus. There are also numerous granules and molecules, varying in size from a point scarcely measurable, to the  $\frac{1}{50}$  of a millimetre in diameter. Weak acetic acid renders the corpuscles more transparent, without producing any other change, and dissolves many of the granules. Ether and alcohol produce little change; ammonia partially dissolves the corpuscles. They are immediately and completely dissolved in a solution of potash.

Softened tubercle presents a similar structure, and is sometimes wholly composed of granules or molecules. Sometimes the corpuscles of tubercle are larger and rounder than those previously described, approaching in form to those observed in serofulous pus. The gray semi-transparent tubercle is composed of similar elements, although more transparent, and not so much defined. In cretaceous tubercle there are masses of earthy salts of irregular form and size, frequently mingled with crystals of cholesterine.

“If then,” says Dr. B., “we are asked to determine what is positively tubercle as distinguished from all other morbid products, we must answer, that deposition which is composed of the peculiar corpuscles and granules we have described and figured. From pus corpuscles they are readily distinguished by the action of acetic acid, which in them causes no granular nucleus to appear. From plastic corpuscles they may be separated by their irregular form, smaller size, and the absence of primitive filaments. With the exudation or granular corpuscle they can scarcely ever be confounded, on account of its large size, brownish or blackish colour, and nucleated or granular structure. The cells of cancer are large, transparent, and distinctly nucleated, and consequently easily distinguished from the small, non-nucleated corpuscles of tubercle.”

Tubercle is frequently conjoined with more or less pigmentary matter, which is greater in amount as the tubercle is older. It occurs in the form of irregular black masses, which consist of numerous minute molecules aggregated together.

After considerable experience in the examination of sputum, he thinks himself warranted in saying, that a knowledge of its minute structural composition is of little use in a clinical point of view. The diagnosis of phthisis, in its various stages, is capable of being so accurately determined by auscultation, that the microscope is in this respect of secondary importance.

Dr. Bennett has never seen the corpuscles of tubercle nucleated; he considers them to be undeveloped cells, which are produced slowly, and have no tendency to form perfect organizations, before they break down into a molecular matter. Hence no danger is to be apprehended from the spread of tubercle itself, and if fresh deposits could be prevented, the tendency of this substance to disintegration is highly favourable to its absorption.

*The chemical composition of tubercle.*—Dr. Bennett quotes at length the observations and analyses of Abercrombie, Hecht, Lassaigue, Preuss, Gueterboeck, Simon, Scherer, Felix Boudet, and Wright. He also inserts a note from Dr. Glover, of Newcastle, giving the results of various analyses of tubercle by him, in its different forms and stages. He concludes the paper with the following summary and observations:—

“In reviewing the different analyses of tubercle which have now been given, we find,—

"1. That tubercle consists of an animal matter, mixed with certain earthy salts.

"2. That the relative proportion of these varies in different specimens of tubercle. That animal matter is most abundant in recent and earthy salts in chronic tubercle.

"3. That the animal matter certainly contains a large amount of albumen. Some chemists have also detected caseine, the existence of which is probable; others gelatine, the presence of which is more doubtful. The statement of Gueterboeck, that it contains a peculiar animal matter, (phymatine,) has not been confirmed by other analysts. Fibrine and fat exist in small, but variable proportion; as a constituent of tubercle.

"4. The earthy salts are principally composed of the insoluble phosphate and carbonate of lime, with a small proportion of the soluble salts of soda. The statement of Boudet, that cretaceous concretions are principally formed of the latter, is directly opposed by other chemists, and is quite incompatible with their long persistence in the body.

"5. That very little difference in ultimate composition has yet been detected between recent tubercle and other so-called compounds of proteine.

"The two problems which the pathologist wishes the chemists to resolve are, 1. What difference exists between tubercle, lymph, and cancer? 2. Does the blood undergo any change which bears a relation to the production of these deposits? These questions are not yet answered; but there is every reason to hope, now that attention is directed to these subjects, something positive will soon be ascertained. No doubt, there are great difficulties to be surmounted. Organic chemistry is yet in its infancy. Only lately, Reichenbach discovered sulphur in considerable quantity, as a constituent of animal bodies. Even now, the existence of proteine, which promised to facilitate our knowledge of organic compounds, has become a subject of dispute. At the last meeting of the Royal Society of this city, (April 6, 1846,) Dr. G. Wilson showed that the fluoride of calcium, which had hitherto been considered insoluble in water, was, on the contrary, soluble to a considerable extent. He detected it in recent and fossil plants and animals, in the blood, and in milk. If, then, the very basis of organic analysis is thus uncertain; if we are as yet ignorant of the chemical constitution of the animal solids and fluids in a state of health, we need not feel surprise at the little assistance pathology has hitherto received from the chemist. We anticipate, however, from the labours of those who follow the track of Simon, Scherer, and Lehmann, important results at no distant date.

"To arrive at positive conclusions, it becomes a matter of the utmost importance that the different morbid products should not be confounded with each other. This has been done even by Scherer. Thus, in his analysis of tubercle from the liver, the appearances under the microscope prove the disease to have been cancer, and not tubercle. Hence, it is incumbent on the chemist to give, with each analysis, an account of the minute structure of the matter operated on. Unless this be done, we must anticipate confusion rather than harmony, from the multiplication of analyses of morbid products."—*Monthly Journ. Med. Sci.* for June.

21. *Hemorrhage from the Ear following Suppressed Menstruation.*—M. ALIBERT relates, in the *Journal de Méd. et de Chirurg. de Toulouse*, a very interesting case, in which there occurred a periodic hemorrhage from the ear after the suppression of the menses.

The subject of this case was a woman 45 years of age, whose menses had stopped from the application of cold to the feet and agitation, nine years previously. The menses have not appeared since that time, but she became deaf, and every month there flowed from the right ear an ounce and a half of blood. The discharge continued twenty-four or forty-eight hours, and ceased spontaneously. It was announced by certain premonitory symptoms, which custom had taught the woman indicated an approaching flow of blood, consisting of an inconvenient sense of weight in the head, noises, and a sensation as if numerous ants were buzzing in the ear affected. The patient herself observed the regular periodic nature of this discharge, and understood that it was supplied by nature to compensate for the absence of the menses.

At present, this loss of blood no longer retains its menstrual type. It comes

back at indeterminate intervals, and differs more or less from the quantity formerly discharged. This irregularity is accompanied by violent cephalalgia, fugitive vertigo, dimness of sight, and, in short, a congestive state of the brain. The general health is otherwise perfect.—*Lond. Month. Journ.* for June.

22. *Excessive Secretion of the Ammonio-Magnesian Phosphate by the Kidneys with long-continued Vomiting.*—Dr. GOLDING BIRD read before the Royal Med. and Chirurg. Society, March 24th last, a case of excessive secretion of the ammonio-magnesian phosphate by the kidneys in confirmation of the value of irritability of stomach as a diagnostic sign of calculous affection of the kidney. A lad, æt. 18, was admitted into Guy's Hospital, under Dr. Golding Bird's care, with all the symptoms of scirrhus pylorus, with the exception of non-existence of epigastric tumour. He had for four years been subject to constant irritability of stomach, and had never for 24 hours consecutively ceased from vomiting, never having retained a meal during the period alluded to, for more than an hour, often only for a few minutes. He was emaciated to a skeleton, and appeared actually sinking from exhaustion. There appeared (unless the vomiting were regarded as such) to be no demonstrable evidence of organic disease; the urine had a fetid fish-like odour, was copious, limpid, rarely whey-like, of moderate specific gravity, and deposited crystals of triple phosphates in abundance. No hippuric acid or other abnormal element. The urine was alkaline on secretion, and always contained a large excess of the crystals alluded to. Treatment directed specially to the gastric symptoms, having failed to relieve in able hands, Dr. G. Bird prescribed strychnine in doses of gr.  $\frac{1}{16}$ . The result was the gradual diminution of the gastric irritability, the restoration of the acidity of the urine, and the disappearance of the phosphatic crystal. After a few weeks' continuance of the medicine the lad became convalescent, and actually fat. The author entered then into the question of the etiology of the disease, and expressed an opinion of its connection with functional derangement of the spinal and ganglionic nerves. In conclusion, he pressed upon the notice of the meeting the value of strychnine as an anti-emetic remedy.

Mr. Lloyd was of opinion, with Dr. Bird, that the secretion of phosphatic urine, in cases of disease of the spinal marrow, was by no means uncommon; indeed, it was a sign for which he generally looked in those cases, but this state of urine was not, in his experience, at all associated with loss of flesh, or with stomach disease, as noticed by Dr. Bird. He had frequently met with these cases, and he mentioned one in particular which had existed for several years, the chief symptom being pain in the lower part of the spinal region; the disease was considered to be dependent on repeated attacks of lumbago. The urine, however, eventually became phosphatic, and the true nature of the case revealed. He was in the habit, in all such cases, of examining the urine by the aid of the microscope, and had always found phosphatic crystals present. He was not aware whether Dr. Bird, in his paper, had alluded to any other morbid products which were found in cases of this description; but he, Mr. Lloyd, had generally detected much mucus, or a great number of pus corpuscles, in phosphatic urine generally. He mentioned one case in which the urine was loaded with phosphates, and contained a great number of epithelial cells and other morbid products. He had never met with a similar example. In this case, also, the stomach did not sympathize with the morbid condition of the spine; the patient was still under treatment, but progressing towards a state of convalescence.

Dr. Golding Bird could not help thinking that two very distinct classes of cases had been confounded by Mr. Lloyd, both of which were characterized by the presence of phosphates in the urine, but the circumstances of each class of cases were widely different. One of these classes of cases, occurring frequently to the surgeon, consisted of those in which a diseased secretion was poured out from the mucous membrane of the bladder, as the result of chronic inflammation of that organ, stricture, enlargement of the prostate, or where, after mechanical injury to the spine, a more or less complete paraplegic condition had been induced. In these cases the alkaline and phosphatic state of the urine was produced from changes taking place in the fluid after it had reached the bladder. The urine in these cases was always ropy, and also frequently fetid. If, in these cases, how-

ever, the bladder were carefully washed out, and the urine secreted during the next few minutes were examined, it would be usually found to be acid. The second class of cases were altogether of a different nature from these, which, from their dependence on mechanical lesion, were strictly surgical. In the second class, from some antecedent cause, very likely having relation to the function of the spinal nerves, a low form of inflammation or irritation was set up in the kidneys, and the consequence was, a secretion of alkaline urine, which urine contained an excess of phosphates. Dr. Bird believed that the mechanical irritation of the tubular structure of the kidneys by the phosphatic crystals was the immediate exciting cause of the irritation of the stomach, or of the vomiting.

Dr. Taylor thought that the distinction to which Dr. Golding Bird had just referred was an important one—viz., that between urine which was secreted in an alkaline condition, and urine which had become alkaline only in consequence of chemical changes taking place in it after its secretion. This distinction had been first drawn, or, at least, had been especially insisted on, by Rayer, who considered that urine, which is secreted alkaline, is an important symptom of simple chronic nephritis. Dr. Taylor had had the opportunity of satisfying himself of the fact that the urine is in some cases alkaline when secreted, having made the experiment, referred to by Dr. Bird, of washing out the bladder with water, and collecting the urine in a very short time afterwards. In more than one of these cases he had also had the opportunity of ascertaining, after death, that the kidneys were inflamed, and there was no other appearance to account for the alkaline urine. Whether this class of cases, however, is attended with more disorder of the stomach than other cases in which the kidneys are diseased, he thought was another question. Nausea and vomiting are well known to be common symptoms of various morbid conditions of the kidneys. In the cases of alkaline urine, observed by Dr. Taylor, the sickness had not been more marked than in other renal affections. The same thing seems to be implied, if it be not directly stated, in the work of Rayer. The great reason why Rayer insists so much upon the importance of alkaline urine as a symptom of nephritis is this, that very often there is no other obvious sign of the disease. In such cases the inflammation runs a very insidious course. A man complains of some languor, and looks a little out of health, and he may or may not have some slight aching about the loins, and these may be the only symptoms of the disease except the alkaline urine.

Dr. Bird could not believe that the mere secretion of alkaline urine, when there was no irritation from a calculus, was sufficient to produce vomiting. He referred to Guy's Hospital Reports, which contained a paper by Dr. Barlow, as evidence in favour of this opinion.—*Lond. Med. Gazette.*—April, 1846.

23. *Neuroplasty or Ganglionic Alteration of the Peripheral Nerves.*—By M. SERRES. In 1843 I called the attention of the Academy to a general disease of the nervous system which had not been previously noticed. This disease I termed *neuroplasty*; its anatomical and fundamental character consisting in a ganglionic transformation of the ramifications of the nerves of life, of relation, and of organic life; no disease within my knowledge affects any organic system so universally, for I have counted two hundred preternatural ganglia on a single limb.

The two former cases which I observed occurred in subjects in the school of anatomy; they were precisely similar to each other, and a third, perfectly identical with them, was observed by MM. Maher and Payen at Brest. The similarity of the alteration in those three cases,—the form of the ganglionic enlargements,—their situation,—their structure,—their resemblance on comparison to the superior cervical ganglion,—the examination of their intimate structure with the microscope,—everything confirmed the idea that the nervous tissue was the seat of the disease, and that it had sustained, under the influence of disease, an enlargement similar to that which exists naturally on certain cords of the sympathetic nerve and of the nerves of relation.

The perfect integrity of the spinal marrow and of the brain in those three cases occasioned, however, some doubts as to the nature of those enlargements; for it was difficult to admit that such extensive disease of the peripheral nerves could exist without any trace of alteration in the cerebro-spinal axis.

These doubts were strengthened by our ignorance of the phenomena which had preceded and accompanied this alteration during life: the disease having been in the three cases only detected after death, and little more being known of the condition of the patients than the one fact common to all—viz., that they had all died of entero-mesenteric or typhoid fever; with regard to the case observed by MM. Maher and Payen, it is, however, further known that during the entire course of the malady there was no symptom indicating any disturbance of the nervous system. The contradiction between the morbid alterations and the absence of phenomena corresponding to those phenomena was striking, and to a certain extent justified the doubts expressed by some physiologists as to the nature of those pre-natural ganglia.

A most remarkable case is at present under my observation. The phenomena which preceded and accompany it indicate so decided a disturbance of the nervous system that I publish it at once—on the one hand, to dispel the uncertainty occasioned by the previous cases,—on the other, to elucidate the diagnosis of an affection so uncommon in its nature and so destructive in its effects. Though the general state of the patient in question has been somewhat ameliorated under treatment, yet I do not expect that he will recover, as every day enlargements, not perceptible the day previous, form on the trajet of the superficial nerves.

P. A. Legrand, aged 26, a smith, was admitted to hospital April 14, 1836. His parents are healthy; his constitution seems robust, though, he says, he has become much thinner latterly. Until within the last year he rather frequently exceeded in drink. In April, 1845, after working eighteen hours daily for several days, he, in the course of one night, without any previous symptom, lost the sight of the left eye completely. That eye remains blind, and there also exists complete paralysis of the left levator palpebræ muscle.

Last January he fell insensible in the street with all the symptoms of a fit of epilepsy; the fit commenced with slight convulsions and rather sharp pain in the right thumb; the other fingers then became convulsed and painful, which caused clenching of the hand; the pain extended along the arm to the larynx, where he experienced such a sense of suffocation that he could not articulate; vertigo, tinnitus aurium, and insensibility then supervened. He cannot state how long the fit lasted. The evening of the same day he experienced another, but much less intense fit, characterized by very severe pain in the arm and forearm, with flexion of the thumb in the palm of the hand, but without insensibility. The day following his health was as usual, save that the right arm remained feeble and somewhat benumbed for several days.

On the 25th February there was a similar attack, with vertigo and imperfect loss of sensibility; and during the night two similar but milder attacks occurred. As the arm now remained numb, and the fingers were affected with tingling, he was admitted to La Charité, which he left in nine days, those symptoms having then disappeared.

On the 4th April he was again obliged to give up work in consequence of pain and weakness in both arms. In the right arm the pain was fixed about the center of the biceps; in the left it occupied the center of the dorsum of the forearm.

On the 8th he was threatened with two fits of epilepsy, which, however, he arrested by crossing the arms, and preventing the flexion of the thumbs.

On the 14th he was admitted to La Pitié, where he was treated at first for rheumatic pains, but in a few days several small hard tumours, painful on pressure, were observed in the substance of the right biceps, and also on the external aspect of the left forearm. He now began to experience pains in the legs; so that, as he was previously unable to use his hands, he was compelled to remain in bed, being unable either to stand or to walk, and a few days subsequently tumours, similar to those occupying the upper extremities, were discovered on the lower.

At present (May 15th), those tumours are disseminated over the entire surface of the body; but they are more numerous on the extremities than on the trunk, and on the upper than on the lower. In the extremities they occur either immediately beneath the skin, under the aponeurosis, or in the substance of the muscles. The skin over them is not discoloured. Their disposition presents this peculiarity, that they do not extend below the lower third of the muscles, and none occur in

the course of the tendons or of the synovial sheaths. In one point only two of them seem adherent to the bone, though they are separable from it by continued pressure. No trace of those tumours can be discovered in the neck, on the hands, nor on the feet.

But few occur on the trunk, and here they are all subcutaneous, while in the limbs the majority are situated in the substance of the muscles, and they are more numerous the more the muscles are developed. Thus in the arm they chiefly occupy the biceps. They consequently are more numerous in the sense of flexion than in that of extension, which explains the position of the patient, to be presently noticed. But one of those tumours occupies the course of a nerve of any size. It is situated over the external popliteal nerve corresponding to the head of the tibia; none occur over the great nervo-vascular trunks of the groin, ham, or axilla.

The size of those tumors is very variable; the smallest nearly equal a pea in bulk, the largest are the size of an almond; some of them are spherical, but they are generally oval, seldom fusiform; their surface is smooth and polished; their consistence firm and resisting; they do not seem to adhere to the subjacent tissue. Those which are subcutaneous are very soft, and nothing can be more variable than the sensation they communicate when compressed. In effect those of older formation are completely indolent and painless on pressure, while others are extremely painful on the gentlest touch, and others again are not painful except on firm pressure. A peculiar character of the pain is, that it is entirely local, and does not radiate to the neighbouring parts. Muscular tension produces the same effect on those tumours as pressure with the finger does, and as they are more numerous in the sense of flexion than of extension, the limbs are permanently semiflexed. The tumours, scarcely perceptible to the eye when the muscles are relaxed, become prominent externally when the muscles are thrown into action.

Generally speaking, those tumours are developed almost symmetrically on both sides of the middle line, and in homologous muscles. The general health of the patient is tolerably good. The functions are natural. On his first admission to hospital there was some diarrhoea, which has entirely ceased for the last fortnight; pulse 75; respiration 18 in the minute. His intellect seems quite unaffected.

Such is the history of this singular affection, the diagnosis of which would be impossible if the anatomical examination of the three preceding cases had not revealed the probable nature of those tumours.—*Dublin Med. Press*, from *Comptes Rendus*.

24. *On the Treatment of Pneumonia*.—The *Lancet* (Aug. 22, 1846), contains an interesting paper on this subject read at the Harveian Society, by Dr. A. T. THOMSON. The indications to be fulfilled in the treatment of pneumonia, the author conceives to be—first, to subdue inflammation; secondly, to excite the capillaries so as to prevent extraneous depositions in the tissue of the diseased organ; thirdly, to diminish excitability, and consequently, the susceptibility of impression, and thus to guard against relapse.

With regard to the first indication, there can be no difference of opinion respecting the propriety of blood-letting in pneumonia. It was the custom of the older physicians to bleed, ad deliquium, in every stage of the disease. It was the practice of Sydenham: his words are, “venæsectionem sæpe repetitam videretur indicare.” Cullen recommended bleeding, in quantity sufficient to cause remission of pain and relief of the respiration, or when these did not appear, the bleeding was to be continued to syncope: he also recommended to repeat the venesection, even in the course of the *same* day, and to the same quantity as before, should the pain and dyspnoea recur. He adds, “sometimes the second bleeding may be larger than the first;”—“a quantity,” he says, “of from four to five pounds, in the course of two or three days, is generally as much as such patients will safely bear.” This practice is still followed by some physicians in the present day, both in this country and on the continent; and occasionally from thirty to forty ounces of blood have been abstracted, in the first instance; and the bleeding repeated, on the following day, to a less extent. Andral and Chomel urge the employment of large and repeated bleedings; and even Laennec, who

does not implicitly rely upon the use of the lancet, recommends ample venesection in the commencement of the disease. There can be no doubt, not only of the propriety but of the absolute necessity of the moderate abstraction of blood, in the commencement of the treatment of pneumonia; but I am convinced that the profuse and repeated bleedings usually resorted to are not only unnecessary but hurtful. In the present day, we are properly more guided by the physical signs obtained by means of the stethoscope, than by the cupped and buffed appearance of the blood; and the existence of the crepitant r le is also regarded as a safer guide for the repetition of the venesection than buffy blood. With respect to the influence of blood-letting, to whatever extent it may be carried, much depends on the early period at which it is performed: for, when hepatisation, or solidification from effusion has taken place, it is seldom productive of benefit: on the contrary, the heart becomes more irritable, and a reaction follows, which quickens the circulation, increases the dyspnoea, and hurries the disorganizing process. Some continental physicians, however, and some practitioners in this country, recommend venesection to be repeated, even under these circumstances. My own experience is decidedly against this practice; and unless pneumonia be complicated with pleurisy, I have never seen any advantage derived either from topical bleeding or from blisters, when consolidation had really commenced.

But admitting all the advantages that can be anticipated from blood-letting, and regarding as visionary the idea that it may cause the fever to assume a typhoid type, except when such an epidemic tendency prevails, I may venture to affirm, that as large abstractions of blood cannot fail to give a severe shock to the constitution, as well as to protract the convalescence, and weaken the future energy of the habit, whatever can lessen the necessity for such large bleedings, and, at the same time, cure the disease, must be regarded as of great importance in the treatment of pneumonia. The chief object of my communication is to suggest that this substitute for the repeated use of the lancet will be found in the judicious administration of full doses of the potassio-tartrate of antimony.

This preparation of antimony has long been regarded as a powerful antifebrile; but its contra-stimulant influence, or its power of subduing acute inflammation, independent of its nauseating or diaphoretic property, was first established by Rasori, an Italian physician, in 1806; and the experience of the profession, both at home and abroad, since that time, has justified the encomiums which he bestowed upon it. The doses, however, which he ventured to prescribe, and which were also given by those who followed his practice in this country, were occasionally productive of harm, and therefore the remedy was either seldom prescribed, or the prescriber returned to small, nauseating doses, totally inadequate to resolve the inflammation. The opportunities which I have had of witnessing its influence have convinced me that doses of a grain, or a grain and a half, administered every third or fourth hour, after a moderate venesection, are sufficient to insure every advantage that can be anticipated from the employment of tartar emetic in pneumonia.

My practice, as soon as I have fully satisfied myself of the existence of the disease, and if the attack has not run on to the second stage, is to order one bleeding to the amount of sixteen or twenty ounces; to follow this, immediately, with three or four grains of calomel and one grain of opium, with the view of preventing that nervous irritability which often succeeds the use of the lancet, and of sustaining the beneficial impression made on the system by the blood-letting. In two hours afterwards, I give one grain of potassio-tartrate of antimony in a fluid ounce and a half of emulsion of bitter almonds, and repeat this dose every third or fourth hour, until a decided diminution of inflammatory action takes place—that is, until the crepitation has nearly disappeared, and the sputa are no longer rusty and tenacious. The intervals between the doses of the tartar emetic are then extended to six hours, and afterwards to eight hours, and so continued until convalescence is confirmed. I prefer the bitter almond emulsion, on account of its containing hydrocyanic acid, which has a sedative quality, and a more decided influence in quieting the nervous system, and abating the cough, than small doses of opium. When the pneumonia is uncomplicated, this plan, with the occasional aid of some mild aperient, has, in my hands, seldom failed to



carry the case to a successful termination. When the attack has passed beyond the first stage, when dullness on percussion indicates hepatization, then the object of the second indication—namely, to excite the capillaries and prevent farther depositions—requires attention; and, in order to fulfil this indication, I order four or five grains of mercury with chalk, or one grain of calomel, to be given in each interval of the administration of the tartar emetic. In the statistical table, twenty-three out of the thirty-eight cases were treated with mercurials in addition to the tartar emetic, because, in hospital practice, the patients are seldom brought in until the disease has progressed, and the solid effusion, if I may employ such a term, has taken place. But in the remaining fifteen cases no mercurial was employed; the tartar emetic was alone trusted to, and it did not disappoint my anticipations; but in no instance was its employment commenced until after the abstraction of blood, either by the lancet or by cupping. I have never carried the mercurial action to ptyalism; but it is requisite to procure the usual symptoms of its decided impression on the habit, by producing slight tenderness of the gums. In ordinary cases, I have seldom had occasion to order blisters; but, from the nature of hospital practice, they are demanded, and they were employed in sixteen of the cases in the statistical table. Such are the advantages of moderate doses of tartar emetic in the treatment of pneumonia. In the same manner as when larger doses are administered, vomiting almost always occurs after the first and the second dose of the remedy; but on the third dose the tolerance is established, and even when the dose is increased, neither nausea nor vomiting afterwards takes place.

If I may be permitted to state a hypothetical opinion, I should venture to offer my view of what has been termed the tolerance of the habit against the well-known influence of tartar emetic, when administered in moderately large doses. It has been supposed by Rasori and others, that the inflammatory state of the system in pneumonia is the cause of this tolerance, and that this would cease if the remedy were continued after every trace of general inflammation had disappeared. I am, however, of opinion, that the tolerance does not depend on the state of the habit, but solely on the topical influence of the tartar emetic on the mucous membrane; and upon its influence, also, on that membrane operating as a counter-irritant; and to this its beneficial effect in subduing inflammation is to be attributed. When a grain or more of tartar-emetic is swallowed, a portion is carried into the circulation, and, acting on the nervous centres, it causes vomiting; but a portion also acts topically on the mucous membrane, and sets up sub-acute inflammatory action there. The second dose also causes vomiting, because the inflammation, although extended, yet is not sufficient to prevent absorption; but when the third dose is given, the absorption no longer takes place, and consequently no vomiting is induced. It is a well-established fact, that an inflamed surface is incompatible with absorption; and to this, in a great measure, are we to attribute the accumulation of serum in serous cavities in dropsy. If my views be admitted, I would farther state, that the determination of blood to so extended a surface as that of the mucous membrane is, in my opinion, sufficient to relieve the congestion in the lungs; and that it is, in truth, the cause of the benefit which follows the employment of the tartar emetic. Such is my opinion of the rationale of the action of tartar emetic in pneumonia; but on this part of my subject it is not my intention to insist, my chief object being to demonstrate, that this powerful therapeutical agent can perform all that can be expected from its administration in much smaller doses than Rasori and his followers have employed; that such doses incur no risk, seldom bringing on diarrhoea, whilst hypercatharsis has followed the administration of large doses; and that in such doses it sets aside the necessity of large and repeated abstractions of blood, and consequently cures the disease without that injurious impression on the habit which has been frequently seen to follow copious blood-letting. Another advantage obtained by this mode of practice in pneumonia is the gradual advancement of the convalescence, and the restoration of the patient to health and vigour without the aid of tonics. By gradually letting down the action of the tartar emetic, the stomach recovers its tone, and the assimilating function rapidly restores any waste which the necessary treatment of the disease may have occasioned.

Dr. T. appends a statistical table of 38 cases treated by the method he recom-

mends, but his views of the advantages of that mode of treatment are founded upon 62 cases, a tolerably ample experience.

25. *Acidity*.—Dr. ROBERT DICK, in an article in the *Lancet*, (Aug. 22, 1846,) remarks that there is a difference between acidity which is merely in the stomach, and acidity which is *from* the stomach; and the treatment of these is, also different. Acidity of the former sort arises usually from natural changes in the food, or from improper articles of diet. If from the former of these, we may infer that there is functional debility of stomach, probably associated with, or dependent on, anæmia of the mucous coat, and a consequent deficiency in the secretion of the gastric juice, bile, pancreatic fluid, and the mucus; that hence digestion is feeble and tardy, so much so that the ordinary chemical, non-vital changes in the food, which the mere heat and moisture of the stomach are apt to favour, take place. The cure of this kind, and from the causes just named, is obviously dependent on tonics, with food stimulant in itself or rendered so by condiments.

Acidity *from* the stomach—that is, consisting of morbid matters actually secreted by the organ itself, requires other measures. And here it may be observed, that sometimes the feeling of acidity, or, to use the familiar word, heartburn, is caused by secretions extremely slightly acid, sometimes not appreciably so. Acrid, however, they must be. In other cases, the feeling of acidity does not seem to depend either on acidity or acridity of the stomachic contents or secretions, but either on a peculiar state of the gastric nerves, or on morbid sensibility, with or without hyperæmia of the gastric mucous membrane. These several kinds of heartburn require separate and different treatment.

1. Acidity caused by morbid secretions. These morbid secretions are, in fact, one or more of the following acids:—the hydrochloric, lactic, oxalic, butyric, acetic, carbonic. The last is, perhaps, seldom the cause of heartburn.

In most cases, heartburn of this nature requires alteratives. This is more especially the case if the acidity be owing to excess of the hydrochloric or lactic acids, or to the presence of the butyric. The alteratives from which we may principally select, are, mercury, iodine, potassio-tartrate of antimony, nitrate of silver, bismuth, ipecacuanha, &c.; several of these may be combined with advantage. That the iodide of mercury may be chosen; and ipecacuanha may be usefully allied to mercury, iodine, or nitrate of silver, severally; all of them may also be united, if necessary, with purgatives, such as the extracts of rhubarb, aloes, or colocynth, with scammony, &c., and sometimes, most beneficially, with the extract of colchicum.

Meanwhile much attention must be given to the state of the urine, and the action of the skin. The condition of the urine is a matter of much importance. It must be our care to detect and re-adjust any departure from the normal equilibrium of that important excretion, any excess or deficiency of its constituents. In general, it may be remarked that morbid stomachic secretions are more often associated with and dependent on a deficiency of some of the constituents of the urine, than with excess of these.

2. When heartburn is attended with a red tongue, thirst, pain at the epigastrium, and a distinct sensation of coolness in the stomach, on cold fluids being swallowed—circumstances indicating hyperæmia and erythema of the mucous membrane—solutions of the acetate of ammonia, nitrate of potass, borax, and even dilutions of the acetic, tartaric, and sulphuric acids, are to be employed. Seltzer water, iced drinks, lemonade, grapes, ripe apples and pears, &c., may also be used as ordinary drink and food.

3. That species of heartburn which is a mere variety of gastrodynia, and is accompanied with no derangement of secretion, no hyperæmia or tumidity of the mucous membrane, is best treated with pills of the tris-nitrate of bismuth and hyoscyamus, nitrate of silver and conium, oxide of zinc and extract of chamomile. To these may be conjoined draughts of the infusions of orange peel, of taraxacum, and of gentian.

26. *Spontaneous Rupture of the Stomach*.—Two cases of this accident have lately been recorded, which are remarkable in not having been preceded by ulceration or violence, but were, strictly speaking, spontaneous. The first, which is by Dr

Morici,\* is as follows:—A man, æt. 30, was seized with intermittent fever, which was cured by quinine in about ten days. He was subsequently seized with symptoms of renal calculus, which had nearly subsided, when he suddenly fell down and expired. After death the thoracic viscera were found healthy, as were also the abdominal organs, with the exception of the stomach, which exhibited a rupture upon its anterior surface of three fingers' length, through which its contents had been extravasated. The parietes of the organ had not undergone any pathological change.

A similar case occurred in the Marylebone infirmary. In this instance the pylorus was blocked up by a piece of potato.†

A singular instance, in which spontaneous rupture of the stomach was followed by general emphysema, appears in the 'Annales de la Société de Médecine de Gand.' Its details are given below.

"A surgeon of Gand was seized with a sudden indisposition after a meal; his skin was cold and corrugated, and covered with perspiration; pulse hard and scarcely perceptible; voice lost; respiration anxious; abdomen tense and tympanitic. An emollient enema was administered to him; suddenly, while making an effort to go to the water-closet, an emphysematous swelling appeared extending up the neck, putting the patient into the most imminent danger of suffocation; the action of the lungs was suspended, the swelling increasing at each inspiration; the patient in the meantime performing the action of deglutition as it were in spite of himself. Presently his appearance was so altered that hardly anything of the human form was discernible; he retained his consciousness, however, and took part in the consultation held by three of his professional brethren about him. He experienced a burning thirst, and was calling every instant for cold water, but it was remarked that the difficulty of swallowing became momentarily greater, while the motions of the act were rapidly performed whether he tried to drink or not. In some minutes he expired in making an effort to get down a small quantity of water. The body was soon inflated like a bladder distended with air. *Dissection.* The enormous swelling of the front of the abdomen did not subside even on cutting into the cavity of the peritoneum, in which there was no blood. A second transverse incision exposed the stomach, which immediately protruded in consequence of its great distension with air, and was so large as to reach from the epigastrium to the iliac fossa; its muscular coat seemed hypertrophied. On drawing forward this viscus and tearing through the layers of omentum which connected it with the parts above, the air it contained rushed out by a large opening which extended from the cardiac to the pyloric orifice, along the lesser curvature. In this region the organ was found softened into a pulraceous mass that involved both the muscular and mucous coats; the latter tunic was injected strongly with dark-coloured blood, which appearance was insensibly lost near the rent. There was no trace of ulceration—no swelling in the edges of the ruptured part—no adhesions surrounding it; the peritoneum was natural. The lungs had subsided round the spine; the cavities of the heart were filled with black blood; there was a collection of serum in the right pleura and in the pericardium."—*Ranking's Abstract*, vol. iii.

27. *Pathology and Treatment of Aponia.* By JOHN BISHOP.—It is well known that many persons, especially females of a delicate and relaxed habit, and otherwise of a nervous temperament, become affected with aponia on being exposed to the cold or humid states of atmosphere. Some labour under loss of voice during the whole winter; whilst in others of a stronger constitution, the malady does not often continue longer than six or eight weeks. In order to ascertain the cause of the extinction of voice under the above-mentioned circumstances, it will be necessary to consider the relation of the functions of the vocal chords to the tissues of which the vocal tube is composed. It is not my present purpose to determine whether the vocal apparatus constitutes a stringed, a reeded, or a wind instrument, or whether it is a combination of all these different kinds of instruments, because this would lead to investigations involving the whole theory of the physiology of the voice. Let us, however, suppose it demonstrable that the action of

\* *Annali Universali.*

† *Lancet*, Jan. 3, 1846.

the glottis in the production of sound resembles that of a reed, connected with a membranous tube; and let us take it for granted, as an established fact, that the membranes surrounding the glottis are susceptible of various degrees of tension and elasticity, enabling them to exercise a certain influence in modifying and controlling the sounds which are generated by the vibratory movements of the vocal ligaments. Let us next examine the effects which would be produced on the glottis, supposing the membranous walls of the larynx and fauces to become relaxed, or inelastic, or to lose their tension. When any stretched membranous surface, such as the head of a drum, (which is well known to be composed of animal skin,) is subjected to a very moist state of the air, it becomes relaxed and inelastic; such also appears to be the effect of moisture on the mucous membranes, and several other animal tissues. Now it is found that if we take a reed, and place it in a membranous tube, and cause it to vibrate by passing a current of air over its tongue, the reed will continue to sound as long as the membranes preserve their tension and elasticity; but the moment they are moistened and become relaxed, it ceases to sound, and will not, by any means that we can employ, again produce a sound whilst it is in contact with the moist membranes; but as soon as they become dry, and resume their elasticity, the reed will again yield its sounds as before. These, then, are the acoustic effects of variations of tension and elasticity in membranes, when placed in juxtaposition with a reed, in the manner above described; therefore, as the vocal ligaments are placed under circumstances analogous to those of the reed in the case just mentioned, we can easily perceive why they should lose the power of producing sounds, when the membranes of the vocal tube either lose their elasticity, or cannot acquire from the connected muscles the tension proper to them in their normal condition.

There are several causes, independently of the hygrometric state of the atmosphere, which tend to relax the fauces and soft parts adjacent to the larynx, such as temperature, nervous excitement, and over-exertion of the vocal organs. It is well known how rapidly the transition from one state to another takes place in the mucous membranes. No sooner is the surface of these membranes exposed to sudden changes of temperature and moisture, than they begin to secrete the fluid peculiar to them. This secretion tends to clog the vibrating surfaces in contact with the larynx, and often impedes the action of the thyro-arytenoid ligaments. The voice sinks in pitch if the relaxation is only partial, as does a reed by partially moistening the membranous tube in which it is placed. When the mucous membranes which line the vocal tube have received such a shock as to induce inflammatory action, the train of symptoms is altogether different, and cough and irritation supervene. In this case the voice is not extinguished, and the complaint requires a very different kind of treatment. It is not, however, proposed to include this condition, which is of an inflammatory character, in our present inquiry. The degree of mischief produced on the membrane often decreases in proportion as the distance from the respiratory apertures increases; and thus we often find that the nostrils are irritated and relaxed, without the disorder proceeding further. The fauces are next affected; and it is the derangement of these parts, in consequence of their having less mechanical support, that chiefly tends to extinguish the sounds of the glottis. The trachea is the part least liable to be affected by this kind of relaxation, owing to its component tissues being chiefly of a cartilaginous structure, and to its greater distance from the origin of the mischief.

If we concur in these general principles, we may now advance a step further, and proceed to consider the methods best calculated to restore the vocal tube, when thus disordered, to its normal functions. The peculiar state of the mucous membrane and adjacent parts, now under consideration, appears to be of an asthenic character; on which account depletion, counter-irritation, and other modes of antiphlogistic treatment, do no good, but generally do harm; and stimulants and astringents, applied locally, are the only remedies which tend to restore the mucous membranes to a healthy condition. A strong solution of lunar caustic, applied to the fauces with a camel's-hair brush, produces the most beneficial effects, and may be used in preference to any other remedy. Gargles of capsicum also do good, but are certainly not so efficacious as the solution of nitrate of silver.

CASE I.—Mrs. H., a professional singer, suffered occasionally from loss of voice, which often lasted from six to eight weeks. On one occasion she was engaged to sing at a concert in the course of a few days, when one of these attacks came on, and she was greatly alarmed at the prospect of not being able to fulfil her engagement. Under these circumstances I applied to the fauces a solution of nitrate of silver, of the strength of ten grains to one ounce of distilled water, and repeated the application on the following morning. Under this treatment the power of voice returned, and she was able to sing at the concert on the third day.

CASE II.—Mrs. M., a lady who had suffered every winter a loss of voice, was placed under the same treatment, with similar results, and is now so sensible of its advantage, that she has recourse to it whenever she happens to suffer from cold, and has not lost her voice for several years past.

CASE III.—Mr. R., a gentleman possessing an extremely fine bass voice, consulted me under the following symptoms. He found that he had begun to lose control over the functions of the larynx: he could not sustain any note of equal intensity during the usual length of time, and the vocal ligaments appeared to be uncertain in their action. This was a case of relaxation combined with nervous irritation. Immediately after the application of the caustic solution to the fauces, I observed that the pitch of his voice in speaking was slightly elevated. This plan was continued, in conjunction with the use of tonics, and he speedily became relieved.

When the relaxed condition of the vocal tube arises from over-exertion in speaking, singing, &c., the same treatment is highly beneficial, and complete rest of the vocal apparatus is indispensable for the recovery of a healthy control over the voice. Many persons lose a portion of their range of voice by exercising it too much when in this state. When Grassini, the celebrated singer, came from Italy to England, the humidity and variable temperature of the air in this country so relaxed the membranous portions of her respiratory tube, that her voice sunk nearly an octave in pitch, and was changed from a soprano to a contralto character. The effect, however, was that she acquired great celebrity, in consequence of the fine quality and range of her lower notes; but on returning to the milder climate of Italy, the soprano range of voice returned, and she lost at the same time the power of uttering that range of notes which had conferred on her voice its attractive character. In this case the relaxed condition of the vocal membranes was productive of beneficial effects; but it is presumed such cases are extremely rare.

When aphonia arises from nervous excitement, we must look for the origin of the affection beyond the mere office of the soft parts of the vocal tube. This condition may often be traced to the nervous centre, most commonly arises from an asthenic state, and is often associated with an hysterical disposition. In some hysterical cases, however, an opposite state to that of aphonia presents itself. A patient was placed under my care, who uttered a constant involuntary bark, like that of a dog, very loud and discordant. It need scarcely be mentioned that no beneficial result can be expected in such a case from mere local treatment. Sudden alarm and over excitement will often cause aphonia. When this is produced by an altered state of the nervous functions, galvanism is said to be beneficial. A case of this description is recorded in the *Encyclographi Médicale*. A young man, having killed a comrade in a duel, became epileptic, and lost his voice from the excitement. All other treatment having failed, his medical attendants had recourse to electricity. A galvanic battery of 30 pairs of plates was employed; the zinc pole was applied to the first cervical vertebra, and the copper pole to the side of the larynx; 200 shocks were given the first day, and 300 on the second, when the parts became somewhat sensible. Two days' respite was then allowed, and at the third sitting the patient received 300 shocks from 70 pairs of plates. On the fourth day, 400 shocks from 70 pairs were administered; and after the latter application the patient began to utter a feeble cry. Six days were then allowed to elapse, after which 300 shocks were given with a pile of 30 plates. The treatment was continued a few days longer, and at the last four sittings 400 shocks were administered through the tongue; the zinc pole being applied to the first cervical vertebra, and the copper pole under the tongue. By means of this treatment the voice gradually returned to its natural state, and at the

end of twelve operations he was convalescent. In another case aphonia resulted from sudden surprise and over-exertion. The patient was a tradesman residing in St. Martin's Lane, who was standing in his shop, when an impudent thief entered, very coolly took up some article before his face, and bolted away. The tradesman ran to the door, crying "Stop thief!" as loud as he could vociferate, and was about to repeat the same exclamation, when he suddenly found himself unable to do so. The larynx was rendered mute, and he remained incapable of uttering a vocal sound for the space of eight months, when his voice returned unimpaired.

The more aggravated forms of aphonia, such as those arising from diseases of the larynx, or morbid conditions of the brain, most commonly baffle every method of treatment hitherto devised; but the investigation of these cases would lead to very lengthened details, besides being irrelevant to the class of cases selected for this communication; the scientific treatment of which often leads to very gratifying and satisfactory results.—*Transaction of London Med. Soc.*, vol. i.

28. *Diagnosis of Disease of Mitral Valve of the Heart.*—Dr. J. J. FURNIVAL relates in the *Lancet*, (March 21st, 1846,) a case of disease of the mitral valve of the heart, and offers some interesting remarks on the diagnosis of that affection. This he observes is founded on the physical and general signs. Of the *physical*, we know that the sounds usually considered distinctive of mitral-valve disease, may not, at times, or in certain cases, be heard at all; and yet disease may exist; though when they are audible, there is, perhaps, hardly any other cardiac lesion which is more easily or more certainly detected by a good practical auscultator than this; or which, on the other hand, would more easily pass undetected by those who rely solely on the general symptoms, and who do not use the stethoscope. Accordingly, no good auscultator is likely, in these days, to deny the possibility of pointing out the diseased valve when signs are in any degree clear. The objection, that as all the valves are so clustered together as to be covered by the open end of the stethoscope, the source of each murmur cannot be made out; however probable it seems, has been removed, by attending to the line of direction in which the murmur is propagated, while our difficulty is further lessened by the fact, that the left or arterial heart is the part mostly affected in valvular disease. To any one who has dissected the muscular fibres of the left ventricle; and has observed how they are connected with the columnæ carneæ, and how they all converge to a point, almost at the apex of the heart, the reason becomes at once apparent, how the murmur from a diseased mitral valve is heard loudest at or near the cardiac apex. The practical man is contented with the fact that it is so, and that fact has been verified by symptoms during life, and by examinations after death.

We must not, however, forget, that although we may have, in the vast majority of cases, no difficulty in diagnosis, yet an exceptional case may occur in which the murmur may be masked by other sounds; or even may not exist; or after having existed, may disappear altogether. As to the masking by other sounds, the auscultator will generally, with care, avoid error: as to the murmur not being present while disease exists in the valve, I have read of such cases, but never met with them; and as to the disappearance of the murmurs, this may arise from the diseased action increasing, and so implicating the valve that too small an opening is left to allow of the formation of a murmur. But here the previous history will have shown what the case has been, and is.

The distinctive physical sign of a patulous mitral valve is an abnormal murmur, generally of a blowing character, heard with the systolic or first sound of the heart, low down, below the line level with the nipple, and between the nipple and the left edge of the sternum. In most cases, this systolic murmur is also heard in the back, to the left of the spine, about the sixth or seventh dorsal vertebra, and near the angle of the left scapula; and this last site of murmur is very diagnostic. The murmur is also sometimes heard in the axilla; yet at the base of the heart the murmur is either very indistinct, or not to be heard at all, the two natural sounds being distinct and clear, as was the case at the head of these observations.

This murmur may mask, or take the place of the whole of the first sound, or of

only a portion of it. The systolic murmur, which may most readily be mistaken for this one proceeding from the mitral valve, is caused by some obstruction to the blood issuing from the left ventricle; and the disorder occasioning this last sound is called obstructive disease of the aortic valves. The distinction between the two is founded on the fact, that in the mitral-valve disease there is no propagation of the murmur up the aorta, and into the neck, which is observed in aortic disease; while, on the other hand, in this last disease there will be no abnormal murmur at the heart's apex.

But there are yet other sources of a systolic murmur; it may arise in a fit of hysteria. Here no mistake is probable; for as the murmur arose with the paroxysm, so it will usually cease with it, or very soon afterwards.

It is a not unfrequent accompaniment of chlorosis and anæmia. Here the history makes manifest the cause, and we shall hear, in the same case, various murmurs in many of the superficial arteries of the neck, and even limbs. In the neck, over the carotid artery, we shall not only have the bellows murmur, (and this alone is distinctive,) but also a humming sound, which has been described to be like that sound which reaches the ear from a marine shell. This sound is said to proceed from the jugular vein; it may be intercepted by pressing the finger firmly on the part of the vein above the stethoscope. If we press the instrument too forcibly on the vein, we shall not hear the sound at all.

We may have a systolic murmur from deformity of the chest so displacing and pushing about the heart, as to cause this first sound to become almost abnormal in character—quite a whiz, as a late writer expresses it. This whiz is produced by mechanical impediment, but the impediment is without the heart, and not within it. Here the deformity, which is visible to the eye, reveals the mode of formation of the murmur.

It has been said that the natural murmur of respiration may closely resemble this murmur, which is, as I have said, of a blowing character, not unlike that of the breathing; but this source of fallacy will never perplex the auscultator who causes his patient to hold his breath for a few seconds; and this precaution is invariably taken by every well-informed auscultator.

Finally, it has been lately said by Dr. Latham, and before him by Dr. Zehetmayer, that tuberculosis may cause a systolic murmur, and the corroboration of an interesting fact thus afforded by two practical men of professional eminence, living in different countries, must be calculated to fix the attention of our medical brethren on this hitherto unnoticed symptom.

Dr. Zehetmayer published in 1844, and Dr. Latham in 1845. The former says:—"The first sound in the pulmonary artery is at times accompanied by a murmur, if there is an insufficiency of the mitral valve, (Skoda,) and it must take its origin from a loosening or softening of the internal coat of the vessel. I have repeatedly heard in phthisical persons, in the second intercostal space, a decided bellows murmur, instead of the first sound of the pulmonary artery, and in a place where one could not doubt that tuberculous infiltration was present. No anomaly in the vessel could be detected by post-mortem examination."

Dr. Latham writes thus, at page 66 of his late excellent volume on Clinical Medicine: "Fancy a line drawn from the left side of the sternum along the upper edge of the second costal cartilage, and continued an inch along the second rib; and another line drawn from the sternum along the lower edge of the third costal cartilage, and continued an inch along the third rib. Between these two lines a space is included, in the whole or part of which a murmur is often audible, coincident with the systole of the heart, when no such murmur can be perceived either in the præcordial region, or in the course of the aorta, or in the carotids, or in any part of the arterial system; but here, and here only. It is a gentle bellows murmur, quite obvious to the ear, and unmistakeable in its character.

"Of such a murmur, often audible in this situation exclusively, I am certain as a matter of fact, and certain, too, of its very remarkable accompaniment: I have witnessed it either in those who were undeniably consumptive, or in those who were too justly suspected of being so. I cannot say in what proportion of the phthisical it occurs; but I am continually meeting with it." \* \* \*

Some German auscultators have attempted to lay down a diagnosis, founded on the sharpness or alteration of the second pulmonary artery sound. Dr. Skoda, of

Vienna, was the first, I believe; to tell us, that when we meet with a systolic murmur in the left ventricle; and also an augmentation of the second sound in the pulmonary artery; we may safely and certainly conclude that there is an insufficiency of the mitral valve; and he attempts to explain the asserted fact, by referring it to distension of the right ventricle and of the pulmonary artery, produced by the impeded circulation, first through the mitral valve, and then through the lungs; for the distended artery reacts with unusual force on the pulmonary semilunar valves. \* \* \* \* \*

I do not recollect reading any author amongst the British who has directed attention to the second sound in the pulmonary artery as a diagnostic sign, except Dr. Hope, and he makes the augmentation of it to depend on hypertrophy of the right ventricle. Drs. Skoda and Zehetmayer both seem to think that this augmented sound is distinctive of, and caused by, three states.—1st, of hypertrophy of the right ventricle; 2d, of distension of it; and 3d, of mitral-valve disease.

There is little doubt, that this increased sound generally accompanies advanced hypertrophy of the right ventricle, because there is an increased power of propulsion in the ventricle, from increased substance and action of its muscular parietes. As to the second cause, this case may be cited as subversive of the opinion; for distension was repeatedly observed and made manifest by the slow upheaving of the ventricle, and the general signs; yet I never heard this increased sound, although on the watch for it. To the third cause of its existence, this case, as well as several others within my personal experience, gave a decided contradiction.

We must not expect to hear this sound so loud as that of the second sound of the aorta; for, according to the seemingly accurate experiments of Dr. Valentin, published in Henle's "Zeitschrift," "the mass of the right ventricle stands to that of the left in the exact proportion of one to two, and it may be concluded that in equal intensity of contractions; the muscular power will exhibit an analogous proportion. The hydrostatic blood pressure of the pulmonary artery must amount to half the hydrostatic pressure of the aorta; and the whole resistance of the bodily circulation must be exactly double that of the pulmonary circulation."

When investigating this sound, the proper place to examine is to the left of the sternum, on a level with the upper part of the third costal cartilage; while the second sound in the aorta is to be heard on the same level, but more on the sternum—i. e., a little more to the right. It may well be supposed that an ear practised in auscultation is necessary, both to distinguish between these two second sounds, and to ascertain the plus or minus of intensity in the sounds themselves. And when I read or hear of gentlemen who deny our power to ascertain these sounds, or to say which valve may be diseased under all circumstances, I cannot but think such gentlemen do not possess an average acuteness in hearing, or that they lack either the necessary practical discernment, or requisite patience; for the practitioner who relies on one, or even more examinations, may miss hearing this sound, as well as other sounds, because we know they vary with circumstances of comparative fullness of the heart or its reverse, or with deficiency or surplus of propelling power.

The general signs of this lesion are very distinctive in the early stages. And first, of dyspnœa. One moment's reflection on the immediate effect of the retro-pulsion of the waves of blood through the patulous mitral valve into the left auricle, and then into the lungs, will teach us to expect dyspnœa as the very first as well as the most constant sign. And so it is. If we find dyspnœa to exist, and yet no signs of pulmonic disease, or of other manifest cause for it present, we may suspect disease of the heart, almost from this symptom alone. In some cases which I have published, of diseases of the heart and their treatment, this shortness of breath was the only symptom for a long time observable, and it appeared long before the characteristic murmurs of diseased mitral valve could be detected. This dyspnœa causes congestion of the pulmonary vessels, and that is for a time relieved by medicines, without expectoration, or, in some cases, by an effusion of mucus on the mucous membrane; and thus the dyspnœa may continue without expectoration, or there may be, also, cough with mucous sputa.

Another early and very distinctive sign is, the peculiarity of the pulse, which peculiarity is not met with in any other disease so early as in this. It is very quick; not intermittent, but irregular, or rather, unequal, in the volume of each



pulsation: first, a full artery is felt, then a seemingly half-filled one, and the coat of the artery cannot be felt distinctly; it is a soft pulse if no inflammation be present. A late reviewer found fault with me for overrating the importance of this pulse as a diagnostic, and affected to think there was some misprint; yet nevertheless, I wrote from very considerable experience; and the more I see of diseases of the heart, the more I am disposed to rely on the accuracy of what I have written thereupon. But it is best to leave the flippancy of reviewers to be dealt with by the good sense of the members of the medical profession, if they will but examine for themselves. I will repeat, that when we meet with such a pulse as the one described, either with or without other signs of heart disease, excepting dyspnœa, at an *early* period, we shall not do wrong, if we conclude that there is a patulous state of the mitral valve; for when it occurs in other diseases, it will do so only at the very latter stages.

In some persons, headache is early complained of; and headaches, both severe and long-continued, cause much suffering. These are doubtless caused by cerebral venous congestion, in consequence of the jugular and other veins being partly prevented from emptying themselves into the right auricle and ventricle. In some instances, this and other cerebral symptoms have been so prominent, as to mask and draw our attention from the original and originating disorder. Here the reflex function is sometimes called into action, and the brain becomes involved in that way. These symptoms are not so frequently met with in disease of the mitral valve as in pericarditis. In this latter case, is the phrenic nerve the medium to transmit the irritation, or is the cause a congestion of the vessels of the medulla spinalis induced by the blood being hindered flowing freely from it, in consequence of the highly impeded circulation in the large returning veins?

The liver also speedily feels the influence of this impeded state of the circulation. The venæ hepaticæ have mouths which are large and valveless; they therefore easily admit of reflux; and the vena cava abdominalis itself is over distended, from not being able to pour its contents into the cardiac cavities. Congestion of the liver follows. Hence the colourless motions so often observable in this disease, and various symptoms of gastric derangement, &c., which results have a pernicious reaction on the original disease, the gastric irritation adding to the cardiac irritation, and aggravating the paroxysms of dyspnœa as well as exciting them; while the retention of those matters which should be excreted from and by the agency of the liver, deprives us of one of our principal means of relieving the load of labour which is imposed upon the heart. This congestion of the liver may lead on to structural disease, to hardness, cirrhosis, or enlargement, which would soon be followed by ascites. The enlargement usually occurring in mitral-valve disease, takes place in the anterior parts of it, and as these parts make room for themselves, by pressing on the abdominal parietes, which easily give way, ascites does not most commonly occur. If the lobules Spigelii, or the parts adjacent to it, were enlarged, direct pressure would probably be exerted on the vena cava abdominalis, and ascites must follow.

Œdema of the feet and ankles is likewise an early symptom, for puffiness of the ankles soon occurs; and if the cardiac disease has taken place suddenly, or there has been a sudden increase of disease, the dropsical effusion involves the whole of the legs and thighs, and creeps up to and over the abdominal integuments with most alarming rapidity. In one of the cases which I have recorded, this increase of the dropsy was manifest at every morning and evening visit, till the skin seemed on the point of bursting, and becoming gangrenous from mere distension.

This effusion is not caused by lack of power in the absorbents, which continue to act with normal activity; and the gangrene, when it does occur, proceeds from distension, whereas the gangrene, which is sometimes met with in this country, but often in Germany, from disease of the aortic valves, partly arises from a want of the proper supply of healthy arterial blood, as well as from local congestion. One of our usual means of checking the progress of the effusion is here of little avail; I mean, laying the legs up on a level with the body, and so long as the mechanical difficulty remains in the heart unrelieved, this œdema goes on increasing, and ultimately augments the dyspnœa, &c.

The rationale of this early œdema is found in the fact that the blood in the vena

cava abdominalis cannot pass freely in its proper course; it presses on the nearest valves in one single column, the apex of which is the heart, for there are no valves in this large abdominal vein or sinus. These valves are situated at the top of each femoral vein, and are raised by the pressure, and then the femoral and crural veins become distended, and relieve themselves by an effusion of serum into those parts which offer the least resistance, and where there is most cellular tissue—viz., about the ankles, from which point the effusion spreads upwards.

Amongst the later and sometimes earlier sequelæ of this disease we have hæmoptisy, arising from the congestion; in some cases emphysema pulmonum occurs.

Effusion may take place into one or both pleuræ, and into the pericardium, whereby the dyspnœa and danger are both much increased. In the head, we may have apoplexy or paralysis from venous congestion, and effusion or extravasation may be found post-mortem; and as the venous blood is often not returned from the medulla spinalis, we may see various signs of spinal irritation, or of failure of the spinal functions, which signs may, through the reflex function, cause some mystification.

The kidneys also suffer; for as the renal veins enter the vena cava at an obtuse angle, they are subjected to the pressure of the delayed blood in that vein, and thus they cannot relieve the kidneys by emptying themselves; effusion into the renal pelvis takes place, the urine becomes charged with albumen, or with components of the blood itself; it becomes scanty, high-coloured, and loaded with deposit, and renal disease is added to the list of evils, soon exciting diseased action in the brain, if it have not occurred before, and causing, more or less speedily, a fatal issue. These are the chief results, and this is the progress, of insufficiency of the mitral valve.

29. *Case of Congenital Cyanosis of forty years' standing, depending upon Patulous Foramen Ovale and Obstruction in the Pulmonary Artery.* By ROBERT J. SPITTA, M. B., Lond., (Proceedings of *Med. and Chirurg. Soc.*, Jan. 13, 1846.)—The patient in this case, was a lady, aged forty, of diminutive stature, and pigeon-breasted, but without any positive deformity. She had cyanosis from birth, indicated by blueness of the cheeks, lip and tongue. The heart's action was said to have been regular when at rest, but sudden exertion produced palpitation and dyspnœa. She suffered very much from cold. In March, 1845, she was suddenly seized (while at rest) with dyspnœa and partial syncope, accompanied by convulsions, and afterwards, with intense pain, referred to the epigastrium, loins, and hypochondria. This condition lasted twenty-four hours, when she died. The patient had previously suffered from four serious attacks of a similar nature—in 1840, in May and September, 1844, and in February, 1845, six weeks before her death. In November, 1843, she had also anasarca of the abdomen and lower extremities, from which she completely recovered, and never afterwards became dropsical.

The author having only seen the patient casually, was unable to give any accurate information respecting the sounds of the heart. She had never suffered from rheumatism.

The following are the post-mortem appearances observed about the heart:—The organ was heavy and firm, and somewhat enlarged. The hypertrophy was chiefly on the right side, the right ventricle being as thick as the left, and the right auricle three times as thick as the left auricle. The foramen ovale was patulous—the opening about four lines in diameter. The auriculo-ventricular valves were thickened, but movable: the aortic valves healthy. Above the semilunar valves of the pulmonary artery an adventitious membrane was stretched completely across; it was a line in thickness, and perforated in its centre, not by a circular foramen, but by a mere slit, with margins of a red colour, and fringed with fibrin of the blood. The three semilunar valves were thrown up, as they are naturally during the systole of the heart, and fixed in that position by the adhesion of (what would have been) their free borders to the adventitious membrane.

The author concludes with the following deductions:—

1. From the presence of cyanosis immediately after birth, and the absence of

rheumatism during life, the adventitious membrane was a congenital malformation.

2. That the hypertrophy of the right side of the heart was the consequence of the obstruction in the pulmonary artery.

3. That as either an obstructed pulmonary artery, or a patulous foramen ovale, may exist separately without the blue disease, the cyanosis in this case was the result of the two lesions combined.

4. That the circulation of imperfectly oxygenated blood impaired no function so much as the formation of animal heat.—*London Med. Gaz.*, Feb. 13.

30. *Ammonia in Asthma*.—M. GUERARD has employed ammonia, Dr. Rogetta states, (*Annales de Thérap.*, Nov., 1845,) in a hundred cases of asthma, and generally with a favourable result. In some, indeed, its effects were truly marvelous, the paroxysm disappearing as if by enchantment. We lately saw in his wards an old woman, aged 60, who on admission was literally simple suffocating, the respiration being nearly suspended, in consequence of extensive pulmonary emphysema; the paroxysms recurred with extreme violence; but these disappeared after a single application of ammonia to the throat, a month ago. At the present time this woman is tolerably well (*assez bien*), she sleeps quietly, her respiration is free, and her physiognomy tolerably calm; she merely complains of an uneasy feeling in the throat, caused by relaxation of the uvula, which is resting on the base of the tongue.

M. G. applies the remedy in the following manner:—He dips a small pencil of charpie into pure liquid ammonia, and then instantaneously into a glass of water, after which he immediately passes it to the back of the throat, touching rapidly the velum, uvula, and œsophagus to a greater or less extent. He seldom has occasion to repeat the operation; when there is merely simple emphysema the improvement has been permanent. The pencil should neither be carried too deeply into the throat, nor allowed to remain too long in contact with the soft parts, more especially the posterior wall of the pharynx; the reaction resulting from such an application is, at times, followed by the most frightful symptoms, menacing instant death. The last case we witnessed at *La Charité* was that of a young man, hemiplegic, aphonic, and previously epileptic; the pencil was carried deeply into the pharynx, and maintained there a considerable time. The most horrible convulsions immediately ensued, and for a time it was feared he would sink. Fortunately the symptoms abated. In another case, recourse was nearly had to tracheotomy. By this mode of operating, the ammonia, exhaled in too great abundance, from the heat of the parts, acts too powerfully on the glottis, causing it to contract spasmodically, and the patient is threatened with asphyxia. It is well known, moreover, that cases have occurred, in which death has ensued from the presence of ammonia diffused through the air. Percy relates a case in which the son of a chemist thus perished,—he had immediate assistance, but fell a victim to the bursting of a bottle filled with ammonia. A similar case has been given by Merat and Delens. The importance of circumspection in the use of the remedy is the more necessary, as the benefits resulting from it are neither to be attributed to its cauterizing or revulsive effects upon the œsophagus, as has been imagined, nor yet to any nervous action; were this the case, any other cauterizing or irritating substance applied to the same spot would produce the same, if not more powerful effects, for the cauterization of the throat by the diluted ammonia is but slight. We shall see by and by that its action on the diseased organ is of a purely dynamic nature. But before proceeding further, we may inquire if this mode of treatment, so much vaunted of late in Paris, is really new. We have no hesitation in stating that it has been taken from the work of M. Giacomini, who is the only one, so far as we know, who has recommended the use of ammonia as a curative means in cases of asthma and catarrh. Three questions naturally arise from the preceding facts, 1st. In what cases of asthma may ammonia be prescribed with advantage, and in what cases is it hurtful, or of no use at all? 2. What is the true principle of action of the remedy in such cases? and, 3d. Is it indispensable to apply the ammonia to the throat in order to obtain its good effects?

From daily observation, it is evident that asthma frequently occurs in a state

of complication with organic lesions of the heart and aorta, or with tubercles; at other times it exists without these complications, although otherwise accompanied with emphysema or catarrh. In the latter case, the disease may be denominated simple, the catarrh and emphysema being regarded as symptoms of the affection. From what we have observed at *La Charité*, it appears, that it is only in cases of the latter description, that is to say simple asthma (idiopathic of authors), that ammonia effects a cure, or affords great relief.

In regard to the second question, as previously remarked; we can only regard the result produced by the ammonia, as a dynamic effect acting immediately on the diseased organ, an action analogous to that obtained by Scarpa from the same remedy, in congestive amaurosis. And the proof that its action is dynamic, and neither mechanical nor nervous, is just this, that if any other irritating remedy, the actual cautery, for instance, boiling alcohol, tincture of canella, or cloth steeped in hot wine, the same results will not be obtained; far from that, the disease will be aggravated. Two things, moreover, must be considered regarding the medication in question, and these are,—its physico-chemical and local action, which is evidently irritant, and its dynamic action, resulting from the speedy absorption of the ammonia by the bronchia, which it reaches with the air. The first of these is unimportant in a therapeutic point of view; it is inconvenient, and would certainly be hurtful, were it not counteracted by the dynamic action, affecting not only the lungs, but the whole system. What, then, is the nature of the dynamic action of the ammonia? From the effects we have witnessed in asthma and congestive amaurosis, we have no doubt that it is hyposthenic. In one or other case you have a humoral evacuation, (mucus, tears,) and afterwards, results similar to those produced by other hyposthenic remedies. If, instead of ammonia, boiling alcohol be used, you will likewise have humoral evacuation, but the disease is increased. From what has been said then, it follows, that the direct application of the ammonia to the pharynx is not indispensably necessary, in order to obtain all the good expected from it in asthma, its absorption by the stomach will lead to the same results, if it be administered in sufficient doses. M. Rayer has already obtained good effects from it in this way, but the dose being too small, they have not been permanent. The remedy must therefore be given in sufficient quantity, through other absorbing media, not excluding the respiratory organs themselves, by placing a phial filled with the medicine near the nostrils, or disseminating it through the air; the rectum, skin, and stomach, may conjointly be made the means of repeatedly administering the remedy. It is well known, for instance, that belladonna applied to the neighbourhood of the orbit causes dilatation of the pupil, produces its hyposthenic effect upon the globe of the eye and the brain, exactly in the same way as if it had been administered by the rectum, stomach, friction over the extremities, or injected into the bronchia. Ammonia forms no exception to this general law of effects produced by absorption.

31. *Hydrocele or Scrous Cyst in the Rectus Abdominalis.*—Dr. MAYNE exhibited to the Pathological Society of Dublin, (Dec. 6th, 1845,) a specimen illustrative of the difficulty which sometimes occurs in the diagnosis of abdominal tumours.

The subject of the case was a man aged 55 or 56 years, an occasional inmate of the South Dublin Union Poor House. He had been liable to attacks of severe bronchitis, for which he was in the habit of seeking admission into the poor house; and when relieved, he used to return to his occupation—that of a labourer.

About three months since, this man applied to Dr. Mayne on account of a new complaint, namely, a frequent desire to discharge the contents of his bladder, which, if not immediately satisfied, caused his urine to pass from him involuntarily. He could retain a moderate quantity without inconvenience, but whenever a certain degree of distension of the bladder took place, an urgent call to evacuate the urine immediately followed. He also directed Dr. Mayne's attention to a tumour in his abdomen, to which he attributed these symptoms. This tumour was seated above the pubis, on the right side of the linea alba, in the track of the right rectus abdominis muscle. It was of an oval shape, about the size of a goose-egg, perfectly uniform on the surface, and yielded an obscure sense of deep fluctuation. There was a very strong impulse communicated to it whenever the

patient coughed. It was not in the slightest degree tender upon pressure, but by compressing it, the inclination to pass water was strongly excited.

At first Dr. Mayne thought it might be a sacculus connected with the bladder, consequent on some disease of the urinary passages, but a full-sized silver catheter passed along the urethra without difficulty, showing that there was no stricture nor any disease of the prostate gland. The urine drawn off was perfectly healthy, from which a sound condition of the mucous coat of the bladder was inferred; and the evacuation of this viscus had no effect whatever in diminishing the bulk or tension of the abdominal tumour, which rendered it unlikely that any communication could subsist between the tumour and the bladder.

Dr. Mayne was led to regard it as a chronic abscess, and the treatment was regulated accordingly; blisters, iodine, etc., were prescribed with little benefit.

After some time, the patient left the poor house, but he returned lately labouring under typhoid pneumonia. He was in a state of profound prostration, with extreme dyspnoea, some anasarca, and general dulness over the whole of one lung posteriorly. He soon sank.

At the autopsy, Dr. Mayne was particularly anxious to ascertain the nature of the tumour,—it was now before the society. They might perceive that it was a hydrocele placed at the back of the right rectus muscle. In this situation the posterior lamina of the sheath of the muscle is deficient, so that the tumour rested upon the peritoneum.

The experiment of inflating the bladder was made before the society, and as it became distended, the superior fundus was observed to come fairly into contact with the tumour. The same occurrence must have taken place during the patient's lifetime, whenever the bladder was dilated, and the pressure in this manner exercised upon it renders a satisfactory explanation of the urinary symptoms under which he laboured.

Dr. Mayne thought it probable that any attempt at a radical cure (supposing the diagnosis to have been made) would have induced a fatal peritonitis.—*Dublin Hospital Gaz.*, Jan. 1st, 1846.

## SURGICAL PATHOLOGY AND THERAPEUTICS AND OPERATIVE SURGERY.

32. *On Displacement of the lower fragment in fracture of the Surgical Neck of the Humerus.* By M. DEBROU.—As fracture of the surgical neck of the humerus is commonly occasioned by direct violence acting on the posterior part of the shoulder or on the upper and outer part of the arm, M. Debrou thinks that the fracture is usually directed obliquely from above downwards, and from within outwards, or from before backwards. In 1843 he saw three cases of this fracture at the Hôtel Dieu of Orleans, in which the obliquity was in this direction, and in those three cases there was considerable prominence of the lower fragment, which was drawn upwards and carried inwards and forwards. When the extremity of the lower fragment is drawn in this direction it encounters a smaller thickness of soft parts than if it were displaced externally, and may come in immediate contact with or even run the risk of perforating the skin. In the three cases above referred to, the extremity of the bone partially (but not completely) perforated the skin, and thence carried the integument backwards with it on the slightest motion of the elbow and of the inferior fragment; from this there resulted a depression of the skin, which became deeper the further the lower fragment was carried backward. In one of these cases the lower fragment was so firmly engaged in the skin that perfect reduction could not be effected until the bone was freed from the skin by means of a subcutaneous section with a tenotome introduced two inches from the site of fracture.—*Dub. Med. Press*, from *Journal de Chirurgie*.

33. *On a new variety of Dislocation of the Humerus.*—By Prof. ROSER, of Taubingen. The Professor discovered in a subject intended for dissection at the University of Taubingen, a variety of dislocation of the humerus not hitherto described by

any author. The head of the humerus lay in front of the short head of the biceps; the sub-scapular muscle was ruptured and completely detached from the lesser tuberosity of the humerus, and the head of the bone elevated the scapular extremity of the pectoralis minor. In ordinary dislocations of the shoulder, the upper extremity of the humerus lies immediately on the outer border of the scapula; in this dislocation it is separated from it by the biceps and the coracobrachialis whose tendons pass behind instead of in front of the humerus. On a careful examination the following dispositions were observed.

The dislocated limb was abducted and slightly everted, the head of the humerus touching the inferior border of the coracoid process. Intimate adhesions existed between the muscles of the shoulder and the ligamentous apparatus of the joint. The brachial plexus was surrounded by a very dense cellular tissue. The sub-scapular muscle, detached from the lesser tuberosity of the humerus, terminated in a bulbous mass which rested on the neck of the scapula, and involved the musculo-cutaneous nerve. The head of the humerus lay in a capsule of new formation below and internal to the coracoid process, was tolerably movable, and the pectoralis minor was expanded over it, adhering very firmly to its capsule. The tendons of the coraco-brachialis and short head of the biceps descend behind the head of the humerus involved in the capsule. The long head of the biceps was entirely displaced from its groove, described a curve round the head of the humerus, and was completely adherent to and confounded at its insertion with the fibrous tissue which filled the glenoid process.

The subject of this observation had fallen seven years previously while carrying a heavy load up a hill. The first attempts at reduction were made by some woodcutters who came to his assistance. Several surgeons to whom he applied on the following day were equally unsuccessful. Very forcible tractions and several methods, including that of De la Mothe, were tried, but every time that the head of the bone was thought to be reduced it was found to be still displaced. The patient regained tolerable use of the limb, being able to dig and thrash, but could not put his coat on the left arm. He often experienced pain and a sense of numbness in the fingers.

Professor Roser easily produced this species of dislocation on the dead body, by cutting the tendon of the sub-scapular muscle, displacing the long tendon of the biceps from its groove, and then forcing the head of the humerus downwards. But what (he asks) was the obstacle to reduction? It was not, he thinks, muscular action, but the interposition of the short head of the biceps between the head of the humerus and the glenoid cavity. To reduce this dislocation, the same principle should be adopted that is followed in other reductions—viz., to cause the head of the bone in re-entering to follow the course it took in its exit, and as while being displaced it probably sustained violent torsion outwards, during reduction it should be forcibly rotated inwards, so as to slide over or else displace the interposed soft parts.—*Gaz. Med. de Paris* from *Archiv. für Physiol. Heilk. Von Roser*.

34. *Vagino-labial Hernia*.—Prof. STOLTZ had occasion to examine a pregnant woman, who, whilst coughing at stool, suddenly felt a large swelling make its appearance in the right labium. The tumour diminished when the patient lay down, but became painful and very large when she stood, or was engaged in fatiguing occupations. The tumour disappeared completely under compression, but reappeared on coughing. These characters enabled it to be recognized as a hernia, but M. Stoltz ascertained it was not an inguinal one, for the hand pressed over the inguinal canal and aperture did not prevent the hernia from descending into the labium. It was remarkable that in this case the hernia did not return after delivery.

In commenting on this case, M. Stoltz confirms the propriety of Sir A. Cooper's naming this form of hernia pudendal hernia; and he considers it to be quite distinct from perineal hernia, with which it might be confounded. The much greater breadth of the perineum in women favours the occurrence of hernia in them. But the broad ligament of the uterus divides the pelvis into two portions, a posterior and an anterior. If a loop of intestine descends in front of this broad ligament along the side of the vagina, and if it pushes before it or separates the fibres

of the *levator ani*, the hernia will occupy the labium and nowhere else. But if, on the other hand, the loop of intestine descends behind the broad ligament, it descends between the vagina and rectum, and shows itself in the perineal region near the margin of the anus. The hernia is then a perineal one.—*Ed. Med. and Surg. Journ.* from *Gaz. Méd. de Strasbourg*, Jan. 1845.

35. *Two Cases of Prolapsus of the Uterus cured by excision of a portion of the Vagina.* By M. LÉLOUTRE.—A married woman, 45 years of age, the mother of five children, suffered from *prolapsus uteri* brought on by a fall on the fundament. Pessaries gave intolerable pain, and for several months the woman was in a deplorable state. Between the thighs there was a tumour of a pyriform shape, 11 inches 9 lines in circumference, and 5 inches in height, composed of the uterus with a portion of the vagina. Three longitudinal portions of the mucous surface of the vagina, extending from the *os uteri* to the vulva were cut out, one on each side and one behind. The edges of the wounds were then cauterized and the uterus replaced in the vagina. No unfavourable symptoms followed. A month after cicatrization was complete, the vagina was considerably narrowed, and the uterus remained *in situ*. Forty-four days after the operation the patient got out of bed, and at the end of eighteen months the prolapsus had not returned.

The second case was a woman 50 years of age who had led an irregular life, and who had a complete prolapsus of the uterus with inflammation. After the inflammatory symptoms were subdued, four longitudinal portions of the mucous surface of the vagina were removed, and the wound treated as in the former case. On the fifty-fourth day the cure was complete, the uterus being retained in its proper situation. Two years after this woman died of an apoplectic attack, when the condition of the parts were examined. The vagina presented four grayish-coloured resistant lines corresponding to the incisions. The mucous membrane had acquired considerable hardness, especially that towards the orifice of the vagina, which had lost much of its ordinary dimensions. The uterus preserved its normal structure, but its volume was evidently much increased.—*Ed. Med. and Surg. Journ.*, July, 1846, from *Archives Gén. de Méd.*, Oct. 1845.

36. *Resection of the four external fifths of the Clavicle.* By M. CHAUMET.—A girl, æt. eighteen, was admitted in the Hôpital St. André, of Bordeaux, June 30, 1845. Nine months previously she experienced vague pains in the right clavicle. A tumour soon commenced which has attained the size of the shut hand; it is immovable, hard, mammillated, and occupies nearly the four external fifths of the clavicle: it is the seat of moderate pain, and does not impede the motions of the right arm, which is not, however, quite so strong as the opposite one. July 15th M. Chaumet laid the tumour bare by a semi-elliptical incision, convex anteriorly and inferiorly, which extended from within half an inch of the sterno-clavicular articulation to the summit of the acromion. After dissecting up the flap, a portion of which was removed, as its size would have interfered with coaptation of the parts after the removal of the tumour, a vertical incision was made at the sternal extremity of the first along the inner border of the sterno-mastoid muscle. The tumour was then detached from its connections without its being necessary to tie a single artery. The clavicle was now divided at its internal fourth with the chain-saw, and the coraco-clavicular ligaments being cut, the bone was removed. Three points of suture were applied, and to diminish the extent of the cicatrix, the shoulder was approximated to the residue of the clavicle by a circular bandage. Irrigation with cold water was commenced two hours after the operation, and continued till the tenth day. On the 54th day the remains of the clavicle were slightly elevated by the sterno-mastoid muscle. On the 70th day the girl returned home, enjoying slight motion of the arm.—*Gaz. Méd. de Paris*, from *Journ. de Méd. de Bordeaux*.

37. *On Spermatic Discharges.*—The *London Medical Gazette* for May, 1845, contains an interesting paper by MR. B. PHILLIPS, on this subject. The cases, he remarks, which have come under his notice have left on his mind the impression, "that though great general depression, and much mental discomfort, are the common accompaniments of those discharges, the instances are unfrequent where

permanent bodily or mental mischief results from them: still I have known cases in which the mind as well as the body have broken down under the complaint. Usually, however, the inconvenience is got the better of; and I think more commonly it is owing to effort on the part of the patient than to the skill of the attendant.

"Of the 109 cases to which I have alluded, 84 were under 22 years of age; 97 admitted that they had practised masturbation to a greater or less extent, and they referred their present distress to that practice. Every one, however, stoutly asserted that the habit had been discontinued—by some for a few months, by others for years—but in many cases I thought I saw reasons to doubt the correctness of the assertion. In two of the older cases, I am of opinion that irritation within the rectum was the exciting cause; in two instances abuse of sexual intercourse; in one, probably in two, stricture of the urethra; in four, the irritation consequent upon congenital phymosis. In the remaining cases I have no doubt that masturbation was practised, though it was strenuously denied.

"In 38 cases the patients were submitted to active treatment; caustic was applied upon the urethra. In 71 instances no medical treatment seemed to be necessary, except for the purpose of exercising some moral influence over the patient, and to gain time. They were advised in many instances to use a mixture of the tinct. ferri sesqui-chloridi, and tinct. lyttæ. In cases where the patient was reasonable, I tried to convince him that it was unnecessary to employ any medical treatment.

"Such is a statement of the general features of those 109 cases. I will now proceed to offer some remarks upon the ordinary characters presented in the cases which have come under my observations.

"Of the patients under 20, a considerable majority deny that they have ever attempted sexual intercourse, but their minds have evidently dwelt much on sexual matters. They complain of occasional emissions during the night, of which they are usually conscious for the most part; they occur during lascivious dreams, and in most cases not oftener, on an average, than once in four to seven days. In a great many cases, however, there is another symptom which they conceive to be much more serious—the escape of a drop or two of a viscid transparent fluid with the last drops of urine, either when simply emptying the bladder, or when at stool. This kind of loss very commonly produces severe mental depression, often amounting to absolute hypochondriacism. In most cases the patients' minds are alive to all the dreadful consequences of these symptoms, for they have usually devoured the contents of books written by the inventors of quack medicines, for the cure of sexual debility, who gain a disgraceful livelihood by pandering to the fears and fancies of young lads who suffer from these affections; and in most cases no reasoning will convince the sufferer that his worst fears will not be realized. And in these cases I have occasionally submitted them to treatment, more with a view to act upon their moral than their physical nature.

"In this class of cases the instances are few in which, if the practice of masturbation be discontinued, and the mind can be directed to other objects, all inconvenience is not in a moderate time dissipated. The difficulty I have experienced is to make the patient think so. My view of these cases is less gloomy than that of some persons who have written on the subject; they have been regarded as the most aggravated form of the malady. In ordinary cases I hold them to be the simplest, the most easily got rid of.

"The way in which I explain this kind of discharge is as follows. A large number of lads soon after puberty inflame their minds by reading lascivious books, from which they derive images of sexual pleasures. A large number, also, are early taught the practice of masturbation. Under both circumstances, the gratification derived by contemplating the image, on the one hand, and by the physical excitement, on the other, keeps up a constant irritation. The testicles, instead of enjoying moderate rest, are incessantly stimulated by the dream at night, the mental or physical stimulus during the day; the secretion is constant, the seminal vesicles are distended, and unless the ejaculation takes place with sufficient frequency to relieve them, the fluid must escape by some other means; and the pressure upon them during the efforts at stool, or in making water, squeezes out a



certain quantity of the fluid by which they are distended, and it usually escapes with the last drops of urine.

"At one time I doubted whether this fluid were spermatic, it is usually so smooth, transparent, and homogeneous; but if it be examined under the microscope, spermatozoa can usually be observed in it. Its thin fluid character has induced some people to think that it was a depraved secretion, become watery by exhaustion of the secreting organ; and sometimes it may be so, but usually it is not so; for if a person who complains of the constant escape of the thinner fluid has an ejaculation, the fluid will be thick and grumous. It is therefore most probable, that the more fluid portion of the secretion which fills the seminal vesicles is most easily pressed out; and this is a reasonable explanation of this particular feature of seminal discharges.

"To cure this symptom, all that is necessary is to keep the testicle quiet, by substituting new images for the prurient fancies which have occupied the mind, and to empty the vesicles occasionally by sexual intercourse; and for this class of cases I think marriage is the best cure. It is true, it is not easy generally to make the persons who thus suffer think so; they believe that their sexual power is destroyed, and that intercourse must be impossible—that their impotence is complete; but spite of such impressions, I have never hesitated in such a case to recommend marriage as a cure. You may be met with the observation, that intercourse has been attempted, but that it had failed—the erection was insufficient, or the ejaculation premature. These things in no wise alter my opinion, because where a person casually attempts intercourse as a means of testing his powers, but with the impression strong on his mind that he shall fail, he does fail; and therefore I dissuade patients from having recourse to these tests. But if a man be married, he ultimately gets rid of that nervousness which may at first have rendered him powerless, and he ultimately succeeds; and a single success gets rid of the mental impression which prostrated his bodily powers.

"It is astonishing, in these cases, how powerful is the influence of the mind in destroying the virile power. I was consulted by a man upwards of 40 years of age, who was at the time apparently in the best health, but who for twenty years had scarcely had an erection during perfect consciousness, though the sexual organs were perfectly well developed, and natural. His own account was, that there was no want of erection before 20. To some extent he had practised masturbation, and emissions during lascivious dreams were not unfrequent. At that time he read Sauvages, where he found a passage containing, as he conceived, a statement that discharges occurring as his did lead to impotency. From that hour he has been practically impotent. On first waking in the morning there is almost always complete erection; but the moment consciousness is perfect all erection ceases, and does not occur again till the next morning.

"Supposing my view of such cases to be correct, it is clear that they would commonly right themselves; but so profound is the mental distress they occasion, so hopeless the condition of the mind, so complete the despair in many cases, that all reasoning is useless, and you are obliged to propose some plan of treatment; and the main point to be carried out is to convince the patient that matters will go right under the use of the remedy employed, but that many months are necessary for the purpose; and that even then, unless the habit of constantly looking for discharge, and dwelling upon the subject, be discontinued, it is very likely it will fail.

"In all cases, however, previous to recommending such a course, I am accustomed to pass a bougie along the canal once or twice, for the purpose of ascertaining whether or not it be in a natural state; because, if there be no increased sensibility in the vicinity of the verumontanum, my experience does not enable me to promise any very certain relief from the direct application of any agent. If there be much sensibility along the whole canal, and this is sometimes the case, it is often difficult to satisfy the mind as to the state of that particular portion of the canal. It then becomes necessary to pass a bougie more than once, to determine what we desire.

"Even in cases of this class, if there be very excited sensibility at the point in question, great good is usually derived from the use of caustic; and, indeed, there

are few cases of sexual debility which are much improved by the use of caustic, unless such abnormal sensibility be present."

38. *New Method of applying Taxis.*—M. GRYNFELTT proposes to effect the reduction of hernia by applying mechanical force to the contained viscera in order to drag the protruded intestine back into the abdomen, instead of adopting the usual proceeding of endeavouring to push it back through the opening at which it has made its exit.

The following is the description given by M. Grynfeltt of the manner in which he has applied his taxis, in the case of a female, who had suffered from a strangulated inguinal hernia for five days:—

"We flexed the thighs as much as possible on the pelvis, leaving room, however, to grasp the tumour easily. The loins, thorax, and head, were also bent forwards to the full extent, so that the whole body was, as it were, coiled up. The patient, being placed in this position, it is easily imagined, that complete relaxation of the abdominal parietes was produced. Next, with the fingers of the left hand, we seized the hernial tumour at its base, and compressed it slightly. At the same time, with the right hand, we brought the coverings of the abdomen as much towards the external inguinal aperture as was practicable, when, by a simultaneous movement with the fingers applied over the parts, we produced a traction on the hernial contents sufficient to bring them back into the interior of the abdominal cavity. At the first attempt we had the satisfaction of feeling under our left hand, which supported the hernial tumour, the movement of fecal matter, and of the gas contained in the protruded portion of the intestines; when, combining compression of the hernia with the traction of the intestines in the abdomen, we succeeded in the reduction in a few seconds."

"Our operative proceeding, then, consists in *drawing* back the protruded portion of intestine into the abdominal cavity, instead of trying to *push* it back."—*Monthly Journ. Med. Sci.*, from *Gaz. Med. de Paris*, May 30, 1846.

39. *Ligature to the common Basilic Vein.*—WALTER YATES, Esq., records in the *Lancet* (July 4, 1846,) the following case, in which he applied a ligature to the common basilic vein, without a single outward symptom occurring.

Ann C—, aged thirty-three, a very stout person, was coming carelessly down stairs with a chamber-pot in her hand, and happening to tread on her dress, she fell all her length to the bottom; the chamber utensil was broken into very many fragments, from some of which she sustained a severe injury of the arm. She immediately got up, and found her dress to be almost completely besprinkled with blood; a stream of the same fluid was also flowing down her arm. Assistance happening most luckily to be at hand, her arm was bound tightly up with six or seven handkerchiefs. She considers the loss of blood in this short time to have been not less than two quarts. (?) She feels faint, is very pale, and dreadfully alarmed lest she shall lose her arm. Having at hand everything necessary for the purpose of effectually securing any vessel or vessels that might be wounded, I removed the handkerchiefs, immediately upon which a terrific gush of blood followed, which I was unable to arrest by the firmest pressure of a bandage, being obliged to have recourse to the tourniquet. Having thus temporarily prevented the hemorrhage, I then searched the wound—which was deep, and was directed horizontally across the front of the elbow-joint, the incision being about three inches in length. I discovered a large vessel, which I at first took to be the humeral artery, but which afterwards turned out to be the common basilic vein. The incision proceeding as it did through three-fourths of its caliber only, allowed the vessel to gape very considerably, hence the tremendous escape of blood. Without further loss of time I then passed a tenaculum through the vein, above the place where it was injured, and a second one through it below the same place, and directly divided the remaining portion of the tube, disconnecting very slightly, and with the point of the scalpel, the vein from its cellular investment both upwards and downwards, so as to admit of a ligature being placed around its two extremities without any stretching of its coats, or of the nervous filaments connected with the coats. This operation occasioned very acute pain, which abated, however, in the space of a few minutes. All danger from immediate hemorrhage

being now at an end, I leisurely, but carefully, examined the wound, in order to ascertain the exact nature of the injury. There being no pulsation of either of the two ligatured ends of the vessel, I placed my finger in the wound, and felt distinctly the brachial artery pulsating directly under the finger, and immediately under the divided vein. It was this want of pulsation which at once led me to suppose that the humeral artery had escaped injury, and that the vessel injured was a large vein, that vein being, of course, the common basilic. The edges of the wound were brought together by a single suture, and by straps of adhesive plaster, a dossil of warm wetted lint being placed over all. The forearm, placed at right angles with the humerus, was then lightly bandaged with a roller thoroughly soaked in warm water, the whole limb being supported and kept in its position by a long pasteboard splint, nicely adapted to the occasion. She was put to bed, and kept on low diet; directions were given that the whole arm should be kept continually moist with warm water. She was placed under the action of a saline antimonial mixture, having first had a draught containing forty minims of the tincture of opium.

One ligature came away on the ninth day, and the second on the eleventh day; and the entire wound healed kindly in the short space of a fortnight.

40. *Injurious effects of Pressure in the employment of the Taxis for the reduction of Hernia.*—P. P. TEALE, Esq., in his *Practical Treatise on Abdominal Hernia*, relates the following case, which is particularly interesting in connection with the elaborate and valuable paper by Dr. Blackman, in the original department of our present number on the reduction of strangulated hernia *en masse*.

A man, aged thirty-nine, was admitted into the Glasgow Infirmary, having had a reducible hernia for ten years, which had been strangulated ten hours. During this time a surgeon had made powerful and continued efforts to return the displaced parts, and on his admission into the hospital, the taxis was again rather forcibly employed. When the patient was seen by Dr. Macfarlane, he considered it improper to make any further attempts at reduction. The scrotum was much swollen and discoloured. When the sac was opened, not less than a pound and a-half of dark-coloured blood escaped, a considerable quantity of which was pressed from the depending part of the scrotum. The hernia consisted of a large part of omentum, which was covered with coagulated blood, and of nearly two feet of intestine. The omentum was bruised and lacerated, and the protruded gut was almost wholly separated from the mesentery; it contained several rents, which passed in a longitudinal direction, and into each of these openings two or three fingers could be introduced. Mr. King possesses the unpublished records of forty cases of strangulated hernia; eight of these proved fatal after reduction by the taxis: six from rupture of the intestines; one from peritonitis, and one from having been reduced in mass. Even in cases which recover it is not uncommon to find peritonitis of such intensity as to endanger life, following reduction by the taxis. The return of the hernia in mass whilst strictured by the neck of the sac is an accident of a most formidable character, which occasionally, though rarely, results from manual pressure; this accident may occur even under the employment of such a degree of pressure as the surgeon was justified in exercising.

41. *Treatment of Panaris by Mercurial Ointment.*—Dr. MARTIN has published in the *Mémoires de Méd. Militaire* a very valuable practical paper on this subject. Panaris prevailed to a considerable extent among the soldiers of Dr. M.'s regiment in Spain, in the year 1834, and the treatment adopted, narcotic and emollient applications, general and local bleedings, incisions, &c., not having resulted satisfactorily, Dr. M. resolved to give a fair trial to the use of mercurial ointment as recommended by M. Serres of Alais. He soon was satisfied not only of its efficacy but was persuaded that it is almost a specific in these inflammatory affections. The first case in which he used it, was the following:—

A soldier presented himself at visit on the 15th March with a swelling, of four days' standing, on the palmar surface of the right hand, which had originated without any known cause. The pain experienced in it was intolerable, continuous, and pulsative; the skin was hot, red, and very sensible to pressure. All the symptoms appeared to indicate its termination in suppuration. M. Martin

prescribed mercurial frictions, according to the method of M. Serres; the pain soon began to abate, the patient was able to sleep, and the signs of reaction diminished. After six days' use of the frictions, the inflammation had entirely disappeared.

M. Martin, after satisfying himself of the efficacy of this mode of treatment in several similar cases, thought that by using the friction more frequently, and afterwards covering the part with a poultice, he would still more increase the tendency to resolution; he did so accordingly, and found the results more favourable and speedy. In this way he effected a cure in several very aggravated cases. The following summary, appended by the author to the end of his paper, will best exhibit the value of this mode of treatment.

In 1834, M. Martin had fifty-three cases under his care. All the resources of the art, with the exception of mercurial frictions, were tried. Of the fifty-three cases, forty terminated in suppuration, three in gangrene, three in caries of the phalanx, and in seven permanent stiffness of the affected part ensued.

In the following year he had forty-eight cases. Mercurial frictions were employed, and there was a marked difference in the results; twenty-seven cases terminated in resolution, and twenty-one only in suppuration. In none of them was there gangrene, loss of the phalanx, or stiffness of the joints. The period of treatment, moreover, was much shorter.

It may be added that during the two latter months of the year, when M. M. commenced to employ the frictions at shorter intervals, and covering the part with a poultice, the success was much more marked than in the previous months.

The modification of M. Serres' method by M. Martin is the following:—Instead of having recourse to the mercurial inunctions every three-quarters of an hour, and with a small quantity of the ointment, as advised by M. Serres, M. Martin recommends them to be made for five minutes at a time, and with the same interval, to be continued for two hours night and morning, and during the interval the affected part to be covered with a poultice.

By the use of these means, to the exclusion of almost all others, M. Martin has obtained this remarkable success. He does not hesitate to say he considers mercurial frictions specific in this painful affection.—*Monthly Journ. Med. Sci.*, June, 1846:

42. *Hydrocephalus—Tapping—Cure.*—Dr. JAMES EDWARD relates in the *Monthly Journal of Med. Sci.*, an interesting case of chronic hydrocephalus in a child eight months of age in which he resorted to tapping. The child when seen by Dr. E. was fretful and feverish, leaning his head alternately on his mother's arm and breast, with small irregular pulse. In the course of Dr. E.'s attendance the child was alternately affected with diarrhoea and constipation of the bowels, the tongue being sometimes clean and at others foul; the face had generally a hectic appearance; the skin was occasionally hot and dry.

Dr. E. scarified the gums, applied a leech to each temple, and had him occasionally bathed to the middle in warm water. The bowels were rectified as occasion required by aperients or astringents. Cold applications were occasionally applied to the head when it felt very warm, but were discontinued when the temperature was reduced. Blisters were applied to the nape of the neck successively for a considerable time, and latterly these were replaced by a liniment of croton oil rubbed over the head and neck, which produced an eruption on the latter, but only an erysipelatous blush over the former.

Notwithstanding the continuance of this treatment during a period of six months, the disease gradually gained ground. Convulsions and squinting made their appearance, and Dr. E. considered that tapping for the evacuation of the effused fluid afforded the only chance of recovery.

The operation was accordingly determined on and performed in the following manner:—The child was placed across the knee of an assistant with his head towards Dr. E. The latter then applied loosely a starched bandage, the ends of which were given to another assistant that he might tighten it as the fluid was being evacuated. Dr. E. then introduced a common hydrocele trocar and canula a little to the right of the lateral angle of the anterior fontanelle, to the depth of about an inch in the direction of the right lateral ventricle. Having withdrawn

the trocar, a reddish fluid was allowed to flow to the amount of  $\frac{3}{4}$  viij, when Dr. E. withdrew the canula, applied a compress on the wound, and tightened the bandage for the thorough support of the cranial bones. A slight oozing of blood took place from the nostril and continued for twenty-four hours: whether from an accidental wound of any small vessel within the cranium, in the course of the operation, Dr. E. is unable to say.

Nourishing diet was ordered; aperient medicine when necessary; and the body to be kept comfortably warm. All the former symptoms gradually disappeared; the child recovered his health, and now, after a lapse of seven years, is a fair scholar, and has every faculty good. He appears, however, to be increasing in height more slowly than is usual at his age.

43. *Treatment of Wounds of the Arteries of the Superior Extremity by compression and forcible flexion of the forearm.* By M. J. FLEURY.—When the forearm is strongly flexed, the pulse, as remarked by Bichat, ceases at the wrist, and Malgaigne, availing himself of this fact, tested the effects of a forced flexion of the arm in a wound of the brachial artery in 1832; he was at that time satisfied also that a strong flexion of the knee would equally arrest the pulsations in the artery of the foot, and consequently in all the arteries of the leg.

M. Fleury has made a successful application of these data in two cases, the one a complete transverse section of the brachial artery at the fold of the arm, the other a complete section of the radial artery.

A man named Nicolini met with a severe injury from the bursting of a cannon. The thumb of the right hand was stretched out (allongé) and in part buried in a deep wound, about three centimeters wide, in the anterior and inferior part of the left arm in an oblique direction downwards and outwards. Compression in the wound itself prevented the spouting of the blood, which, however, continued to flow in torrents. The moment the compression was removed, pure arterial blood escaped in a continuous and jerking stream. The inspection of the wound, its situation, direction, depth, with the colour and jerking of the blood, left no doubt as to the brachial artery being completely cut through transversely a little above its division into the radial and cubital arteries. The simple indication was to tie the two ends of the artery, but M. Fleury was prevented doing this by the arrival of several other wounded patients, and the operation was deferred, the wound being dressed as follows:

The brachial artery was compressed about its middle, the forearm flexed to  $45^\circ$ , bringing together the edges of the solution of the continuity, two diachylon bandages maintained them together, a large pledget of cerate placed transversely covered the wound, then two tents of charpie and two tight bandages fixed the whole.

A roller was applied from the fingers to the elbow, the forearm was then flexed as tightly as possible on the arm, the bandage was continued by figure of eight turns which extended to the middle of the arm. The compression being complete, the forearm strongly flexed on the arm, was immovable. Nicolini had received also a dangerous wound in the abdomen with extrusion of the intestines.

Examined some time afterwards, the bandages were scarcely tinged by a drop of blood, the compression was supportable and no inconvenience was observed respecting the apparatus. An attack of peritonitis supervened, from which he was soon convalescent. After the expiration of three days, the patient complaining that he suffered much from the arm being too tight, the bandages were cut behind, care being taken as to the position of the limb. It was bathed with cold water, and all the dressings which did not adhere to the wound were removed. New dressings were substituted, and the bandage replaced by one not quite so tight. On the seventh day the bandage was soaked, the charpie, imbued with pus of a good character, fell off, and the wound was seen almost with a complete cicatrix. The blood had resumed its course by the collateral vessels, the pulse was perceptible, but smaller than in the right arm, and the patient perfectly recovered.

In the second case a man received a transverse cut to the bone in the right hand, with a china plate; the blood gushed out instantly. The radial artery was

cut completely through. The artery was compressed and the limb washed; the edges of the solution of continuity were brought together by flexing the hand on the wrist, two small agglutinative bandages being applied. The wound was covered transversely with a pledget, and with dry charpie in large folds. A rounded form was given to the wrist by the application of graduated compresses dipped in cold water, and applied to the dorsal and palmar surfaces of the wrist and forearm; these were sustained by a long damp tight bandage. A roller was then applied, commencing at the fingers, and having reached the elbow, the forearm was flexed as tight as possible upon the arm, and the remainder of the bandage was employed in figure of eight turns, which rendered the forearm fixed and immobile. The whole of the dressing was bathed in cold water and the limb was placed in a scarf. The patient recovered without any untoward circumstance.

M. Fleury, after making several remarks, arrives at the following conclusions:

1. Forced flexion of the forearm, upon the arm, aided by a compressing bandage well applied, and by the limb being rendered immovable for a sufficient length of time, will suffice for the radical cure of any transverse and complete section of the inferior parts of the humeral artery, and of its terminal branches. By analogy, as much may be said respecting the terminal branches of the popliteal artery, since the forced flexion of the leg on the thigh suspends or relaxes the course of the arterial blood in the leg and foot.

2. Every incomplete division of the arteries may be cured in the same manner, but the chances of success would be less than when the vessel is completely divided. Accordingly, in many cases it might be necessary to imitate the practice of our predecessors, by completing the section of the vessel injured below the bend of the arm or the hand.

3. Any aneurism whatever of the arteries mentioned is curable by the same means. The ligation of the arteries therefore should be limited to certain rare cases, as an anchylosis, or fracture, or some other disease, which would prevent the forced flexion of the forearm or of the leg.

4. Lastly, these two cases prove in a decided manner that one can and ought to dispense, in certain cases, with tying an open artery in a recent wound.—*Ranking's Abstract*, vol. iii. From *Gaz. Med. de Montpellier*.

44. *Œsophagotomy*.—This operation was performed with success by M. DELAVACHERIE, Professor of Clinical Surgery at Liege. A man during a meal suddenly experienced a painful sensation in the region of the œsophagus, which he attributed to something having stuck there. This was soon followed by pain and great difficulty of deglutition. Emetics were employed unsuccessfully. At the termination of six days, and during forty-eight hours subsequent, he threw up at intervals large quantities of blood. A gum-elastic catheter was then passed, and confirmed the diagnosis of a foreign body being in the œsophagus. The catheter sometimes passed into the stomach without resistance, and sometimes met with an obstacle, in striking against which a noise was distinctly heard by the bystanders. Various instruments were employed for the purpose of withdrawing the foreign substance, or propelling it towards the stomach, but unsuccessfully, it was so impacted in the soft parts that violent efforts must have resulted in a rupture of the tube, and from the trials made it was doubtful whether it would be possible by any effort to displace it.

Long-continued sleeplessness, pain, anxiety, deprivation of food, and large losses of blood reduced the patient to a state of extreme debility, and his life was in imminent danger; œsophagotomy was accordingly performed by the method of M. Bégin.

The patient was laid on his back, the shoulders and chest being moderately elevated, and the head turned back and supported on pillows, so that the neck was inclined to the right. The operator placed himself on the left, and made from above downwards an incision through the skin, parallel to the trachea, reaching to about a finger's breadth above the sterno-clavicular articulation, below; and to the level of the superior edge of the thyroid cartilage, above. The muscles and cellular tissue were then successively divided, until the cellular space, between the trachea and œsophagus on the one hand, and the deep vessels and nerves of the neck on the other, were cleared. An assistant placed to the right

of the patient during this dissection, drew the trachea and its dependences towards him, while the operator separated the external lip of the division with the ends of the three first fingers of the left hand introduced deep down to protect the vessels and nerves. In dividing the muscular fibres the wound was suddenly inundated with blood from the division of a transverse cervical vein, which had to be tied. The superior fasciculus of the omohyoid muscle had not to be cut. After separating that muscle, the bodies of the vertebræ were reached, but the œsophagus could not be distinguished either by the sight or by the touch. The catheter introduced into the tube did not project into the wound. The patient now swallowed a little water, when bullæ of air and a small quantity of water escaped at the bottom of the wound. The finger being placed over this spot, a hard very acute foreign substance was felt; this was fixed with a forceps to serve as a guide for the opening of the œsophagus, but before doing so, slight traction having been made to ascertain if the forceps held firmly, the foreign body escaped. On examining the neighbouring parts, the common carotid and internal jugular were contained in their sheath intact, but a branch of the superior thyroid artery had been divided. The foreign substance was a large piece of bone. Considerable fever and cough followed the operation; and the wound suppurated and sloughed; emollients and antiphlogistics were employed; and the patient left the hospital on the twenty-sixth day after the operation, perfectly cured.—*ANSEL'S Report in Ranking's Abstract*, vol. iii.

45. *Unusual Injury of the Elbow-Joint.*—Mr. R. W. SMITH exhibited to the Dublin Pathological Society a preparation and cast of an injury of the elbow of a very unusual and rare description: it was taken from the body of a man æt. 46, who died of pulmonary disease, and who had met with the injury of the elbow several years before his decease; the forearm formed a right angle with the arm; it could not be flexed any further, nor could it be extended; the hand was fixed in a state of pronation, the power of supinating was almost completely lost: no motion whatever could be imparted to the ulna; it was in fact obvious that it was ankylosed to the humerus: the head of the radius formed a striking prominence, seen, (in the flexed position of the forearm,) below and behind the natural situation of the capitulum of the humerus; the olecranon process likewise, was placed behind and below the plane of the condyle of the humerus, but was by no means as prominent as in the ordinary case of luxation of both bones of the forearm backwards. Upon the inner side of the joint, three distinct osseous processes could be felt, placed upon the same vertical plane; two of them were constituted by the inner margins of the olecranon and coronoid processes; the third, (which was inferior,) corresponded to the inner condyle of the humerus; at the outer side of the joint, was also seen a remarkable projection, placed above the dislocated head of the radius, and reaching upwards about three inches along the outer margin of the arm. When it ceased, a great irregularity could be felt in the shaft of the humerus as if it had been the seat of an oblique fracture; from these characters, it was conjectured that the injury had originally been a combination of fractured humerus, with luxation of the forearm at the elbow. Upon examination, *post-mortem*, the following condition of parts was found. A fracture commencing about four inches above the outer condyle of the humerus, traversed the bone obliquely downwards and inwards, terminating about an inch above the inner condyle; this fracture had united with considerable deformity and overlapping, the lower end of the upper fragment projecting forwards and inward: the ulna was ankylosed at a right angle with the humerus, and somewhat displaced inwards: so that a line prolonged vertically downwards from the centre of the olecranon fossa of the humerus, would pass completely external to the olecranon, the inner margin of which was exactly in the same vertical plane as the inner condyle; the head of the radius lay in the situation which it occupies in what is termed luxation backwards; it lay below and towards the posterior part of the outer condyle: the inferior surface of which, enlarged and flattened, presented a very shallow socket for the displaced head of the radius: the capitulum of the humerus had altogether disappeared: a strong capsule surrounded the new radio-humeral articulation, and from its inner surface was detached a bundle of ligamentous fibres, resembling the round ligament of the hip-joint. This new ligament was

fixed in the depression which the head of the radius presents, and which was here much deeper than natural: the specimen then was an example of oblique fracture of the lower end of the humerus, with displacement of both bones of the forearm, inwards and slightly backwards; but, Mr. Smith remarked, it was involved in some obscurity, in consequence of the ankylosis of the ulna to the humerus; for ankylosis was not an usual result of accidental luxations, that have been left unreduced: in such cases the efforts of nature are rather directed towards the formation of a new articulation: so that in the absence of any of the commemorative circumstances of this case, the question naturally suggests itself, were the lesions of the humerus and of the elbow-joint, that had been described, the results of one and the same accident? or should the abnormal state of the elbow rather be considered as the result of chronic disease of the joint, occurring totally independent of, and at a period different from that at which the fracture of the humerus took place? Mr. Smith remarked that it would now be extremely difficult to arrive at a correct conclusion upon the question, but he was inclined to maintain the opinion, that the two lesions were coeval, and the results of one and the same accident; he had upon a former occasion exhibited to the society an example of an analogous injury, viz., fracture of the neck of the humerus with luxation of the head of the bone into the axilla; in all such cases the luxation remains unreduced, and the fracture unites with great deformity, as in the preparation on the table, for it almost amounts to an impossibility to reduce a luxation when either of the bones forming the joint have been fractured close to the articulation. With respect to the ankylosis between the ulna and the humerus, it may have been the result of a wound of the synovial sac of the articulation (by the pointed extremity of the upper fragment), whereby were induced synovitis, and ulceration of cartilage; this, however, was merely a conjecture. Such an occurrence is not unfrequently met with when the synovial sac of the knee-joint has been penetrated, in cases of fracture either of the femur or of the tibia. Mr. Smith in concluding alluded to a case described at page 465 of the edition of Sir A. Cooper's work, edited by Mr. Bransby Cooper, of an injury of the elbow-joint, analogous to that which he had just described.—*Dublin Hospital Gaz.*, Feb. 1, 1846.

46. *Contracted Cicatrix from a Burn—Autoplastic operation—Considerable improvement.*—The *Provincial Med. and Surg. Journ.* for 27th May last, contains the following abstract, of a case of contracted cicatrix from a burn, in which an autoplastic operation was performed by Mr. CARDEN, of Worcester. This case was originally published in the 12th vol. of the Transactions of the Provincial Med. and Surg. Association, and seems worthy of notice, as it is supposed to be the first case in which this particular method of operating was employed.

“Mary Ann Barnett, aged 14, was admitted into the Worcester Infirmary, September 9th, 1839, with contracted cicatrix after a burn, which had occurred seven years previously. The movements of the head were greatly restricted; the mouth remained permanently open, the tongue protruded, the lower incisors projected horizontally, and there was constant salivation. On attempting to raise the head, the eyelids were drawn considerably downwards. The patient, as well as her friends, being anxious to have something done for her relief, a consultation was held upon the case, and with the consent of his colleagues at the Infirmary, Mr. Carden performed the following operation:—

“The patient being placed on a well-cushioned table, with her head and shoulders somewhat elevated, the operation was commenced by carefully gathering up the cicatrix from below the left ear to the top of the sternum, between the fingers and the thumb of the left hand, which allowed, of the whole of that side to be transfixed and divided at a stroke; the same was repeated on the right side, and a short cut over the top of the sternum connected the two incisions. In this manner the whole transverse extent of the cicatrix was rapidly divided, the wound terminating in sound skin on each side. The chin was then drawn upwards by an assistant, and every tense band of cicatrix successively divided by repeated strokes of the scalpel, until the head was released into nearly its natural position. By this mode of dissection, although nothing had been removed, the hiatus produced was very great, and extended from the chin and edge of the lower jaw to below the upper border of the sternum, exposing the greater part of both sterno-



mastoid muscles, and external jugular and thyroid veins, the latter being particularly large and prominent. The quantity of blood lost was very trifling, scarcely requiring the torsion forceps. As soon as all bleeding had ceased, Mr. Carden proceeded to select a portion of sound skin on each side, about three inches long and two and a half wide; these were raised and detached, except at their junction with the outer edges of the wound, and brought together across the centre of the neck, and there united by hare-lip needles. The side wounds left by the flaps were then brought together, and the exposed parts covered with lint. The flaps were carefully supported by adhesive plaster, leaving apertures for the points of the needles, and the whole of the wound and surrounding integuments were well supported by long plasters and bandages.

The operation, which was severe and necessarily protracted, was borne with great fortitude, and without fainting. The needles were withdrawn two days after the operation; the dressings were not removed until the sixth day, when the flaps were found to have retained their position; but the upper border of each, being composed of old cicatrix, had perished, diminishing the breadth of each to less than two inches. The complete healing of the wound occupied nearly twelve months, during which time various contrivances were had recourse to for keeping the head in the erect position; but the bodily and mental suffering was so great, each time the wound was dressed, from these repeated stretchings, that they were altogether discontinued.

She was made out-patient in May, 1840, and in November following, presented the following appearances:—Wound healed; position and movements of the head greatly improved; can close the mouth, retain the saliva, and articulate distinctly; teeth regaining their natural position. A narrow cord has sprung up between the flaps, which threatens to draw down the centre of the lower lip, and also to prevent the further expansion of the flaps, which has hitherto been steadily going on and forming the most satisfactory feature of the case.

This band was divided by a curved bistoury, and pressure and further extension enjoined; but from that time she avoided attendance at the hospital, and neglected all directions. No more was seen of her until October 23d, 1843. She had then become stout in person; the teeth were quite upright; the flaps measured three inches on the right side, and two inches and three-quarters on the left, from above downwards; but there was an increased contraction in the central cord. This she consented to have divided and separated from the flaps, which would then, it was thought, be permanently united, and her appearance considerably improved.

## OPHTHALMOLOGY.

47. *Successful case of the Employment of Iodine in removing Sparks of Iron from the Cornea.* By M. REINIGER.—A small particle of steel struck the eye of a cutler while at work, and fixed itself in the thickness of the cornea, from which it was found impossible to remove it either by the forceps or needle. It soon excited inflammation of the cornea, and after eight days the eye was still red, painful, hot, and the patient complained of a pricking sensation on its being moved. The fragment of steel could be distinctly seen, still retaining its polish. As a strong magnet also failed to remove it, it was resolved to have recourse to a chemical solvent, but one which would not act deleteriously on the eye. A weak solution of iodine and iodide of potassium was therefore used as a collyrium, and its very first application sensibly deadened the lustre of the spark of steel. By its continued use the steel was rendered soluble, and was gradually removed, when the sight was restored. The treatment was therefore most successful, and seemed to be preferable to that formerly recommended, viz. the employment of a dilute acetic acid, which is apt to impair the vision.—*Ed. Med. and Surg. Journ.*, July, 1846, from *Journal de Pharmacie*, June, 1845.

48. *Cases of Melanosis, with observations.* By HOLMES COOTE, Esq. (*Proceedings of Royal Med. Chirurg. Soc.*)—The author, after relating several cases of melanosis, gives a brief historical notice of the works in which a description of the disease

is contained. He then observes that "melanosis in the human subject occurs as a primary disease only in the eye and in the skin. In the horse it is seen chiefly about the anus, parotid gland, or spleen. Mr. Spooner, of the Royal Veterinary College, has never met with it but in horses of a light colour; most of them were becoming white from age." After some remarks upon the necessity of not confounding true melanotic tumors with other dark-coloured swellings, the author gives a brief account of the chemical composition and microscopical anatomy of true melanosis. He argues from the latter, that melanotic tumours, though sparingly if at all supplied with blood-vessels, are not unorganized; the cells which compose them having their periods of growth, maturity and decline. He denies the correctness of regarding melanosis as a variety of cancer. He enumerates many points in which the two diseases essentially differ. Melanosis of the eye commences between the choroid and the retina; the tumour, pushing the contents of the globe on one side, makes its way externally by irregular periods of increase. Melanotic tumors of the skin are either cutaneous or subcutaneous.

The development of internal disease, to which the fatal termination of the case is owing; is marked by general lassitude, and undefined pains over the trunk and limbs. The patient is exhausted by sickness and diarrhœa, but in most cases retains his mental faculties to the last.

Secondary melanotic accumulations are found in all structures except the cornea, synovial membranes, tendons, and aponeurotic expansions. The liver often attains an enormous magnitude: in one case examined by Mr. Lawrence it weighed seventeen pounds and a half. From a table of fifteen accurately-watched cases, it would appear that the average duration of life after the removal of the primary disease by operation does not amount to more than thirteen months. The author concludes by endeavouring to show that the operation, though justifiable for the purpose of relieving a patient from a source of suffering, ought not to be held out as a means of eradicating the disease, or of materially prolonging life. He rests these conclusions very much upon examinations made by himself; having found by the aid of the microscope, that in one case melanotic matter existed in the blood-vessels, in apparently healthy muscles, nerves and other tissues immediately surrounding the primary tumour.

49. *Entozoon in the chambers of the Eye.* By M. ALESSI.—A magistrate, aged thirty, applied to M. Alessi, in the month of July, 1844, to consult him for keratitis, from which he had suffered for nine months, and to which chronic vascularity of the conjunctiva was added. The inflammation, which appeared and disappeared irregularly, had produced several nebulæ of the cornea, and a constant lachrymation and weakness of vision. This disease had been attributed to various causes by different practitioners, and had been submitted to various kinds of treatment. On examining the eye, M. Alessi recognized keratitis affecting all the layers of the cornea, together with slight muddiness of the aqueous humour. By continuing the examination with the aid of a lens, he saw a worm pass from the posterior into the anterior chamber. Seen with the naked eye, this worm appeared to be about two lines and a-half long. It was of a dirty white in its two inferior thirds, and fusiform; its superior third was of a milky colour. From the last portion four processes grew, one inferiorly, the shortest, one superiorly, the longest, and two laterally. M. Alessi gives no opinion whether this animal approached sufficiently near to cysticercus to pass under that name. It had, however, two opposite motions; when the superior appendages approached the inferior separated, and *vice versa*. It remained two or three minutes in the anterior chamber, and then retreated into the posterior, where it remained behind the lower edge of the iris for some time; returning into the anterior chamber, it lay against the outer edge of the iris. The chief cause of the ophthalmia having been thus discovered, M. Alessi was afraid to attempt to destroy the entozoon, according to M. Gescheit's plan, by exciting severe inflammation in the eye. In the present case inflammatory disease already existed, and its increase would not have been without danger. With regard to the extraction of the worm by an operation which had been done by Sæmmering, M. Alessi feared he would not be able to effect it in this case, on account of the free motion present. After having reflected on the indications which this rare case presented, M. Alessi determined first to combat a syphilitic

taint which existed in the patient's family. This was done by means of anti-syphilitic treatment during forty days. M. Alessi then prescribed the application of three blisters, each two inches long by one wide. The first was placed over the eyebrow, the second on the temple near the orbit, and the third near the inferior border of the orbit. These blisters were dressed night and morning with an ointment containing equal quantities of calomel and santonine. The worm was, shortly after this treatment, deprived of all motion, and in less than forty days it was entirely absorbed. The keratitis and conjunctivitis of the eye of course soon disappeared, and the functions were completely restored.—*Med. Times*, June 27, 1846.

50. *Amaurosis from Concussion of the Retina.—Recovery.*—W. B. PAGE, Esq., reports in the *Lancet*, (June 6th, 1846,) a very interesting case of amaurosis of the right eye, in a man 30 years of age, produced suddenly by a blow with the fist, on the organ. When admitted into the Cumberland Infirmary five weeks after the injury he was just able to distinguish night from day. The pupil was dilated and insensible. It was resolved to administer mercury until the system became slightly affected by it, and to keep up its effect for some time, and also to abstract blood occasionally by cupping, in such quantities, and at such intervals, as the state of the patient, and the benefits resulting from it, seemed to warrant; carrying it to such an extent as to relieve any local congestion, without materially lessening the powers of the patient.

Accordingly, on the following day, Dec. 18th, he was ordered to be cupped on the right temple to six ounces; to take five grains of blue pill three times a-day, and to be put on milk diet. The next day he stated that he could distinguish light from darkness more plainly than he had hitherto done, although he was still unable to point out in what part of the ward the windows were situated.

23d.—The breath has a slight mercurial fœtor. He is able to see the hand if held up between the eye and the light, and to count the number of fingers. Cupping to be repeated to eight ounces, behind the mastoid process.

25th.—The gums being slightly affected, the blue pill was ordered to be given only twice a-day.

30th.—The system being now evidently under the influence of mercury to as great an extent as appeared desirable, the blue pill was ordered to be given at night only. The sight has continued uninterruptedly and rapidly to improve, inasmuch that he is able to distinguish one object from another, as a pencil-case from a pocket-knife. Ordered to be cupped to six ounces.

Jan. 4th.—Is able to read some large printed letters on a board in the ward, when held close to the eyes.

14th.—Can now make out the ordinary type of an octavo volume, although with some difficulty. Little obvious improvement now took place until the

26th.—When he was ordered cupping to four ounces.

Feb. 3d.—The cupping having been apparently attended with much benefit, it was repeated.

12th.—Was again cupped. He was then able to read with comparative ease.

18th.—Discharged, being able to see perfectly.

A month afterwards this patient came to the hospital as desired; from the time he left he has been employed in ploughing. He can read the smallest type; in short, the sight of the eye is fully restored.

#### MIDWIFERY.

51. *On Incision of the Neck of the Uterus in cases where, from the contraction of the organ, delivery is difficult.* By Dr. LABORIE.—The author commences his paper by stating, that whilst in many cases the progress of labour appears to proceed naturally, the membranes having ruptured, and the pains succeeding each other regularly, yet the os uteri does not dilate, and labour is indefinitely retarded. The woman being otherwise well formed, it is evident the obstacle to delivery is to be sought for in the cervix, which being rigid, does not yield to the contractions of the body of the uterus itself. In general, simple means, such as bleeding, baths,

emollient and narcotic injections, are found sufficient to overcome this state of rigidity. But in a certain number of cases these means are insufficient, the labour is lingering, and the life both of mother and child is in danger. It is in such cases that incision of the neck must be had recourse to, in order to induce dilatation, and this has now been so often performed, as to rank as a legitimate operation.

Three cases occurring in the practice of M. Dubois are detailed by M. Laborie; to one of these we invite the attention of our readers, as the operation was followed by a result not usually met with.

A young well-formed girl was admitted into the Clinical Hospital in the month of June, 1844. She had suffered from hemorrhage at various times during the course of her pregnancy. On the 6th the pains of labour commenced, and continued during the whole day. In the evening the membranes burst, the neck was thick and rigid, and the os uteri dilated to the extent of two centimetres.

On the morning of the 7th, a face-presentation was recognized. The cervix was still rigid, but dilatation had advanced to the extent of three centimetres. Although the woman was much weakened, M. Dubois still deemed it prudent to allow things to take their course without interference. At half-past one, labour having made no advance, and the patient being much exhausted, incision of the cervix was then determined on. One incision made in the right side produced slight relaxation, but was followed by no other consequence worthy of remark. A second was immediately practised on the left side, but scarcely had it been done, when there ensued a copious hemorrhage of bright red blood. M. Dubois had immediate recourse to the forceps, but could not apply them owing to the rigid state of the cervix; he was equally unsuccessful with the lever. Injections of cold water were then had recourse to, but without effect in stopping the hemorrhage. The woman was now so exhausted that it was necessary to plug the vagina. The bleeding was thus stopped, but the patient continued in a state of extreme distress, and delirium supervened. Nevertheless the plug was removed in the course of half an hour; dilatation had now become sufficient, and labour was terminated by the forceps. All the unfavourable symptoms ceased, and the woman soon recovered.

M. L. remarks that cases of this kind are of rare occurrence, so much so, that M. Dubois had never met with one similar. He conceives that it must have been owing to the placenta being inserted near the mouth of the uterus.

The following rules laid down by M. L. as to the mode of operating are so important, that we give them *verbatim*:—"We have seen," he says, "M. Dubois have recourse sometimes to the knife, sometimes to the scissors, in order to relieve the strictures at the mouth of the uterus. M. Danyau, on the other hand, invariably uses the scissors. Should a preference be given to one or other of these instruments, or may they be used indifferently?"

"No decided answer can be given to the question, for either of them may be had recourse to according to circumstances, and according to the spot at which it is necessary to relieve the stricture. As a general rule, however, we should not hesitate to employ the bistoury, provided the cervix be not displaced; and we should choose that form of bistoury employed in removing the amygdalæ. When it is wished to practise an incision, either to the left, or anteriorly, or posteriorly, the index finger of the left hand must be introduced into the vagina, and its palmar surface applied to the spot where it is desired the incision should be made. The bistoury must then be introduced by means of the right hand, its flat surface sliding along the surface of the index of the left, until its point, passing within the cervix, comes to be in contact with the end of the finger. By means of a semicircular motion, the cutting edge of the instrument must then be directed perpendicularly towards the free edge of the orifice; should the latter be tense, the incision will be most readily executed, by giving to the instrument a sawing motion. We recommend all surgeons to limit the incision to the extent of a centimetre, conceiving it preferable to multiply them, should the desired result not be produced. If the incision be made to the right, the right hand must be used as the guide to the instrument.

"When the cervix lies far back, it is impossible to use the bistoury for the purpose of relieving its posterior lips; in such a case, recourse must be had to

the scissors. The mode of operation is nearly the same; but in order to insure that the incision does not go beyond the extent of a centimetre, a most careful examination must be made by means of the finger. The scissors are also applicable in cases of retroversion, when the anterior hip has to be relieved, as well as for lateral incisions, in those cases in which the cervix lies very much to the right or left. It will be readily conceived, that in all such displacements, the most elevated edge of the orifice would be reached with difficulty by the bistoury, as its point would be in danger of coming in contact with the walls of the uterus, and wounding them. If the use of the scissors be entirely prohibited, a bistoury with a very convex cutting edge, must be made use of.

"The operator can, in general, immediately ascertain the effects of his incision; the cervix, from being hard and resisting, becomes more pliable. The influence of the operation should be particularly watched during a pain; it will then be found, that the head of the child, pressing strongly on the orifice, advances more perceptibly than before. The dilatation, in a quarter of an hour after the operation, is generally sufficient to permit of the application of the forceps, should it be thought desirable, to terminate the labour in that way. But should this not be the case, new incisions must be had recourse to. M. Dubois affirms, that he has never failed in accomplishing his end by following these precepts.

"There is one counter-indication to the operation, which we wish particularly to specify, and that is, thickening of the cervix. When the latter has not become thin, the operation, instead of being of advantage, may be the very reverse. In this latter case, hemorrhage is most to be feared; but the incision over such an extended surface may also be the primary cause of laceration so extensive as to prove fatal.

"Attachment of the placenta, near the cervix, may, from what we have previously stated, be also held as a circumstance sufficiently serious to forbid the operation."—*Monthly Journ. Med. Sci.*, June, 1846, from *Encyclographie Méd.*, April, 1846.

52. *Results of Auscultation at a Triple Birth.*—Prof. NÆGELE, Jr., has published the following interesting account of a case of triplets in the *Medicinische Annalen*. The woman was a primipara, aged thirty-two. At the moment when her labour commenced auscultation was used, and the double tick of a fœtal heart, with the simple *bruit de souffle* of a cord, was heard in the left hypogastric region. The same phenomena were present in the right hypogastric region, together with other double sounds. Two auscultators applied the stethoscope, one on the right, the other on the left side of the woman; they noticed that the fœtal pulsations were more frequent on the right side than on the left, in the proportion of thirty-eight to thirty-six, in the quarter of a minute. Soon after, when the right fœtus moved, forty pulsations in the quarter of a minute were counted for its heart, thirty-six for that of the fœtus on the left side, making thus a difference of sixteen pulsations in the minute between the two infants. After the birth of the first child with the aid of the forceps, the mother was examined again with the stethoscope, and the pulsations of two fœtal hearts were still heard distinctly, of which one was situated in the left hypochondrium, and the other in the right umbilical region. The head of the second child presented in the second position, and the pulsation heard to the right belonged to this child, which was also delivered by the forceps. After the birth of this child, the pulsation of a third fœtal heart was still heard distinctly, and this child was born by the unaided efforts of nature. The entire duration of the labour was fifty-seven hours. The first diagnosis given was, that there was more than one child in the uterus, and was founded on the distinct hearing the pulsation of two fœtal hearts. It was only by auscultating after the birth of the first child, when double pulsation was again heard clearly and distinctly, that it became possible to ascertain the existence of three infants. The pulsation of the third fœtal heart was not heard at the commencement of labour, because the infant was most likely situated behind the others towards the spine of the female. The uterine *bruit de souffle* was neither louder nor more extensive than usual, and could not cause the presence of the placenta to be suspected.—*Med. Times*, Aug. 15, 1846.

53. *Fractured Sacrum impeding Labour—Delivery by Craniotomy in two successive labours.* By J. R. GIBSON, Esq. (*Lancet*, June 13).—The subject of this case was thirty-six years of age, the mother of seven living children, whose previous labours had always been natural. She had been run over by a baggage wagon and sustained severe injury of the pelvis. Soon after her recovery she became pregnant and advanced to her full period. When seen by Mr. G., July 23d, she had been two days in labour. She had been bled and otherwise treated, but no advance had taken place in the progress of the labour, although the pains had been vigorous. Upon examination per vaginam Mr. G. found the liquor amnii had escaped: the head presented at the upper aperture of the pelvis, but no portion had entered the pelvis. The scalp was tumid; the os uteri dilated to nearly the full extent of the superior antero-posterior diameter of the pelvis, which could not be more than two inches and a half, as near as Mr. G. can judge.

The sacrum appeared to be that part of the pelvis which had sustained the chief injury, inasmuch as its promontory projected forward, and so much contracted its antero-posterior diameter. Upon examining externally, there was a deep depression, in which almost the two hands might be buried, corresponding to the sacrum. In this case there could be no doubt that one of two operations should be immediately performed—craniotomy, or the Cæsarian section; and as the latter operation must entail greater suffering, and far greater risk to the safety of the mother than the former, Mr. G. proceeded to deliver by perforating the head. This he did, not only “cautiously, but with great care,” well breaking up the brain, particularly at its base. But in order to accomplish the extraction of the fetal head, it was not only necessary to destroy the brain and evacuate it, but to remove the parietal and the greater portion of the occipital bones. The delivery was completed in about an hour and a half. Two grains of opium were given, and the case proceeded, with some slight uterine inflammation, which was readily overcome, to a favourable issue.

About five months after her recovery, she again became pregnant, but did not apply to Mr. G., or inform me of its occurrence, until within the ninth month of utero-gestation. At this late period, Mr. G. did not deem it advisable to induce labour, but to await the commencement of natural labour, which took place on Oct. 1, and proceeded slowly during the whole of that day. The os uteri being somewhat dilated and readily dilatable, without any advance, as in the preceding labour, Mr. G. considered it advisable to accomplish the delivery without further delay. The uterine efforts had been vigorous, although not very frequent, and the head seemed fully developed. The delivery was accomplished in the same manner as on the former occasion, and at this time she recovered without one unfavourable symptom.

54. *Influence of Galvanism upon the action of the Uterus during Labour.* By J. Y. SIMPSON, M. D. (*Monthly Journal Med. Sci.*, July, 1846).—The employment of galvanism as a means of exciting or increasing uterine contractions seems to have been first suggested by Herder in 1803. It has since been proposed and employed by others with this view, but without any very striking results. The attention of the profession has recently been strongly drawn to this measure by the publication, in the *Provincial Med. and Surg. Journal* (Dec. 24, 1844), of a paper “on galvanism applied to the treatment of uterine hemorrhage” by Dr. Radford, in which this writer states that galvanism produces an effective and powerful contraction of the uterus. With a view of ascertaining the exact degree of influence which galvanism possesses over the contractile action of the uterus during labour, Prof. Simpson instituted a series of experiments, the details of which are given in this interesting paper, and which lead to results the very reverse of those which he expected.

Galvanism was employed in eight cases, and the results may be summarily stated as follows. Prof. S. says:

“In one instance the pains were more frequent in their recurrence, but shorter in their duration during the application of the galvanism. In five other cases, the employment of the galvanism neither increased the average frequency of the pains, nor their average duration. In one, the pains ceased whilst the galvanism was applied, and returned upon its removal. In the instance which I have last

detailed, the uterine action ceased while the galvanism was applied, and did not return upon the withdrawal of the galvanic action, nor for twenty-four hours subsequently. There was no reason whatever at the time to expect this as a probable occurrence, independently of the galvanism. But even admitting, for the sake of argument, that the cessation of the uterine action was not the result of the galvanic influence used, still the fact is amply sufficient to show that the galvanic current had not, at least, the power either of increasing the pains, or even of continuing and maintaining them when they offered to fail. It may be proper to add, that during the galvanic action, in none of the experiments did Dr. Barry or I find, in the intervals between the *clonic* uterine contractions or pains, any evidence whatever of unusual *tonic* contraction of the uterus, as shown either by any degree of hardness in the general uterine tumour, or by any degree of tension in the pressure of the bag of membranes or the child's head against the cervix uteri.

"It would be hasty and logically incorrect to deduce from the preceding observations, that under no modification, and under no manner of application, does galvanism possess the power of directly exciting or increasing the contractile action of the uterus. Forms or methods of employing it may yet possibly be detected or devised, affording a different result. But I believe I am justified in inferring from the preceding inquiry, that as employed at the present time, and in its present mode, it is not a means which can be in any degree relied upon for the purpose in question; and is so far practically and entirely useless as a stimulant to the parturient action of the uterus."

54. *On Thrombus of the Vulva.* By Prof. VELPEAU.—Thrombus of the vulva, although mentioned by Kronauer (1734), by Levret, and since by Boer and Audibert, has been omitted in nearly all modern works on pathology and midwifery. M. Deneux is the author who has given the best description of it.

Thrombus of the vulva may invade both sides of the vulva, but, generally speaking, it only occupies one of the labia majora, or a part of the interior of the vagina. It may be of the size of a walnut, of an egg, of the fist, or even of a new-born child. Of a dark, black colour, it presents a globular, spheroidal, or elliptical form, protruding principally on the free surface of the labia or vagina.

These sanguineous tumours of the vulva are generally indolent and soft, and offer slight fluctuation at the most prominent part of their surface. The pubic or femoral side is, on the contrary, rather dense and resistant, fluctuation being obscure. On pressing the tumour between the finger, as if to strangulate it, or to draw it towards the exterior, it becomes at once evident that the mass is not connected with the pelvis, that it is entirely formed in the soft parts. When a tumour presenting the above characters has formed rapidly,—in the space of a few hours for instance,—the diagnosis is very easy. An abscess, a cyst, or any other tumour, would develop itself in a very different manner, and present other anatomical characters.

The frequency of thrombus of the vulva is very great. I have myself seen more than thirty cases. M. Deneux has collected about sixty in his essay. If the disease has been considered rare hitherto, it can only be because it has often been confounded with diseases of another kind. Thus, Formi describes a case of this malady as gangrene of the vulva. He says that a tumour appeared in the right labium of a lady who had been wounded, and having been neglected for three or four days, became gangrenous. He made a deep incision into the tumour, and withdrew a pound and a half of blood, after which all healed.

The essay of M. Deneux would make it appear that thrombus of the labia is a disease confined to lying-in women, or to women recently confined. Such, however, is not the case; women who are not married, and women who are not in the puerperal state, are often the subjects of these tumours. I have seen as many as six cases in the same year, in women who were not, and had never been pregnant. I have seen a thrombus of the vulva in a young girl of fourteen, who stated that she had been violated. I have also met with the disease in women of eighteen, twenty, twenty-five, thirty, thirty-five, forty, fifty, and even sixty years of age. De la Motte and other authors, during the last two centuries, have like-

wise published cases in which thrombus of the vulva has occurred apart from the puerperal state.

Parturition is only one of the causes of thrombus of the vulva, which is merely the result of the laceration of some vessel or vessels, with or without a wound of the integuments. A collection of blood thus created, may take place at the vulva, as elsewhere, under the influence of all kinds of shocks, pressure, or mechanical violence. Thus it is that non-pregnant women may become affected with thrombus of the vulva from a fall on the angle of a chair, or of any article of furniture, from a kick, or from venereal excesses. This latter, indeed, is one of the most frequent causes of the lesion. The mechanism, in such cases, would be the same as in women in the puerperal state, in whom the tumour is generally occasioned by the passage of the head of the fœtus through the inferior brim of the pelvis. We must not, however, forget, that in pregnant women the pressure exercised by the uterus, or by some parts of the child, on the organs contained in the pelvis, or in the abdomen, must, by interfering with the return of the blood towards the centres, favour the dilatation of the vessels of the vulva, so as to render their laceration and the subsequent extravasation of blood more easy. In parturient females, the tumour acquires a much larger volume than in those who are not pregnant. Sometimes, also, a thrombus will appear, as it were spontaneously, in women in the puerperal state.

Once established, thrombus of the vulva may be assimilated in its symptoms and changes to sanguineous tumours in other parts of the body. It may undergo various transformations; either the effused blood disappears insensibly, and the disease terminates by resolution, or the fluid part alone of the collection is absorbed, and the coagulum which remains forms, in the course of time, a concrete tumour, the precise nature of which it is sometimes difficult to determine. In some cases it is the concrete part that disappears, whilst the fluid part remains, and increases in quantity. Thus is formed a cyst, filled with red pultaceous matter; with a greasy, semi-transparent substance, or with serum. Sometimes the tumour becomes inflamed, and is transformed into a true sanguineous abscess.

It is more especially in women who are not in the puerperal state that these different phases of the disease are seen, as the medical attendant is seldom consulted at the onset, from the dislike which women, so situated, feel to mention the nature of the disease under which they are labouring. At the epoch of parturition it is different; the tumour originates, as it were, under the eyes of the accoucheur, and when recognized, may be treated at once. Its existence is also more likely to be recognized in parturient women, as with them it is much more painful than with women who are not in that condition. In the course of a few days, the pain which the tumour may occasion, even when not inflamed, passes away, and it becomes indolent, and like any other form of hæmatocele. When the thrombus exists in the superior half of the labia, or near the pubis, it occasions less pain, and is less disposed to inflame, than when occupying the perineal half of the vulva.

When a thrombus situated near the perinæum inflames and suppurates, the pus emitted, when it is opened, has a very fetid odour, which might lead to the supposition that the abscess communicated with the intestine, were it not borne in mind that all purulent collections contract easily the odour of the contents of the mucous canals in the vicinity of which they lie.

The prognostic of sanguineous tumour of the vulva is not very serious, although it may be rather more so than that of sanguineous tumours in general. The danger of this form of thrombus has been exaggerated; if death has followed its manifestation in some instances, it has been owing to excessive hemorrhage. Thus, Stendal gives a case in which seven pounds of blood escaped from the tumour. When the thrombus has had time to transform itself into a concrete tumour, or a pultaceous or serous cyst, the disease belongs to another class of tumours, and no longer forms a part of sanguineous tumours, properly so called.

The treatment of thrombus of the vulva varies according as the tumour is small or large, with or without solution of continuity of the skin, with or without thinning of the teguments, with or without inflammation. I need scarcely say that simple sanguineous infiltration of the labia does not demand any particular curative measure, and that it is dissipated by the same precautions, and by the same



mechanism, as in ecchymoses, infiltration of blood of greater or less extent and depth, in other regions of the body. Unless there be division of the integuments, resolute agents must be first resorted to, such as linen dipped in a solution of acetate of lead. If by the end of fifteen or twenty days the tumour has not become absorbed spontaneously, or under the influence of these means, other measures will probably be necessary. When this is the case, the absorption of the effused fluid is probably prevented by a kind of cyst by which it is surrounded, which cyst must be opened. This may be done in two ways,—by introducing a kind of lance into the tumour, cutting or tearing the parietes of the cyst with it, and then forcing, by pressure, the blood to infiltrate into the surrounding tissues, or by simply crushing the tumour. Crushing a thrombus of the vulva, with a view to disseminate its contents in the adjoining parts, becomes impossible, if the skin is broken or painful. When it can be effected, it is preferable to puncture, as the small wound thus occasioned may inflame, and be followed by suppuration of the thrombus.

In women in the puerperal state, the thrombus is generally too voluminous, and, more especially, too painful, to admit of crushing, which is also often inapplicable under other circumstances. In such cases, and when the thrombus resists all resolute treatment, the definitive remedy is incision of the tumour. The incision should be large, so as to allow of the thrombus being fairly emptied of the clots which it contains. The minute arteries which may be divided are too small, or too easy to tie or twist, to modify the practice of the surgeon. The cyst having been freed from clots, should be washed, and then filled with small rolls of lint.

The region of the cyst in which the incision is performed is of importance. When the thrombus is very thin and prominent in one part of its surface, and appears likely to burst, the incision must be made on the most prominent spot, otherwise it should be made as near the cutaneous edge of the labium as possible.

When thrombus of the vulva occurs in pregnant women, it constitutes an obstacle to the exit of the child, and there is scarcely any way of avoiding an incision.—*Journal de Chirurgie*.

55. *Vomiting of Pregnant Women*.—Dr. STACKLER has communicated to the Medical Society of the Bas Rhin two cases of obstinate vomiting in pregnant women, in which the symptoms yielded to the black oxide of mercury, given in the dose of five centigrammes (three-quarters of a grain), daily. There was not the least trace of salivation nor any other inconvenience after the use of this medicine. Dr. Jauger referred to cases of hysterical convulsions and vomitings sympathetic with the condition of the uterus, which had been cured by the black oxide of mercury. According to this physician the medicine is equally appropriate in irritated states of the organ, whether in pregnancy or otherwise. Should further experience confirm this property of the black oxide of mercury, its importance will be readily comprehended by those who recollect how extremely severe are the obstinate vomitings with which females are occasionally attacked during gestation. Professor Forget took occasion of the communication of Dr. Stackler to quote the case of a woman who had been reduced to the last degree of emaciation by these nervous vomitings, and at length died during the sixth month of pregnancy.—*Prov. Med. and Surg. Journ.*, July 1, 1846, from *Gaz. Méd. de Strasbourg*.

56. *Pregnancy and Delivery during complete Prolapsus of the Uterus*.—In the "*Bull. delle Sc. Med. di Bologna*," is recorded the case of a female, who first suffered from prolapsus of the uterus in her 15th year; in her 22d year she became pregnant, and her pregnancy proceeded to the seventh month, without any particular inconvenience. In the seventh month the uterus began to sink, and in the commencement of the eighth month it projected as much as six fingers' breadth beyond the vulva. The author was not called until the patient had been four days in labour. When he arrived he found the woman much exhausted, and the organ, of a dark-brown colour, hanging as low down as the middle third of the thigh. Labour proceeded in the usual way; the child was extracted with forceps; and

on the twentieth day the woman was able to leave her bed.—ANCELL'S Report in *Ranking's Abstract*, vol. iii.

57. *Imperfect condition of the Os Uteri, with Malformation of the Vagina—Treatment during Labour.*—Mr. S. H. DAVIS records, in the *London Med. Gaz.*, March 1846, a case, not of occlusion, but of an imperfect condition of the os uteri, with malformation of the vagina, producing difficult labour, for which it was deemed necessary to make an incision. On placing the patient on her back, separating the thighs, and opening the vagina with the fingers, the upper part was not more than two inches from the vulva, and a small aperture large enough to admit a crow's quill, having the appearance of a circular hole made with a punch, was seen, and through it the hairy scalp of the fœtus.

A director, bent at an obtuse angle, was introduced into the uterus, and passed directly backwards, in which direction an incision nearly an inch in length was made with a probe-pointed bistoury, a catheter having been introduced into the urethra; the cutting edge of the bistoury was turned forwards, and an incision made to the extent of a quarter of an inch. The structure divided was of a firm texture, a quarter of an inch in thickness.

About three hours after the operation the pains continued strong; the os uteri was dilated to the size of a crown-piece, the margin thin, and of a natural feel; but a longitudinal band, extending along the posterior wall, was still firm and resisting. Five hours and a half subsequently the pains continued very powerful, but symptoms of threatened exhaustion came on. The head was low down in the pelvis, the swollen scalp, in the form of a cone, pressing on the os uteri, which was much more dilated; but six distinct, firm, and unyielding bands were observed taking a short course from the vagina to the os uteri, as if formed by puckering of the vagina; one on each side of the urethra, two laterally and two posteriorly, much stronger than the others; the bands became very tense during a pain.

Each of these bands was divided, and a little additional room obtained, but still the head did not make progress during the very powerful pains, being obstructed partly by the approximation of the rami of the pubes, but chiefly by the malformation of the soft parts.

Dreading rupture of the uterus, and the patient being more exhausted, it was decided to open the cranium of the fœtus, and endeavour to deliver at once, which was easily effected without laceration or hemorrhage, the uterus contracting firmly, and the patient recovered without a bad symptom.

On examination after recovery, an inch within the vagina a distinct and firmly resisting ring was observed, and within this the os uteri flattened, not protruding into the vagina, and of an irregular form; from the left margin of the os uteri, extending between it and the ring, was a firm, thickened band, resembling somewhat the contracted cicatrix of a burn, and a second band took its course along the posterior wall of the vagina.—*Ibid.*

## MEDICAL JURISPRUDENCE AND TOXICOLOGY.

58. *Diseases of Workmen engaged in making Lucifer Matches.*—This subject has attracted a good deal of attention in Germany and France, and as we have in this country similar manufactories, it may be of some use to state what has been noticed concerning it.

An interesting communication was published in the *Medical Gazette of Strasburg*, in 1845, by Dr. STROHL. He states, that about a year previous, a female was received by him into the hospital, whose inferior maxillary bone was completely denuded of the soft parts, and its surface was grayish, rough, rugous, and covered with a fetid suppuration. She informed him that she had been for many years engaged in making chemical matches, and had thus contracted the disease. After remaining about a month, she left the Hospital, and died soon after.

Dr. Strohl could scarcely, from this solitary case, imagine any connection be-

tween the disease and its assigned cause, but in June, 1845, he was consulted by another female aged twenty-four years, in a similar condition. She had laboured during five years in the manufactory; her particular occupation being to dip the matches into the phosphoric mass. During all this period, she had experienced no inconvenience. She then left the establishment, to assume another occupation, which, however, it is added, was not laborious. In three months after leaving, she experienced violent pain in the right upper jaw, which was soon succeeded by swelling and abscess of the gums. Eight teeth in succession became rapidly carious, and were removed. This had some effect on the swelling, but it soon increased again, extending to the cheek and part of the neck. According to the patient's account, the suppuration at its commencement had a strong odour of phosphorus.

When first seen by Dr. S. she had been eighteen months away from the manufactory. Six weeks previous to this time, she had been brought to bed, but did not suckle the child. Pregnancy on the whole appeared to have proved favorable to the disease, but on new examination, there was found to be a necrosis of the superior maxillary. Various remedies were employed. Cod-liver oil was one of them, but she could not continue its use. Iodine internally and a solution of creasote for an injection, to the fistulous opening, with chloruret of lime, gargles, proved successful in arresting the progress of the disease, and restoring the parts to a healthy condition.

Two additional cases presented themselves to Dr. Strohl, in July of the same year. One a female aged twenty-two, had been two years employed in the same manufactory as the preceding patient. Her health had continued good until the last two months. Then the gums began to soften and swell—violent pains in the teeth of the upper jaw followed, with swelling extending over the whole cheek. The teeth becoming carious, fell out, and a fetid suppuration proceeded from the sockets. On examination, the bone was found largely diseased.

In the other instance, a female also, aged twenty-two, had been employed six years. Her particular business was to make up parcels, but this was done in the same room where the matches were dipped. At first she experienced a defluxion from the nose, which soon became permanent. At the end of three years, her face began to swell and she then left the business. The swelling gradually increased, and she entered the hospital. Several teeth were extracted, with but little relief. She appears to have repeatedly left and returned to the hospital, with the swelling continuing, until at last an abscess formed in the superior maxillary bone, which left it carious.

In addition to these, Dr. S. also knew of three others. Death ensued in two instances, and the third was cured.

The information that he obtained was not very satisfactory. There were two manufactories of these matches; six of the diseased came from one and but a single person from the other. In the first, the work was for a long time done in a small chamber, but the persons employed are now transferred to a more spacious apartment. Few remain interruptedly in the business; they come and go for a longer or shorter interval. As the phosphoric emanations are very irritating, a great number are taken with cough, which sometimes is so severe as to oblige them to leave off work. Pain in the teeth and serous catarrhs are also common. Indeed, the above seven cases all commenced in this way, and it will be observed that in one instance the affection of the bone did not occur until a considerable time after leaving the establishment. The nature of this, it is difficult to characterize; it is not a simple caries, nor is it a simple necrosis; the soft parts detach themselves from the bone to a large extent and leave below a bony, gray, rough, but solid surface, and a gray, fetid suppuration exhales. After an uncertain period, this separates, without any appearance of new bone being formed.

Dr. Strohl does not doubt the affinity between the employment and the disease produced. How else are we to explain the great similarity in the symptoms? If it be urged, that out of fifty workmen, only six have been attacked, he replies that all of these last have been long engaged in this occupation, while most of the others were so only temporarily. It is possible that the disease may still break out in these. At least one of the patients informed him, that three others

were now labouring under permanent fluxions, and she doubted not, but that they would be similarly attacked.

Dr. S. finds some difficulty in explaining the mode of action of these phosphoric vapours. The affection is purely local; no general symptom precedes it; no other part of the osseous system, except the mouth is attacked, and the general health is not affected, unless the suppuration be very profuse. In his opinion, the vapours of the phosphorus become changed, by contact with oxygen, into hypophosphoric acid, and this breathed in, is dissolved by the saliva, and becomes phosphoric acid. Again, this last acid is frequently produced by the combustion of the phosphorus, since it often happens that the vessels in which the masses of phosphorus are melted take fire, or again, the matches when prepared, will also frequently inflame.

This acid, then, when dissolved in the saliva, renders it corrosive, and as the saliva rests longest against the gums, these will be softened; they will imbibe the liquid acid, which in turn will attack the teeth, and, filtering down, at last reach their sockets. This explains also why the tongue is not affected, although the gums and cheeks are. The tissue of the tongue is probably more refractory; at all events, the saliva on that organ is more rapidly swallowed or rejected, than that at its sides or retained in the various cavities. Hence also the nasal cavities and the bronchial membranes are not affected. In the first, the acid meets only a small quantity of fluid, of a viscous nature with small dissolving power; in the bronchiæ, it is the same, and as the passage of vitiated air must be here very rapid, the solution will be small in quantity. Still the habitual cough of the workmen proves the irritant quality of these vapours.

At the meeting of the Royal Academy of Sciences of Paris, held February 16, 1846, Mr. T. ROUSSEL presented a memoir on the same subject.

Many physicians, he remarks, have already noticed diseases produced in this manufacture. Gendrin has spoken of the severity of the bronchitic attacks among the workmen, and several German physicians are of opinion, that necrosis of the jaws originates from the same cause.

After a careful examination of the workmen in the French manufactories, he found that cough and bronchitis existed with more or less intensity amongst them, and also that diseases of the gums and maxillary bones, terminating in necrosis and sometimes in death, were not uncommon. So far as he could ascertain as to the last complaints, they were not ascribable to syphilis or scrofula, but only appeared after a sojourn in the manufactory for at least two years, and in individuals habitually exposed to the fumes of phosphorus. All the workmen affected, whom he had observed, had decayed teeth before the disease commenced, not unfrequently before commencing the manufacture. Many other workmen with sound teeth have preserved them in the midst of phosphoric vapours, a fact which induces him to believe that carious teeth constitute a predisposing cause of the disease.

As to the idea of some German physicians that the necrosis is owing to the vapours of arsenic, which in some manufactories is mixed with those of phosphorus, he shows that the effects of arsenic have nothing in common with these here observed, and again that necrosis of the jaw-bones happens where arsenic has never been employed. He thus attributes the principal action to the vapours of phosphoric acid.

The sanitary measures proposed by M. Roussel, are 1. To carry on every operation in a separate locality, and by this means greatly diminish the number of those now exposed to these fumes, and 2. To establish a perfect system of ventilation in the workshops of these who must necessarily continue exposed.

At the meeting of the same body, on the 9th of March, M. SEDDILLOR of Strasburg, presented observations on necrosis of the bones of the face and pulmonary diseases in workmen of this description, which had occurred under his observation. His cases are very similar to those of Dr. Strohl. Three females were admitted in succession, each having laboured about four years in the manufactory. All had necrosis of the severe and large extent as in the worst instances already related, and the third of these was in addition suffering under phthisis, of which she died. The affected parts, in those who recovered, remained solid, and covered apparently with a mucous membrane of a red colour.

But on the other hand, BRICHTEAU (*Journal de Médecine de M. Troussau*, March 1846), is disposed to doubt the accuracy of the above opinions. He remarks, that Heyfelder, Professor of Clinical Surgery at Erlangen, has published a number of cases of necrosis of the maxillary bones in workmen of this class, and that they are ascribed to the phosphoric fumes, and further, that in the manufactory at Nuremberg, nine cases have occurred, and the same number in the workshops of Vienna, according to Lorinser.

These facts appeared so extraordinary and alarming, that Chevalier and himself were induced to examine the manufactories of Paris, whether similar affections existed there. The result was, that among about two thousand workmen, male and female, no case of caries or necrosis is stated by the proprietors to have been noticed.

They collected however, the following: 1. A workman had been attacked with a disease of the jaw-bone, but no further information could be obtained concerning him. 2. A superintendent laboured under caries of the jaw-bone, but his attending physician considered it to be a syphilitic affection. 3. A female had been attacked with the same and died in one of the hospitals of Paris. 4. A female had an ulcer of the cheek, but her attending physician certified that this existed, before she entered the manufactory.

The workmen state that the vapours arising from the combustion of these matches, consist of sulphurous, phosphorous and phosphoric acids, that these produce cough among them, and this is always more severe in winter than in summer, as in the latter season, the windows being open produce a free ventilation, which it is impossible to obtain in the cold season.

Dr. BELFOUR, in the *Northern Journal of Medicine*, has added some interesting facts in relation to these manufactories. The dipping of pieces of wood in the phosphoric mixture and the drying of the matches, are carried on in ill ventilated rooms, from twelve to thirteen hours daily. In each establishment, from three to four pounds of phosphorus are daily employed in the production of from one to two millions of matches, the mere drying of which must give no inconsiderable quantity of phosphoric fumes, to which also must be added the quantity of metaphosphoric acid produced by the burning of sundry parcels, which, in spite of care, is not uncommon. It would seem that continued exposure to the fumes for a length of years, is requisite to produce the disease, as no cases were observed at Vienna until the manufactories had been at work upwards of eleven years. Scrofulous subjects suffer most and in them the disease is most fatal. Almost all the girls employed have the gums more or less affected, and at their junction with the teeth, a red ulcerated line, like that produced by mercurial salivation, is apparent. When the individual is robust and the necrosis is confined to a small portion of the bone, exfoliation takes place and a gradual cure follows, but where there exists any tendency to scrofula, phthisis becomes developed, and the patient sinks under the combination.

It is mentioned in *Chambers' Journal* for July, 1846; from which this last notice is taken, that the Austrian government, in order to counteract as much as possible this distressing malady, has ordered the observance of the following precautions: 1. That the matches must not be permitted to be dried in the workroom, and if possible this must be done in one situated above it. 2. That every second hour, the girls be obliged to wash their mouths with acidulated water, and 3, that they be sent out twice a day to take their meals and get some fresh air. These precautions are ordained on the recommendation of a medical commission.

T. R. B.

59. *New test of Prussic Acid*, proposed by Mr. RICHARD AUSTIN, jun., of Dublin. The precipitate of the cyanide of silver, say half a grain, obtained in the usual manner, is mixed with a small quantity of oxide of iron and carbonate of potash, and the whole fused together in an iron or platina capsule. The fused mass is then dissolved in half an ounce of distilled water, filtered and rendered slightly acid by the addition of a few drops of hydrochloric acid. The liquid thus treated is next divided into two portions, to one of which a few drops of a solution of sulphate of copper is added, which immediately causes the evolution of the chocolate-brown colour, so characteristic of the ferro-cyanide of copper; and to the other,

a few drops of the muriated tincture of iron, or any persalt of iron, when the solution becomes intensely blue by the formation of the ferro-cyanide of iron, the ordinary Prussian Blue.

In Mr. Austin's opinion, "these two tests, with the well known odour of prussic acid, are, independent of all others, sufficient to convince the medical jurist of the presence of free prussic acid."

The precipitates above mentioned, are very distinctly obtained with half a grain of the cyanide of silver.—*Lancet*, from *Dublin Hospital Gazette*. T. R. B.

60. *On the Natural Period of Delivery.*—By Dr. LE RAY.—1. The natural period of delivery, as premature delivery has a certain connection with the menstrual periods. 2. The return of these periods during the whole duration of pregnancy agrees with the period of the month corresponding to the date of the day on which the catamenia commenced to appear for the last time, whatever may be the number of days reckoned to each month. 3. The premonitory symptoms of delivery at the natural term, or premature, commence, in the majority of females, at the date mentioned, or during the seven days immediately succeeding. 4. Nevertheless, the commencement of the expulsive pains may still occur in the normal manner, but much less frequently at the fifteenth day of the tenth month. 5. Every delivery, whether at the natural term or premature, which occurs before the date mentioned, may be considered as accelerated. 6. Every delivery which occurs after the period mentioned may be considered as protracted. 7. The accelerations are proportionably much less numerous than the protractions; most commonly they do not precede the term specified by more than five days. 8. The protractions, on the contrary, are not limited by any period. 9. In either case, the causes of acceleration and protraction are very appreciable, although cases do occur where no cause can be assigned.—*Monthly Journal of Medical Science*, July, 1846. T. R. B.

61. *What constitutes the intent to commit murder?*—The following case is given in Carrington and Kirwan's *English Nisi Prius Reports*, vol. i. p. 176.

The indictment was for administering to Mrs. Day, a certain poison, called oxalic acid, with intent to murder.

It appeared that on the night of Saturday, the 1st of April, 1843, Mrs. Day had accused the prisoner, who was her servant, of stealing a table cloth, and that, it being Mrs. Day's custom to take her breakfast in bed, the prisoner, on the morning of Sunday, the 2d of April, brought to her, into her bed-room, the tea-pot and cup and saucer, and there left them and went down stairs, and that on Mrs. Day's having helped herself to some of the tea from the tea-pot, she found it had an acid taste, and on its being analyzed it was found to contain oxalic acid.

The jury found the prisoner "guilty of administering the poison, but not with intent to murder."

*Baron Parke.* That is tantamount to a verdict of not guilty.

It was urged by counsel that the prisoner might be convicted of an assault, under a recent English act, but the judges were of a different opinion. *Regina v. Draper*. T. R. B.

62. *On the presence of Sulpho-cyanogen in the Human Saliva.*—In experimenting for opium in suspected cases, it may be necessary to prove the presence of meconic acid. An excellent test for this purpose is the perchloride of iron. Even an extreme watery solution of the acid acquires a cherry-red colour on the addition of the perchloride. But unfortunately the sulpho-cyanic acid is affected in the same manner, and some chemists have asserted that sulpho-cyanogen exists in the human saliva.

It appears, by an article of Dr. J. L. SMITH, in *Silliman's Journal*, for September, 1845, that Pettenkofer, finding there was a great diversity of opinion on this subject among chemists; that Gmelin, Ure, Liebig and Wright assented to its presence, whilst Berzelius, Kuhn and Muller disagreed to this, determined to re-investigate the subject.

The saliva used was collected from the author himself, and its secretion was promoted by smoking tobacco. "It was evaporated almost to dryness, exhausted

with strong spirits, again evaporated, and the residue dissolved in water. The solution was very strongly reddened by neutral chloride of iron, and let fall some brown flakes, but it could not be caused to disappear by the addition of chloride of sodium or ammonium. The extract was boiled with sulphuric acid and a moist piece of lead paper held over it, which latter was rendered brown by sulphuretted hydrogen. It was submitted to other characteristic tests for sulpho-cyanogen, and responded to all of them."

According to Professor Christison, (*Treatise on Poisons*, fourth edition,) the best distinction yet proposed is that suggested by Dr. Percy. Acidulate the red fluid with sulphuric acid, drop in a piece of pure zinc, and suspend; at the mouth of the tube, a bit of paper moistened with solution of acetate of lead. If the redness be caused by sulphocyanic acid, hydrosulphuric acid gas is evolved and blackens the paper. Dr. Pettenkofer, it will be observed, pursued this process. No such effect ensues, if the redness be owing to meconic acid.

T. R. B.

63. *Poisonous Fish*. *Royal Society*, April, 1846.—Sir William Burnett, M. D., communicated a report which he lately received from Mr. JAMESON, Surgeon of the Flag-ship at the Cape of Good Hope, of the rapidly fatal consequences ensuing from eating small portions of the liver of a fish known at the Cape by the name of the Bladder or Toad Fish, the *Aptodactylus punctatus*, or *Tetrodon* of Cuvier. The symptoms were chiefly pain and burning sensation at the epigastrium, constriction and spasm of the fauces and muscles of deglutition, rigidity of the tendons, coma, paralysis, and convulsions, following one another in quick succession, and terminating in death within twenty minutes after the poisonous food had been taken. Several other instances of the same kind are next related, and a narrative is subjoined of the case of a seaman who lost his life with similar symptoms, from the bite of a water snake in Madras roads; the *Coluber laticaudatus* of Linnæus, (*Hydrus Colubrinus* of Shaw,) and of a ship's company who were all severely affected by eating portions of a large *Banacuda*. (*Perca Major*.)

The author ascribes the symptoms induced by these deleterious substances to their action on the nervous system alone, these being evidence of congestion only, but not of inflammation, in the stomach and other viscera.—*London, Edinburgh and Dublin Philosoph. Magaz.*, Aug. 1846.

T. R. B.

64. *Upas Tree*.—"The Dyak Darrat or Land Dyaks, seem to differ in no essential particular of language or customs, from the men of the sea, except in as far as depends on their inland position. The only remarkable difference of usage noticed by Mr. BROOKE is, that the latter use and the former do not, the curious weapon called the *sumpitan*, or blowpipe, for shooting poisoned arrows. The wounds inflicted by these are curable," says Mr. Brooke, "by antidotes known to the natives; nor are they regarded, apparently, with much terror. And we suspect the whole romantic history of the poisonous trees of the Indian isles must be banished with so many other marvels, to the province of legends, since a friend of Mr. Davidson, in Java, to prove their absurdity, climbed up an upas tree and passed two hours in its branches, where he took his lunch and smoked a cigar."—*Edinburgh Review*, July, 1846.

T. R. B.

65. *Spontaneous Combustion*.—This summer, the banks of the Cam exhibit an unusual multitude of those singular phenomena—cases of spontaneous ignition and combustion in growing willows. About a week ago, we observed, in one instance, at a point not far from Granchester, the process rapidly going on. It was really astonishing to look upon a fine willow, in the full vigour of robust vegetable health, pouring fourth clouds of smoke from its half-burned stem, and doomed speedily to expire—itself its own funereal pile. How explain this? How account for the fact that this tree, yet hale and green, covered with a rich mass of foliage, and flourishing "like a green bay tree on the river bank," should suddenly burst forth into ignition, burn like tinder to its very core, and to-morrow be prostrate? There is no putrescence—we think there can be no fermentation in this process. If instances of spontaneous vegetable combustion thus frequently happen, why dispute the analogous case of spontaneous animal combustion? The tree which we

observed last week, as stated above, is now prostrate—its very foliage charred—a vegetable ruin—as if stripped, shattered, blasted and half consumed by the electric fluid.—*Athenæum*, July 25, from the *Cambridge Advertiser*. Has anything similar been noticed in the willows of this country? T. R. B.

66. *Alcohol*—*Paris Academy of Sciences*, July 13.—MM. BOUCHARDAT and SANDRAS completed their series of communications on the digestion of food, by an article on the effect of alcoholic liquids. They state that these liquids do not undergo in the apparatus of digestion, any other change than that of being weakened by the gastric juice and mucus, the saliva, and the other liquids which may be present. The absorption of alcoholic liquids is effected by the orifices of the veins. It is particularly in the stomach that this takes place, when these liquids are taken in excess, or mixed with sugar. The absorption, however, may be continued throughout the intestines. The chyloferous vessels perform no part in this absorption. Spirituous liquors, when introduced into the circulation, are not eliminated by any of the secretory organs; a small portion only is evaporated by the lungs. If the quantity taken be very great, the arterial blood preserves the colour of the venous blood, and alcohol may induce apoplexy. Alcohol, under the influence of oxygen incessantly introduced into the system by the respiration, may be immediately converted into water and carbonic acid, but in many cases, acetic acid has been obtained.—*Ibid*. T. R. B.

67. *On the Blood in bodies killed by Strangling*.—CICCONE made the following communication to the scientific association at Naples. After he had observed that in persons destroyed by asphyxia, the quantity of fibrine in the blood is diminished, and that the circulation continues for some time after respiration has become suspended, he further found, that in cases where the respiration, as also the return of the blood in the jugular veins to the heart, had become prevented by a ligature placed round the neck, whilst the greater circulation, and hence also the metamorphosis of arterial into venous blood, which, although now imperfect, takes place in the capillary vessels, the blood accumulating in the jugular veins; that portion of this fluid which is above the ligature contains the normal quantity of fibrine, while in that which is below the ligature the quantity is diminished. This observation is thus far of value, that if the body has not been killed by strangling, and the ligature has not been applied until after death, these changes in the blood will not be found. At the same meeting, Cappa detailed his experiments; he found that in ten strangled fowls, death was in six instances caused by asphyxia and apoplexy combined; twice by asphyxia alone; and twice by apoplexy alone. In the last cases, on the microscopic examination of the blood above the ligature, its corpuscles were found distended, and generally without a central nucleus; some of them were elongated, and others again in conglomerated masses, while the blood corpuscles in all the other parts of the body presented their natural appearance. Where death had been the result of asphyxia alone, all these alterations in the corpuscles were, on the contrary, observed only in the parts of the body below the ligature; and lastly, where death had been caused by a complication of apoplexy and asphyxia, the alterations in the blood corpuscles could be seen in all parts of the body without distinction. The above test could therefore, in his opinion, be applicable only in the two first circumstances, viz., where death was from apoplexy or asphyxia alone, and not where it was caused by these two conjoined, which occurrence was by far the most frequent. Where death was the result of apoplexy, he had certainly seen the jugular veins thicker and more full of fibrine, and he had seen exactly the reverse where asphyxia had been the cause; but where death was accompanied with symptoms of apoplexy and asphyxia conjoined, this test was of no avail.—*Monthly Journ. Med. Sci.*, Sept. 1846, from *Annali Univ. de Med.*, Nov. 1845.



## MISCELLANEOUS.

68. *On the Composition of Air at different heights in close Apartments.*—M. LASSAIGNE has drawn the following conclusions from a series of experiments on this subject:

1. In rooms where the air is confined, and has been respired for some time without renewal, the carbonic acid expired is not found exclusively, as some have asserted, in the lower strata.

2. In accordance with the laws of physics, confirmed by experiment, the carbonic acid is nearly equally diffused throughout the whole volume of confined air which has been respired by a certain number of persons.

3. The slight differences observed would lead to the inference, that the proportion of carbonic acid was, under these circumstances, greater in the upper strata, were it not that the differences may depend upon errors in the quantitative estimation of the gaseous components of the air.

4. These facts show the erroneous principles upon which some modern theories of ventilation are based: for it is clear that the *whole* mass of air which has been respired by many persons requires renewal, so that the vitiated air may be entirely expelled.

5. The uneasiness experienced from respiring the heated air in the upper part of crowded and badly ventilated theatres, is due rather to its rarefaction than to its chemical composition; for the latter is almost identical in the upper and lower strata. The acts of respiration are more (?) full and frequent when rarefied air is breathed; hence certain physiological effects are induced which are not observed in the respiration of air at the common temperature.—*Lond. Med. Gaz.*, from *Comptes Rendus*.

69. *Means of neutralizing exhalations of Sulphuretted Hydrogen.* M. FAUCILLE, being charged with the direction of the works for exploring the fountain Lucas at Vichy, had to contend against a disengagement of carbonic acid gas so abundant, that it had become dangerous to enter the pits. He had in vain tried ventilation by heated air, clearing by means of compression, projection of water, either in a stream or a shower, lime water, &c. M. F. placed on the edge of the pit a small boiler, a pipe from which descended the excavation, conveying to the bottom of it the steam generated in the apparatus. The steam, on issuing from the mouth of the tube, became, after a few moments, opaque, and as it were, full of sooty particles: it then gradually recovered its transparency. At the end of twenty-five or thirty minutes the pits could be entered without danger. The transmission of this steam was required to be continued during the whole period of the works, which were then advanced as far as necessary. M. F. had been able, under the circumstances, to apply the same process to destruction of the vapours of sulphuretted hydrogen.—*Monthly Journ. Med. Sci.*, Sept. 1846, from *Annales d'Hygiène*, April, 1846.

## AMERICAN INTELLIGENCE.

## ORIGINAL COMMUNICATIONS.

*Address to the Medical Profession, in relation to the objects of the National Medical Association, by the committee appointed for that purpose.*

Abstract from the minutes of the proceedings of the National Medical Convention, held in the city of New York, in the month of May, 1846.

*Resolved,* That a committee of seven be appointed to prepare, and issue an address to the different regularly organized medical societies, and chartered medical schools in the United States, setting forth the objects of the National Medical Association, and inviting them to send delegates to a Convention to be held in Philadelphia, on the first Wednesday in May, 1847."

In obedience to the above resolution, the committee appointed for the purpose present the following remarks to the members of the medical profession generally, as well as to the several bodies named in the resolution:—

In compliance with an invitation from the Medical Society of the State of New York, delegates from various medical societies and schools met in Convention in the city of New York, on the first Tuesday of May, 1846. About one hundred members were present, representing thirty-four different medical associations, and residents in sixteen different states of the Union. The object of the Convention was, by a concert of action of medical men from every part of the country, to advance the interests, the honour, and the usefulness of the profession. The result of their deliberations, as shown in their published proceedings, has been widely circulated, so that all who take an interest in such matters, are probably fully informed of it; still, some remarks upon the course which was pursued, together with the reasons for it, may be appropriate:

The first object which engaged the attention of the Convention, was the necessity of forming some plan of organization, by which medical men in every part of the country may communicate with each other, and act in concert, in regard to their common interests. The general opinion was, that this desirable object would be best attained by the formation of a National Medical Association, to consist of members from every part of the country, who should meet together for consultation and action, at such times and places as they might deem expedient. Although no doubt was expressed of the propriety of such an organization, there were other subjects connected with it, such as, who should be members of the association? how and by whom should they be selected? if delegates, what should be their number? what power should be conferred upon them, and how far should their acts be binding upon the bodies whom they represent? concerning which there would probably be difference of opinion. As no one had been charged with the consideration of these subjects, no definite plan of organization had been prepared, upon which the Convention could act with that careful deliberation which the importance of the business demanded. It was thought best, therefore, to postpone all definite action upon it until a future Convention. This opinion was strengthened by the fact, that although the number of members present was larger than had been anti-

ipated, yet many most respectable medical societies and schools, who are doubtless equally solicitous with those who were present, for the welfare of the profession, and as ready to promote its interests, were not represented. This deficiency, it was hoped, would be remedied in a future Convention. In the meantime, the consideration of these subjects was intrusted to a committee, who will prepare a plan for a National Medical Association, which will be presented to the proposed future Convention.

The other subjects which engaged the attention of the Convention, related principally to medical education; such as the qualifications which should be required of those about to engage in the study of medicine; the course of study which should be pursued by them; the mode of examination which should be adopted, and others of a similar kind. For reasons like those given above, these several matters were also placed in the hands of committees to examine, and report upon them. The same course was also pursued in regard to the preparation of a code of medical ethics, by which the intercourse of physicians with each other, and with the public, shall be regulated.

From this statement it will be seen that matters of great interest are presented for consideration, and that if wise measures in regard to them are adopted, such errors and abuses as may be found to exist, will be corrected; the profession will be guarded against the admission of incompetent or unworthy members; the purity, professional and moral, of those who are allowed to continue in it, will be preserved; and that thus full assurance, such as it has a right to demand, will be given to the community; that all who are acknowledged by medical men to be of their number, are worthy and competent to perform the duties, and to sustain the responsibilities of an arduous and honourable profession.

It remains to be seen whether these desirable objects shall be accomplished. It is obvious that this can be done only by general and united exertion. All partial or divided action will be unavailing. It is equally true, that whatever is done, must be accomplished by medical men themselves. In some countries, the interests of the medical profession, like others of great importance to the community, are regulated, sustained, and protected by public law. In this country, no general law, even if it were desirable, can ever exist. In many of the states, no laws upon this subject have ever been enacted; in several, where they have formerly existed, they have been repealed, and in those where they still remain, there is a general complaint of their inefficiency. In this state of things, the only resource which remains, is, for medical men to establish and enforce among themselves such regulations as shall purify and elevate their own body, and thus more fully command the respect and confidence of their fellow-men. The proposed association; if it becomes general, may be the means of accomplishing much of this good work. The opinions of such a body of the most respectable members of the profession, enjoying the confidence of their brethren, and of the public, freely expressed after full consultation and careful deliberation, although not clothed with the authority of law, will still command respect, and for the most part, compliance. A public opinion in regard to the subjects decided upon, will be created, which will be more controlling than law. It is by creating and sustaining a sound and healthy public opinion, that the association will prove most beneficial.

In calling the attention of the physicians of this country to such an effort at this time, it is not intended to express the opinion, for no such opinion is entertained, that they are behind those of other countries, or the mem-

bers of the other professions in this, in general intelligence, in scientific attainments, or in practical skill. Still it is to be remembered that the present is peculiarly an age of advance in every department of science, and that at such a time to rest satisfied with present attainments, and to make no provision for increased acquisitions, is practical retrocession.

In this state of things, the committee feel themselves fully authorized to call upon the medical profession throughout the country to consider, carefully and deliberately, the matters which have been presented to them, and upon which a future Convention will be called on to act. They also earnestly invite all medical societies, the faculties of all medical colleges, and all similar associations, to appoint delegates to meet in Convention in Philadelphia, on the first Wednesday in May, 1847.

It is confidently believed that a Convention thus constituted, embodying the wisdom, and acting under the sanction, and with the authority of the united profession, will devise such measures as shall command the respect of all who are interested in the promotion of medical science, and the physical welfare of man. In behalf of the committee,

J. KNIGHT, M. D., *Chairman.*

*Legal Medicine.—1. Protracted Gestation.—2. The right of Physicians to compensation for post-mortem examinations at inquests.*

*Philadelphia, July 29, 1846.*

TO ISAAC HAYS, M. D.

DEAR SIR:—The Hon. Ellis Lewis, of Lancaster, Pa., has kindly forwarded to me, for your Journal, two of his charges, bearing on medico-legal questions. One refers to the case of the "Commonwealth v. Elisha F. Hoover," indicted for fornication and bastardy; the other to a suit brought by me against the county of Lancaster to test the principle of compensation for post-mortem examinations at inquests.

In reference to the case of fornication and bastardy, I will give you memoranda of two cases, occurring in my practice several years ago, viz:—

CASE I.—The wife of Valentine Shaeffer, Rapho Township, Lancaster County, lost her catamenia, March 22d, 1832, quickened August 5th, 1832, was delivered March 22d, 1833, with forceps, of a female child. First presentation, fourth child.

CASE II.—The wife of Samuel Henry, same township, lost her catamenia August 6th, 1832, quickened December 25th, 1832, was delivered August 13th, 1833, of a female child. First presentation, third child.

Both the above children were living, healthy, and unusually large, and the mothers enjoyed excellent health.

In these cases there was no possible motive for deception, nor is it probable that the women were deceived. They experienced the same symptoms as in previous pregnancies, and made their calculations as before, engaging my services for a period long before the time they were actually required. From the moment of quickening, they continued to feel the motions of the children daily, until the time of parturition.

I have not the least doubt of the truthfulness of the evidence in the above cases. The circumstances were so extraordinary to me at the time that I closely investigated them so as to satisfy myself on this point.

If these are not cases of protracted gestation, how are they to be explained?

Very respectfully, yours, &c.,

WASHINGTON L. ATLEE.

LANCASTER QUARTER SESSIONS—*Hon. Ellis Lewis, Pres't.*

1. *Com. vs. Elisha F. Hoover.* Indicted for fornication and bastardy, with Catharine E. Rife. This trial lasted two days, and excited considerable interest from the vigour with which it was conducted and the novelty of the defence. The complainant swore that the child was begotten on the 23d of March, 1845, and born on the 30th of January, 1846, making the period of gestation 313 days—being 33 days over the usual time. The defence relied mainly on the *time*, and called several physicians to prove the *impossibility* of gestation being protracted so much beyond the usual period. Doctors Kerfoot, Burrowes, Alex. Cassidy, J. S. Carpenter, Smith and Leonard, testified with more or less positiveness against the possibility of protraction. Dr. Kerfoot considered that nature had established nine calendar months as the period of healthy gestation, and that that period could not under any circumstances be materially extended. Dr. Burrowes had formed his opinion from the absence of facts: he had never known gestation to exceed nine calendar months, and did not believe it possible. The other medical gentlemen called on the part of the defence concurred in substance with these, though they all admitted that *the books* generally held differently.

The prosecution called Doctors John L. Atlee, F. A. Muhlenberg, P. Cassidy, H. Carpenter, Fox, and Baker.

Dr. J. L. ATLEE was of opinion that the ordinary period of utero-gestation was nine calendar months, or from 270 to 280 days—that, although improbable, there was a *possibility* of its being protracted to 313 days. He had formed his opinion from two cases which had occurred in his own practice, in which, by all the usual methods of calculation, the patients must have gone at least ten calendar months—from the testimony and opinions of standard authors—such as Wm. Hunter, Burns, Merriman, Blundell, Velpeau, Moreau, Dewees, &c. &c.—and from analogous cases among domestic animals, as proved by experiments made with great care, particularly by M. Tessier, in France.

The other gentlemen called for the prosecution concurred in the opinions of Dr. Atlee, for various reasons stated, which we have not space to report.

*Charge of the Court.*—*COM. vs. ELISHA F. HOOVER.* The defendant is indicted for fornication and bastardy.—The prosecutrix, CATHARINE E. RIFE, is a competent witness, but her credibility is for the jury. According to her account, the child was begotten on the 23d of March, 1845. It was born on the 30th January, 1846—a male, fine, large and healthy. The period of gestation was 313 days. It is conceded that the defendant had no intercourse with the mother after the 23d of March, 1845, and the time of delivery is fixed with equal certainty. A question of science has arisen respecting the possibility of protracted gestation.

The usual period is nine calendar months, or from 273 to 275 days. What has been denominated the *extreme* of the *usual period* is 280 days, or ten lunar months. But whether any, and if any, what longer time may be allowed as possible, are the questions which this case presents for decision. Medical writers of celebrity and authority are arrayed on both sides of these questions. And the medical witnesses upon the stand are, in like manner, divided in opinion. In construing this evidence, so far as respects the *FACTS* narrated by each, it is proper to consider that writers and witnesses are respectively relating only the results of their own knowledge: and, when one states that no case of protracted gestation has fallen under his observation, it is but negative testimony, and cannot justly be relied upon to invalidate the affirmative evidence of others equally entitled to credit, who enumerate cases of the kind, which they positively affirm to have come within the range of their practice and knowledge. In the most familiar transactions of life, witnesses will differ in their narration of the circumstances. In an account of a simple assault and battery, the bystanders frequently vary in their statement of the facts. Some narrate incidents which others omit. Conceding all the witnesses to be equally worthy of credit, the rule is to reconcile their evidence so that all will stand consistently together, if this be reasonably practicable. Some witnesses observe circumstances which others have not seen. Negative evidence is therefore deemed insufficient to outweigh affirmative statements from witnesses equally entitled to credit. One gentleman, in a long course of practice, may have failed to observe any case of the kind. Another, in a very brief period, may have

noticed several. And it is reasonable to believe that where such a diversity of opinion exists, each will be in some measure influenced by his own professional experience; and that this will also, to some extent, affect his belief in the cases reported by others. There are doubtless many of these cases where the struggle for character and property, and the circumstances of the parties whose interests have been involved, have furnished temptations to falsify, and may have influenced the decisions of the tribunals. But, after making all proper allowances for cases of this description, the whole evidence on the question, when fairly considered, appears to show that cases of protracted gestation are not impossible, although their existence is very unusual.

The heads of wheat in the same field do not all ripen together. The ears of corn on the same stalk do not all come to maturity at the same time. Even the grains of corn on the same ear ripen at different periods. The fruit on the same tree shows the like deviation. A portion will ripen and fall while other portions remain comparatively green upon the parent stalk. The eggs of the fowl, under process of incubation at the same time, are subject to the same variation. In quadrupeds, if the testimony of M. Tessier be believed, we have proof of the like irregularity. Whatever may be the causes, operating in each case, to divert nature from her accustomed course, to accelerate or delay her usual progress, the human species, like the rest of creation, seems occasionally under their influences. The developments of puberty, although generally shown at a certain age, are far from regular. Some individuals approach it earlier—others later in life. Intellectual maturity is subject to the same irregularities. Some are precocious, others astonishingly tardy in arriving at the usual degree of discretion. The intervals between the catamenial visits, although in general regular and fixed, exhibit remarkable deviations. The final departure of the catamenia, although generally to be expected at a certain age, is as irregular as their first approach, and as subject to variation as were their periodical returns. A certain period of life has been usually assigned for the termination of a mother's perils, but the instances of extensive deviations from this general rule are numerous and well-established. The gestation of one child at a time is according to the usual course of nature, but the births of twins, triplets, &c., furnish indubitable proofs of astonishing departures from the usual course. The sensations of the mother produced by the elevation of the fœtus from the cavity of the pelvis, (called quickening,) although usually occurring at a certain period, are known to be subject to the like departure from the usual time.—It has been said that human life does not generally extend beyond 70 years. But if this be the general rule, the departures are numerous. The most distinguished jurist perhaps, now living in the whole world, (Chancellor Kent,) will be 83 years old on the 31st of July next; and yet, within a few days I have been honoured by the receipt of a letter from him, under date of the 18th inst., in which he states that he is still in "good and active health—that his relish and ardour for studies and legal learning continue unabated—that he has the blessing of good eyes, and that he is still an observer of what passes with lively sensibility." This instance may serve to illustrate not only the occasional deviation from general rules respecting the duration of human life, but the like variation in respect to intellectual vigour, by which one individual attains a pre-eminence over the generality of mankind. All Nature abounds with occasional departures from her general customs. Even the compass, which guides the mariner on the trackless ocean—which enables science to fix with reasonable certainty the boundaries of kingdoms and farms, and the truthfulness of which to its accustomed law has been perpetuated by a proverb—is subject to mysterious but acknowledged variations.

From analogy, and from the statements of distinguished authors and eminent witnesses, after making every allowance for mistakes, and the operation of unfavourable influences, we are led to the belief that, although Nature delights in adhering to her general usages, she is occasionally retarded in her progress, and otherwise coerced, by causes not always apparent, into extensive deviations from her accustomed path.—And we are induced to believe that protracted gestation, for the period of 313 days, although *unusual* and *improbable*, is not *impossible*. The evidence to establish the existence of such a considerable departure from the usual period, should be clear and free from doubt. The witness should possess a character

beyond reproach, and her testimony should be consistent and uncontradicted in all material facts. If the jury are satisfied that the evidence for the commonwealth is of this character, the unusually long period of gestation does not require them to disregard it. The law fixes no period as the *ultimum tempus pariendo*. The usual period has been stated, but longer time may be allowed, according to the opinions of physicians and the circumstances of the case. The question is, therefore, open for the decision of the jury. If they believe the witness, they may find the defendant guilty.

[Here the court drew the attention of the jury to the prominent facts, tending on the one side to impeach, and on the other to support, the credit of the prosecutrix; and then left the case to the jury, with the direction that, if they entertained reasonable doubts of the defendant's guilt, he was entitled to an acquittal.

April 24th, 1846.

ELLIS LEWIS.

NOTE.—A lady of respectability was examined on oath, in the course of the trial, and stated that she had been the mother of nine children—that to the best of her judgment and belief, the period of gestation, in the case of the 7th child, was over ten months. That in addition to the usual data she relied upon the time of quickening, which happened, as she believed, at the usual time, and that the birth of the child did not take place until seven months after that event.

The jury found the defendant GUILTY, and the usual sentence was passed upon him.

Frazer and Mathiot for the commonwealth—Stevens for the defendant.

2. *Has a physician a right to compensation for making a post-mortem examination at the request of a coroner?* Opinion of the Hon. Judge ELLIS LEWIS.

Dr. W. L. Atlee vs. Lancaster County. No. 50, January, 1844.

Opinion of the Court.—This is an action to recover compensation for professional services rendered by the plaintiff, as a physician, at the request of the coroner, upon a *post-mortem examination*. It is the duty of the coroner, in cases of sudden death, or where any are slain, to hold an inquest, *super visum corporis*, to inquire into the cause of death, "by the mark on the neck, or by a mark on any of the limbs, or *by any hurt found on the body*." 4 Ed. 1. The statute of Edward the 4th, regulating the duties of the coroner, in this respect, is in affirmance of the common law, one of the great advantages of which is, its constant adaptation to the progress of business, the advanced state of the sciences, and the habits of the people. In this enlightened age, a coroner who would undertake to consign to the grave, the body over which he had held an inquest, without availing himself of the lights which the medical science has placed at his disposal, would be regarded as unfit for the office, and unworthy the age in which he lives. A thorough examination, aided by the highest professional skill within reach, is absolutely necessary to the administration of justice. Without such examination, groundless suspicions may be entertained, and prosecutions commenced, at once cruel to the objects of them, expensive to the county, and wasteful of the time and talents of all persons engaged in them. But this is not all. Without a thorough examination of the body, and a complete demonstration, from the evidence thus in the power of the commonwealth, that the death was caused by violence, the guilty agent cannot be convicted. When, from ignorance in the examination of the body, the cause of death is left in doubt, the accused will, in general, escape the just punishment due to his crime; because in all cases of doubt, he has a right, under the law, to demand an acquittal. Thus, the guilty are again let loose upon society, and the public is deprived of that protection which the laws were intended to provide. There can be no doubt of the

duty of the coroner to require such aid as was given in this case; and it seems equally clear that his powers are commensurate with his duties.

It is true that medical witnesses may be called upon to give evidence of any *facts* within their knowledge, and also of the *opinions* which their researches and experience have enabled them to form on questions of science pertinent to the case. But they cannot be compelled to devote their time and talents, without compensation, to the unpleasant duties of a *post-mortem examination*—to assume the responsibilities of an investigation, upon which the life of a fellow-being may depend—and to encounter the painful ordeal of a thorough cross-examination, generally conducted by gentlemen of science and courtesy, but sometimes by individuals not remarkable for either.

Even where nothing further is asked of the medical practitioner than his attendance as a witness, his case is one of peculiar hardship. His business is of a character which requires the employment of his own skill and judgment. It is in these alone that his patients confide, and no deputy or substitute can supply these qualities. To him the ordinary fee of a witness is a very insufficient compensation. "The same policy which exempts medical practitioners from attendance on other public duties may suggest the propriety of allowing them some adequate indemnity when their assistance becomes indispensable; and this not only for their private and immediate advantage, but ultimately for the public benefit; for, if properly remunerated for their attendance, practitioners of a superior class would not be unwilling to devote some portion of their time to the assistance of public justice; whereas, under the existing system, it is notorious that all who can will avoid the burden; and the duty, therefore, devolves on those who are least competent to its execution; this evil is particularly apparent on coroners' inquests, where the opinion of a shop boy has often been allowed to determine a question *in limine*, which, properly investigated, might have required the first science to obtain a satisfactory result." 1 Par. & Fonbl. Med. Jur. 157.

As attendance is more burdensome on a professional man than on others, so also is it more frequently called for. Other men are only called to testify to *FACTS* within their knowledge. The professional man is called upon for his *OPINIONS*. Those, therefore, who stand highest in public estimation, and whose time is most valuable to themselves and their patients, are the most frequently called to discharge these unprofitable duties. A consideration of these circumstances, has doubtless led to the practice in England of allowing medical witnesses more than the usual witness fees, as a compensation for the loss of time. *Willis v. Peckham*, 4 Moore; *Severn v. Olive*, Mich. Term, 1821, C. B. Two guineas per day is the usual allowance to medical witnesses in England, "but no allowance is made for the time spent in making chemical experiments, for the purpose of perfecting themselves in the science which they profess, because it is alleged that they ought not to acquire their knowledge at the expense of the parties." Ap. to 3 Par. & Forb. p. 203; *Severn v. Olive*. How far they should be allowed for the time spent in the application of that knowledge to the particular case under investigation is a question entirely different. The principle by which they are compensated for their loss of time as witnesses, and considerations of policy and justice, sanction an allowance for the loss of time spent in a *post-mortem* examination.

This expense is a necessary incident to the administration of justice for the county. The coroner of the county is charged with the duty of hold-



ing the inquest, and has, as an incident to his authority in this respect, the power to summon such aid as the purposes of justice may require. In doing so, he acts not in his individual capacity, but in his official character as the officer of the county. "The power to order, implies the promise to pay." *Commissioners v. Hall*, 7 Watts, 291. It is upon this principle that the county is liable for the contingent expenses of the court—for the fire-wood, candles, &c., consumed in the court house—for the expenses of keeping a jury together at a hotel when ordered by the court, as is usual in capital cases—as for the expenses of medical services ordered by the court for a juror suddenly ill. 7 Watts, 291.

The plaintiff is, therefore, entitled to recover in this action, such a sum as the jury shall deem a reasonable compensation for his services.

Nov. 23th, 1844.

ELLIS LEWIS.

The jury rendered a verdict in favour of the plaintiff for the whole amount of his claim.

*Case of a Catheter left in the Bladder, in drawing off the Urine for Retroversion of the Uterus.* Communicated by E. W. THEOBALD, M. D., of Baltimore, Md.

I have met with the following case recorded in the *Medical Facts and Observations*, London, 1791, p. 96, by Wm. Ford. Since reporting one in the April number of the *Amer. Jour. of the Med. Sciences*, bearing a very striking resemblance to it; and it being, so far as my observation extends, the only one of the kind previously on record, I beg leave to subjoin it.

"Mary Wilding, a thin delicate woman, about twenty-five years of age, was admitted in January last, as a patient at the Westminster General Dispensary. She complained of a painful and involuntary discharge of urine, mixed with blood and matter, from the urethra; and also of a discharge of purulent urine, which was continually flowing from a fistulous sore, situated in the buttock, near the middle of the glutæus muscle. She was in a weak and emaciated state, and had been confined to her bed for several months; every attempt to move from thence being attended with most severe pains, both in the neck of the bladder, and at the fistulous wound in the nates.

"Upon introducing a sound into the bladder, an extraneous substance was easily felt within its cavity; and from its hardness, I judged that it might be a calculous concretion. At the patient's desire, I then proceeded to examine the fistulous sore on the buttock, and she told me there was a loose bit of bone in the wound, which frequently made its way outwards beyond the skin, but as often seemed to be retracted with considerable force. I found, by examining it with the probe, that it lay loose in the sinus, and I endeavoured to remove it with the forceps, gradually drawing it outwards. This process was not attended with much pain; but when the extraneous substance was brought forward about half an inch beyond the integuments, a further removal of it seemed impracticable, as it was strongly held back by the contraction of the muscles. Whilst it was retained externally by the forceps, I viewed it closely to ascertain whether it was an exfoliation of carious bone, or a calculous concretion that had made its way outwards from the bladder, but was much astonished to find that the substance, protruded from the wound, was evidently the bulbous end of a silver catheter.

"This discovery instantly induced me to suspend any further operation, as it was clear that an attempt to remove the catheter by extracting it forcibly through the wound, must occasion a considerable laceration of the fundus of the bladder; and I was anxious to collect from the patient such circumstances as might explain her unfortunate situation. She professed herself totally ignorant by what means the catheter had been lodged in her bladder, and could with difficulty believe the information I gave her. The narrative she furnished me with was, that she had been brought to bed four months; that in the third month of her last pregnancy,

she had been seized with a difficulty of voiding her urine, which had been several times drawn off by means of a catheter; that she had experienced great relief from this operation, but that the last time it was performed, she had felt great pain, and had ever since been unable to remove from her bed without great distress; that she had been safely delivered at the expiration of the ninth month: and that she had since suckled her infant, though in the most wretched and debilitated state. It was obvious from this account, that the catheter had escaped from the hands of the operator the last time the urine had been drawn off; that it had slipped into the bladder, and had been suffered to remain there; and that the only method of relieving her was to extract it through the meatus urinarius.

"From the weak state in which she lay, exhausted by suckling her infant, by pain, and by the discharge from the wound, I declined performing the operation, till her health should be a little invigorated by weaning the child, and by a more nourishing diet. When this was accomplished, I was favoured with the assistance of Dr. Jackson, Dr. Bland, and Dr. Combe, all of whom were anxious to see so singular a case.

"The patient was laid upon a table, and secured in a manner usually adopted in the operation of lithotomy. The urethra was dilated by the blunt gorget introduced upon a female staff, and the catheter was then attempted to be taken out by the forceps. This part of the operation was attended with much difficulty, as the catheter lay transversely in the bladder, the handle of it resting on the arch of the pubis, and its other extremity on the crura ischii. It was dislodged from its situation by drawing the blunt end outwards through the posterior wound, so that the handle of the instrument being detached from the pubes, was more easily brought forward through the opening in the urethra, and extracted. The catheter, which is now in my possession, was found covered with a slight incrustation.

"The operation was finished by extracting a few small calculi from the bladder. The patient was then put to bed, and the same regimen pursued as after cutting for the stone. A slight fever came on, but was apparently more owing to the state of her breasts, as she had just weaned her child, than to the operation. The fistulous opening on the buttock healed in a few days, the urine passing entirely through the natural passage; and in one month she was perfectly well. She now retains her urine, and suffers no inconvenience from this extraordinary calamity.

"The foregoing case is, I believe, unprecedented in medical history. It affords a singular example of an accident occurring from an operation in surgery, which has usually been deemed easy to perform, and free from hazard. The natural structure and situation of the female urethra, warrant the general opinion of the safety of this operation; but where an alteration takes place in these parts, either from pregnancy or other causes, the operation of drawing off the urine may become liable to difficulty.

"In cases of retroverted uterus, we find, by the testimony of Dr. Hunter, and other practitioners, that this operation is not always to be done with facility, and that in some cases it has been impracticable. The poor woman who is the subject of this paper, had been liable to a retroversion of the womb, both in this and in a former pregnancy. Her urine had been drawn off a few days before this accident by a man-midwife of eminence; but being suddenly taken ill, she applied to a person in her neighbourhood, from whom this accident happened. His business obliging him to leave London, he heard no more of his patient, and imagined, I suppose, that the catheter had been expelled by the efforts of the bladder."

Golden Square, May 16th, 1791.

*Case of Injury of the Foot.—Amputation, excessive hemorrhage from the stump.* By J. C. BUTLER, M. D., of E. Berkshire, Va.

Mr. J. H. Hazletine, æt. 32, of sanguine temperament, consulted me in a case of long standing disease of the right foot. The history the patient gives of it is, that in the winter of 1839, a large mill-log rolled from a sled upon his foot, producing serious injury of the phalangeal and metatarsal bones, and the soft parts nearly as high as the ankle. The toes were

removed at first dressing; subsequently two metatarsal bones; but there yet remained an extensive and obstinate ulcer upon the external and inferior surfaces, extending back to the heel, measuring fifteen and a quarter inches in circumference, discharging from six to ten ounces of pus per day since the accident. Very painful, particularly during the night. The ankle-joint was healthy; muscles of proper consistence; skin appeared sound; the hair was as numerous and thrifty as upon the other leg. I recommended amputation of the leg, and accordingly proceeded to operate.

February 19, 1846, assisted by my father S. S. Butler, M. D., and Drs. Babcock, Cushman and Stone, and in the presence of several students, I performed the flap operation about the middle of the leg; the muscles and bones appeared to be perfectly sound. The two tibial, the peroneal, and one large cutaneous artery were ligatured. Upon loosening the tourniquet we were saluted with a torrent of blood little anticipated. Compression was instantly applied upon the femoral artery by means of the tourniquet, and before hemorrhage could be completely controlled, another was applied to the popliteal space. Upon close examination I found it to be venous as well as arterial blood gushing from every portion of the stump. We applied *twenty-seven* ligatures to both arteries and veins; four ligatures included several vessels each. We think we can safely estimate over forty arteries and veins *tied*, besides many smaller arteries arrested by torsion. The venæ comites were enlarged to nearly or quite double their natural calibre. The usual dressings were then applied and the patient put in bed. The wound healed by first intention, and patient dismissed the seventeenth day, with directions to use a spare diet, moderate exercise, and gentle laxatives if necessary.

March 15th. Was called; found him complaining of severe pain in the head, loss of appetite and great prostration; pulse 65. Right angle of the mouth and nostril of the same side elevated; cannot restore them to their proper positions by an act of the will; an incapacity to close the eyelids of the left eye. R.—Seton to left arm, blister to the back of the neck; drastic cathartic composed of calomel jalap and gamboge; frictions to extremities.

16th. Pain in the head somewhat abated; disfiguration of the face continues; repeat cathartic; blisters to the arms.

17th. An evident improvement. Slight pain in the head, but complains of pain in the back. Pediluvium and frictions to extremities.

18th. Much improved from yesterday. Free from pain. Appetite returning; volition extends to the muscles of the face; repeat blister to the neck.

23d. Found him still improving; face assuming more natural appearance; dismissed the patient with directions to take a light and spare diet.

I have seen Mr. H. from week to week to the present time. There yet remains a slight elevation of the angle of the mouth, and paralysis of the eyelids of his left eye. His health and strength are good. The case presents two important features—the extraordinary hemorrhage after amputation, and the train of symptoms which followed healing of the stump.

#### DOMESTIC SUMMARY.

*Report upon the Hemostatic Virtues of the Brocchieri Water and Ergotine.*—The *Southern Journal of Medicine and Pharmacy* (March and July, 1846) contains an interesting report by the editors, Drs. SMITH and SINKLER, on the hemostatic virtues

of the Brocchieri water and ergotine. We have not space for the experiments in detail, but must restrict ourselves to the results, and which, it will be perceived, accord with those of the Committee of the Virginia Medical Society, in our preceding number, p. 146.

"From the numerous experiments performed, and from the manner in which they were modified, as well as the care bestowed upon them, without reference to their favourable or unfavourable results, we are warranted in coming to the following conclusions:

"1st. That when Brocchieri Water, ergotine, or a watery emulsion of creosote, is applied to the divided artery of a sheep, it depends greatly, if not altogether, upon the manner in which the lint is applied to the wound on the artery, whether the hemorrhage is arrested or not. If it be placed immediately upon the orifice of the cut vessel, the success is certain; if, however, the vessel shrink from contact with the lint, the animal is almost certain to bleed to death. The former is shown by the experiments 4 and 5 of the first report, and by 3, 5 and 6 in the present one; and the latter, by experiments 1, 2, and 4, in this report.

"2d. That by a small pledget of *simple lint*, placed immediately upon the incision made into a sheep's carotid artery, that the hemorrhage is arrested in a few moments; and after a lapse of from twenty to thirty minutes, the animal may be let loose, without any apprehension of the return of the hemorrhage. If the lint be applied so as not to touch the wound in the artery, all effort to arrest the hemorrhage becomes ineffectual; this is proved by reference to experiments 1, 2 and 3 of the first report, while experiments 7, 8 and 9 give evidence of what was first stated. From these results, it will be seen how many difficulties often attend the simplest experiments, and how important it is to leave no point, not the most apparently trivial, without close examination; it is true, it requires time and trouble, but both are more than compensated for, by a knowledge that we become in possession of truths that are important to ourselves and to others.

"3d. The sheep is an unfit animal to try the hemostatic power of substances, with reference to what their fitness may be in the case of the human subject; for although sheep will bleed to death by a wound in one of the larger arteries, still, by the application of a small pledget of lint, sustained with a little pressure *immediately upon the wound in the vessel*, the hemorrhage will cease, and the animal survive. The same we are convinced may be said of all like experiments upon the lower classes of animals, as in many of them the hemorrhage from the larger vessel will be arrested spontaneously; this is true of the dog; in fact, so far as our knowledge extends, the sheep is more readily bled to death than most any other of the quadrupeds. Furthermore, the blood of animals is more plastic, coagulating with far greater rapidity than that of man; and as the arresting of the hemorrhage in these experiments is dependent upon the formation of a clot around the opening and in the cavity of the vessel, it ought therefore to happen more readily in them than in man.

"4th. If the hemostatic virtues of the agents already alluded to, are to be correctly ascertained, it is only *by experiments upon the human subject*, and no value should be given to those made in any other way. Whether the Brocchieri water, ergotine and creosote will stand this test, we are not as yet prepared to say, although several experiments made with them have come to our notice, but they are of so contradictory a character, that no definite conclusion can be formed. These substances no doubt hasten the coagulation of blood, and may, under some circumstances, arrest hemorrhage coming from the smaller arteries; but in the case of the larger vessels, they are of no manner of use, at least not more so than the lint without them.

"The experiments on the human subject that have come to our notice are,—wound on the hand, oozing, some time after the operation of hydrocele; oozing from a tumour on the back;—tried with Brocchieri water. In the first case, there appeared to be no effect; in the last two, some slight effect; the oozing in the case of the hydrocele, although diminished, could not be arrested. In a case of hemorrhoids that was operated upon, application of the ergotine was made with apparently good effect. In two cases of operation for hydrocele, the emulsion of creosote was used; in one of them, the bleeding from the small vessels was

arrested; in the other no effect. So it is clear, that as yet, there is no danger of ligatures being supplanted by either of the above agents."

*Excision of the Elbow-Joint in a case of Caries of the Articular Extremities of the Bones.* An interesting case of this is recorded by Dr. GURDON BUCK, Jr., in the *New York Journ. of Med.*, for July last. The subject of it was a steamboat fireman, 25 years of age, of temperate habits and good constitution, admitted into the New York Hospital, June 6th, 1844, with inflammation of the right elbow-joint of nearly two years' duration, which originated without injury or known cause. It commenced with a swelling between the olecranon and the outer condyle, attended with stiffness and slight pain. On the swelling being punctured, a glairy fluid with lumps of solid substance was discharged. With occasional interruptions, the patient has been able to use the limb, though the joint has continued stiff, and its motions have been impaired. Medical treatment has been employed for a year past, such as blisters and cupping, of which numerous marks are visible about the joint. Till recently, patient has suffered but little pain, and his general health has continued good.

On the 1st of July, when he came under Dr. Bilou, the condition of the limb was as follows:—A uniform swelling involves the elbow, tapering off to the middle of the forearm below, and of the arm above, where the limb becomes small and wasted. The prominences of the olecranon and condyles, as well as the anterior fold of the joint, are obliterated. The swelling is formed by thickening and induration of the soft parts, the integument covering which, particularly over the posterior part, has lost much of its suppleness and mobility. Pressure over the olecranon and condyles causes pain. Synovial fluid is discharged from two openings, one of which is situated at the outer edge of the ulna, three fingers' breadth below the olecranon, the other at an inch above the inner condyle; the former leads to rough bone at the head of the radius, the latter, though it extends more than an inch inwards, does not communicate with exposed bone.

The habitual position of the limb is that of incomplete extension. A slight degree of flexion from this position is admissible, as well as partial pronation and supination, all of which are attended with pain: No sensation of roughness is perceptible in performing these motions.

The surface of the swelling is pale, the temperature above that of the rest of the limb. Two issues have been established near the outer condyle since his admission, and an attempt made to preserve the limb flexed at a right angle, it being the most favourable position in which to allow anchylosis to take place. This, however, had to be abandoned in consequence of the increased pain and inflammation that followed.

Patient's general health is good, pulse sixty-two, tongue somewhat furred, bowels good.

July 25.—Preparatory to the operation to be performed this day, the issues have been allowed to heal up. The swelling about the joint has rather diminished.

The upper opening extends two inches downwards and outwards, and now communicates with denuded bone.

At a distance of two fingers' breadth above the condyles, the os brachii appears to be uneven and enlarged. The patient's general condition continues favourable; appetite good, and bowels regular.

*Operation.*—The tourniquet having been applied at the insertion of the deltoid muscle, and the patient placed in the recumbent posture, on his left side, with the head and shoulders elevated, and his back towards the operator, a longitudinal incision six inches in length, was made over the olecranon, extending to an equal distance above and below it, and penetrating to the bone. The triceps muscle and tendon thus split, were raised towards the outer condyle, care being taken to keep close to the bone and avoid dividing the connections of the tendon with the aponeurosis of the forearm. The same course was pursued in the dissection toward the inner condyle, the ulnar nerve being drawn inwards to prevent its being wounded. The integuments and subjacent aponeurosis were next raised on either side, below the olecranon, the olecranon itself denuded, and more than an inch of this process removed with the amputating saw. The edges of the wound being drawn forcibly to either side, the articular extremity of the os brachii

was detached from its connections by dividing the lateral ligaments and the muscles arising from the condyles, which allowed it to be projected from the surrounding soft parts, while a transverse section was made above the condyles, separating a portion of an inch and a half in length. The head of the radius was now ascertained to be rough and denuded, as well as the smaller sigmoid cavity in which it rotates upon the side of the ulna. The division of the annular ligament allowed the head to be cleared from the soft parts sufficient to excise it at its neck. An additional portion of the ulna, including the coronoid process and lesser sigmoid cavity, remained still to be excised, which was effected by carefully dissecting up the insertions of the brachialis anticus muscle as far as was necessary, and then making a section from before backwards, with a metacarpal saw applied just below the coronoid process. The rough angular edges of bone were then pared away. Several ligatures were applied to small vessels, and the edges of the wound brought together with seven sutures, between which adhesive straps were applied, passing half round the limb.

The diseased parts presented the following appearances. A grayish, jelly-like substance covered the synovial surfaces, being most abundant where the synovial membrane passes from the bone and lines the ligaments.

This morbid product could be easily scraped off, and brought to view the spongy tissue of the bone, destitute of cartilage as well as compact outer shell. The spongy tissue thus exposed, was red and softened, and could easily be penetrated by the scalpel. Small patches of cartilage still remained at the margins of the articular surfaces, which were only loosely adherent to the subjacent spongy tissue, the exterior shell having been absorbed.

Between the outer condyle and the small head of the humerus, a deep ulcerated excavation, capable of containing a white bean, had been formed. The surface of the posterior fossa of the humerus, lodging the olecranon, was rough and bare. The articular surface of the ulna presented similar appearances to the os humeri, with two or three superficial excavations at the bottom of the greater sigmoid fossa.

The surface of the coronoid process and of the olecranon, as well as the inner margin of the sigmoid fossa, are studded with spiculæ of newly-formed bony matter.

The head of the radius was more completely deprived of cartilage and bony shell than the parts already noticed, and their place supplied by a thick layer of gelatinous deposit.

After the dressing of the wound, the limb was placed nearly at a right angle, on a flat splint padded with cotton, with a joint at the elbow allowing it to move edgewise. By means of this splint the arm was suspended from the ceiling after the patient was conveyed to his bed. This arrangement afforded great comfort; the limb swinging clear of his body, allowed him to vary his position within certain limits, an advantage he could not enjoy in any other way, compatible with perfect rest of the new joint. Sixty drops tinct. opii were then given.

At evening, about five hours after the operation, the patient began to suffer severe pain in the elbow, and the wound appeared distended with blood, which oozed from between the sutures. Thirty drops tinct. of opium, spirits mindereri  $\overline{ss}$ , were ordered to be repeated every two hours.

July 26. Patient suffers less pain; swelling of the elbow has increased, with tension and slight lividity of the surface; oozing of blood continues; removed two sutures, and an intermediate strap from the middle of the wound, and readjusted the bandages that had become too tight; pulse ninety-seven. Ordered light poultice of flax-seed meal.

At 6 P. M. Oozing of blood has ceased; complains of pain in the left hypochondrium, extending to the right side; no evacuation for forty-eight hours. R.—Tinct. opii gtt. xxx; ol ricini  $\overline{ss}$ .—At 10 P. M. to take his anodyne draught of tinct. opii gtt. xxx; aq. menth. pip.  $\overline{ss}$ .

27. Has passed a good night; general appearance is improved; free from pain in the abdomen; pain in the arm slight; serous fluid oozes from the wound; swelling is not increased; pulse ninety-four, soft; tongue coated with whitish fur; bowels still confined. R.—Infus. sennæ comp.

28. Passed a good night after taking the same anodyne as the night previous; has had two evacuations following a laxative enema; pulse seventy-four; removing the dressings, the elbow appeared well, tension has diminished, swelling the same; reapplied only two adhesive straps; continued poultice.

29. Doing well; suppuration is commencing; five ligatures came away; omitted adhesive straps; continued poultice; bowels confined; pulse seventy-two; ordered *mistura eccoprotica*.

30. Pulse sixty-eight; one evacuation from the bowels; tension of the elbow still further diminished, as well as redness of the surface; one ligature came away; and removed one suture from each extremity of the wound.

31. Doing well, bowels confined, pulse seventy-four; removed two ligatures, and the remaining sutures were removed; ordered *pil. cathart.*, three.

Aug. 1. Tension and redness of the elbow have disappeared; the three remaining ligatures came away.

The farther progress of the case continued favourable, with the exception of an attack of erysipelas on the 17th Sept., which was, however, soon subdued. One or two collections of matter formed near the elbow, but after being opened, discharged and soon healed. The limb gradually acquired increased strength and facility of motion. The power of grasping bodies with the hand, which had been very much impaired by the disease, was recovered in a good degree; pronation and supination could be performed almost to the original extent; patient was able, unassisted, to raise the hand to the head, and could handle a broom in sweeping, carry half a pail of water, and perform other useful functions to his own great satisfaction. He was also sensible of progressing improvement in his limb. His general health was good; the only symptom of which he complained was a stricture across the chest. He was discharged this day, Feb. 1, 1845, to return to his family at Buffalo.

Dr. Buck remarks that the reason for preferring, in this case, the operation with a single longitudinal incision, to an incision in the form of the letter H, or to the crucial incision, more generally recommended by authorities, was with a view to preserve undivided the lateral connections of the tendon of the triceps with the fascia investing the forearm, which must necessarily be sacrificed by either of the other modes of operating. The important advantage of so doing was, that a point of action would be preserved for the triceps muscle, which might compensate in a good degree for the sacrifice of its connection with the olecranon. All the other essential objects of the operation, such as the preservation of the ulnar nerve, &c., were attained by this method, without any increased disturbance of the soft parts, or embarrassment in its several stages. The subsequent progress of the case could scarcely have been more favourable under any circumstances, and from the experience derived from this case, together with one reported in No. VIII. of *New York Medical and Surgical Journal* for April, 1841, in which the H incision was employed, I should be disposed decidedly to give preference to the method adopted in the present instance. Rather more time, perhaps, is required for this operation, but the great advantage already noticed should certainly be considered more than an equivalent.

Statistics of the Medical Schools of the United States for the Session 1845-46.

	Students.	Graduates.	
University of Pennsylvania - - -	462	168	2+
Transylvania University - - -	171	58	3
Medical Institution of Geneva College - - -	179	39	4½
Medical Department Willoughby University - - -	164	30	5½
Albany Medical College - - -	115	42	3-
Louisville Medical Institute - - -	345	73	4½
Western Reserve College, (Cleveland) - - -	161	52	3
Jefferson Medical College - - -	469	170	2
Rush Medical College, (Chicago) - - -	50	9	2
Harvard University, Boston, - - -	180	31	2
College of Physicians and Surgeons, N. Y. - - -	219	38	6
Med. Dep. of University of the city of N. Y. - - -	425	131	3½
Ohio Medical College - - -	192	46	3

University of Maryland	-	-	147	40
Medical College of Georgia	-	-	112	30
Med. Dep. of Hampden Sidney College	-	-	74	17
Med. Department University of Missouri	-	-	92	29
Med. Department Illinois College	-	-	-	13
St. Louis University	-	-	53	11
Med. Coll. of the State of South Carolina	-	-	210	74
University of the city of N. Y.	-	-	407	130
Indiana Medical College, (Laporte)	-	-	71	17
Berkshire Medical Institution	-	-	142	35
Medical College of Louisiana	-	-	103	19
Pennsylvania Medical College	-	-	70	38
Medical School of Maine	-	-	73	19
Vermont Medical College, (Woodstock)	-	-	-	24
Yale College	-	-	53	19
Castleton Medical College	-	-	140	36

*Early Pregnancy.*—Dr. J. B. WALKER, of East Stoughton, Mass., relates in the *Boston Med. & Surg. Journ.*, Sept. 9, 1846, a case in which the menstrual function was established at the age of 11½ years, and the patient was delivered of a living child when only 12 years and 8 months of age.

*Ectrotic treatment of Small-pox by Tincture of Iodine.*—Dr. SAMUEL JACKSON (late of Northumberland), was led in April, 1845, to make an experiment of aborting small-pox by the tincture of iodine, from contemplating its wonderful influence over erysipelas. He applied it to one arm of a child eleven months old, in confluent small-pox, on the third day of the eruption, and to the arm which appeared the worst, rubbing it freely on with a sponge, three times that day and twice the next. On the 11th day, when the pocks over the whole body were at their height, elevated with hard bases, those of the medicated arm were entirely flat, with thin, purulent matter under the dead cuticle, without any swelling of the part. There are, however, some very slight pits now to be seen, but they are very inconsiderable when compared with those on the other arm.

Drs. Goddard and Sargent have since tried the application.

Dr. Sargent used the iodine on one side of the face in twenty-five cases—"the swelling, soreness and tenderness were very much less than on the sides not covered; each pock remained flattened; but I cannot say that it prevented pitting."

Dr. Goddard writes that he tried the medicine in five cases—"not one of the patients shows the least pit or mark; none of them had been vaccinated, and the disease was confluent in most of them."

One advantage of this treatment, Dr. Jackson remarks, is, "that it removes the cuticle and leaves the part free from those disgusting discolorations which commonly remain for months."—*Med. Examiner*, Aug. 1846.

*Medical Rank in the Navy.*—The late Secretary of the Navy, Mr. Bancroft, just before retiring from office, issued the following General Order, which will be gladly received by the medical officers in the navy.

"GENERAL ORDER.

"Surgeons of the fleet, and surgeons of more than twelve years, [date of commission,] will rank with commanders

"Surgeons of less than twelve years with lieutenants.

"Passed-assistant surgeons, next after lieutenants.

"Assistant surgeons, not passed, next after masters.

"Commanding and executive officers, of whatever grade, when on duty, will take precedence of all medical officers.

"This order confers no authority to exercise military command, and no additional right to quarters.

GEORGE BANCROFT.

"Navy Department, August 31st, 1846."



According to this order, by reference to the Navy Register, we find the following named surgeons will now rank with commanders in the navy.

- |                       |                            |
|-----------------------|----------------------------|
| 1 Jonathan Cowdry.    | 18 George Terrill.         |
| 2 Wm. P. C. Barton.   | 19 Edmund L. Du Barry.     |
| 3 Thomas Harris.      | 20 Waters Smith.           |
| 4 Wm. Turk.           | 21 Benjamin F. Bache.      |
| 5 John A. Kearney.    | 22 Thomas Dillard.         |
| 6 Bailey Washington.  | 23 Stephen Rapalje.        |
| 7 Wm. Swift.          | 24 James M. Greene.        |
| 8 Thomas B. Salter.   | 25 Benjamin R. Tinslar.    |
| 9 Peter Christie.     | 26 George W. Codwise.      |
| 10 Samuel Jackson.    | 27 Gustavus R. B. Horner.  |
| 11 Thomas Williamson. | 28 W. S. W. Ruschenberger. |
| 12 Benajah Ticknor.   | 29 William Johnson.        |
| 13 James Cornick.     | 30 Samuel Mosely.          |
| 14 Charles Chase.     | 31 Robt. J. Dodd.          |
| 15 David S. Edwards.  | 32 Wm. Fairlie Patton.     |
| 16 Isaac Hulse.       | 33 John F. Brooke.         |
| 17 John S. Wily.      |                            |

The following named surgeons will now rank with lieutenants in the navy.

- |                        |                          |
|------------------------|--------------------------|
| 1 William Whelan.      | 19 Samuel C. Lawrason.   |
| 2 Samuel Barrington.   | 20 Edward Gilchrist.     |
| 3 Thomas L. Smith.     | 21 John A. Lockwood.     |
| 4 George Blacknall.    | 22 Daniel C. M'Leod.     |
| 5 H. N. Glentworth.    | 23 Lewis W. Minor.       |
| 6 Lewis B. Hunter.     | 24 Wm. J. Powell.        |
| 7 John C. Spencer.     | 25 J. Frederick Sickels. |
| 8 George Clymer.       | 26 N. C. Barrabino.      |
| 9 Isaac Brinkerhoff.   | 27 Henry S. Reynolds.    |
| 10 Wm. Maxwell Wood.   | 28 M. G. Delaney.        |
| 11 John Vaughan Smith. | 29 Wm. F. M'Clenahan.    |
| 12 Jones W. Plummer.   | 30 Wm. L. Van Horn.      |
| 13 George B. M'Knight. | 31 Daniel S. Green.      |
| 14 Solomon Sharp.      | 32 James C. Palmer.      |
| 15 Daniel Egbert.      | 33 Ninian Pinkney.       |
| 16 Amos G. Gambrell.   | 34 Robt. T. Barry.       |
| 17 W. A. W. Spotswood. | 35 Charles A. Hassler.   |
| 18 Jonathan M. Foltz.  | 36 David Harlan.         |

The youngest surgeon on this list of those who rank with lieutenants, has been nearly twelve years in the navy, and under the order must serve upwards of eleven years more, before he will rank as a commander.

# INDEX.

## A

Abdomen, wound of with expulsion of intestines, 379.  
 Absorption of narcotic substances, by lymphatics, 464.  
 Acidity, its causes and varieties, 493.  
 Aconite, Fleming on, 189.  
 Accumulation of medicines in the system, 211.  
 Address relative to the objects of the National Medical Association, 533.  
 Air, composition of, at different heights, 532.  
 Aldridge, treatment of urinary diseases, 211.  
 Alcohol, digestion of, 531.  
 Allen's report of Kentucky Lunatic Asylum, 187.  
 Alessi, entozoon in eye, 517.  
 Alkaline remedies, abuse of, 220.  
 Amaurosis from concussion of retina, 518.  
 Ambrosia trifida as a remedy for mercurial salivation, 382.  
 Amenorrhœa, tincture of water pepper in, 279.  
 Ammonia in asthma, 502.  
 Amorphous quinine, 207.  
 Amputation, excessive hemorrhage, 541.  
 Anatomical specimens, preservation of, 195.  
 Andry on diagnosis of diseases of the heart, 171.  
 Aorta, bifurcation of, 197.  
 Aphonia, pathology and treatment of, 494.  
 Apoplexy in fœtus and new-born child, 225.  
 Arsenic in intermittent fever, 223.  
 Arteries, treatment of wounds of, 512.  
 Asthma, ammonia in, 502.  
 Atlee, protracted gestation, 535.  
 Auscultation, results of, at a triple birth, 520.  
 Austin, new test for prussic acid, 528.  
 Autoplastic operation for contracted cicatrix from burn, 515.  
 Axillary artery, ligature of subclavian for wound of, 235.

## B

Baldwin on trismus nascentium, 353.  
 Basilic vein, ligature of, 509.  
 Bavoux, polypi of female urethra, 237.  
 Bed-sores, means of preventing, 477.  
 Behrend, periodical nocturnal cough of children, 226.  
 Bellingham, latent endocarditis, with large deposit of lymph on valves of the heart, 215.  
 Bell's report of M'Lean Asylum, 184.  
 Bennett, minute structure and chemical composition of tubercle, 484.  
 Bernard, digestive powers of stomach, 463.  
 Bidder, minute anatomy of kidneys, 465.  
 Bile, presence of copper and lead in, 466.  
 Bischoff, absorption of narcotic substances by lymphatics, 464.

Bird, excessive secretion of ammonio-magnesian phosphate by the kidneys, 487.  
 Bishop, aphonia, 494.  
 Blackman, reduction of strangulated hernia in masse, 336.  
 Bladder, inverted, displacement of, 197.  
 Bladder, labour complicated with prolapsus of, 372.  
 Blighted fœtus, 309.  
 Blood in bodies killed by strangling, 531.  
 Boling on remittent fever, 18.  
 Boston, statistics of, 177.  
 Boudin, arsenic in intermittent fever, 223.  
 Bouchardat and Sandras, digestion of alcohol, 531.  
 Bowditch, the young stethoscopist, 171.  
 Brain, disease of, following ligature to carotid, 233.  
 Bresciana, lithotomy, 239.  
 ——— mode of curing obstinate ulcers, 251.  
 ——— extraction of ball from orbit, 259.  
 Bretonneau, intermittent fever, 219.  
 Brigham's report of Utica Insane Asylum, 181.  
 Bright's disease of kidneys, diagnosis of, 477.  
 ———, treatment of, 480.  
 Bricheteau, diseases of workmen engaged in making lucifer matches, 528.  
 Brooke, upas tree, 530.  
 Brocchieri water, supposed hemostatic properties of, 146, 542.  
 Buck, amputation of elbow-joint, 544.  
 Burn, contracted cicatrix from, 515.  
 Burrows, disorders of cerebral circulation, reviewed, 391.  
 Burwell, case of placental presentation, 144.  
 Butler, injury of foot, 541.  
 Butcher, death from puncture of membrana tympani, 250.

## C

Calomel in urethral discharges, 238.  
 Calculi of prostatic gland, 239.  
 Campbell, epilepsy, 370.  
 Cappa, blood in bodies killed by strangling, 531.  
 Capitaine, hydrate of lime in diarrhœa, 223.  
 Carpenter, Elements of Physiology, 179.  
 Cardiac disease, slow pulse from, 218.  
 Carotid, disease of brain following ligature of, 233.  
 ——— ligature of, 233, 234.  
 Cazenave on lithotritry, 240.  
 Carbonic acid, removal of from wells and cellars, 262.  
 Carden, autoplastic operation for contracted cicatrix from burn, 515.  
 Cæsarian section, 386.  
 Catheter left in bladder, 540.

Cell development, theory of, 462.  
 Cephalæmatoma, 225.  
 Cerebral circulation, disorders of, 391.  
 Chaumet, resection of clavicle, 506.  
 Chervil in ophthalmia, 258.  
 Chemical fumes, death from inhaling, 380.  
 Children, periodical nocturnal cough of, 226.  
 Chomel, erysipelas of the head, 227.  
 Chorea, sanicula Marilandica in, 374.  
 Ciccone, blood in bodies killed by strangling, 531.  
 Clavicle, resection of, 506.  
 Clay, retained placenta, 260.  
 Clymer on Fevers, 190.  
 Colour, change of, in a negro, 13.  
 Colic, painter's, 224.  
 Columbia Insane Hospital report, 454.  
 Constipation, efficacy of large purgative clysters in, 222.  
 Corpus callosum, absence of, 466.  
 Coote, melanosis, 516.  
 Coxe's translation of writings of Hippocrates and Galen, 452.  
 Crampton on lithotrity, 240.  
 Crosse, inverted displacement of bladder, 197.  
 Croton oil in delirium tremens, 222.  
 Cyanosis, congenital, 501.  
 Cyst, serous, in rectus abdominalis, 503.

## D

Davis, imperfect condition of os uteri and malformation of vagina, 525.  
 Death, new sign of, 262.  
 Debrou, displacement of lower fragment in fracture of surgical neck of humerus, 504.  
 Delaracherie, œsophagotomy, 513.  
 Delivery, natural period of, 29.  
 Delirium tremens, croton oil in, 222.  
 Desaussure, melanosis of eye, 276.  
 Deval, chervil in ophthalmia, 258.  
 Diagnosis, physical, 171.  
 Diarrhœa, hydrate of lime in, 223.  
 Dick, acidity, 493.  
 Digestive powers of stomach, 463.  
 Doepp, cephalæmatoma, 225.  
 Duncan, ulceration of gums of children, 475.  
 Dunglison's Human Physiology, 458.

## E

Eager, ovarian dropsy, 249.  
 Ear, hemorrhage from following suppressed menses, 486.  
 Ebrard, new method of employing muriate of morphia in neuralgia and toothache, 221.  
 ——— ioduret of potassium ointment in rheumatism, 225.  
 Ectrotic treatment of small-pox, 547.  
 Edward, hydrocephalus, tapping, 511.  
 Elbow-joint, unusual injury of, 514.  
 ———, excision of, 544.  
 Endocarditis, latent, with large deposit of lymph on valves of the heart, 215.  
 Epilepsy, 370.  
 Ergotine, hemostatic power of, 542.  
 Erysipelas of the head, 227.  
 ———, phlegmonous, 229.  
 Evans and Worthington's report of Frankford Asylum, 188.

Extra-uterine sœtation, 279.  
 Eye, gum-Arabic and iodine, for removal of foreign bodies from, 257.

## F

Faucille, removal of carbonic acid from wells and cellars, 262.  
 ———, means of neutralizing exhalations of sulphuretted hydrogen, 532.  
 Fevers, quinine in, 65.  
 Fleming on aconite, 189.  
 Fleury, treatment of wounds of arteries of superior extremity, 512.  
 Ford, catheter left in bladder, 540.  
 Forman, injury of head, 54.  
 Furnival, diagnosis of diseases of mitral valve, 497.

## G

Galvanism, influence of on action of uterus during labour, 521.  
 Gardiner, labour complicated with prolapsus uteri, 387.  
 Gastrotomy, 279.  
 Gerhard, diseases of the chest, 171.  
 Gerdy, phlegmonous erysipelas, 229.  
 Gestation, protracted, 535, 536.  
 Gibson, labour impeded by fractured sacrum, 521.  
 Giehl, substernal goitre, 230.  
 Gibbs, wound of abdomen, expulsion of intestines, 379.  
 Gintrac, arsenic in intermittent fever, 224.  
 Girard, influence of milk of nurse on health of child, 474.  
 Glycerine in squamous eruptions, 222.  
 Goblin, mercurial frictions in small-pox, 225.  
 Goodsir, supra-renal; thymus and thyroid bodies, 195.  
 Goitre, substernal, 230.  
 Gruber, anatomy of middle thyroid and crico-thyroid arteries, 193.  
 Grynfeltt, new method of applying taxis, 509.  
 Guerdard, ammonia in asthma, 502.  
 Gums of children, ulceration of, 475.

## H

Hall, case of congenital ptosis, 143.  
 ———, efficacy of large purgative clysters in constipation, 222.  
 Harden on isopathia, 87.  
 Hartford Retreat report, 455.  
 Hasse, description of tubercle, 469.  
 Head, injury of, 54.  
 ———, erysipelas of, 227.  
 Heart, inflammation of, 215.  
 ——— hypertrophy of, 216.  
 Heller, urostelith, 202.  
 ———, copper and lead in bile, 466.  
 ———, analysis of urine in dropsy, 467.  
 ———, use of nitrate of silver, 476.  
 Hemorrhage, excessive after amputation, 541.  
 Hemostatic powers of Brocchieri water, 146, 542.  
 Hemeralopia, cauterization of cornea for, 257, 258.  
 Hippocrates and Galen, writings of, 452.  
 Herndon, Cæsarian section, 386.  
 Hermann, obstetrical forceps, 448.  
 Hernia, vagino-labial, 505.

- Hernia, injurious effects of pressure in, 510.  
Hess, mechanical ophthalmology, 451.  
Holmes, quinine in diseases of Florida, 260.  
Howell, case of placenta prævia, 259.  
Hudson, labour with prolapse of bladder, 372.  
———, separation of bones of pelvis, 373.  
Hughes on auscultation, 171.<sup>7</sup>  
Humerus, excision of head of, 235.  
———, removal of part of head of, 330.  
———, displacement of lower fragment in fracture of surgical neck of, 504.  
———, new variety of dislocation of, 504.  
Hutchinson, capacity of lungs, 469.  
Hydrocele of tunica vaginalis, 334.  
Hydrate of lime in diarrhœa, 223.  
Hydrocele in rectus abdominalis, 503.  
Hydrocephalus, tapping, 511.  
Hymen, imperforate, 139.
- I
- Insane Hospital reports, 181, 453.  
Insane, meeting of association of medical superintendents of, 273.  
Intermittent fever, curative medication of, 219.  
———, arsenic in, 223.  
Iodide of iron, new method of making, 468.  
Iodine, for removal of iron from eye, 516.  
———, tincture of, in small-pox, 547.  
Ioduret of potassium ointment in rheumatism, 225.  
Iron in eye, removed by iodine, 516.  
Isopathia, 87.
- J
- Jameson, poisonous fish, 530.  
Johnson, varicose aneurism, treated by pressure, 378.
- K
- Kemp, nitrogen in elementary substances, 202.  
Kentucky Lunatic Asylum, report for 1845, 180.  
Kidney, minute anatomy of, 465.  
Kirkpatrick, ovarian cyst, 249.  
Kirkbride's report of Pennsylvania Hospital for Insane, 180.  
King, hypertrophy of heart, 216.  
Kop, new method of making iodide of iron, 468.
- L
- Labour complicated with prolapsus uteri, 387.  
Laborie, incision of neck of uterus, for contraction of that organ, 518.  
Labour, impeded by fractured sacrum, 521.  
———, influence of galvanism on action of uterus during, 521.  
———, with imperfect condition of os uteri, 525.  
Lane, small-pox and vaccination, 119.  
Larynx, anatomy of, 141.  
Lasserre, apoplexy in fœtus and new-born child, 225.  
Lawrence, melanosis of eye, 252.  
Lanza, thoracic percussion, 223.  
Lead, action of on animal body, 443.  
Leloutre, prolapsus of uterus, 506.  
Le Ray, natural period of delivery, 529.
- Le Ray, amorphous quinine, 207.  
Leidy, anatomy of human larynx, 141.  
Lenoir, calculi of prostate gland, 239.  
Lewis, opinion in cases of protracted gestation, 536.  
———, opinion as to whether a physician has a right to compensation for making post-mortem examination at inquests, 538.  
Liebig, formation and discrimination of urinary calculi, 198.  
Life, 193.  
Liston's Surgery, 192.  
Liston, wounded arteries, secondary hemorrhage, false aneurism, 231.  
Lithotomy, 239.  
Lithotripsy, 240, 263.  
Lippich, sulphate of iron for excessive perspiration, 469.  
Lopez, blighted fœtus, 309.  
Lower jaw, excision of a portion of, 335.  
Lucifer matches, diseases of workmen employed in making, 525.  
Lungs, structure of, 194.  
——— capacity of, 469.
- M
- Mackenzie, ligature of subclavian, 235.  
Maine Insane Hospital Report, 453.  
Mans, case of death from inhaling chemical fumes, 380.  
Maryland Hospital Report, 456.  
Martin, mercurial ointment for panaris, 510.  
Mayne, serous cyst in rectus abdominalis, 503.  
Mayor, calomel in urethral discharges, 229.  
Massachusetts State Lunatic Asylum, report for 1845, 184.  
M<sup>l</sup>Lean Asylum for the Insane, report for 1845, 184.  
Medical schools of the U. S., 279.  
———, statistics of, 546.  
Melanosis of eye, 252, 276, 516.  
Membrana tympani, death from puncture of, 250.  
Menses, hemorrhage from ear following suppressed, 486.  
Mendenhall, quinine in fevers, 65.  
———, effects of quinine on the pulse, 79.  
Mercurial frictions in small-pox, 225.  
Mercurial salivation, cure for, 382.  
Metcalf, imperforate hymen, 139.  
Mialhe, accumulation of medicines in the system, 211.  
Miller's Surgery, 191.  
Milk, influence of on health of child, 474.  
Mitral valve, diagnosis of disease of, 497.  
Montain, oxalis crassicaulis as an astringent, 207.  
Monster, double-headed, 80.  
Mount Hope Institution, report for 1845, 183.  
Murder, what constitutes the intent to commit, 529.  
Muriate of morphia, new method of employing for neuralgia and toothache, 221.
- N
- Naegele, results of auscultation at a triple birth, 520.

- Narcotics, absorption of by lymphatics, 464.  
 National Medical Convention, 266.  
 Naval Surgery, 423.  
 Navy, general order, 457.  
 Necrosis of bones of face, 527.  
 Negro, change of colour in, 13.  
 New York State Lunatic Asylum, report for 1845, 181.  
 Neuralgia, new method of employing muriate of morphia for, 221.  
 New Hampshire Asylum for Insane, report of, 457.  
 Neuroplasty, 488.  
 Nitrate of silver, use of, 476.  
 Nogaro, ophthalmia caused by larvæ; 258.  
 Nurses, rheumatic paralysis in, 222.
- O.
- Obstetrical forceps, 448.  
 Œdema of new-born infants, 474.  
 Ogier, tincture of water pepper in amenorrhœa, 279.  
 Ophthalmia from larvæ, 258.  
 ——— chervil in, 258.  
 Ophthalmology, mechanical, 451.  
 Orbit, musket ball in orbit 24 years, 259.  
 Organizing, capacity for, 193.  
 Œsophagotomy, 513.  
 Ovarian dropsy, cured, 249.  
 ——— cysts, paracentesis, 249.  
 Oxalis crassicaulis as an astringent, 207.
- P
- Page, hemeralopia, 258.  
 ———, amaurosis from concussion of retina, 518.  
 Painters' colic, 224.  
 Panaris, mercurial ointment for, 510.  
 Pebbles, intestinal obstruction, 376.  
 Pelvis, separation of bones of during labour, 373.  
 Pennsylvania Asylum for Insane, report for 1845, 180.  
 Perspiration, excessive, sulphate of iron for, 469.  
 Pfeiffer, double-headed monster, 80.  
 Phillips on Scrofula, 149.  
 ———, spermatic discharges, 506.  
 Physicians, their right to compensation for post-mortem examination, 535.  
 Pigne, preservation of specimens of morbid anatomy; 195.  
 Pinkey, removal of one-third of the head of humerus, 331.  
 ———, disarticulation at shoulder joint, 332.  
 ———, hydrocele of tunica vaginalis, 334.  
 ———, excision of portion of lower jaw, 335.  
 Placenta, intra-uterine perforation of, 260.  
 ——— retained, 260.  
 Plague and quarantine question, 260.  
 Placental presentation, 144, 259.  
 Pneumonia, treatment of, 490.  
 Polypi of female urethra, 237.  
 Polygonum hydropiperoides, tincture of, in amenorrhœa, 279.  
 Poisonous fish, 530.  
 Posterior nares and palate, new muscle of, 194.  
 Pregnancy, early, 547.
- Prostate gland, calculi of, 239.  
 Prolapsus of uterus, pregnancy during, 524.  
 Prussic acid, new test for, 528.  
 Ptosis, case of congenital, cured, 143.  
 Pulse, effects of quinine on, 29.  
 Purefoy, means of preventing bed-sores, 477.
- Q
- Quarantine, 260.  
 Quinine in fever, 65.  
 ———, effects of on pulse, 79.  
 ——— in diseases of Florida, 297.
- R
- Rainey, structure of lungs, 194.  
 Randolph, case of lithotripsy, 263.  
 Rees, diagnosis of Bright's disease of kidneys and its relations to albuminous urine, 477.  
 Registrar-General's fifth annual report, 427.  
 Reiniger, iodine for removal of sparks of iron from eye, 516.  
 Remittent fever, Boling on, 18.  
 Retina, amaurosis from concussion of, 518.  
 Rheumatic paralysis in nurses, 222.  
 Rheumatism, ointment of ioduret of potassium in, 225.  
 Ripault, new sign of, 262.  
 Robertson, remedy for mercurial salivation, 382.  
 Roger, œdema of new-born infants, 474.  
 Rognetta, erysipelas of head, 227.  
 Roser, new variety of dislocation of humerus, 504.  
 Roussilhe, cauterization of cornea for hemeralopia, 257.  
 Roussel, diseases of workmen engaged in making lucifer matches, 527.  
 Rumpelt on action of lead on the animal body, 443.
- S
- Sacrum, fractured, impeding labour, 521.  
 Saliva, sulpho-cyanogen in, 529.  
 Sandras, painters' colic, 224.  
 Sanicula Marilandica in chorea, 374.  
 Savage, desquamation and change of colour in a negro, 13.  
 Schonlein on urine in typhus, 202.  
 Schlossberger and Kemp, proportion of nitrogen in alimentary substances, 202.  
 Scrofula, Phillips on, 149.  
 Sedillot, diseases of workmen engaged in making lucifer matches, 527.  
 Serres, neuroplasty, 488.  
 Shattuck, statistics of Boston, 177.  
 Shoulder-joint, disarticulation at, 332.  
 Simpson, influence of galvanism on action of uterus, 521.  
 Simon's Animal Chemistry, 426.  
 Slow pulse from cardiac disease, 218.  
 Small-pox and vaccination, statistics of, 119.  
 ———, mercurial frictions in, 225.  
 ———, ectrotic treatment of, 547.  
 Small Books on Great Subjects, 459.  
 Smith, unusual injury of elbow-joint, 514.  
 Spermatic discharges, 506.  
 Spitta, congenital cyanosis, 501.  
 Spinal cord, general physiology of, 461.  
 Spontaneous combustion, 530.  
 Squamous eruptions, glycerine in, 222.

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