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Analytical and Critical Reviews.

I.—Thudichum on the Chemistry of the Brain.[1]

BEFORE attempting to analyse and criticise the very elaborate article in which Dr. Thudichum has incorporated the results of his laborious researches on the chemical composition of the brain, we shall endeavour to give our readers as clear and accurate an account as we can of the state of our knowledge on this subject at the time when Dr. Thudichum commenced his work, pointing out as briefly as we can how much is due to the chief workers who immediately preceded him. At the same time our endeavours will be directed rather to giving the reader some knowledge of the subject of brain chemistry than to furnish him with a complete historical account of *all* that has been written in reference to it.

Of the early writers on the chemistry of the brain we mean to say very little, but would refer the reader to the really full and accurate historical account which Dr. Thudichum has given of previous researches.

We may merely mention that Vauquelin was the first to show that alcohol extracts from the brain a fatty substance which separates when the solution is cooled and which contains phosphorus as an essential element. He was followed by Gmelin, who was the first (in spite of Couerbe's attempt to claim the discovery for himself) to prove the existence of cholesterin in the brain; by Couerbe, who, in a memoir which, we think, Dr.

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[1] *Researches on the Chemical Constitution of the Brain.* By J. L. W. THUDICHUM, M.D. Reports of the Medical Officer of the Privy Council and Local Government Board. New series, No. III.

Thudichum greatly overrates, announced that the fatty matters of the brain are far more numerous than Vauquelin had imagined, and who by the names of stearocenote, cephalote, éléencéphol and cerebrote, designated various impure products which he extracted from brain by the action of solvents.[1] Couerbe's researches were followed by those of Frémy.[2] This writer criticised, and, as we think, very justly, the attempts of Couerbe to establish his brain-products in the position of proximate principles. A definite proximate principle must, M. Frémy reasons, always possess the same chemical composition, and analysis, therefore, affords one of the best means of establishing the purity of a substance. Couerbe analysed his bodies and obtained discordant results; but instead of concluding from these that he was dealing with impure substances, with mixtures rather than with isolated principles, he assigned to the brain constituents a certain mutability of composition or a modification of composition corresponding to their physiological functions. Frémy's own researches led, however, to the most unsatisfactory results; he states the brain constituents to be—

1st. A white matter which he terms cerebric acid.

2nd. Cholesterin.

3rd. A particular acid fat which he terms oleo-phosphoric acid; and

4th. Traces of oleine, margarine and fatty acids.

These researches of Frémy must be stated to have led to no other results than to confirm the previous statements as to the brain containing phosphorus in the form of an organic compound, and to establish that similar bodies are contained in the nerves and in the liver.

Gobley[3] now engaged in a series of researches on the substances contained in the yolk of egg, in the brain, and in the milt of the carp, and extracted from them by boiling with alcohol and ether two phosphorized principles, to one of which he applied the name of lecithine (from λεκιθος, pulse-porridge, presumedly because of its glutinous character when moist and impure), and to the other that of cerebrine; the latter body, viewed by the light of subsequent researches, appears to have been certainly no definite proximate principle, but

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[1] "Du Cerveau, considéré sous le point de vue chimique et physiologique." Par J. P. COUERBE. Présenté à l'Académie des Sciences' le 30 Juin, 1834. 'Annales de Chimie et de Physique,' 1834, p. 160-193.

[2] "Recherches sur le Cerveau," par M. EDMOND FREMY (Mémoire lu à l'Académie des Sciences'). 'Annales de Chimie et de Physique,' 1841, p. 463-488.

[3] 'Journal de Pharm. et de Chimie,' t. ix, 1846, p. 1, 83, 161; t. xi, p. 409; t. xii, p. 5; t. xvii, 1850, p. 401; t. xviii, p. 107; t. xix, 1851, p. 406; t. xxi, p. 241.

a mixture of a substance not containing phosphorus with lecithine.

Gobley, however, was the first to give clear and correct information as to the phosphorized body which he had separated. His lecithine was little soluble in cold, but abundantly soluble in hot alcohol, from which it was deposited on cooling. This lecithine from yolk of egg, when ignited, left an acid charcoal, the residue containing phosphoric acid. When boiled with water it did not decompose, but under the influence of weak acids and heat it yielded margaric and oleic acids, and glycerin-phosphoric acid. Gobley subsequently corroborated the statements of Frémy by discovering the phosphorized constituents in blood and in bile.

In spite of the immense labour of the investigations which had been made in the interval, we must look upon Gobley's identification of lecithine by the study of some of its chief products of decomposition as the one important step made in the study of brain chemistry since the day when Vauquelin first announced the discovery of his white fatty matter. The experiments of John, of Couerbe, of Frémy, may, for any facts with which they have enriched science, be left out of consideration except by the actual investigator, who in the fruitless experiments of his predecessors in research may often find precious guides in his work.

After Gobley's work on the phosphorized principles of brain came the researches of Liebig, of Von Bibra, and of Müller. These researches added nothing to the knowledge which had been furnished by Gobley of the phosphorus-containing body or bodies of brain, but they succeeded in demonstrating that brain contains many of the proximate principles which are found in the juice of flesh, as creatine, xanthine, hypoxanthine, and inosite. To these the attention of the reader will be again called.

In 1865 Liebreich, who was then assistant to Professor Hoppe-Seyler, at Tübingen, published a paper entitled " Ueber die Chemische Beschaffenheit der Gehirn-Substanz." In this paper he stated that the various bodies which had by various writers been designated cerebrine, cerebric acid, lecithine, or phosphorized fats, did not exist as immediate principles of brain, but that from the brain could be extracted by an easy process a body to which, as indicating its probable importance, he assigned the name of Protagon ($\pi\rho\omega\tau\alpha\gamma o\varsigma$, the leader).

In order to obtain this substance the following process was followed. An animal was bled to death from the carotid, and a stream of water was allowed to flow through the vessels of

[1] 'Annalen der Chemie und Pharmacie,' Bd. cxxxiv, 1865·6, p. 29-44.

the head so as to remove all blood-colouring matter. The brain was then removed, freed from its membranes, pounded in a mortar to the condition of a pulp, and then shaken in a flask with ether and water. It was allowed to stand at a temperature of 0° until the ether had separated.

The ether and water were removed as far as possible by filtration from the insoluble brain matter, which was then treated with 85 per cent. spirit and heated in a water bath to 45° C. The fluid was filtered through a hot funnel, and the filtrate cooled to 0° C. A flocculent precipitate then separated, which was collected on a filter and treated with cold ether until it ceased to dissolve any cholesterine. The insoluble mass was dried in vacuo, and dissolved in spirit at 45° C. From the alcoholic solution protagon separated in microscopic needles, differing a little in arrangement and form according to the concentration of the solution. The substance admitted of being crystallized over and over again. The results of nine carbon and hydrogen determinations were remarkably concordant, and the same remark applies to three nitrogen and three phosphorus determinations. The results of these analyses led Liebreich to give to the new body the formula $C_{116}H_{241}N_4O_{22}P$.

Below is exhibited the composition of Liebreich's protagon according to this formula :

					Theory.	Found.
C	.	.	.	.	67·21	66·74
H	.	.	.	.	11·59	11·74
N	.	.	.	.	2·70	2·80
P	.	.	.	.	1·50	1·23
O	.	.	.	.	17·00	17·49
					100·00	100·00

According to Liebreich protagon is soluble with difficulty in cold but more easily in warm alcohol and ether. At higher temperatures than 55° C. alcohol appears to decompose protagon. In water protagon swells and presents the appearance of an opaque jelly, ultimately dissolving so as to form an opaque solution. Liebreich found that protagon was soluble in glacial acetic acid, from which it was deposited, if subjected to the long-continued action of cold, in a crystallized form.

By far the most important of the observations made by Liebreich had reference to the products of decomposition of protagon. He found that when boiled with a solution of barium hydrate for twenty-four hours it yielded as products of decomposition glycerine-phosphoric acid, fatty acids of which he isolated in a state of approximate purity stearic acid, and a base to which he gave the name of neurine, and to the platinum compound of which he ascribed the formula $C_5H_{14}NCl_3Pt$.

This base was afterwards, by the researches of Dybkowsky,[1] shown to be identical with choline, which had already been separated from bile by Strecker.[2]

Diakonow,[3] another pupil of Hoppe-Seyler's, had undertaken the examination of the phosphorized proximate principle of the yolk of eggs, which had received the name of lecithine from Gobley. His investigations showed that this body yielded the same products of decomposition as Liebreich's protagon, but that it was richer in phosphorus. According to him, lecithine has the following per-centage composition :[4]

C	.	.	.	.	.	.	.	64·27
H	.	.	.	.	.	.	.	11·4
N	.	.	.	.	.	.	.	1·8
P	.	.	.	.	.	.	.	3·8

And from these numbers he derived the formula for lecithine $C_{44}H_{90}NPO_9$.

When heated with a solution of barium hydrate, lecithine, according to Diakonow, yielded as products of decomposition glycerine-phosphoric acid, stearic acid, and neurine, according to the following equation .

$$\underbrace{C_{44}H_{90}NPO_9}_{\text{Lecithine.}} + 3H_2O = \underbrace{C_3H_9PO_6}_{\substack{\text{Glycerine-phos-}\\\text{phoric acid.}}} + 2\underbrace{(C_{18}H_{36}O_2)}_{\substack{\text{Stearic}\\\text{acid.}}} + C_5H_{15}NO_2$$

The lecithine which he had at first obtained from the yolk of eggs was now obtained by Diakonow from brain.[5] This organ was, after repeated treatment with ether, digested at a temperature of 40° C. with absolute alcohol. The alcoholic solution was then cooled to 0° C., when a precipitate separated which was collected on a filter, washed with a little cold absolute alcohol, and treated with ether. The ethereal solution (containing all the lecithine) was evaporated to dryness, and the residue dissolved in absolute alcohol at 40 C. On being cooled to —7° C. or —10° C. the solution deposited lecithine.

Lecithine, according to Diakonow, was distearyl-glycerine phosphate of neurine.

Strecker, who had already in his study of the lecithine of bile come to the conclusion that this body was a derivative of glycerine-phosphoric acid, in which two atoms of hydrogen had

[1] Dybkowsky, "Ueber die Identität des Neurin und des Cholin" ('Erdmann's Journal,' Bd. c, s. 153).

[2] 'Annalen der Chemie und Pharmacie,' Bd. 123, 356).

[3] Diakonow, "Ueber die Phosphorhaltigen Körper der Hühner und Storeir Medicinisch-Chemische Untersuchungen herausgegeben von HOPPE-SEYLER. 2 Heft. Berlin, 1867.

[4] 'Centralblatt f. d. Med. Wissenschaften,' 1868, No. 1.

[5] 'Centralblatt,' 1868, No. 7.

been replaced by the radicals of fatty acids,[1] now wrote a paper[2] on the constitution of lecithine. In this paper he homologated the opinion of Diakonow that Liebreich's protagon is lecithine mixed with a body containing no phosphorus (almost certainly Müller's cerebrine); but stated that his own experiments on lecithine from the yolk of eggs had shown him that this body yields not only stearic acid, but oleic acid and palmitic acid. Just as there are several fats and several mixtures of fats, so Strecker contended there may be and are many lecithines.

Diakonow had looked upon lecithine as a neurine salt of a glycerine-phosphoric acid, in which two of the hydrogens of the hydroxyl groups had been replaced by the radical of stearic acid. Now, Strecker suggested that these two hydrogens might be replaced by two different fatty-acid radicals, or by one radical of a fatty acid and one of oleic acid; and further, he maintained that in all probability neurine was not a salt of distearyl-glycerine-phosphoric acid, but an ether-like compound of neurine and distearyl-glycerin-phosphoric acid.

The researches of Wurtz had established the constitution of neurine to be trimethyl-oxyethyl-ammonium hydrate. To the lecithine which he had separated from the yolk of egg Strecker assigned the rational formula—

$$C_3H_5 \begin{cases} O, C_{16}H_{31}O \\ O, C_{18}H_{33}O \\ O, PO \begin{cases} OH \\ O, C_2H_4N(CH_3) \\ \quad\quad\quad H^3 \end{cases} \end{cases} O.$$
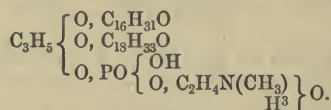
According to this view lecithine is glycerine-phosphoric acid:

$$C_3H_5 \begin{cases} OH^* \\ OH^* \\ O, PO \begin{cases} OH \\ OH \end{cases} \end{cases}$$
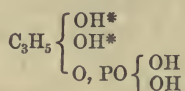
in which the two atoms of hydrogen in the hydroxyl group (marked by asterisks in the above formula) are replaced by the radicals of oleic and palmitic acids; whilst choline, a compound which is at the same time an ammonium base and an alcohol, forms with glycerine-phosphoric acid an acid compound ether. Lecithine is, therefore, at the same time a fat, a base, and an acid.[3]

We may now, that we have alluded a little in detail to some of the more salient points in the history of the phosphorized principles of the brain prior to the time when Dr. Thudichum

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[1] 'Ann. Ch. Pharm.'
[2] "Ueber das Lecithin," 'Zeitschrift für Chemie,' von Beilstein, Vittig, und Hübner. 1868, p. 437.
[3] See Schorlemmer's 'Chemistry of Carbon Compounds,' p. 259.

commenced his researches, summarise the whole of the positive knowledge which had been accumulated on brain chemistry.

It had been shown by many observers that the grey matter of the brain was richer in water than the white ;[1] that the phosphorized organic constituents are more abundant in the grey matter than in the white, whilst the latter contains the greater part of the cholesterin. The albuminous matters of the brain had been examined (Hoppe-Seyler) and shown to contain a casein-like body, in addition to the gelatigenous constituents of the neuroglia. Many of the substances which had been looked upon as characteristic of muscle metamorphosis, specially inosit and creatine, had been proved to be regular and not insignificant constituents of brain matter, whilst from the mass of bodies which, under the different names of white fatty matter (Vauquelin), cerebrine (Couerbe), cerebric acid (Frémy), protagon (Liebreich), various writers had described as containing phosphorus, one had been sufficiently studied to give us a clear idea of its constitution ; chemists had been able, as a result of their researches, to arrive at the conclusion that one, at any rate, of the mysterious phosphorized bodies of the nervous system was a fat, bearing so close a relationship to the well-known fats of the body that without any great stretch of the imagination its formation in the economy might be conceived.

And here, leaving for the moment our purely critical functions, let us pause to consider whether it *is* conceivable that such a body as lecithine can be built up in the animal organism. It is usual to speak of the chemical operations of the animal organism as being operations in which complex are broken up into more simple bodies, and on the whole this is true ; the processes of disintegration which go on in the body are far more obvious and on a more obvious scale than those of integration, yet processes of integration—true syntheses—do occur even in the animal body. How else could we explain the building up of iron into the complex molecule of hæmoglobin? But actually syntheses are known to occur; the animal swallows benzoic or chinic acids, and linking the aromatic nucleus to glycocine forms hippuric acid ; or, to take an illustration which is of greater use to us here, the animal can assimilate a soap and convert it into a fat, *i.e.* may link the radicals of fatty acids to glycerin.

Similarly, it is most conceivable that the animal may actually form the fat called glycerin-phosphoric acid ; and, assuming this, there is no difficulty in conceiving of the further production of lecithine.

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[1] Petrowski, 'Archiv f. gesammte Phys.,' Bd. vii, p. 367.

Perhaps we shall have noticed all that is worth noting as having been definitely ascertained before Dr. Thudichum's research, if we say that Müller had, by the action of baryta water and heat on the substance of brain, obtained a body called cerebrine of apparently definite composition, having the formula $C_{17}H_{33}NO_3$, and capable of explaining by its admixture in various proportions with lecithine many of the discrepant results of the investigators who preceded Diakonow and Strecker.

The survey which we have made of the work done by Dr. Thudichum's predecessors in research will have stimulated the curiosity of the reader, who will feel anxious to know how far their discrepant statements could be reconciled.

Is lecithine the only phosphorised ingredient of brain? Does no such crystalline phosphorized principle as Liebreich's protagon exist? Is Müller's cerebrine actually a proximate principle of brain, or nothing more than a product of the action of barium hydrate on certain organic constituents? These and many other similar queries naturally suggest themselves as we commence the study of the elaborate investigation which Dr. Thudichum has carried on.

But when we have read Dr. Thudichum's formidable paper we find that none of our doubts are solved. Dr. Thudichum's paper bristles with new names for old facts, and with the names of numberless new substances which the author discovered at each step of every investigation. Every substance, however impure, or rather however inconclusive the evidence of its purity, is subjected by Dr. Thudichum to organic analysis, every analysis furnishes the materials for a new formula, and every formula the excuse for a new name. No wonder, then, that in alphabetical list of chemical educts and products stated to have been found in or produced from the brain of man and animals there are eighteen marked with an asterisk, indicating that they " are believed to be now described for the first time as ingredients in brain matter."

These are, in alphabetical order—

Apomyeline,	Kephalic acid.
Base new, from kephaline,	Myeline,
Buttery matter,	Oily last, matter,
Cerebrinic acid,	Oleate of ethyl,
Glyceramine,	Oxykephaline,
Hydrothion,	Paramyeline,
Kephaline,	Peroxykephaline,
Kephaloidine,	Phrenosine,
Kerasine,	Pyrokephole.

We cannot pretend to give a detailed account of all Dr.

Thudichum's results, but we shall analyse with considerable care some of the chief of these.

Dr. Thudichum's researches are always conducted on a large scale. "As material for the present very large inquiry ox brains have in most part been preferred, and of these more than a thousand have been used."

The paper commences by an account of the methods used in the isolation of the various proximate constituents of the brain, followed by their classification. To this succeeds the description of certain chief principles which have been isolated. We shall select for special examination the author's research on myeline (p. 156).

The author commences by distinguishing his new body from the myeline of Virchow, which is a mixture of various substances.

Preparation of myeline.—Brains are washed in water, broken up, and kept in alcohol for some time. After hardening, the brains are triturated and heated for some time in 85 per cent. alcohol. The alcoholic fluid is cooled, whereby a crystalline and granular precipitate of so-called "white matter" is obtained. This is dissolved again in cold alcohol; the solution is concentrated and cooled until a precipitate is obtained, and the process is repeated until crystalline myeline is obtained. Other methods of preparation by precipitation with lead acetate and with platinum chloride respectively are described. In his remarks on the "differences and separation from other cerebral principles" the author states that "from lecithine myeline can be separated by cold absolute alcohol, in which this body is more soluble than myeline; also by cold ether, in which it is also more soluble than myeline." It has never occurred to the author that it would have been of the first importance to place this statement on a firm foundation, and that a research which appears to be of the most elaborate character might have included a determination of the relative solubilities of lecithine and myeline in alcohol and ether, even if such determination had not been absolutely required in order to disprove that which we believe all physiological chemists will conclude after reading Dr. Thudichum's research, viz. that *myeline is but impure lecithine.*

Before describing the reactions of myeline or the results of his analysis Dr. Thudichum devotes two pages to the consideration of the probable constitution of myeline, a consideration which might almost be imagined to have been facetiously as an intended parody on those chemists who are rather inclined to indulge their imagination too freely in their speculations on its constitution.

"Myeline contains," says the author, "four principal or proxi-

mate radicals, of which one is the radical compound itself, common to all the cerebral phosphorized substances, namely, glycero-phosphoric acid. To this is attached a nitrogenized radical, choline or neurine, and to these radicals are attached two fatty acid radicals, of varying value and composition, but most commonly found consisting of margaric acid, with 16C, or of the amidated form of this acid. Myeline combines with lead like a dibasic acid, with $PtCl_4$, and $CdCl_2$, like an ammonium base. From these facts we must attribute to its radicals positions which permit them to act alternately, if not together. These I have endeavoured to represent in the following diagram :

Substitution pole.

	R	
Acid pole.	R + P	R + N	Basic pole.
	R	

Substitution pole.
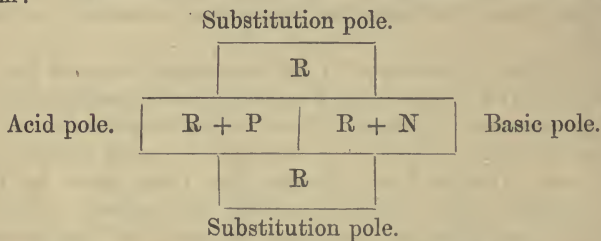
" We find sometimes myelines which contain more nitrogen than phosphorus in the proportion of 2 : 1, or 3 : 2, or 5 : 3. In these cases I assume the increase of the nitrogen to be due to the presence of an amidated fatty acid radical, such as I have proved by chemolysis to occur in the rarer forms of kephaline. To such amidated forms I attribute the following constitution :

R + Amide	
R + P	R + N
R	
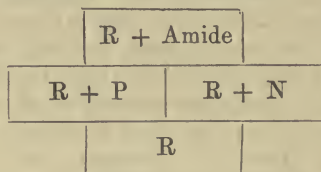
" This most simple form I have never directly isolated, but I am compelled to assume its existence by the observation of salts, which could only be explained by any of the following schemes :

R + Amide	
R + P	R + N
R	
R	
R + P	R + N
R	

$PtCl_4 2HCl.$
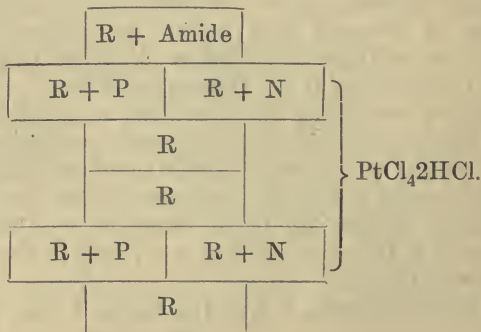
" Such a compound was sometimes united with $PtCl_4$ only, or with $PtCl_4$ and one HCl, or with $PtCl_4$ and 2 HCl, variations which made the preparations and analysis of these bodies extremely difficult and laborious.

" But I have also met with compounds which contained when fully expressed six molecules of myeline, of which four were amidated (or two twice amidated), and which require the following diagram for representation :

$$
\begin{array}{|c|c|}
\hline
\multicolumn{2}{|c|}{\text{R + Amide}} \\
\hline
\text{R + P} & \text{R + N} \\
\hline
\multicolumn{2}{|c|}{\text{R}} \\
\hline
\multicolumn{2}{|c|}{\text{R}} \\
\hline
\text{R + P} & \text{R + N} \\
\hline
\end{array} \Bigg\} PtCl_4
$$

$$
\begin{array}{|c|c|}
\hline
\multicolumn{2}{|c|}{\text{R}} \\
\hline
\multicolumn{2}{|c|}{\text{R}} \\
\hline
\text{R + P} & \text{R + N} \\
\hline
\multicolumn{2}{|c|}{\text{R + Amide}} \\
\hline
\multicolumn{2}{|c|}{\text{R + Amide}} \\
\hline
\text{R + P} & \text{R + N} \\
\hline
\end{array} \Bigg\} PtCl_4
$$

$$
\begin{array}{|c|c|}
\hline
\multicolumn{2}{|c|}{\text{R}} \\
\hline
\multicolumn{2}{|c|}{\text{R}} \\
\hline
\text{R + P} & \text{R + N} \\
\hline
\multicolumn{2}{|c|}{\text{R}} \\
\hline
\multicolumn{2}{|c|}{\text{R}} \\
\hline
\text{R + P} & \text{R + N} \\
\hline
\multicolumn{2}{|c|}{\text{R + Amide}} \\
\hline
\end{array} \Bigg\} PtCl_4
$$
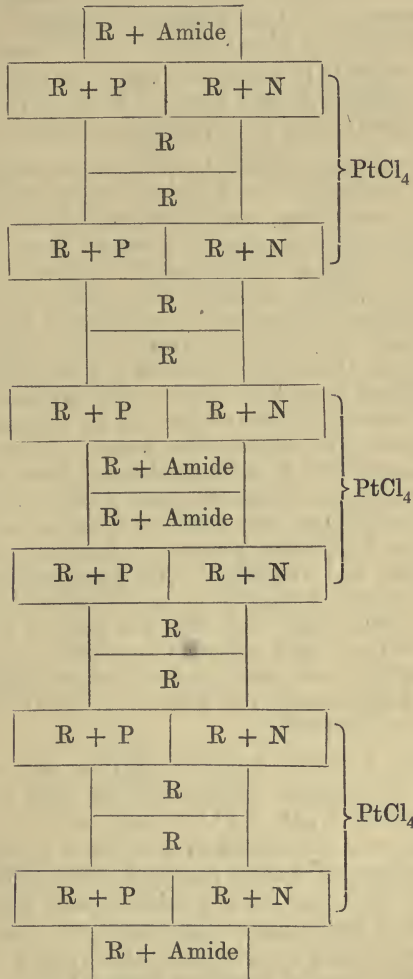
" I am yet doubtful whether the formula of myeline as above given must not be doubled, and whether the molecules are of equal value,

or if isomeric are not somewhat differently constituted. At least myeline, like kephaline, yields to chemolysis products in which the constituting radicals are presented in two different forms. The further study, confirmation, or refutation of the hypothesis I must reserve to the future."

After these misty speculations the author gives the results of his analyses of myeline compounds or of myeline. It must here be remarked that, although the author gives numerous analyses of various compounds of this supposed chemical individual, he never gives the results of two analyses of the same sample.

The first analysis is of a lead compound; the mode of obtaining this compound will give an idea of Dr. Thudichum's method of preparing the pure substances which he analyses.

" *Myeline-lead.* $C_{40}H_{73}PbNPO_{10}$.—The ether solution from white matter, after exhaustion by freezing, was precipitated by alcohol; the bulky precipitate was filtered, washed, and dried in vacuo, and during this process repeatedly pounded in a mortar. It was now dissolved in water and subjected to fruitless dialysis; it formed a thick, sticky, gummy, or starch-like emulsion, in which many small crystals formed like mica. The addition of watery Pb. acetate produced a dense curd, which separated easily from fluid; it was placed on a cloth filter, and allowed to drip over night. The precipitate was placed in alcohol and warmed, whereby little else but water was extracted (one litre alcohol left on evaporation to dryness a little brown matter). More warm strong alcohol now extracted much cholesterine and a little cerebrine. Hot boiling absolute alcohol extracted much cholesterine and a little yellow smeary lead-salt. The insoluble part was soft, waxy, but on cooling granular. It was now placed in ether, whereby a yellowish fluorescent lead-salt of kephaline was extracted. This latter salt was precipitated by absolute alcohol, deposited as a yellowish oily body, which became hard on standing. This has been treated under kephaline. A white pulverulent salt remained insoluble in the ether, was thoroughly washed with ether on the filter, also shaken with ether in a bottle, and again washed on filter. It shrunk much on drying. It was insoluble in benzole."

Then follows a single analysis of this salt, and from this analysis Dr. Thudichum deduces for the body combined with lead the formula $C_{40}H_{75}NPO_{10}$, and to it he assigns the name of myeline. Then follow a series of analyses either of separated myeline, or of some compound of myeline, such as the compounds of myeline with platinum and with chloride of cadmium; Dr. Thudichum's analyses show that no definite compounds with these salts exist. Calculating the amount of carbon in 100 parts of myeline from all his analyses of the supposed isolated substances and its compounds the following percentages are obtained :—

63·409 per cent. of carbon.
62·651   ,,   ,,
65·266   ,,   ,,
66·101   ,,   ,,
61·990   ,,   ,,
63·460   ,,   ,,
63·500   ,,   ,,
63·09   ,,   ,,
64·66   ,,   ,,

And yet Dr. Thudichum speaks of a definite individual myeline!

After the description of the supposed compounds of the hypothetical myeline there follows an account of the products of its "chemolysis," or decomposition by means of caustic soda, from which it appears that these consist of glycerin-phosphoric acid, *choline* and *neurine*, and fatty acids.

After reading this research on myeline it is curious and interesting to find that Dr. Thudichum was unable to separate lecithine from the brain; it always decomposed so rapidly that he was only able to obtain products of decomposition of this body. "Perhaps the above processes followed by me, if carried out with the greatest despatch by working it during night and day without intermission, would lead to the desired result" (to obtain lecithine).

But is it not obvious that if myeline is anything it is Diakonow's lecithine? The process by which it is obtained is such as would yield lecithine; the appearance of the precipitated myeline resembles that of lecithine; its chemical composition agrees closely with that of lecithine, and its products of decomposition are the same.

Below are given the percentage of composition of Thudichum's myeline and lecithine:

	Thudichum's myeline. $(C_{40}H_{75}NPO_{10}.)$	Lecithine. $C_{44}H_{90}NPO_9$ (Diakonow).
C	63·409	64·27
H	9·833	11·40
N	1·794	1·80
P	4·087	3·80
O	20·874	18·73

But myeline is only one of thirteen entirely new bodies discovered by Dr. Thudichum, of which we append a list.

Apomyeline.
A new base from kephaline.
Cerebrinic acid.
Kephaline, $C_{42}H_{79}NPO_{13}$.
Kephaloidine.
Kerasine.
Kephalic acid.

Myeline.
Oxykephaline.
Paramyeline.
Peroxykephaline.
Phrenosine.
Pyrokephole.

Kephaline is described as the principal and most remarkable phosphorized ingredient of the brain of man and the ox. It was contained in the cephalote of Couerbe, but missed or neglected by all subsequent inquirers. It occurs in part regularly combined with K, Na, Ca, Mg, Fe, Cu. To it Thudichum attributes the formula $C_{42}H_{79}NPO_{13}$.

It is obtained from the "white matter" referred to in the preparation of myeline, by treating it with ether, which dissolves kephaline and cholesterine. To the fluid alcohol is added, which precipitates crude kephaline.

To purify it, this body is dissolved in water and precipitated as a hydrochlorate by means of HCl. It is a strange property of this hydrochlorate that the hydrochloric acid is removed from it by washing with water, pure hydrated kephaline (!!) being left.

This supposed chemical individual forms indefinite compounds with $CaCl_2$ and $PtCl_4$.

A critical mind fails to make out what kephaline can be, certainly no definite substance. It would be as rational to analyse bread and butter and attribute a formula to it as to do so with kephaline.

Oxykephaline.—"When the white matter of ox's brain has been extracted with ether and the kephaline removed from the ether solution by precipitation with absolute alcohol, there remains a bulky solution containing all lecithine (?), much myeline, and some kephaline, together with cholesterine previously contained in the white matter. When to this solution $CdCl_2$ is added, a voluminous precipitate ensues, which, after washing, yields to ether a quantity of coloured salt. This, after concentration, is precipitated by alcohol and purified by repetition of this treatment. It was of interest to know the composition of this precipitate."

The analysis led Dr. Thudichum to the formula $C_{42}H_{79}NPO_{14}CdCl_2$. Now, as the results of the analysis of this compound chanced to agree with the composition of the theoretical $CdCl_2$ salt of kephaline, plus one atom of oxygen, Dr. Thudichum thinks it "necessary to consider this substance as a genuine compound of a kephaline containing an atom of oxygen more than the normal kephaline, to which it will be convenient to apply the term oxy-kephaline."

According to Dr. Thudichum, then, the brain substance contains two new phosphorized constituents, viz.—

<div style="text-align:center">

Kephaline and

Myeline,

</div>

in addition to lecithine, if, indeed, this substance be present.

In addition to the new phosphorized constituents Dr. Thudichum's list includes—

Cerebrine, in its several varieties.

Kerasine, in its several varieties.

Phrenosine, in its several varieties.

Extractive acids and *secretory* (sic) acids and their salts, such as uric acid and congeners.

But, after all Dr. Thudichum's toil in the laboratory, after all the trouble of writing out the innumerable experimental data, after all the ingenuity which he has expended in framing rational formulæ which we venture to think no chemist will ever be able to decipher, when we ask what has been gained to science we must answer, without hesitation, little or nothing.

The mantle of Couerbe, as an investigator of brain chemistry, seems to us to have fallen on Dr. Thudichum; he has not only adopted many of the names which that observer assigned to his products, but also the methods of investigation which he proposed; he has imitated him, too, in his habits of rash reasoning and in his tendency to speculate where no speculation is possible, for according to the criteria which guide chemists in their determinations of the purity of chemical substances not one of the numerous bodies described by Dr. Thudichum has been proved to be a definite chemical individual.

Interpreting Dr. Thudichum's researches in our own way, we should summarise the most important facts as follows :—He has by his experiments and observations confirmed the existence of lecithine in the brain, though he applies to it the new name of myeline; he has, however, shown that, besides lecithine, there exists a distinct phosphorized body, which is chiefly distinguished from lecithine by its solubility in ether, but which has not yet been isolated in a state of purity; this is the body termed kephaline by Dr. Thudichum.

We confess that we are not yet prepared to throw over Liebreich's researches on protagon as completely as Diakonow, Strecker and Thudichum have done. We think it would be of great service to physiological chemistry were some one to repeat, in all their minuteness, the experiments of Liebreich, and were frankly to avow whether the statements of that observer as to the crystalline nature of the proximate principle which he believed he had discovered are absolutely untrue.

The trustworthiness and the undoubted correctness of most of the subsidiary facts which Liebreich published cannot be disputed, and it therefore seems to us to require very much stronger evidence than any we have adduced before the cardinal fact can be denied.

As Liebreich's protagon is a body which contains far less phosphorus than lecithine, it has been assumed that he analysed as a definite chemical principle what was nothing but a mechanical mixture of lecithine and cerebrine; but we are altogether at a loss to accept this explanation. Liebreich's descriptions of the crystalline nature of protagon, of the possibility of recrystallizing it repeatedly from alcohol, of dissolving it in glacial acetic acid and again crystallizing it, are, unless he be altogether untrustworthy, better evidences of the individuality of protagon than any characters assigned to lecithine by Diakonow or Strecker.

Then none of the facts with which we are acquainted in reference to Müller's cerebrine render it at all likely that it can be the body which, mixed with lecithine, constituted Liebreich's protagon.

We cannot help thinking that the method employed by Liebreich was far less likely than that of subsequent observers to lead to the decomposition of brain substance. What right have we to assume that, after being hardened in strong alcohol until the water has been in great part removed, the proximate constituents of brain remain undecomposed? None whatever. Let us ask ourselves what would have been discovered as to the nature of myosine by Denis and Kühne had they commenced by hardening the muscular tissue which they investigated in alcohol; or what Hoppe-Seyler, Stokes, and Preyer would have learned in reference to hæmoglobin if blood coagulated by alcohol had been employed to prepare the raw material of their researches, and we shall be in a position to estimate whether we can look upon the products of an alcohol extract of brain which has been hardened in alcohol as at all likely to represent the actual chemical individuals occurring in the nerve centres?

II.—Curling and Smith on Diseases of Rectum.[1]

As in many other branches of surgery, it is difficult to conceive in what direction any great advance can be made in the treatment of diseases of the rectum. A perusal of the two works before us clearly illustrates this fact, as we may truly say, that, although each has been carefully revised and enlarged since the appearance of the former editions, there is nothing in either that can really be said to be new. Increased experience has not led either of the authors to alter to any great extent the views expressed in the earlier editions of their works.

Mr. Henry Smith has, indeed, added to his small work a chapter on "Painful Ulcer of the Rectum," in which he states his belief that many such cases may be successfully treated by the local application of solid nitrate of silver, or by "an ointment made of the cinereous oxide of mercury in the proportion of half a drachm of the mineral to one ounce of lard," without having resort to operation. The conditions necessary for success he considers to be an absence of spasmodic contraction of the anus and an ulcer easily within reach. We think, however, that the majority of the profession will agree with Mr. Curling that such cases are rare, and that as the patient's sufferings are usually very great and the operation for their relief is so slight and so certain, it can seldom be worth while to waste time over less efficient means. The operation of simple dilatation of the anus, or rather forcible rupture of the floor of the ulcer and the superficial fibres of the sphincter, which seems to be somewhat popular at the present time with the French and German surgeons, finds but little favour either with Mr. Curling or Mr. Smith. The advantage claimed for it is, that after a few hours' rest the patient can follow his usual occupations without any confinement to bed, but against this Mr. Curling quotes some cases mentioned by Giraldé, in which the operation failed, and therefore he sees no reason, as the treatment "by incision is so simple, so effectual, and so harmless," "to substitute for it a proceeding so rough and uncertain as forcible dilatation" On the other hand, we find that M. Le Fort, in the new edition of Malgaigne's 'Médecine Opératoire,' speaks of forcible dilatation as the only justifiable mode of treatment, and mentions a case operated on by Laugier in which the simple incision terminated

fatally from pyæmia.   With such opposite views before us it is
evident that further experience is required before it can be
finally determined which operation is to be preferred.

The treatment of internal piles is discussed at considerable
length by both authors.

Mr. Henry Smith's increased experience has led him to speak
more strongly than ever in favour of the operation by means of
the clamp and cautery, which in this country is so intimately
connected with his name, while, on the other hand, Mr.
Curling sees no reason to discontinue his preference for the
ligature.   On this subject he says :

"I continue to give the preference to the ligature as more con-
venient to the surgeon and less alarming to the patient. The opera-
tion by the cautery is more tedious than by ligature, a matter of
little moment when the patient is insensible, and spec   care is
necessary in using the cautery to guard against after-hæm.
In a favorable case the patient is able to leave the couch somewhat
sooner, and the recovery is, on the whole, somewhat more rapid than
after ligature, as the healing of the sores does not commence till
after the separation of the ligature, a process occupying four or five
days, whereas the eschar, or superficial slough produced by the
cautery, is cast off sooner."

On the ground of convenience to the surgeon no one can
doubt the superiority of the ligature over the clamp and cautery ;
but if any real advantage to the patient can be shown to arise
from the use of the latter method no surgeon would hesitate to
put himself to a little inconvenience during the performance of
the operation.   The first question which requires to be settled
is the relative mortality of the two operations, but we know
of no statistics at present before the profession sufficiently
extensive and sufficiently detailed finally to settle it.   In
the hands of Mr. Smith there can be no doubt the cautery
has proved extremely successful, while he seems to have been
somewhat unfortunate with the ligature.   Mr. Curling states
that in his lengthened experience only one fatal case has oc-
curred to him after the use of the ligature, and in that one the
patient died after five months' illness from chronic blood-poison-
ing.   He also states that erysipelas and pyæmia have followed,
" though very rarely, the operation by the cautery."   There is
probably a general impression in the profession that these grave
accidents are less frequent after the use of the cautery, but we
do not know of any direct evidence in proof of the idea.   It is
probable that the frequency of pyæmia after the use of the
ligature varies with the mode in which it is applied.   Except
in very large and broad piles, Mr. Curling simply notches the
base of the pile and ties the ligature tightly in the notch.   Mr.

Smith, on the other hand, in describing the operation of ligature in Holmes's 'System of Surgery,' vol. iv, first edition, recommends that a needle should be passed through the base of the tumour, carrying a double ligature with which each half is to be tied. Now, it is well known that almost every internal pile contains a large dilated vein in its centre, and it is quite evident that if the needle be passed through this and the vein tied open instead of being occluded, not only will there be an increased danger of hæmorrhage, but pyæmia would be very likely to occur from the entrance of septic matter into the circulation.

Another danger accompanying the operation for piles is hæmorrhage, either primary or secondary. As regards the former, the advantages are decidedly on the side of the ligature, for if it be applied with ordinary care and skill hæmorrhage is almost impossible during the operation, or until the ligature has separated. The application of the clamp and cautery, however, is not free from this danger except in experienced hands. Thus, we find in several of the cases recorded by Mr. Smith that smart hæmorrhage occurred on unscrewing the clamp, or that the bleeding was very free but was ultimately arrested by the cautery. If such accidents occur to Mr. Smith it is evident that they would be more frequent in less experienced hands, and on this account alone it is not likely that the clamp and cautery will ever completely supersede the ligature. Secondary hæmorrhage is a rare occurrence after any operation for internal piles, and occurs probably with equal frequency after the ligature and the cautery.

The minor troubles which follow the operation, such as retention of urine, pain in the rectum, and delayed healing of the sores, are decidedly less frequent after the operation by the cautery than after the ligature, and this forms the chief argument in favour of the former method of treatment. On the whole, we should feel inclined to think that, although there is not sufficient evidence to justify us in assuming that in equally skilful hands one operation is more likely to be followed by grave or fatal complications than the other, yet the cautery presents the great advantage of being a speedier and less painful mode of treatment than the ligature, and is on this ground to be preferred. On the other hand, it requires special instruments, which are not always at hand, and to comparatively inexperienced operators presents many difficulties and dangers. It is not probable, therefore, that with the ordinary practitioner it will ever displace the ligature.

In treating of fistula in ano both authors adhere to the commonly received view that in almost all cases the fistula penetrates beneath the external sphincter, and that the division of

this muscle is a necessary part of the cure. This view we believe is not absolutely correct. Not a few fistulæ commence as small submucous abscesses which borrow downwards in the submucous tissue until they reach the anus, and then extend beyond this into the subcutaneous tissue superficial to the sphincter, and in such cases the operation would not involve the division of the fibres of that muscle. Many more certainly open externally at a point considerably internal to the outer margin of the internal sphincter, and in these the whole muscle will not be divided in the operation. It is probably due to this that incontinence of flatus and fæces is such an uncommon occurrence even for a short time after the operation for fistula. Mr. Smith, however, thinks that in such superficial fistulæ the operation will be generally useless unless the sphincter is divided after laying the fistula open, and states that " in order to get it thoroughly cured it is necessary to paralyse the action of the sphincter for a time by dividing it." In proof of this statement he mentions a case which occurred in his own practice, in which the first operation entirely failed in consequence of the sinus only being laid open, but he does not tell us in what condition he found the parts. Was there a new fistula, or was it a superficial sore? If the former, probably by slitting it up and cutting away the thinned edges of the mucous membrane and skin, a cure would have been obtained, and if the latter, we see no reason why it should be necessary completely to divide the sphincter so as to paralyse its action any more than it is in the treatment of painful ulcer and fissure of the anus, in which, as is well known, a superficial incision, only affecting a few fibres of the sphincter, is sufficient to cure. In the treatment of fistulæ which pass for a considerable distance upwards along the side of the rectum above the internal opening, Mr. Curling differs from Mr. Syme and Sir Benjamin Brodie, in believing that it is not always sufficient to connect the inner and outer openings by an incision to effect a cure.

"My own experience," he says, " does not enable me to coincide altogether in these views, for in many cases I have found that the sinus running up the rectum will not close without a free division of the parts. The wound below is apt to assume an unhealthy aspect, and a free discharge continues. In a case of this kind with a sinus burrowing close to the mucous membrane, I have sometimes passed up to its extremity a straight director, and carried along its groove one of the blunt-pointed blades of a pair of knife-cutting scissors, whilst the other has ascended the rectum, and, by closing them, have divided the intervening membrane and exposed the sinus."

He states that these sinuses are so superficial that there is but little fear of hæmorrhage after the operation. We cannot

but think that such an operation must be very rarely necessary, and that it falls to the lot of but few surgeons to meet with such cases as Mr. Curling describes.   Mr. Smith thinks that a high division of the gut, involving, as it may, a complete division of the internal sphincter, is a cause of the want of control of the bowels occasionally met with after the operation, and advises, therefore, that the incision should be kept as low down as possible.   We should be inclined to think that this view is not correct, and that the internal sphincter is much more often completely divided than the external.   The opportunities of dissecting a fistula in ano are not frequent, at any rate in cases which come under the care of the surgeon, and its accurate pathological anatomy and exact relation to the sphincter would form an interesting subject for investigation.   Mr. Smith lays great stress upon the frequent coexistence of fistula in ano and stricture of the rectum.   He states that his own observations have led him to believe " that in a considerable proportion of cases a stricture will be found to exist with fistula in ano, and that a failure to cure the latter disease by operation has simply been the result of overlooking the other morbid condition of the bowel."   In many of these cases Mr. Smith thinks the contraction of the gut is secondary to the fistula.   We do not think that the experience of most surgeons would justify such statements as the above.   Few operations are more successful than that for fistula, and few surgeons find it necessary to employ bougies in the after-treatment of a " considerable proportion " of their cases ; and yet Mr. Smith tells us that it is useless to try to cure a fistula, complicated with stricture, without treating the latter as the chief disease, and that the coexistence of the two occurs in a " considerable proportion " of cases.   The treatment by the elastic ligature finds favour with neither author, and in this they are in accord with the great majority of surgeons.

One of the most interesting parts of Mr. Curling's work is that which treats of colotomy.   During the last few years this operation has doubtless been performed with much greater frequency than formerly.   Many surgeons no longer look upon it as a last resource to be employed only when complete obstruction of the bowels threatens speedily to end in death, but as a means of relieving the sufferer in an early stage of malignant disease, or even in extensive syphilitic ulceration, from the constant pain and anxiety which such conditions give rise to.   The statistics furnished by Mr. Curling are so favorable as fully to justify this extended employment of the operation.   Out of twenty-one operations in which he has taken part fourteen were successful.   In two of the fatal cases the death was chiefly

attributable to chloroform, in one the obstruction had lasted thirty days, another died from peritonitis springing from the cancerous disease for which colotomy was performed, two more died from exhaustion shortly after the operation, and one from pyæmia. It is but reasonable to suppose that some of these fatal cases might have recovered had the operation been performed earlier. It will be noticed that only two of these cases died from peritonitis, and that in one of these it existed before the operation, and in the other sprang, not from the wound, but from the original disease. It is clear, therefore, that the danger of peritonitis is not so great as could at first be supposed, and if this can be avoided the risk of the operation is but slight when performed before the patient is exhausted by prolonged pain and obstruction of the bowels.

The treatment of cancer of the rectum by any operative interference with the disease itself is, in Mr. Curling's opinion, very rarely possible, no such case ever having come under his observation. Epithelioma of the anus is, however, more manageable, and he mentions some interesting cases in which the disease was successfully treated by excision, and in which it was observed that, although a large part of the sphincter muscle was excised, the retentive power of the anus was not seriously weakened, nor was there any inconvenient contraction of the anal opening.

The final chapters of Mr. Curling's book are devoted to the congenital imperfections of the anus and rectum, a subject on which he is a well-known authority. After discussing the treatment of those cases, in which the gut can be reached from the perinæum, the question of the advisability of colotomy and the best method of performing it when the simpler operations have failed is considered at some length. There can be no doubt that the operation in the left groin is considerably easier of performance in a new-born infant than that in the loin. In order to prove this point Mr. Curling performed both operations twenty times on the dead body. In eighteen out of the twenty the colon was easily found and opened in the groin. In the two in which difficulty was found the colon passed across the body of the fourth lumbar vertebra, the rectum commencing on the right instead of on the left side, a condition which, according to M. Boucart, occurs only once in twenty-five subjects. On the other hand, in only eight of the twenty cases was the operation easily performed in the left loin. In six it was rendered difficult by the depth of the gut, its empty condition, and the presence of the kidney in front of it. In six the gut was completely surrounded by peritoneum, forming a long mesocolon. In respect to the comparative dangers of the operations, Mr. Curling

thinks the number of cases too few for the purposes of accurate comparison, but is inclined to consider that in the groin the safer. There is ample evidence to show that, if the infant survive, the artificial anus, whether in the groin or loin, is amply sufficient for its purpose, and that the inconvenience arising from it is surprisingly small. It would seem, therefore, that both on account of the greater ease of performance and greater certainty the operation in the groin is to be preferred to that in the loin, and from the great success that has followed it in some cases no surgeon would be justified in letting a child die without the attempt being made to establish an artificial anus.

There is much more of interest in both these works that we would gladly notice did space permit. The fact that each has reached its fourth edition shows that it is highly appreciated by the profession, and it is satisfactory to note that neither book is the work of a specialist, both the authors holding high positions in connection with general hospitals.

III.—Diseases of the Peripheral Nerves.[1]

This is one of the fifteen volumes of the cyclopædia which the eminent professor of Munich is editing for the profession. What a learned man this professor must be who is capable of supervising the work of fifteen or more specialists! Does he really do so we wonder, and has Professor Erb possibly been compelled to submit to the editorial pruning of his chief, to suggestions, criticisms, and emendations? Or is Von Ziemssen merely the scientific adviser of a firm of German publishers, whose work is limited to the selection of authors for the men who find the capital ; or does he stand in the light of a director who lends his great name to " float " some scheme into popular favour? All this does not concern the reader, and we should not enter into the discussion of the subject did it not strike us as a monstrous absurdity that the name of the author of a book does *not* appear on the cover, while the " editor's " does. Even on the title-page the editor's name is twice as big as the author's, and, following the same principle, the name of the American editor of the English translation is printed in letters twice as big as those which serve for the English translator. Did Mr. Power's proof sheets go to America and back?

We are, in short, quite at a loss to understand the craze which exists at present for systems, cyclopædias, and dictionaries, where jackal authors do all the work for lion editors, who get the glory. An author is certainly placed at a great disadvantage in having to appear in company which is not of his choice, and it has not unfrequently happened that a good article has foundered in the sea of learning because the craft was overloaded with ponderous ballast. We think that Professor Erb is to be pitied because his book can only be purchased in company with fourteen other volumes, and we think that the medical public may justly complain that so great an obstacle is placed between them and an author who has much that is valuable to teach.

This particular volume of the ' Cyclopædia ' is, on the whole, excellent, and the translation leaves nothing to be desired. Mr. Power has done his work in a finished and scholarly manner, and the book reads as if it had been originally written in English. This translation is a great contrast to the miserable productions in Hans Breittmannese which the Americans have given us of Rindfleisch and Wagner.

The one fault of the book is its length, and there are parts of it in which the author may fairly be asked for " more matter and less words." This is particularly the case in the first chapter, " On Neuralgia," which is, we think, the worst chapter in the book, and contains so little that is new that it will not convey any information to a student of average diligence. On the first page we are told that neuralgia is a disease " the chief and most important symptom of which is pain. The pain appears to be localised in particular nerve-trunks or branches, with all their ramifications; it is characterised by its remarkable acuteness, and is either intermittent or at least undergoes very well-marked remissions." On the opposite leaf we are again informed that the pain of neuralgia is limited to definite nerve-trunks and their ramifications, and that it is paroxysmal and intermittent. A few lines further on and the announcement is made for the third time the " pain, then, is the most prominent symptom in neuralgia." On page 37, and again on page 40, we have the chief facts about the pain recapitulated twice over. Having had the disease dealt with " generally " and at length, it is next entered upon particularly, and the various kinds of neuralgia are described with a tedious recapitulation of what we have previously been told, and the other needless application of generalities to particulars, till we feel inclined to say as did the Vicar of Wakefield when listening to Mr. Jenkinson, " Surely I have heard all this before."

But now that we have exercised the critic's right in the way

of grumbling and fault-finding, we will endeavour to give a few samples of what appears to us in every way good. The account given of diaphragmatic or phrenic neuralgia will be a novelty to most readers. Erb does not seem to make himself responsible for the facts concerning this disease, but gives them on the authority of Peter, who in a paper in the 'Archives Générales de Médecine,' in 1872, drew attention to " nevralgies diaphragmatiques."

We must confess that the grounds upon which certain symptoms are referred to a disorder of the phrenic nerve seem to us to be scarcely sufficient, and the assertion that they are so referable must, we think, be received with great caution.

Neuralgia of what has always been looked upon as a motor nerve is something so contrary to ordinary experience that we have a right to demand before accepting it that the sensory functions of the phrenic shall be fully demonstrated. To say that " its mode of origin from several branches of the cerebral plexus renders it highly probable that it contains sensory fibres," or that " Peter regards it as a mixed nerve," or that Henle is " satisfied that it contains sensory fibres," is hardly sufficient. Again, the course of the phrenic nerve through the neck or thorax is so deep and removed from ordinary methods of investigation, and the reference by patients of their subjective symptoms to certain internal regions of their body is always such a matter of very great doubt, that we should only be justified in referring subjective phenomena to disease of the phrenic upon the maximum amount of evidence.

" Pain in a line corresponding to the course of the phrenic through the chest " might surely be caused by a multitude of different things. The same may be said of " acute pain at the lower and especially the anterior part of the thorax," of " pain at the point where the nerve rises," or of " pain in the shoulder." *The* point where the phrenic arises is a term we are seldom justified in using, for it, as often as not, arises from two or even three of the cervical nerves. We are at a loss to conceive upon what grounds, physiological, clinical, anatomical, or pathological, pain in the shoulder and lower jaw can be regarded as indicative of phrenic disease. Again, the " painful spots " which help to diagnose this neuralgia seem to be situated in very doubtful quarters. They are—1, the spinous processes of the cervical vertebræ from the second to fifth; 2, the phrenic nerve itself as it pursues its course along the supra-clavicular fossa; 3, the line of attachment of the diaphragm; and 4, a point over the cartilage of the third rib. The first and last seem quite valueless as indicating phrenic disease, and as for tenderness of

the nerve itself in the neck, the difficulties of accurately compressing it without at the same time compressing many other nerve-twigs and trunks are so great, that it would be rash to attribute much diagnostic importance to this point.

"Now, although phrenic neuralgia is not unfrequently an independent and primary disease (especially in nervous and anæmic individuals, with whom it is a consequence of exposure to cold, &c.), it is the more commonly a concomitant of some other disease, especially of angina pectoris and of cardiac and vascular lesions, Basedow's disease, enlargement of the spleen, and hepatic disease; and Peter is inclined to refer a part of ordinary or extraordinary pains occurring in these affections to neuralgic implication of the phrenic, and supports his view by the histories of numerous cases. Phrenic neuralgia especially affects the left side."

All these facts or alleged facts are to be borne in mind, no doubt; but, nevertheless, the grouping of them together does not strike us as something familiar—something of ordinary occurrence. We must suspend judgment for a time in the matter of this neuralgia; and if Peter be correct in his assertions, confimatory evidence will, no doubt, be forthcoming from many quarters. In the mean time we do not think that a strong *primâ facie* case has been made for the introduction of a new disease into our plethoric catalogue, and our impression is that to talk of "phrenic neuralgia" is to give to an "airy nothing" a local habitation and a name which it does not deserve.

The section on neuralgia is followed by an "appendix" on headache, the most remarkable feature of which is the fact that Erb has evidently never read Liveing's splendid and thoughtful monograph on megrim.

In the diagnosis of headache no directions are given for the use of the ophthalmoscope, which is certainly a most remarkable oversight for an author who writes in these times, and we think we are right in saying that not once in the whole volume are any directions given for the employment of this the faithful and constant companion of the neuro-pathologist. It is true that the work deals with "peripheral lesions," but it is no less true—as, indeed, is constantly admitted throughout the book—that peripheral symptoms are often caused, in whole or in part, by central change, and yet this valuable instrument for the detection of central lesion meets with the most astonishing neglect at the hands of our author.

In all doubtful cases of headache, ocular spasm and paralysis, trigeminal neuralgia or anæsthesia, and facial paralysis or spasm, it should be a matter of routine to use this instrument; but we have not found advice of this kind anywhere in the volume before us. We are quite ready to admit that so dazzling has been the light

thrown of late on the physiology and pathology of the central nervous organs, that there appeared to be some danger that the peripheral nervous system would be forgotten, and that men's minds would be devoted too exclusively to the more fascinating but less practically useful problems which belong to the study of the highest cerebral functions. On this account we welcome Professor Erb's volume, but it is certainly strange that he should have so completely neglected that valuable aid to diagnosis which we have in the power of exploring the fundus oculi by means of the ophthalmoscope.

The section on neuroses of the gustatory nerves is chiefly interesting for the discussion on the part which the chorda tympani has in conveying gustatory impressions. After weighing the evidence, both clinical and anatomical, the conclusion arrived at is that " the greater part of the fibres ministering to the sense of taste pass from the nervus lingualis into the chorda, and by means of the latter into the fascialis, but finally return again into the trigeminus." The route which the fibres of the lingual take to get from the facial to the trigeminal is a matter of doubt, and Erb very wisely leaves the question open.

The testing of the sense of taste is a very difficult matter, and the methods ordinarily employed are very inadequate for the purpose. The galvanic method of testing the sense of taste is strongly recommended by Erb, and the following are his directions for this mode of investigation :

"Two fine wires, provided with small knobbed ends, and carefully isolated from one another by means of sealing-wax, are to be attached at a distance of a few millimètres from each other to a non-conducting handle, such as a glass rod or an elastic catheter ; these wires, which form the electrodes, are then connected with the poles of one or several galvanic elements. If these are placed upon the tongue a slight burning sensation is felt, with a distinct sensation of taste, which is described as being sourish, saline, metallic, coppery, &c., and which is somewhat stronger at the anode than the cathode. By this means a very exact localisation of the galvanic gustatory stimulus is obtained, and it is possible to determine to a hair's breadth, by moving the small pair of electrodes over the surface of the tongue and palate, the limits of the gustatory and non-gustatory area, as well as to form an accurate conclusion in regard to the intensity of the sense of taste in symmetrically situated parts."

The neuroses of the olfactory nerves are treated of at some length, and this part of the book is enriched by the valuable investigations of Dr. W. Ogle. It is pointed out that what we call "flavour" is a quality which we appreciate by the help of two senses, smell and taste, and it is asserted that olfactory paralysis affects the sense of taste far more than gustatory. The

fact also is alluded to that, though in some cases we may be unable to appreciate odours through the anterior nares by the blocking of the nostrils, that, the passage from the pharynx to the posterior nares remaining open, the sense of "flavour" is not lost.

In paralysis of the trigeminus it is well known that the sense of smell is impaired owing to the diminution of the secretion of nasal mucus, and in facial palsy the same effect is produced owing to the tears escaping over the cheek instead of finding their way into the nasal cavity.

Diseases characterised by the loss of the olfactory sense are grouped together under the name of *anosmia,* a word which has been in general use now for some time. We do not know who was answerable for loading medical science with this piece of unnecessary Greek, and we should not stop to notice the word had we not doubts of its correctness from an etymological point of view. We have very grave misgivings as to the wisdom of adhering to the old fashion of cloaking our ignorance in the dead languages, but surely if there is any useful purpose served by such a course it is this, that a scholar may find in the word some clue to the condition which the word connotes. Now, when we were at school, the Greek work ὀσμή was said to signify scent or aroma, as of a flower, and the word anosmia would, therefore, mean without scent or aroma, which should be, practically speaking, a healthy condition in the human being, although with beavers, musk-deer, foxes, and skunks, the case would be otherwise. The sense of smell is represented by the word ὀδωδή, so that, if we are to have a Greek word at all for paralysis of the olfactory and other conditions which deprive us of the pleasures of aroma and flavour, it should surely be anododia, and not anosmia.

There is one point in which all writers should try to imitate Professor Erb, viz. the progressive improvement which each section of his work shows on that which precedes it. The various subjects are handled throughout in a scientific spirit, but with some he necessarily seems more conversant than with others. The chapter on spasms is a thoughtful one, and a comprehensive review on the whole subject forms a most acceptable introduction to the study of this difficult and obscure department of nerve pathology. The old division of spasms into clonic and tonic is adhered to.

"The essential difference between these two forms of spasm is perhaps this, that in tonic spasms the several motor excitations are so closely approximated to one another in point of time that they coalesce to produce a continuous tetanic contraction, whilst in clonic spasm the several excitations are separated from one another

by intervals of sufficient duration (varying also in intensity and the part affected) for each excitation to be separately recognised."

These two main divisions of spasms are again subdivided—the clonic spasms into tremor and convulsion, and the tonic into cramp and contracture. The etiology of spasm is touched upon with characteristic German precision. Thus, it is said that spasm may depend upon—1, increase of strength of stimulus, and 2, increase of excitability of motor apparatus. Again, it may be produced by *direct, indirect,* or *reflex* stimulation. Then we are told that direct spasm may originate in many ways, as (1) by irritation of the fibres themselves or the motorial plates ; (2) by irritation of the motor nerves ; (3) by irritation of the cord ; and (4) by irritation of the brain. Reflex spasms in a similar way may be due to increased excitability of the centripetal fibres or of the reflecting centre. What is meant by indirect spasm apart from reflex spasm is not very clearly made out. In this chapter we notice, as elsewhere in the book, a great want of knowledge of English work, and in dealing with the subject of convulsions Reynolds's contribution to the pathology of epilepsy and Hughlings Jackson's (to use a horrid German expression) "path-breaking" and "epoch-making" investigations into the cause and localisation of "discharging lesions" are alike ignored. Interesting as this chapter on spasm undoubtedly is, we wish it bore a little more strongly the stamp of clinical observation and rather less the appearance of having been evolved from an inner consciousness.

The section on writer's cramp and allied neuroses will be read with interest, although in this chapter, as in the last, there is too much of the inner consciousness and too little of clinical observation. There is not much, if anything, that is new in this subject, and here, again, there is evidence of want of knowledge of the work which has been done in this country and America, and it is evident that Erb has never perused the observations either of Poore or Weir Mitchell. The old-fashioned nebulous theory is again given. Thus :

"It is probable that the co-ordination of these voluntary impulses occurs at different and definite points at the central organs (apparatuses of co-ordination), or it may be that the associated excitations of the will pass at certain points of the grey substance over routes which, from long and frequently repeated exercise, offer only a small resistance, so that ultimately the excitations of the will pass at once along these co-ordinating fibres."

Is it not, we ask, very much out of harmony with the spirit of the times to waste words in idle speculations on matters concerning which our knowledge is absolutely nil, albeit that we

may make-believe to write learnedly concerning these co-ordinating apparatuses! Where are they? What are they? Is there any evidence for their existence? What is their number, size, shape, and composition? Do we need a co-ordinating apparatus for every conceivable combined movement, and can they be bred up and trained at will? When any of these questions can be answered, then, we think, will be time enough to indulge in speculative theories which benefit neither doctor nor patient nor physiological science. If medical writers would limit themselves to a discussion of facts, or at least to theories founded upon facts, how very much time would be saved!

A considerable amount of space is devoted to the discussion of " tetany," a disease which was first described by Dance in 1831, and was popularised by Trousseau in his famous clinical lectures. The spasms affect chiefly the muscles of the arm, and very often the muscles supplied by the ulnar nerve, so that the hand is drawn together like the hand of an accoucheur. It is particularly stated that the disease is not a " business neurosis," although the malady which Clemens described as *schuster krampf*, or the spasmodic contraction, occurring in shoemakers, is included under the heading of " tetany." The fact also that the disease affects principally the arm, and is (as was pointed out by Trousseau) very liable to occur in nursing women, in which the arms are often subjected to the strain of nursing, would at least make it doubtful whether this troublesome malady is not, at times, dependent on the occupation of the patient. An interesting point in this disease is the relationship which it apparently has to the state of contraction of the arteries. The observations of Bauer have shown that poisoning with secale cornutum is capable of directly inducing tetany, and Dr. Moxon has also recently called attention to the fact that the symptoms of ergotism and of tetany are strikingly similar.

Kussmaul observed in one case that an attack of spasm could be induced by pressing upon the large artery of the limb. These are interesting points, and seem clearly to show that there is some relationship between tetany and the amount of blood traversing the arteries.

The section on paralysis shows a more intimate knowledge of clinical facts than any of those which precede it, and may fairly be looked upon as the best chapter in the book. With the important subject of the degeneration of nerve and muscle which results from disease or injury of the peripheral nerves Professor Erb shows himself completely at home, and talks with that clearness which is the sure evidence of accurate knowledge. The degeneration of nerve and muscle are essentially alike. Both consist of the degeneration of the tissue elements of high

function, the fatty degeneration of the medullary sheath of the nerves and the atrophy and waxy degeneration of the muscular fibres, and the abnormal growth of the connective tissue elements which ultimately amounts to a state of " cirrhosis " both of nerve and muscle.   Upon these points histologists seem very generally agreed, but the matter in doubt is the behaviour of the axis cylinder of the nerve during these degenerative changes ; and while some say it persists, others deny that it does so.

It is certainly a very great advance in the diagnosis of nervous diseases that we are able with tolerable accuracy to estimate by means of electricity the degree of degeneration to which nerve or muscle has reached ; and the " degenerative reactions " which Erb was the first, we believe, to place lucidly and coherently before the profession, are now recognised as points which have to be settled in the examination of every case of paralysis.   The account which is given of these reactions is accompanied by very useful diagrams, which are the only illustrations in the book ; and here we would suggest that a future edition would be very much improved by a few woodcuts to supplement the text.   The price of the volume is certainly such that the publishers might well afford a few pictorial helps to the better understanding of the work.

The last fifty pages of the book, we think, ought to have come first ; and, indeed, we have wondered why the whole arrangement has not been reversed.   The author begins with "functional " diseases, concerning whose pathology we know nothing that is sure, and scarcely anything that is even doubtfully probable ; and, opening with neuralgia, he passes on to anæsthesia and spasms, and then, after considering paralyses, concludes with the study of definite histological changes affecting nerve trunks, hyperæmia, neuritis, and the neoplasms, and the symptoms which result from them.   If the last chapter had been placed first the student, in studying the physiological results of definite nerve lesions, would have formed an acquaintance with neuralgia, anæsthesia, paralysis, and spasm, which would have proved of great service to him in studying these groups of symptoms more fully.

The treatment of the diseases which form the subject-matter of Professor Erb's volume is based upon common sense and a sound physiological basis.   He does not push this method of therapeusis or that, and is not unduly wedded to any doubtful theories to warrant a line of treatment or to account for results. In this matter the work stands in very favorable contrast to many others, and we believe the practitioner may safely turn to this work for very wholesome, sound, and practical advice in the matter of managing patients afflicted with lesions of the

peripheral cerebro-spinal nerves.   In conclusion, we would say that, although we have exercised the critic's right of growling, perhaps a little freely, our general impression of the book is good, and that the reader who can peruse it without adding to his knowledge must be exceedingly well informed.

IV.—Text-Books on Practical Histology.[1]

THE publication of several manuals of practical histology is a natural outcome of the general resolution of medical examining bodies throughout the kingdom that an acquaintance of the main facts of structural anatomy shall, as far as possible, be acquired by students themselves from a study of their own preparations, instead of being, as formerly, picked up more or less correctly from looking at woodcuts copied and recopied into the various physiological handbooks, from the classical works of Kölliker, Frey, Bowman, and others.

Practical histology having thus become part of the regular curriculum of a medical student, it may be worth while to inquire what knowledge of microscopical details it is fair to expect the average second or third year man to acquire from his own work, with the time at his disposal, and whether this knowledge, when gained, is of such an extent as to be of practical value to him in after life.

As Latham eloquently says, "It is a truth, that the whole circle of sciences is required to comprehend a single particle of matter; but the most solemn truth of all is *that the life of man is threescore years and ten.*"

Now, this point is one which it is very important not to overlook, having regard to the fact that there is but a limited time at the disposal of many students, not only to get enough anatomical and physiological knowledge to pass their examinations, but also to lay in such a stock of clinical experience as must, at least, be sufficient to make them safe practitioners, and recollecting also that a course of practical physiology in addition to histology is now added to the programme.

We shall probably be safe in assuming that a course of demonstrations lasting from four to six months, the average

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[1] 1. *Handbook to the Physiological Laboratory. Histology.* By E. KLEIN, M.D., F.R.S.
2. *Practical Histology.* By E. A. SCHÄFER.
3. *Outlines of Practical Histology.* By Professor RUTHERFORD.
4. *Practical Physiology.* By M. FOSTER, M.D., F.R.S., and J. N. LANGLEY.
5. *Demonstrations of Microscopic Anatomy.* HARLEY and BROWN.

attendance of each student being twice a week, and the length of these demonstrations being at most three hours, will be as much time as it will be found possible in medical schools to allow for the subject of practical histology.

In what respects will the average student, fairly diligent, but having no special taste for the subject, be the better at the end of such a course?

In answering this question, it must be remembered that microscopical work is in many respects a peculiar study ; putting aside some of the most complicated methods of preparation of tissues (such as gold preparations of the cornea), and the various ways of injecting, it will be found that one or two lessons are sufficient to teach the ordinary working methods of hardening, cutting, and mounting the common tissues. But this is not enough ; the hand and eye have to be educated, and for the latter, especially, is this education difficult, for not only do we find that many men are incapable of recognising differences of structure which to others are quite apparent, but the infinite number of deceptive appearances produced by position, slight obliquity of section, dragging of the specimen, &c., have all to be recognised almost instinctively before the conclusion drawn by the observer can have any value.

Further, when the student is sufficiently advanced to understand his preparations, the structure of the various simple tissues and compound organs of the body is required to be studied, and here again the ground to be covered is of such an extent that if the investigation is to be done thoroughly a far longer time will be required than it is fair to demand of one and all alike.

It is, then, well worth consideration whether the practical teaching of histology could not be done better and with greater economy of time by a well-arranged course of demonstrations by skilled workers, in which the methods of preparations of tissues were explained and the structure of the various organs shown in specimens, the study of which would serve to make clear those points which must always be imperfectly understood if left to mere word painting.

If such a course as this were made imperative on all, we believe that there would be no lack of students who would be stimulated to work for themselves with the microscope, and it is for these, and these only, that a good course of practical histology is capable of being a training which will be useful to them in their professional work.

Those students who have to pass the earlier part of their medical education at the universities, and those who under other conditions have time to take their professional course

leisurely and to win the higher qualifications, would probably come under this head in any case, but it will be understood that our remarks do not apply to these favoured ones, but to the greater number, whose circumstances render it advisable that they should pass their examinations and become qualified practitioners with as little delay as may be.

The first text-book which it is necessary to notice as at all representing the recent methods of working and recent opinions on disputed points in structural anatomy, is that portion of ' The Handbook to the Physiological Laboratory '[1] which deals with practical histology, the author of which has deservedly a very high reputation as a microscopic worker. Though this book appeared so far back as 1873, it has not hitherto been noticed in this Review, and we are glad to be able to compare it with later works, some of which bear evident marks of its influence.

Too high praise can hardly be given to the beauty and faithfulness of the engravings, and to the careful way in which many of the processes of preparing and examining specimens is described in this work ; its general plan and arrangement also seem in some respects better than that adopted in some more recent text-books.

But altogether we cannot but think that the author has missed a great opportunity, the book is neither a complete account of histological methods nor a text-book of structural anatomy, but a mixture of both these subjects, in which, while some structural points are described with very great care and detail, many more are of necessity either briefly noticed or omitted altogether.

For example, while the lymphatic system of the central tendon of the diaphragm is described at very great length, the histology of the whole of the respiratory tract is not mentioned at all ; and the methods of preparation of the organs of respiration are dismissed in a little over a page.

We think, also, that too great prominence is given to the silver method of preparation, for, beautiful and valuable as the results of this process undoubtedly are, yet they are somewhat unsafe, and the conclusions arrived at from them should be as far as possible checked by the employment of other methods.

The first part of Dr. Klein's book—that which deals with the elementary tissues—is, we think, certainly the best. Especially admirable is the chapter on the connective tissues, which is not only very clear and precise, but is so arranged that the reader readily understands the grounds on which tissues apparently so essentially different, as, for instance, fatty tissue and bone, are properly placed under a common heading.

In the chapter on muscular tissue we find a very elaborate account of the optical properties of striped muscular tissue, in which the author, although almost painfully accurate, yet fails to render his description easily intelligible to students who have no previous knowledge of the subject.

The structure of the cardiac muscle-fibres and the methods for its demonstration are, on the other hand, dismissed in a few words, the impression conveyed being that the muscle-fibres of the heart in no way differ essentially from ordinary striped muscular tissue, but are only peculiar in dividing. This, of course, is not the case, and the omission is somewhat unfortunate, inasmuch as the cardiac musculature forms the link which establishes the homology of striped muscular fibre with other varieties of nucleated cell-formations.

The description of the nervous tissues and the illustrations accompanying it leave little to be desired, especially noteworthy being the drawings of the nerves of the substance of the cornea and those of the distribution of nerve-fibres to muscular tissue and blood-vessels.

Before describing the compound tissues, to which the second part of this practical histology is devoted, an account of the various methods of injecting, hardening, and cutting sections is given, and here we are glad that preference is given to the razor rather than to any form of microtome.

In the description of the lymphatic system the histology of the endothelium of the serous membranes is very fully described, to the exclusion of much else of great importance, as, for example, the lymphatic sheaths of arterial vessels. The plates illustrating this part of the work also bear an unfairly large proportion to the whole number.

From this point the proofs of want of space become plainer at every page; the respiratory and digestive systems, the skin, genito-urinary apparatus, and the organs of special sense, are all discussed in less space than is given to the study of blood-corpuscles and epithelium, and the work concludes with two short chapters on embryology and the process of inflammation.

Mr. Schäfer's recently published text-book (2) is in many ways a marked contrast to the one we have just noticed; in it, microscopical drawings are conspicuous by their absence, and there is a careful abstention from descriptions of tissue-structure, while throughout the book the author rigidly keeps to his purpose of affording " to those engaged in the practical study of histology plain and intelligible directions for the suitable preparation of the animal tissues."

And it is, we believe, owing to this wise resolution that Mr. Schäfer has produced an exceedingly valuable work within a

very reasonable compass, for from beginning to end, wherever a lengthened description of any process has been required, fulness of detail is rarely if ever wanting.

The following extract will serve as a good example of the author's careful way of going to work :

"The animal having been killed by bleeding, the skin is snipped through around the upper part of the fore limb, and is then forcibly reflected from the limb. In this operation care must be taken to avoid besprinkling the subjacent parts with the cut hairs of the animal. A piece of the tissue over or between the muscles is then seized with the forceps, and snipped off with fine, sharp scissors. The snipped-off tissue shrinks immediately around the end of the forceps, and appears very unsuited for microscopical examination. But place it on a clean slide without the addition of any fluid, and with a pair of mounted needles. Endeavour, by drawing out first this corner and then that, to again reduce the gelatinous-looking piece to the condition of a thin film, and it will be found that this can be effected without much difficulty, for when not floated up by fluid the thin edges of the film tend to stick to the glass, and cease to shrink away from the position to which they are drawn by the needles. At the same time, while it is important not to add fluid to that which naturally moistens the piece of tissue, it is equally important never, during the whole process of stretching, to let the film become actually desiccated, for this would altogether ruin the tissue for microscopical purposes. The best way to prevent such an untoward result is to breathe now and then on the object while it is being prepared."

As to the order in which the tissues are examined, with some changes, the plan is similar to that followed by Dr. Klein; thus, the cornea is described with the rest of the eye instead of being used as an example of the connective tissues. The structure of the heart is given a chapter to itself, and embryonic and inflamed tissues are purposely omitted.

Purely as a matter of arrangement, it would surely have been better to group the spleen with the circulatory system rather than tack it on to the genito-urinary tract, with which its only obvious connection is that the so-called Malpighian bodies are found in both the spleen and the kidney.

The subject-matter of the book proper, naturally begins with the study of the blood and the application of various reagents to it; mammalian blood is first taken, and then that of the frog or newt. This order would, we think, be better reversed, owing to the greater ease with which amœboid movements can be studied in the corpuscles of the amphibia.

If a student, at the commencement of his histological education, can be induced to spend two or three hours at a stretch watching and drawing the movements of a colourless

blood-corpuscle, he will have made a great step towards that education of the eye which is necessary to make him an observer; and it is, therefore, as well to avoid harassing him at first with the complication of a warm stage, which for the observation of mammalian blood is indispensable.

To the description of the blood follows that of the various kinds of epithelium. The action of reagents on the phenomena of ciliary motion is well described, but the student will hardly find it worth while to attempt to preserve ciliated epithelium in the manner described on pp. 53, 54.

The silver method of staining is described *àpropos* of the preparation of the connective tissues; but we do not think that the *rationale* of the process, which, after all, is a sufficiently simple matter, is made plainer by the rather lengthy and confused explanation given in the text.

In this description the author uses the word epithelioid to express an epithelium-like arrangement of connective-tissue-cells. "The appearance is similar to what is observed in an epithelial tissue after the silver process." Here the word is used in a sufficiently limited sense; but we find later that all that group of cell-membranes generally known as endothelium are called "epithelioid cells," thus introducing a needless source of confusion, and substituting a clumsy word for one which is in very common use, and which serves its purpose well enough.

In the article on cartilage an account of the potash method of preparation described by Dr. Thin[1] would have been useful, and we are somewhat surprised to find no mention of cellular cartilage, which although in a sense an embryonic tissue, still has such an important relation to the other members of the cartilage group that its omission is to be regretted. The descriptions of the methods of examination of muscular tissue are very clear and possess more than ordinary interest, since this is a subject which the author has made peculiarly his own, and on which he is entitled to speak with authority.

The processes of embedding and cutting sections are detailed in the chapter on the circulation. This part of the subject is very completely treated, the description of the circulation in the frog's tongue being especially admirable. We have not found, however, any demonstration of the nerve plexuses in vascular walls, and a few words on the method of staining and clearing delicate sections without removing them from the slide on which they have been originally placed would have been useful, as it is a procedure which is not very troublesome and is sometimes valuable as a means of diminishing the chances of error by over-manipulation.

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[1] 'Journal Microsp. Sci.,' p. 1, vol. xvi.

It is right, however, to say that this proceeding is incidentally referred to later on.

For the injection of blood-vessels the pressure-bottle apparatus is recommended; for that of lymphatics acupuncture.

Up to this point we have followed Mr. Schäfer rather closely through his book because to the student the first is by far the most important part. Any one who conscientiously works through the programme here laid down for the examination of the simpler tissues will be nearly or quite able to stand alone by the time he comes to study the structure of the compound ones, and if, indeed, we were to discuss fully that part of the work which deals with the latter, there would be little but a monotonous reiteration of praise to give. Omissions, as a matter of course, there must be, and among the more noteworthy we would mention the examination of the renal epithelium when treated with monochromate of ammonia as described by Heidenhain. Eberth's method of examining the muscular substance of the heart by the silver process also might have been mentioned. The chapter on the central nervous system seems to us to be too short; that on the eye, however, is singularly complete and clear, being, indeed, quite the best account of the investigation of the organ which we have ever read.

In taking leave of Mr. Schäfer's 'Practical Histology' we feel that the thanks of both demonstrators and students of microscopical work are due to him for the conscientious way in which he has fulfilled his task.

We may fairly assume that the work just noticed reflects pretty accurately the manner in which a course of practical histology is desired to be carried out at the only medical school in London which possesses a physiological laboratory worthy the name. It would be interesting, therefore, to compare Mr. Schäfer's plan with that on which the class for practical histology is conducted at Edinburgh. Professor Rutherford's manual (3), is, however, professedly nothing more than a bare outline of the work there done, and a comparison would be manifestly unfair; the "outlines" being, indeed, in most parts little more than a carefully drawn up list of the various tissues to be examined. Professor Rutherford evidently finds that in order to render his course at all complete, a very careful arrangement of the work to be done and a rigid economy of time is requisite. Even as it stands, although the pruning knife has been used with no unsparing hand, we cannot but think that it must be nearly impossible to teach, in "thirty lessons of from an hour to an hour and a half each," the histology of the various tissues and organs mentioned in the text.

As regards the detail of the preparations we find the freezing

method is largely employed in investigations ; this, though undoubtedly a most valuable means of hardening many structures, and especially muscular tissue, is a somewhat dangerous method for general use, for there can be no doubt that it does alter, often to a considerable extent, the structural relations of parts.   As might have been expected, a greater prominence is given to the microtome for section cutting than either of the previous authors have accorded to it, but we are glad to see that the razor is recommended for all ordinary purposes.

To the histological notes proper is appended a very concisely written chapter on " Microscopical Methods," in which quite a wonderful amount of information is given in a small space.

The students of the physiological class of Edinburgh will find, no doubt, their labour of note-taking much lightened by having this little book ; away from there, however, it will probably be of more value to teachers, by affording hints how best they may arrange the time at their disposal, than to the students themselves.

Turning to Cambridge, in Professor Michael Foster's ' Practical Physiology' (4) we have presented the histological and physiological work done in his elementary class, the subject-matter of this book being worked over in about five months. The most noteworthy feature of the scheme is, that in it histology is regarded as one method only of physiological investigation, and the study of the structure of tissues and organs forms a part of the study of their physiological properties.   It would, indeed, be a great thing if all elementary physiological teaching could thus be associated with rational microscopic work, and we cannot but hope that the publication of this plan of blending practical histology with practical physiology will cause it to be, in part at least, generally adopted.   The difficulties of such a course will, we think, be found to lie rather with the teachers than the students, for while men fairly well qualified to teach structural anatomy and practical microscopic work are common enough, good practical physiologists are rare, and the combination of both these qualifications in one man rarer still.

While thus freely admitting the merits of Professor Foster's position so far as elementary teaching is concerned, we do not think it is advisable to carry the combination further, or to attempt to mix demonstrations of advanced physiological work with the more difficult and obscure histological investigations.

Indeed, the paths diverge at a wide angle after running but a short way together, and the best microscopical work is, and we believe must for a long time yet be done by workers whose single idea is to make out facts of structure in whatever way they

best can, without preconceived notions of what the physiological bearing of these facts ought to be.

A histological fact being once found out, he who has seen it is as well, but no better, qualified to reason on its meaning, as another to whom it has been described by drawings or in some other way. In the present state of our knowledge it is, we believe, choosing the safer if not the more excellent way to regard the practical study of microscopical anatomy as a skilled labour by means of which facts are to be established without regard to their physiological significance.

One other recent publication, or rather new edition, we should notice before closing this article. The 'Demonstrations of Microscopic Anatomy' (5), by Dr. Harley and Mr. Brown, professes to be an account of both normal and morbid histology. It, however, fails altogether to give even the most elementary account of modern microscopical science. The descriptions of tissues are often quite extraordinarily inaccurate, and when not that, are couched in such general terms as to render them valueless, while the illustrations in very many cases are copies of old figures, and the comparatively few original ones are in no way praiseworthy.

The student will be ill-advised who trusts to these " demonstrations" when he has text-books to choose from which show such honest work as those of Klein, Schäfer, Michael Foster, and Rutherford.

V.—Baumler on Syphilis.[1]

THE pathology of syphilis has lately been prominently brought before the profession by the two discussions at the Pathological Society, of which the last, on visceral syphilis, was, we think, by far the most valuable. There is a decided advantage in bringing together a number of specimens illustrative of the pathology of a particular subject; and even if no great addition be thus made to ascertained facts, such discussions serve the useful purpose of defining more clearly the limits of our certain knowledge, and indicating the directions in which future investigations may most usefully be pursued.

The most notable advance has certainly been made in the pathology of visceral syphilis, and it is only during the last

few years that the real character of many of the late manifes-
tations of the disease has been recognised. Herein, too, our
clinical knowledge has been much improved, and especially
that concerning the remote and modified symptoms both of
acquired and inherited syphilis. The value of such knowledge
will be apparent when we consider how rapidly many such
symptoms yield to antisyphilitic remedies, and how obstinately
they resist any other treatment. For those, then, who have not
the time nor opportunity to collect and master the various
contributions that have thus been made to the literature of
syphilis, a clear and concise statement of our present know-
ledge of the disease and its treatment becomes very desirable.
Dr. Bäumler's essay admirably supplies this need ; it is written
with great clearness and impartiality, is exceedingly complete,
and, besides exhibiting a thorough acquaintance with the work
of others, it gives evidence of much careful observation on the
part of the author himself.

Syphilis is defined as " a chronic infectious disease, whose
course, though protracted, is not on this account less typical
than that of other infectious diseases." And further on it is com-
pared particularly with the exanthemata, so that in this respect
the author looks upon syphilis, much in the same way as Mr.
Hutchinson and others, as strictly comparable to a specific
fever ; differing, however, from Mr. Hutchinson's view of the
sequelæ. Mr. Hutchinson's definition is that " in syphilis we
have to deal with a specific fever of prolonged but definite
stages, which is produced by contagion only, which has a period
of incubation, a period of outbreak (known as primary sym-
ptoms), a period of efflorescence or exanthem (known as the
secondary stage), and which in exceptional cases differs some-
what from its more short-lived congeners, by being followed
by sequelæ to which we give the name of tertiary symptoms."

To a considerable extent we admit that this comparison
holds good, but we think in the present state of our knowledge
it should be taken rather as a *comparison* than as a *definition ;*
for there are numerous important points in which syphilis
differs from the ordinary specific fevers, and those who adopt
such a definition are obliged to allow important exceptions, on
the part of syphilis, to the rules to which they wish to make
that disease conform.

For instance, as Sir William Gull[1] said, " It is a fever with-
out pyrexia;" also, " if it be placed among fevers, it is a fever
not limited in time as they are," or, as Dr. Bäumler puts
it, " the different stages are so far removed from each other

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[1] 'Path. Trans.,' vol. xxvii, p. 414.

that the connection between their symptoms is not at once manifest."

Again, its effect on the offspring of those who suffer from it is quite unlike anything we see in other fevers; so also is its mode of propagation, "by contact and by hereditary transmission," as Mr. Berkeley Hill pointed out.[1]

Mr. Hutchinson[2] is, moreover, obliged to admit that the irregularity of the fever is due to treatment, and that "if mercury were entirely put aside we should soon see that syphilis is quite as regular in its stages as variola, and also that it varies quite as little in its degree of severity in different persons. When mercury does not cure, it delays, and the retardation of stages sometimes witnessed under its use is very remarkable."[3] But recent researches into the subject of visceral syphilis have shown that gummata, which Mr. Hutchinson regards as sequelæ, may occur quite early in the disease, and in cases of inherited syphilis which have undergone no treatment. "Some of the most typical of visceral gummata are seen in still-born children."[4]

Again, Mr. Hutchinson speaks of tertiary symptoms as exceptional,[5] and corresponding to the occasional sequelæ of fevers; but surely tertiary symptoms occur with much greater frequency in syphilis than the so-called sequelæ in ordinary fevers.

A better definition, we think, is that of Dr. Bäumler:—"The disease consists essentially in an intoxication, which proceeds from a local infection, and gradually pervades the entire organism, manifesting itself in all the various tissues of the body under the form of an inflammatory process, of greater or less intensity, which, to a certain extent, pursues a course peculiar to itself." "The only conditions necessary to the inception of the disease are the presence of the specific poison, and its inoculation at any part of the skin or mucous membrane of a healthy, that is, not yet syphilitic person, though the disease may also be inherited."

It must be remembered, however, that, "etiologically speaking, syphilis and morbus venereus are not synonymous terms; syphilis may be a morbus venereus, as in fact it is in the great majority of instances, but not necessarily."

In the chapter on the history of the disease Dr. Bäumler shows that although syphilis may probably have existed at an earlier period, it was not recognised as a distinct disease until the severe outbreak in Italy at the end of the fifteenth century. At first

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[1] 'Path. Trans.,' vol. xxvii, p. 387.
[2] Ibid., p. 342.   [3] Ibid., p. 342.
[4] Ibid., p. 386.   [5] Ibid., p. 342.

gonorrhœa and other local affections of the genitals were distinguished from the constitutional disease, syphilis ; but after a time " all affections of the genitals springing from sexual intercourse were included in common under *morbus gallicus*." Then, in the latter half of the eighteenth century, Balfour and Benjamin Bell maintained the non-identity of gonorrhœa and syphilis, but their views were not generally accepted, in consequence of the great authority of John Hunter being in favour of the identity of gonorrhœa, chancre and lues venerea. Hunter's opinion was founded on his well-known experiments, in which constitutional syphilis followed the inoculation of a urethral discharge. But Ricord's experiments, made in 1831, have conclusively proved that the matter of gonorrhœa will not produce syphilis. He made many hundreds of such inoculations, and failed to produce a chancre or constitutional syphilis in a single instance, his explanation of Hunter's experiment being that a chancre concealed in the urethra had furnished the syphilitic virus. Mr. Lee, however, has shown that among the secondary symptoms of syphilis is a urethral discharge which is capable of communicating syphilis ; and he supposes, we think with good reason, that Hunter's inoculation was made from such a case. Whatever may be the explanation, however, of Hunter's observation, the experiments of Ricord have led to a definite separation of gonorrhœa from syphilis. Subsequently, in 1852, Bassereau proposed to separate from syphilis the soft chancre and its accompanying bubo.

The history of the disease shows, moreover, that its virulence has varied at different periods, and that there are now few parts of the world where it is not to be met with.

Dr. Bäumler's general description of the course of syphilis is exceedingly good.

After about three weeks from the period of inoculation, " a little red papule makes its appearance, which slowly increases in elevation and circumference, and after a few days may be felt with the finger as a distinct induration. The redness and hardness of this papule are quite sharply defined from the surrounding parts." This is the *primary lesion*. A few days later the lymphatic glands in whose district the infected part lies, take on the characteristic indolent swelling. Then upon the papule, at the point of inoculation, there forms a slight crust, which, being removed, reveals a small ulcer exuding a scanty secretion. Then, in from six to eight weeks, come the symptoms of constitutional infection ; the patient becomes pale and indisposed, eruptions occur upon the skin, the throat ulcerates, and flat condylomata appear upon the mucous membranes. Later follow the periostitis, alopecia, the iritis, and

the general indolent swelling of the lymphatic glands. In favorable cases the disease may now subside, and in about a year may have come to an end. Usually however, fresh crops of eruption come and go, papules appear on the tongue, or psoriasis upon the palms, and in debilitated persons ulcerations occur both of the soft parts and the bones.

The disease may, however, become latent for an indefinite period and then break out afresh, manifesting itself, after perhaps many years, by some form of tumour development, the growths of which exhibit a marked tendency to disintegration and necrosis. These "gummata" may occur in any organ, the necrosis of those superficially situated leading to indolent serpiginous ulcers and osseous caries, while those in the deeper parts undergo central degeneration, the periphery of the tumour becoming thickened into a fibrous envelope of connective tissue. "The internal organs which are most commonly attacked by gummy tumours are the liver, the testicles, and the brain with its membranes. The gummata proceed from the connective tissue, and especially from the adventitia of blood-vessels; they not unfrequently occasion grave symptoms, particularly when occurring in the brain." "Besides these local processes, in numerous cases, though by no means in all, a high grade of marasmus takes place, with marked pallor and great emaciation; and often, especially in affections of the bones, accompanied with fever of a decided remittent type." Amyloid disease may also ensue, and "at length the patients succumb either to anasarca, sometimes with dysenteric affections, or to uræmia or caseous pneumonia, unless beforehand some syphilitic local process implicating an important organ, as the brain or heart, puts a speedier termination to life." Thus, the course of the disease is as follows :— 1. The *infection.* 2. The *local affection.* 3. The *acute general affection,* the period of whose manifestation lasts for six or eight months, and is then followed by either a termination of the disease or—4. By a period of *latency,* having a variable duration, and passing into—5. The period of *gummous formations* and ulceration. 6. Syphilitic *marasmus.*

Ricord divides the disease into—" 1. *A primary period,* comprising the development of the local process of infection, and the indolent glandular swellings." 2. A *secondary period,* including the early superficial manifestations of the general infection. 3. A *tertiary period,* the symptoms of which occur late in the disease and affect the deeper tissues. Ricord considers that the disease in the tertiary stage is "not transmissible by inheritance, but may very likely give origin to scrofula and rachitis in the succeeding generations." Such a division, viewed

from the clinical point of view, is doubtless attractive and convenient, and indeed has been widely adopted; but no such sharp divisions in reality exist, any more than those which Zeissl and others have founded upon the type of the lesion (the *condylomatous* and the *gummous*); for, as Virchow has pointed out, many of the symptoms allotted to each of these stages may in some cases be seen simultaneously; and in inherited syphilis the most marked changes of the tertiary period may be seen in the viscera, while the skin exhibits equally well-marked secondary eruptions. Moreover, "it has been established by numerous observations that a man who at the time is apparently free from disease, or has been so for a number of years, but at some former time was syphilitic, is still capable of transmitting syphilis to the child he begets" (Bäumler, p. 53); and Mr. Pollock has quoted a case in which a woman suffering from tertiary syphilis gave birth to a syphilitic child.[1] We know of no facts which support Ricord's assumption that syphilis is the parent of scrofula or rickets; on the contrary, there are abundant reasons for believing the diseases to be perfectly distinct. At the same time we agree with Sir James Paget,[2] that syphilis may modify, or be modified, by other diseases, and that such mixed cases are by no means uncommon. Others, again, have founded a distinction between the stages of syphilis upon the effects of remedies, maintaining that the symptoms of Ricord's secondary stage are cured by mercury, while iodide of potassium has but little effect upon them; and that those of the tertiary period yield rapidly to the iodide, but that for these mercury is even pernicious. This division does not hold good, however, for it must be remembered that when the tertiary symptoms appear, mercury has usually already been administered for the secondary, and also that many of the late symptoms yield only to a combination of the two medicines.

Mr. Hutchinson, again, has separated the tertiary from the other stages in a still more decided manner, and maintains that at this period syphilis ceases to be a blood disease at all, and that "the gummata of the tertiary stage are purely local, and result from renewed growth in formations left over from the exanthem stage."[3] This view is not, however, by any means generally accepted, as the discussion at the Pathological Society showed. In a recent number of this review[4] we gave our reasons for differing from Mr. Hutchinson upon these points. We cannot but think that this careful observer, for whose work we have the greatest admiration and respect, has in this case allowed his judgment to be warped by a too

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[1] 'Path. Trans.,' vol. xxvii, p. 441.   [2] Ibid., p. 370.
[3] Ibid., p. 341 et seq.   [4] No. cxv, p. 40.

great desire to bring the phenomena of syphilis into an exact correspondence with those of the specific fevers. Mr. Pollock has made some very striking remarks upon this subject,[1] in which he shows by the relation of cases that " the tertiary disease gives rise to the secondary in the offspring, and that the offspring that has secondary disease produces the primary sore on the breast of a healthy woman;" and he pertinently asks how this sequence of events is explained on the theory that the tertiary stage is not a blood disease. We confess ourselves also unable to conceive that the power of hereditary transmission can persist after the cessation of blood contamination ; that—as Mr. Thomas Smith plainly stated it[2]—" a man may beget syphilitic children when he himself is no longer the subject of constitutional syphilis." Mr. Smith disposed of the objection that this happened in gout; *i. e.* " that a man may transmit to his son a something which may give no sign of existence until it becomes gout in the grandson ;" by showing, as Mr. Simon did also, that the two diseases were not really analogous. The hereditariness of gout depends upon the sperm giving a potentiality of development—

" Which requires years to breed the fully developed disease, for a man generally grows up into gout, or, perhaps, more often down into it ; and, indeed, it may require more years than there are in one man's lifetime, for in the case where gout survives a generation this potentiality takes one whole lifetime and part of another to produce the actual disease.

" In strong contrast to this are the circumstances attending the hereditary transmission of syphilis, in which, from the earliest contact of the parental sperm with the ovum. there is distinct evidence of the existence and transmission of a deadly disease, which may destroy the fertility of the seminal fluid, may blight the ovum, may kill the fœtus in utero, may attack the newly born infant, may poison the mother through the fœtus, in her pregnancy or during parturition."

Syphilis does not miss one generation and appear in the next, and, as Mr. Smith says, no one ever heard " of a mother contracting gout by bearing in her womb the child of a gouty father ;" yet—

" Who has not known of syphilis communicated in this way ; and how is the disease passed to the mother? Through the blood of the fœtus and in the placental villi, to the blood of the mother in the uterine sinuses."

We agree with Mr. Smith that these facts indisputably show syphilis, even in the tertiary stage, to be a blood disease in any reasonable sense of the term.

[1] ' Path. Trans.,' vol. xxvii, p. 441.
[2] Ibid., p. 411.   [3] Ibid., p. 418.

As Mr. Simon[1] stated it, "the hereditariness of gout is like the hereditariness of a Roman nose, it is part of the family likeness;" but that of syphilis more resembles the case of the silkworm disease, "it is like the passage of a parasite in the ovum from one generation to another. It is not the case of the law of development inherited from one to the other, but the case of a material something passing from one to the other."

Then, against Mr. Hutchinson's view of the tertiary symptoms being sequelæ and not the manifestations of a blood disease, we have the fact that the so-called tertiary gummata may be developed quite early in the disease; for instance, there may be gummata in the liver at the same time that there are secondary eruptions on the skin. Now, if the secondary eruption is a manifestation of a blood disease, surely the gummata are also. Moreover, as Mr. Pollock pointed out, there is nothing specific about the sequelæ of ordinary fevers; they are accidental, and may be symmetrical or not, as, for instance, the nodes on the tibia after typhoid; but the gummata of syphilis are specific, they are seen in syphilis, and nothing else.

Mr. Hutchinson maintains that the gummata of the tertiary period "result from renewed growth in formations left over from the exanthem stage," and he says they show by their non-symmetry that the blood is not concerned. But if symmetry is the test of blood disease, and if the secondary symptoms are symmetrical, Mr. Hutchinson has to explain, as Sir James Paget suggested, how it is that these residues, in which the tertiary gummata occur, are not symmetrical, although they belong to the secondary period; "why, being symmetrical in the forms of the secondary period, it becomes unsymmetrical in the manifestations of the third." Mr. Simon pointed out that this hypothesis involved the existence of an *active* sequela, and "when any one talks of a morbid *process* as the sequela of a given cause that means that the cause is still operative." Besides, if symmetry be taken as characteristic of a blood disease (though so great an authority as Sir William Jenner disputes this, instancing the non-symmetry of the typhoid eruption) we should still hold the facts to be against Mr. Hutchinson, for tertiary syphilitic ulcers and cerebral gummata are very frequently symmetrical, while secondary lesions are often not at all so. We have gone at some length into this question, for we consider it to be one of much importance. It seems to us a dangerous thing to teach that tertiary syphilis is not a communicable blood disease; on the other hand, we consider that there is abundant evidence to show that it is a blood disease which is

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communicable to the offspring, and may, through the offspring, be conveyed from father to mother.

Passing to the *pathology* of syphilis, Dr. Bäumler shows that we know little or nothing about the changes which take place in the blood, but that the distinctive character of its local manifestation is a cellular infiltration of the connective tissue. The cells have a close resemblance to those of lymphoma, and have no specific microscopic character; Virchow has compared thee ntire formation to granulation tissue. One of the most noticeable of the local manifestations of syphilis is the *gummy tumour* or *syphiloma* so frequently seen in "the subcutaneous cellular tissue, the skin, in and upon the bones, the liver, the testicles, the brain, the kidneys, and, especially in children, the lungs."

These growths " present the appearance of a greyish-red, soft, homogeneous mass, either without fluid contents or yielding a scanty juice-like mucus." They are seen either as minute infiltrations scattered throughout an organ, or as more or less defined tumours of varying size. As they develop externally, central degeneration takes place, into either a gummy or caseous material, which eventually becomes surrounded by a peripheral layer of fibrous tissue.

Of the etiology of syphilis, it can only be said that all we know of the disease, points to its being due to the introduction into the body of a specific contagium. But we know this contagium at present only by its effects; it has never yet been isolated.

There has been much discussion about the possible vehicles of the syphilitic contagium. It is certainly proved that the secretion of both primary and secondary syphilitic sores is highly contagious. Ricord for many years contended that the primary ulcer was the only source of syphilis, and denied the inoculability of the secretion of secondary sores. But this opinion was based upon experiments made on persons already syphilitic, and it has now been conclusively shown that secondary syphilis can be inoculated upon healthy persons, a fact which Ricord subsequently admitted. Dr. Bäumler, indeed, asserts " that the condylomata lata are the most common source of syphilitic contagion," a statement in which we are inclined to agree.

Von Rinecker, of Würzburg, has also related an instance of the successful inoculation of the secretion from an acne pustule in a child the subject of inherited syphilis.

The blood of syphilitic persons has also been proved to be capable of conveying the poison of syphilis, and this is probably the medium by which the disease has been conveyed, in those unfortunate cases in which syphilis has been communicated by vaccination. Dr. Bäumler goes carefully into this question of vaccino-syphilis, and shows that an investigation of Mr. Hutchin-

son's and other carefully recorded cases, points to the separate transmission of the syphilitic and vaccine poison ; and he states that "the negative results of experimental inoculations of healthy persons, from vaccine vesicles in persons manifestly syphilitic, so far as the transmission of syphilis is concerned, speak on behalf of the supposition that the pure contents of a vaccine vesicle developed in a syphilitic child, do not comprise the syphilitic poison."

Mr. Hutchinson believes, however, "that when the vesicle has been irritated for some time the serum of the blood transuding from the vessels may contain the syphilitic poison;" and Dr. Bäumler has repeatedly convinced himself " that a few blood-corpuscles, both red and white, occur in the purest lymph that first exudes from a seven-day-old vaccine vesicle." The physiological secretions of syphilitic persons, with the exception of the semen, do not appear to be vehicles of the syphilitic poison. The milk, for instance, does not seem to convey the disease. How far the pathological non-syphilitic secretions of syphilitic persons, are capable of conveying the disease is not yet clearly known, for in the hitherto recorded cases bearing upon this point, there has usually been an admixture of blood with the material which has been inoculated. Mr. Lee and others believe that the secretion of any mucous membrane in a syphilitic person, if in a state of irritation, is capable of conveying syphilis, and it has been proved by experiment, that the inoculation on a healthy person of a blennorhagic discharge from a person with constitutional syphilis may produce the disease.

Mr. Lee has, besides this, drawn attention to the fact that there is a syphilitic urethral discharge, which is sometimes the result of primary infection, and is also one of the secondary symptoms, and that such a discharge is a medium of contagion.

With regard to the question of infection by the semen, Von Bärensprung considers that this takes place only in the event of conception ; but Mr. Lee is of opinion that the seminal fluid may cause infection, without conception, not by means of the semen itself, but by the admixture with it of syphilitic virus from the urethra.

That the semen of a syphilitic father will produce syphilis in the child he begets is indisputably proven, as is also the fact that when the ovule is developed in a syphilitic mother, the fœtus will be syphilitic even though the ovule be impregnated by healthy semen. These are the conditions which give rise to inherited syphilis. The influence of the father seems more potent in this respect than that of the mother, the infection of the fœtus being most severe when both parents are diseased, and in the earlier stages of the parental disease. Usually the severity

of the fœtal infection decreases with the lapse of time from the
parental infection, excepting in those cases where the mother,
by successive pregnancies, becomes more and more infected, so
that in the later children the influence of the mother is added
to that of the father.   The women who are infected in this way
exhibit a modified form of the disease, and often escape most of
the secondary symptoms, suffering more often from a general
deterioration of their health, with alopecia, glandular enlarge-
ment, gummata, and other late symptoms.   It seems probable
that when a mother acquires syphilis during gestation she does
not infect the child, unless she acquires the disease before the
seventh month.

That syphilis is transmissible to the third generation, is proved
by a case related by Mr. Hutchinson.

The virulence of syphilis is usually considered to be most in-
tense in the primary lesion, though we think it probable that
some of the secondary lesions, as the flat condylomata, furnish a
secretion almost, if not quite, as infectious.   Opinions differ as
to the inoculability of tertiary secretions ; we do not doubt that
the poison is less active in these, but it must be remembered
that many tertiary lesions are characterised by degenerative and
ulcerative processes, which would probably be destructive of
their infective qualities.

The period of incubation in syphilis is established by numer-
ous observations, to have an average of from three to four
weeks, and is not affected by the part of the body which re-
ceives the poison.   Dr. Bäumler gives a table of thirty-one
accurately recorded cases, which exhibit a striking uniformity
in the incubative period.

After an excellent description of the primary lesion, Dr.
Bäumler devotes a section to the consideration of " the doctrine
of the unity and duality of the syphilitic and chancre poisons."
We think the authorities and facts herein adduced, are sufficient
to show that Mr. Hutchinson is not justified in saying that
" dualism is dead."   The question is argued with great care
and impartiality, and some of the sources of error clearly pointed
out.   For instance, it is shown that hard sores, being irritated,
furnish a secretion which is inoculable upon syphilitic subjects,
producing, without incubation, soft sores, the secretion of which
is further inoculable; also, that ordinary non-specific pus, when
inoculated on syphilitic persons, will sometimes produce precisely
similar ulcers.   Again, Kaposi has proved that " non-syphilitic
pus, such as we find in acne and scabies pustules in non-sy-
philitic persons, produces pustules whose pus proves to be inocul-
able in generations ; that loss of substance was caused by the
pustules, which healed by cicatrization, and that with the in-

crease in the number of pustules produced by the inoculations, the contagiousness of the pus diminished and finally became quite extinguished," characters exactly those of the soft chancre. From whence it follows, that certain kinds of pus have an irritative quality which renders it inoculable, and capable of producing sores which furnish an inoculable secretion. But "the real syphilitic virus, that is, the scanty, thin, transparent secretion of an induration, when inoculated upon the bearer, has really a quite different effect from that produced by the abundant purulent discharge which is excited in the sore by means of artificial or other irritation." The real syphilitic secretion is, however, only auto-inoculable before constitutional infection has taken place, *i. e.* during the first few weeks after the appearance of the sore. The soft chancre, therefore, is regarded as a purely local contagious affection, which, while it may stand in some remote relation to syphilis, does not necessarily do so. "There has never yet been a single instance where syphilis was caused by inoculation of the chancre-pus (soft chancre) from an individual who had only chancre and not syphilis."

Doubtless the distinction between the two kinds of sores may be sometimes obscured, but this is no argument against their being essentially different; and the observations of Mr. Lee have done much to explain the manner in which these confusions have arisen. He has shown that *twofold inoculation* may occur from the mixture of the secretion of a suppurating sore with that of an infecting sore, for the two actions do not exclude each other. In such a case the suppurating sore (having the shorter period of incubation) appears first and it runs its course, and subsequent induration will occur at the seat of inoculation, and be followed by constitutional syphilis. Bassereau based his distinction, not so much upon the form of the sore, as upon the fact that the local sore will only give rise to a local disease, while the sore which is accompanied or followed by constitutional symptoms, will communicate the constitutional disease. The doctrine of unity, on the other hand, affirms that, "under whatever form the poison manifests itself, this form may become again converted into the other," as Dr. Bäumler clearly expresses it. "Practically, the doctrine of unity can only mean that the venereal poison is capable of producing syphilis, whether the primary affection appears under the form of a soft chancre, or after a considerable period of incubation, under the form of a papule, with subsequent superficial ulceration and induration; and hence that an infection from the secretion of a soft chancre, may be either followed by a soft chancre again, or by an induration and constitutional syphilis; and that, on the other hand, an infection from the secretion of an induration or other

syphilitic local affection may be followed by a simple soft chancre."

We do not agree with Mr. Hutchinson that this is the generally accepted view, neither do we think with him, that it is a far simpler creed to believe that the soft chancre is attributable " to contagion with inflammatory products produced by syphilis, but not, as a rule, containing its germs," than that there are two distinct kinds of sores, one of which gives rise to a local, the other to a constitutional disease.

The conclusions to be drawn from the ascertained facts relating to this question are formulated with admirable clearness by Dr. Bäumler, who concludes this portion of his treatise as follows:

" When we remember the multiplicity of secretions which are often mingled together at the injecting source, there is no wonder that, in practice, syphilitic infection does not preserve the pure and simple character which is observed in pathological experiments. Hence we accept only one syphilitic poison, but in another sense from that given to it in the doctrine of unity. We do not regard the contagious principle of chancre as identical with this poison. It is a pathological agent by itself, but a far less constant and uniform one than the poison of syphilis, since we have seen that it may be generated *de novo* under the co-operation of certain influences, and quite independently of syphilis."

The microscopical character of the indurated tissue of the primary lesion is that of a dense cellular infiltration. Nucleated cells are closely crowded together in the tissues of the part, and especially in the adventitia of the blood-vessels.

The induration varies very much in amount and area, the differences depending chiefly upon the tissue affected. Dr. Bäumler has ascertained that well-marked induration is not so uncommon in females as has been by some asserted. The time required for the hardness to disappear is also very variable; it may leave no vestige of its existence, or may be marked by a scar, often pigmented. In some few rare cases a relapse of the induration may take place, long after its apparent removal, and without any fresh contagion.

The characteristic enlargement and induration of the lymphatic glands first in order from the primary lesion, usually becomes evident in about a week after the appearance of the latter, and is often valuable as an indication of the situation of the original infection.

Dr. Bäumler does not agree with Mr. Lee that the glands second in order never become affected from the primary lesion, and quotes two cases in which this was observed; it is doubtless, however, a very rare occurrence.

The period which elapses between the appearance of the primary affection, and the eruption of general symptoms, the so-called second incubation, may vary from two to twenty weeks, but is usually from six to seven weeks. Then comes the eruptive fever, which precedes the appearance of the secondary symptoms. It varies much in intensity and is often of a remittent type; sometimes it is absent, or at least is not noticed. The disease has now become *constitutional*.

The earliest constitutional symptom is usually an eruption upon the skin and mucous membranes. These eruptions have certain characters which are often valuable aids in the diagnosis. The coppery red colour of syphilitic eruptions is well known; it is especially seen when the eruption is fading, but is not absolutely peculiar to syphilis; a striking feature also is their polymorphous character; " it is very common to see macules, papules, vesicles and pustules side by side." The anatomical basis for them all is a cellular infiltration, and the most diverse may ultimately assume the same form. Syphilitic eruptions are often of annular form, especially those of later appearance and chronic character; they give rise, as a rule, to very little itching. After giving the usual division of syphilides into exanthematous, vesicular, pustular, papular, squamous, and tubercular, Dr. Bäumler proposes a more anatomical nomenclature as follows:

I. Circumscribed hyperæmias with but slight infiltration.
<p style="text-align:center">Macular syphilide—Roseola.</p>

II Marked infiltration of the papular body.
    1. In the form of papules. Papular syphilide.
    2. In large patches—squamous syphilide on mucous membranes or at favorable points on the cutis. Moist papules. (Condylomata lata.)

III. Especial implication of the immediate vicinity of the hair or sebaceous follicles.
    1. Simple infiltration with either scanty or no exudation in the follicles.
<p style="text-align:center">Lichen syphiliticus.</p>
    2. With acute suppuration in the follicle.
<p style="text-align:center">Acne syphilitica.</p>
    3. Exudation into small, markedly infiltrated groups of follicles, with rapid formation of crusts.
<p style="text-align:center">Impetigo syphilitica.</p>

IV. Infiltration with sub-epithelial suppuration and superficial ulceration.
<p style="text-align:center">Pustular syphilide.
Varicella syphilitica.
Pemphigus syphilitica.
Ecthyma syphilitica.
Rupia syphilitica.</p>

V. Infiltration with disintegration to a considerable depth (gummous development).

Tubercular syphilide (lupus syphiliticus).

We think this a very good arrangement, and much better than the absurd attempts to give a name to each of the innumerable varieties which syphilitic eruptions present.

Dr. Bäumler gives an admirable description of the syphilides, with the modifications they exhibit on the mucous membranes, and points out the highly contagious character of the secretion of the condylomata lata. We are sure that these are a common source of contagion. In speaking of the bone affections, a distinction is drawn between the early swellings leading to the formation of tophi, and the later gummous tumours, ending in ulceration or absorption and corresponding depressions. The increase in the pain of the early periosteal affections which occurs at night, is attributed to the exacerbation of the fever which takes place in the evening, causing a dilatation of the peripheral blood-vessels and a determination of blood to those parts of the periosteum and bones which lie nearest the skin.

A short description is given of the peculiar epiphysial disease which is met with in inherited syphilis, and of which Dr. R. W. Taylor has lately published an elaborate description. This disease, which has only lately been recognised as a manifestation of syphilis, and of which the present writer has seen a considerable number of cases, is one of great interest, inasmuch as it is sometimes almost the only symptom of inherited syphilis, and if seen early, yields with great rapidity to mercurial treatment. As no other treatment is of any avail, the recognition of its true character is very important. The affection consists of an inflammatory affection of the ossifying layer at the epiphysis, which leads to the formation of a granulation tissue instead of the natural osteo-blastema, and consequently to a separation of the epiphysis from the shaft of the bone. It is sometimes accompanied by suppuration in the surrounding tissues, though the joint itself is not usually involved. Clinically it is distinguished by a pseudo-paralysis of the affected limbs, which hang down as if powerless or fractured, or are sometimes held fixed as in articular disease. There is a swelling to be felt just above the joint, and the epiphysis can often be distinguished as entirely separated from the diaphysis. According to Parrot the bones are affected in the following order of frequency—the femur, humerus, tibia, ulna, radius, fibula, ribs, ilium, scapula, clavicle, tarsal and carpal bones.[1]

It usually develops within the first few months of infancy,

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[1] See No. cxii of this Review, p. 420; also No. cxi, p. 30.

and Dr. Bäumler asserts that it is not met with in acquired syphilis. Dr. Taylor has recorded a case, however, in which the disease was observed in a child who acquired syphilis by means of an inoculation upon the lip. The rheumatoid joint affection sometimes seen in syphilis, is characterised by pain and effusion into a joint, accompanied by a distinctly remittent fever.

The general enlargement of the lymphatic glands is a very constant symptom of constitutional syphilis.

"These glands vary in size from a shot to an almond, seldom larger; are elastic, firm, movable, defined, isolable, painless, and remain for a long time without alteration; they do not tend to caseation. Although, therefore, on a single examination they may closely resemble other forms of glandular enlargement, yet if they are watched they are seen to differ from these by their indolence; they do not grow like those in lymphadenoma, nor caseate like the scrofulous."[1]

Enlargement of the spleen is seen both in inherited and in acquired syphilis.

The liver may be affected by a general or a circumscribed interstitial hepatitis, the latter of which is the cause of the fibrous cicatrices met with in the organ, in cases of syphilis. Gummata also are common in the liver.

A good description is given by Dr. Bäumler of the condition of the teeth found in inherited syphilis, for the knowledge of which we are indebted to Mr. Hutchinson. Allusion is also made to the occurrence of retro-pharyngeal gummata, an important point to remember in the surgery of this region. Syphilitic ulceration occurs rarely in the small intestine, very frequently in the large, especially the rectum, of which it is often the cause of stricture.

Ozæna is common both in inherited and acquired syphilis, and when occurring in the tertiary stage, usually depends, we think, on bony necrosis. Such cases are only curable by the removal of the sequestra, for which we would point out that Ronge's operation, of lifting the soft parts of the nose from their bony framework, affords great facilities.

The nasal catarrh, or " snuffles," of inherited syphilis is one of the most constant symptoms.

Ulceration of the larynx, trachea, and bronchi, is seen both in the secondary and tertiary stages, and may lead to necrosis of the cartilages.

The lungs of children with inherited syphilis are liable to a change described by Virchow as " white hepatization," which is due to a diffuse infiltration of the alveolar septa; it is probable that similar changes occur in adults, and lead to the formation

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[1] No. cxiii of this Review, p. 201.

of fibrous cicatrices. Gummata are also seen in the lungs, and in their degeneration may closely resemble tuberculous disease.

We are hardly yet justified in speaking of a *syphilitic phthisis;* but cases are unquestionably met with clinically in which syphilitic persons exhibit the ordinary symptoms of phthisis, which clear up under the administration of mercury and iodide of potassium. Such cases were alluded to by Dr. Pye-Smith at the discussion on visceral syphilis at the Pathological Society; and on the same occasion Dr. Green and others exhibited specimens of lungs from syphilitic persons, which seem to point to the existence of a chronic inflammation of the lung, of a truly syphilitic character. The histology of such cases is certainly not yet conclusive of their syphilitic character. The small arteries of the lung do not exhibit the changes in the inner coat seen in those of the brain, and which are, perhaps, more than aught else, histologically characteristic of syphilis; yet the specimens exhibited appeared to indicate that the new growth originates around the small arteries, and not around the bronchi, which only become secondarily involved, that it begins around the interlobular vessels and not in the alveolar walls, and that it exhibits an extraordinary vascularity—characters which, with the analogy such changes present to syphilitic lesions in other organs, and their clinical histories, make their syphilitic origin at least probable.

It must be remembered that our diagnosis of the specific character of a lesion, must often depend very much upon the associated symptoms and the effect of treatment, and also that there are syphilitic affections, as, for instance, the enlargement of the spleen, which may leave behind nothing that can be recognised post-mortem. The heart and pericardium are the seat of gummata, which probably also affect the large blood-vessels; by the degeneration of the latter it is supposed that syphilis may be indirectly the cause of aneurism.

Very important are the changes which occur in the small blood-vessels, and which, by their disturbance of the nutrition and function of the affected organ, often give rise to serious symptoms. This is especially the case in the brain, wherein these changes have been most carefully studied.

Heubner has published a careful description of syphilitic disease of the cerebral arteries, and at the discussion at the Pathological Society on visceral syphilis excellent specimens were exhibited by Drs. Greenfield, Barlow, and Gowers, illustrative of this condition. The essence of the disease is a thickening of the adventitia and of the intima, especially the latter, due to infiltration with a small-celled growth. This growth becomes organized, and may subsequently undergo rapid

degeneration. The lumen of the vessel becomes narrowed or occluded, either by the growth alone, or partly by the growth and partly by thrombosis, and thus the blood supply is cut off from that portion of the brain to which the vessel is distributed, and the part undergoes consequent softening. Dr. Greenfield showed that the same changes occur in the vessels of the kidney and of other organs in cases of syphilis. This is one mode in which the brain becomes diseased in syphilis ; it is also invaded by the small-celled growth extending into its substance along the vessels, and leading to the formation of gummata, with consequent secondary changes of the tissues pressed upon or interfered with. The ganglia of the sympathetic are also liable to connective tissue growth and consequent atrophy of the nervous elements.

Besides these forms of disease, there may be a chronic, slowly progressive meningitis, a specimen of which was exhibited by Dr. Barlow from a case of inherited syphilis. In this case there were characteristic changes in the cerebral vessels and disseminated syphilitic growths in the chorio-capillaris. The nerves may also suffer injury by narrowing of the bony or membranous sheaths through which they pass, owing to syphilitic disease. The organs of special sense are variously affected by syphilis. The nerves may be interfered with by pressure in the manner just described, or by the necrosis of surrounding or adjacent bone ; in this way the sense of smell or sight may be destroyed.

Iritis is usually a secondary symptom, and the result of acquired syphilis, but it has been also occasionally but rarely seen in the inherited disease. Syphilomata are also met with in the iris, usually near its margin. Interstitial keratitis Hutchinson has shown to be almost exclusively confined to the inherited form of the disease ; it is seen about the time of the second dentition or puberty. The choroid, the retina, and the optic nerve may, any of them, be affected by syphilitic infiltrations. Deafness may result either from inflammatory destruction of the tympanic structures, starting often from the throat affection, or from some obscure lesion of the labyrinth or auditory nerve.

Syphilis causes, besides gummata in the kidney, a diffuse interstitial infiltration, accompanied by albuminuria. Several cases are on record of complete recovery from syphilitic albuminuria. It must also be remembered that syphilis may indirectly lead to amyloid degeneration of any of the viscera.

Syphilis of the testicle is met with either in the form of gummata or as an interstitial growth ; it is usually a late symptom. It is well to bear in mind that gummata are sometimes met

with in the breast, where they may simulate other growths less amenable to treatment.

The placenta is not always the seat of disease when the fœtus is syphilitic; it may, however, exhibit either gummata or a cell-infiltration of the villi.

The subject of inherited syphilis has recently been treated in this Review,[1] and many of its manifestations have been alluded to in the course of this article, so that our space will not allow of our entering further into its consideration. Dr. Bäumler has a short chapter in which he enumerates the usual symptoms; his treatment of this part of his subject is, however, less full than we could have desired.

The question of how far syphilis may be a cause of scrofula or tuberculosis was one of those introduced for discussion at the Pathological Society by Mr. Hutchinson. Both Dr. Bäumler and Mr. Hutchinson believe that there is no connection whatever between these diseases and syphilis, an opinion which we think is amply justified by the facts of the case, among the most striking of which, are the results of treatment.

We are not inclined, however, to go so far as Mr. Hutchinson in his belief that syphilis and other constitutional diseases do not, as a rule, modify each other, although he was supported herein by so weighty an authority as Mr. Simon. We are rather inclined to the opinion expressed by Sir James Paget and Mr. Pollock, that syphilis does undergo very considerable modifications in persons of different constitutions who contract it; and we believe a very useful study is that of mixed and diluted constitutional diseases, and of the variations to be observed in them. As to the possibility of the *cure* of, or recovery from, syphilis, Dr. Bäumler gives a favorable opinion, based upon the fact that there are on record cases in which a second infection has occurred; but he admits the difficulty in deciding when such recovery is complete, and the necessity, therefore, of a cautious prognosis. " It is to be regretted," says our author, " that we possess no criterion of the actual cure of syphilis; when we see, after the acute stage, a period of many years of undisturbed health, and the procreation of healthy offspring, it is highly probable that there has been a complete and permanent cure, but there is no absolute certainty as to the future." Mr. Lee, however, believes that in the condition of the inguinal glands we have a very important index of the presence of or recovery from the constitutional disease, although in any given case he would not rely upon any one indication exclusively. Mr. Lee has paid especial attention to this point, and as the

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[1] No. cxi, p. 23.

result of his inquiries says, "If these glands have become enlarged in the manner described (amygdaloid) at the time the patient contracts syphilis, and if they remain enlarged, as they very often indeed do, then the patient is still syphilitic and cannot be reinfected. If, on the contrary, the enlargement has disappeared, as I am satisfied it has done in a number of well-marked cases, then the patient may again be subject to the constitutional form of the disease."

The treatment of syphilis is well described by Dr. Bäumler, and may be summed up in the word mercury. The administration should be commenced as soon as the nature of the primary lesion is determined, and prolonged over several months. There are good reasons for believing that in some cases secondary symptoms may be thus prevented. Dr. Bäumler lays great stress upon the danger of a too early cessation or interruption of the treatment, but we think he does not give quite due weight to the advantages of introducing mercury through the skin, over any other method. How often when it is given by the stomach has its administration necessarily to be interrupted on account of the digestive disturbance it sets up, a trouble the more desirable to be avoided seeing that we wish to keep the patient for a long time under the influence of the drug.

Due value is given to the iodide of potassium also, which is shown to have a remarkable and rapid influence upon the fever, as well as upon the later manifestations of syphilis.

The practice of so-called "syphilization" is alluded to, and Dr. Bäumler shows that it has no recommendation either theoretical or practical.

VI.—Hypertrophic Cirrhosis of the Liver.[1]

Rokitansky,[2] under the head of 'Cirrhosis of the Liver,' describes the first form as "a morbid development of the capillary gall-ducts; an accumulation of their secretion and probably also a hypertrophy of the parietes of those vessels;" in this disease he

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[1] 1. Hanot.—*Cirrhose Hypertrophique du Foie.* Thesis. Paris, 1875.

2. Charcot and Gombault.—1. *Note sur les Alterations du Foie Consecutive à la Ligature du Canal Cholédoque,* 'Arch. de Phys.,' Mai, Juin, 1876. 2. *Contributions à l'Étude Anatomique des différentes Formes de la Cirrhose du foie.* Ibid., Sept., Oct., 1876.

3. Cornil and Ranvier.—*Manuel d'Histologie Pathologique.* Paris, 1876.

4. Kelsch and Kiener.—1. *Contribution à l'Histoire de l'Adenome du foie,* 'Arch. de Phys.,' Sept., Oct., 1876. 2. *Note sur la Néoformation des Canalicules Biliares dans l'Hepatite,* Ibid., Nov., Dec., 1876.

[2] Rokitansky, 'Path. Anat.,' Syd. Soc., vol. ii, p. 141.

says, "the liver is frequently enlarged, but certainly not diminished in size." On the other hand Frerichs[1] writes :—

" Rokitansky refers one form of cirrhosis to a morbid development of the capillary bile-ducts. I have never met with cases which would admit of such an explanation. In the various grades or forms of obstruction to the flow of bile which have come under my notice the liver never presented any granular character. The enlargement of the bile-ducts did not extend to their origin, but apparently terminated earlier in the form of elongated hollow spaces, surrounded by hepatic cells, loaded with pigment. Thus we regard the various conditions just described as essentially different from cirrhotic induration which we refer to a chronic interstitial hepatitis."

Such a contradiction from so eminent an authority must have had weight, and it is apparent when we read the first edition of Dr. Murchison's ' Lectures on Diseases of the Liver' that, although in certain cases of jaundice the account of the autopsy describes " a granular appearance," or a " remarkably firm and tough" condition of the liver (see cases 29 and 30), he does not give it any place even among the spurious forms of cirrhosis. On the Continent there was a gradually growing opinion that there existed a form of cirrhosis not conforming to the dictum of Bichat, " cet état ne se complique jamais du volume extraordinaire du foie."

In 1866 Wyss published an account of the structural changes in jaundice, and recognised the existence of some degree of new growth of connective tissue which, he says, never invades the lobules; his researches did not give much support to the theory of a biliary form of cirrhosis. Still, many observers, especially in France, from time to time noticed cases of enlargement of the liver persisting up to the last, and in which the organ presented manifest appearances of cirrhosis, but the microscope was not employed to explain these changes. In 1871 M. Ollivier[2] in a series of papers contributed to ' L'Union Medicale' sought to establish the existence of a hypertrophic cirrhosis differing clinically from the common form, and deserving to be considered not a mere variety or stage, but an independent pathological condition. In 1872 Heinrich Mayer, experimenting on animals, found in jaundice an increase of the connective tissue, both around the vessels and in the lobules.

In 1874 M. Hayem[3] supplemented Ollivier's work by adding the anatomical details of two cases which he had studied. In only one of these cases was jaundice present, and that was accounted for by the presence of inflamed glands in the fundus of the liver com-

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[1] Frerichs, ' Clin. Treatise on Dis. of the Liver,' New Syd. Soc., vol. ii, p. 32.

[2] ' L'Union Méd.,' 1871.

[3] Hayem, " Contribution à l'Etude de l'Hepatite Interstitielle Chronique," ' Arch. de Phys.,' 1874, p. 126.

pressing the ducts ; he did not connect the disease in any way with obstruction to the bile-ducts, but considered it a chronic inflammation following the course of the blood-vessels, and he pointed out that it not only circumscribed but invaded the acini.   Ascites was absent in both cases ; he describes the ducts as normal ; the new growth was a formation of connective tissue with abundant nuclei, in which no development or dilatation of biliary canaliculi is described or figured.   In the same volume some months later M. Cornil[1] drew especial attention to the numerous biliary canaliculi present especially in cases of " hypertrophic cirrhosis" of the liver. In a previous article (' Archives de Phys.,' 1871, p. 462) he had described a similar appearance in a case of acute yellow atrophy, and in the present paper he correlated the various observations and adopted the view that these are pre-existing intralobular bile-ducts which have become visible by the destruction of the hepatic cells. He says :

" These canals, hollowed out in the midst of connective tissue, possessed a very then, not isolable tunica propria, and, in their interior they have little cuboid cells arranged very regularly, planted perpendicularly on their wall and leaving a central empty lumen. As you approach the centre of an acinus they become a little more slender and the cells have a less regular form, often elongated in the direction of the vessel and irregularly pavement-like.   In the parts where there are liver cells the ducts are very difficult to discover."

These appearances had been observed by Waldeyer and by Klebs ; the first of these writers considered them to be a proliferation of the cells of the bile-ducts destined to replace the loss of substance caused by the distinction of the liver-cells ; Klebs, on the other hand, regarded them simply as altered or atrophied liver cells and not ducts at all.

Dr. Beale has published a case, entitled " Liver presenting many of the characters of cirrhosis, resulting from the obstruction of common duct ;" the obstruction was caused by an enlarged lymphatic gland compressing the duct ; the liver was a little larger than normal, hard, pale, with uneven surface, the newly formed tissue surrounded each lobule and seemed to be substituted for an equal amount of liver substance destroyed ; the biliary canaliculi were distended and formed a sort of network.   Dr. Beale distinctly ascribed the cirrhosis to the obstruction of the ducts.

In 1873 Dr. Wickham Legg published his paper " On the Changes in the Liver which follow Ligature of the Bile-ducts."[2] This described the results of ligature of the common duct in sixteen experiments on cats, and the general result was to show that this

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[1] Cornil, " Note pour servir à l'Hist. Anat. de la Cirrh. Hepatique," ' Arch. de Phys.,' 1874, p. 265.

[2] ' St. Bartholomew's Hosp. Reports,' 1873, p. 161.

was followed by enlargement of the liver and a formation of new connective tissue in the portal canals and around and within the lobules. He makes no mention of any abnormal appearance of biliary canaliculi. Unfortunately Dr. Legg, although comparing these changes to those which occur in obstruction of the duct from congenital inflammation, attributes the new growth to the extension of an inflammatory process set up in the fibrous sheath of the duct by the irritation of the ligature. This view is disproved by certain of his own experiments, as in two cases the bile found its way into the intestines in spite of the ligature and as changes resulted, while in another case in which the left duct was tied the changes were found only in the left lobe. Indeed this explanation, if true, would render the contribution valueless, as it would introduce an element seldom present in the pathological processes of the human subject, to elucidate which we presume the experiments were undertaken.

The appearance of M. Hanot's thesis in 1875 marks an epoch in the history of cirrhosis of the liver, not only by its intrinsic value, but by the researches by which it has been followed. M. Hanot's personal observations extend to four cases which clinically presented the phenomena of chronic icterus associated with hypertrophy of the liver. He was unable to study the development of the hypertrophy. When the patients presented themselves the condition was already well marked; the liver was resistant to palpation; its surface was generally smooth, and the organ distended the abdominal walls; sometimes the presence of chronic perihepatitis gave the organ a rugged feel. The spleen was also hypertrophied; ascites was notably absent or supervened to a slight degree late in the disease. There was no abnormal distension of the subcutaneous abdominal veins. The general symptoms, as the disease progressed, were pain in the hypochondrium, fever, anorexia, wasting, constipation, tympanitis, with periodical exacerbations; death was preceded by augmentation of these, delirium, intestinal and gastric hæmorrhages, extreme marasmus, and finally coma.

Macroscopic Appearances.—The enlargement of the liver was very great, weighing in two of the three cases over 2500 grammes. There was generally perihepatitis, with adhesions to the diaphragm, to the anterior wall of the abdomen, and to the stomach, the intestine, &c. The surface of the liver, apart from the perihepatitis, was granular, covered with little yellow or greenish projections, but generally not so marked as in atrophic cirrhosis. In some cases the liver was coloured buff, greenish, yellow, or olive green; on section grey fibrous bands were seen passing through the liver substance. The form of the liver was not altered, although enlarged; there were no cicatrices. The external bile apparatus presented slight changes; sometimes some thickening of the mucous membrane was present. In those cases associated with calculi a marked catarrhal inflamma-

tion of the bile passages was present. In his cases he never saw
hypertrophied or inflamed glands in the hilus.

Microscopical Appearances—The Cells.—Very generally their
shape was little altered, but they were granular and fatty ; some con-
tained clear yellow granules of bile pigment. They were separated
from each other by spaces as great as their own diameters, in which
were embryonic elements and pigment granules. Towards the peri-
phery of the lobules bands of fibrous tissue separated the cells,
which were flattened and compressed by zones of connective tissue
often two or three times their diameter, the cells being reduced to
small irregular masses of granular protoplasm. In the olive-green
livers the cells were, for the most part, only masses of pigment.
granules, mingled with oil-globules, and showing no nuclei.

The Connective Tissue.—In some parts the interlobular connec-
tive tissue appeared scarcely modified ; generally the lobules were
separated by bands of fibrillated connective tissue, in which ramified
a more or less abundant plexus of bile-ducts. Between two neigh-
bouring lobules these bands were sometimes three times as broad as
the average diameter of a lobule, and rarely less than a millimètre in
breadth. From these extralobular bands processes passed within
the acini, separating and compressing the cells as described above ; in
some places no interlobular invasion was apparent, in others the
acini were reduced to some atrophied, granular, pigmented cells, lost
amid a fibrillar tract which traversed everywhere the remains of the
lobule. Generally at the exterior of the lobule the sclerosis was
marked, while towards the centre embryonic elements alone occupied
the spaces between the cells. Sometimes small masses of embryonic
tissue were found between the bundles of the hypertrophied connec-
tive tissue within and without the lobules. The distribution of the
connective-tissue bundles was parallel to the course of the bile-ducts.

The Bile-ducts.—The most striking appearance in the sections
was the great development of biliary canaliculi in the extralobular
connective tissue ; these formed a very abundant network forming
loops ; they mostly divided dichotomously and joined neighbouring
loops or doubled on themselves, making figures of eight ; they were
irregularly dilated, and sometimes moniliform. These ducts con-
tained a varying number of polygonal cells, heaped one upon the
other, and which contained nuclei and granules ; in some places
cells were arranged along the inner surface of the duct in one or two
rows, leaving a distinct lumen ; in others they completely filled the
whole interior. Here and there the cells were coloured greenish
yellow, and contained granules of bile pigment. In the olive-green
livers these cells were infiltrated with bile pigment, which also was
free in the lumen of the ducts. Often around the canaliculi the
connective tissue was closer and formed a sort of fibroid sheath ; the
arrangement of the bundles of connective tissue seemed to follow the

course of the ducts. These abnormally developed canaliculi could be traced to the periphery of the lobules, where they became very fine capillary tubes and were lost in the fibrous zone which encroached upon the lobules.

The Vessels.—No important modifications were found in these structures; often they were quite normal, frequently they were dilated; the divisions of the portal vein appeared as sinuses hollowed out of the perilobular connective tissue, their walls being often indistinguishable from the surrounding tissues; the young cells were not more numerous around the blood-vessels, while they were greatly multiplied around the bile-ducts which seemed to constitute formative centres. The hepatic artery showed no alterations. The lymphatic system presented nothing noticeable except well-marked lymph spaces in the newly formed tissue.

M. Hanot believes that the point of departure of the cirrhosis is an inflammation of the ducts and their fibrous tissues. In support of this he quotes two cases; the first, examined by M. Pitres in the laboratory of Professor Charcot, was a case of chronic jaundice, and the liver showed islands of newly formed connective tissue in the interlobular spaces which contained dilated interlobular bile-ducts, and radiating from those smaller canals which lost themselves in the adjoining acini. The second case is one of Professor Kussmaul's, taken from the Berl. Klin. Wochenschrift, in which a certain amount of cirrhosis is described following chronic obstruction of the ducts by a gall-stone. M. Hanot considers the disease he has described to be characterised clinically by the enlargement of the liver, the early and marked jaundice, and the absence of ascites; microscopically, this form of cirrhosis takes its point of departure from the bile-ducts, originating in an inflammation of their coats; he looks upon the new formation of bile-ducts as a result of this inflammation which leads to obstruction to the passage of bile, dilatation of the capillary ducts, and the extension of a catarrhal inflammation which fills them with small cells, and subsequently their dilated canals become lined with epithelium; consequently he considers this new formation as characteristic of the condition. Besides these features, he lays stress on the invasion of the lobules by the new growth of connective tissue.

In support of M. Hanot's views, and in order to give substantial proof of the true relations of the changes, MM. Charcot and Gombault undertook a series of experiments similar to those of Dr. Wickham Legg.

These experiments, seven in number, were made on guinea pigs; of these seven animals, five died between the fifth and the twelfth days, the sixth was killed on the tenth, and the seventh on the twenty-third day. In no case did the bile find its way again into the intestines. During life the animals presented no special phenomena; they wasted,

lost their appetites and died ; there was in no case any trace of jaundice ; the urine never gave a reaction with the nitric-acid test.

Macroscopic Appearances.—There was usually only a slight degree of peritonitis localised about the inferior surface of the liver and the abdominal wound. The liver seemed in every case larger than normal, its colour was pale, yellowish, often nutmeggy ; in the latter case the central substance was yellow and translucent, while the peripheral zones were slightly red ; there were never any granulations. The external bile apparatus was always dilated, sometimes considerably ; its walls were thickened, and had lost their normal transparence. The gall-bladder contained thick bile mixed with a large quantity of mucus and bile-stained epithelium. In one case in which the bile was examined microscopically immediately after killing the animal by pithing, it was found to contain vibriones, which manifested active movements.

Microscopical Appearances, the cells.—Although the lobules were extremely atrophied by the connective-tissue changes to be immediately described, the cells which remained in most examples preserved their form, volume, and regular arrangement ; frequently the cells possessed two nuclei—a circumstance, the authors say, is normal in animals, and even in man ; fatty degeneration was observed exceptionally only ; usually the cells atrophied by diminishing in volume, assuming an angular shape, their contents becoming homogeneous, brilliant, and slightly yellow, or glass-like ; this changed protoplasm resisted carmine-staining, and took no special tint when tested with the reagents of amyloid matter or with osmic acid. This vitreous change was only found in the neighbourhood of microscopic abscesses, or of appearances which they consider due to the rupture of a small bile-duct and the extravasation of biliary matter.

The Connective Tissue.—In the portal canals the connective tissue had become embryonic without having augmented notably in quantity ; in the interlobular fissures and spaces, however, there was a very great increase which enlarged the areas of these spaces at the expense of the surrounding gland-structure, and completely filled the spaces themselves. This new growth circumscribed the lobules and invaded them from all points, penetrating towards their centres. It accompanied the changes in the bile-ducts, being embryonic and infiltrated with lymphoid cells, where these were little marked, and became organised in proportion to their development. The lobules were not simply compressed by the new growth, but destroyed piece by piece as it advanced, following the rows of cells.

The Bile-ducts.—In the portal canals the large ducts were greatly dilated, attaining a diameter larger than the accompanying portal vein ; they were lined with large cylindrical epithelial cells ; their

cavities generally were empty, sometimes they contained concretions of bile pigment. Their walls were confounded with the connective tissue around. In the interlobular fissures and spaces the canaliculi were dilated, generally empty, sometimes containing microscopic calculi; they were lined with cylindrical epithelium like that of the larger ducts. In some cases, in place of these canals, or accompanying them, other smaller ones were seen whose cavities were filled with small polygonal cells compressed together; in the sections these appeared to be like circles or rows of epithelium, and they were often observed passing from the spaces into the fissures, which they more or less greatly distended. Where the new growth in the fissures and spaces was great these canaliculi were observed in great numbers; they formed irregular meshes by frequent anastomoses, and their contents left no doubt as to their being bile-ducts. Often three zones could be distinguished; first, the great canals, few in number, near the vein; next, separated from these by a band of connective tissue, smaller canals running at right angles to the rows of the hepatic cells; and lastly, a third zone of short ducts, running perpendicular to the last, directed in a course parallel to the rows of hepatic cells, *with which they seemed in some places to be continuous.* These canals contained a more or less irregular lining of epithelium, presenting all the intermediaries between the small square epithelium of the lesser interlobular ducts and the cylindrical epithelium of the large ones. When they possessed a free cavity it was sometimes narrow, sometimes dilated; in the former case the epithelium was bulky, in the latter flattened; often the cavities were filled by a mass of bile pigment.

The Vessels.—The branches of the portal vein presented differences in their conditions; sometimes they were dilated and filled with clots containing abundant leucocytes; in these cases the endothelium was undergoing proliferation, but the other coats preserved their normal structure. The central veins of the lobules were sometimes dilated and contained many leucocytes, which were also abundant in the radiating vessels. There was nothing to note concerning the arteries or the lymphatics.

They compare these results with the following appearances observed by M. Charcot in the liver of an old woman aged 80, who died after jaundice due to partial obstruction of the duct by a calculus.

Macroscopic Appearances.—The liver was of normal size, deep olive colour, distinctly lobulated, but nowhere granular on section. The gall bladder and external ducts were dilated and filled with bile.

Microscopical Appearances—the Cells.—Almost all the cells were small, thinned, angular, with distinct nuclei, sometimes containing yellow granules; the vascular spaces between them were dilated.

The Connective Tissue.—In the portal canals the vessels were surrounded by thickened zones of connective tissue poor in cells; in the fissures and spaces the growth was greater, sometimes fibrous, sometimes cellular, in parts there were true miliary abscesses.

The Bile-ducts.—In the portal canals these were dilated and their epithelium had disappeared; there were many accessory ducts, provided with cylindrical epithelium. The canaliculi in the fissures and spaces were abundant, but much less numerous than in the guinea pigs; they sometimes contained cuboid cells, sometimes they were lined by cylindrical epithelium, and possessed a very narrow central lumen, in which there were never any bile concretions. The intralobular canaliculi were not observed to be dilated, nor were there any masses of bile pigment in them.

The Vessels.—The branches of the portal vein were dilated and their muscular walls were thin; the arteries were not altered; both were surrounded by connective tissue poor in cells; generally the vessels had fibrous walls which were not distinct from the neighbouring connective tissue. The central veins of the lobules were notably thickened.

These facts undoubtedly support M. Hanot's view of the possible biliary origin of cirrhosis, and the identity of the lesions in the liver after ligature of the ducts with those in chronic obstruction from gall stones, seem sufficient to have induced MM. Charcot and Gombault to endorse M. Hanot's views, and to claim for biliary cirrhosis certain distinguishing anatomical characters which differentiate it from all other forms. In their second paper they elaborate these views, and divide cirrhosis into three classes distinguished anatomically by the distribution of the newly-formed tissue, and they connect these with the clinical varieties. According to them biliary cirrhosis is mainly distinguished by the occurrence of the new formation around each lobule, to which they give the name of monolobular cirrhosis. In the second or venous form the connective-tissue zones circumscribe several lobules at once, hence called multilobular; while in the third variety the liver substance is destroyed cell by cell, or monocellular cirrhosis. Besides these names they also use the terms intralobular to denote the first, perilobular for the second, and intercellular for the third variety. They quote two new cases of cirrhosis following obstruction of the ducts, in one case by a calculus, in the other by cancer of the head of the pancreas. In the first case the liver was of normal size and smooth; the large bile-ducts were dilated, there was embryonic connective tissue in the spaces, with leucocytes accumulated around the ducts; these, however, were not dilated, nor more numerous than usual. In the second case there was dilatation of the perilobular spaces in which was fully-formed connective tissue; in some of the spaces the

bile-ducts were numerous. In addition, they reprint some cases
from M. Hanot's thesis, and they conclude that they have established
the existence of a form of cirrhosis which takes its origin in an
inflammation of the ducts ; this primary affection is often due to an
obvious cause (*e. g.* calculus), but at other times develops under the
influence of general conditions which they are unable to indicate
precisely. Venous cirrhosis, on the other hand, following certain
observations of Klebs and Cornil, shows itself first around the veins,
and they instance the experiments of M. Solowief, who induced
cirrhosis of the liver by ligaturing the portal vein. They say that
although usually venous cirrhosis is accompanied by diminution in
the size of the liver, it is not so always, and they give the details of
a case which, during life, had presented the symptoms of common
or venous cirrhosis ; in this the liver was enlarged, granular, tough,
and pale ; the cirrhosis was in many places monolobular, and
invaded the acini ; in certain places they found a considerable
number of bile-ducts, but this was only occasional. This is followed
by a second case, in which the liver presented the ordinary macro-
scopic appearances of common atrophic cirrhosis, and was reduced
in size, the cirrhosis was intralobular, and there was in the new
tissue an abundant network of bile-ducts. The disease in this case
commenced by an attack of jaundice, and MM. Charcot and Gom-
bault regard it as a case of biliary cirrhosis, gone on to the (excep-
tional) last stages of shrinking and atrophy. As to the monocellular
form they simply indicate it, and say that it is seen in, amongst
others, some cases of congenital syphilis, as had been already noticed
by MM. Cornil and Ranvier.

From this short account it may be seen that MM. Charcot and
Gombault believe biliary cirrhosis to be usually, but not always,
accompanied by enlargement of the liver, there being a rare final
atrophic stage; anatomically the intralobular disposition of the
new growth is its chief characteristic ; but they are of opinion
that the multiplication of the canaliculi is no less important, and
they explain its absence in certain cases by assuming that the
ducts undergo fatty metamorphosis and disappear ; in support of
which view they quote a case of chronic jaundice which had lasted
eight years, wherein "the biliary canaliculi were abundant and
formed true networks, but were difficult to distinguish, as they
were only represented by very slender moniliform bands, in which
the cuboid epithelium was replaced by some heaps of fatty
granules."

The respect which M. Charcot's lectures in pathology have
justly earned for him, his untiring zeal and energy, and his great
ability, give to all that comes from his pen a right to earnest
consideration, so that criticism is difficult. Locke[1] tells us, " Some

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[1] 'The Conduct of the Understanding,' sect. xxxiii.

embrace doctrines upon slight grounds, some upon no grounds, and some contrary to appearance. Some admit of certainty, and are not to be moved in what they hold; others waver in everything; and there want not those that reject all as uncertain. What then shall a novice, an inquirer, a stranger do in this case? I answer—Use his eyes." We have endeavoured to follow this advice so far as our opportunities have permitted. Last summer we examined the liver of a man who during life had suffered from jaundice, which had persisted five months. There was no ascites. The organ was much enlarged, its surface was slightly granular, it was dense and tough on section, and the cut surface showed yellow granulations surrounded by greyish zones of connective tissue; under the microscope the portal canals and the interlobular fissures and spaces were dilated and filled by an embryonic tissue, consisting of small highly refracting nuclei, which passed into the acini where it lay between the rows of cells, some of which were cut off from the rest by circumscribing zones of the new formation. The appearances were very like those figured by Hayem ('Archives de Physiologie,' 1874, pl. vii, fig. 1), and the growth seemed to follow the course of the capillary blood-vessels in the acini. M. Hanot attempts to dispose of Hayem's facts by saying jaundice was not present, but this is not exact; M. Hayem's first case dated the commencement of his illness from "jaundice and dysentery," fifteen years before, and on admission "there was a yellow straw-coloured subicteric colour of the skin," though there was no bile in the urine; the second case had been "attacked by jaundice for some months" before admission, and the urine contained a little of the bile-colouring matter, which, however, passed off. We cannot admit that in our case MM. Charcot and Gombault's explanation suffices, as the affection was too recent to have undergone everywhere atrophic changes, and no traces of such were visible. The hepatic cells showed no signs of atrophic changes, and M. Hayem says of his first case "in all the preparations, without exception, and even in the most irregular lobules the hepatic cells have a completely normal aspect;" of the second he says "the hepatic cells are remarkably preserved." In the descriptions of M. Hanot and MM. Charcot and Gombault, changes in the cells are always noticed. We shall see that this difference explains the discrepancy between the various observations. We believe that the evidence is incontrovertible that chronic obstructive jaundice, or the resulting inflammatory changes, cause a form of cirrhosis of the liver, and we admit the truth, at least provisionally, of M. Hanot's three clinical features, (*a*) jaundice, (*b*) enlargement of the organ, and (*c*) absence of ascites as characterising this special form. Moreover, from a careful review of a large number of specimens of cirrhosis we admit that the growth differs from ordinary cirrhosis by extend-

ing within the lobules more frequently and to a greater extent than in the latter. But we are not inclined to admit that this new formation of canaliculi is essential or pathognomonic, but is related to certain secondary changes which may or may not be present. It will be remembered that these appearances were first seen in cases of malignant jaundice or yellow atrophy, which had run a somewhat chronic course, and were associated with some hyperplasia of connective tissue. We have quoted the opinion of M. Cornil, who considered them to be pre-existing ducts brought into view by the destruction of the hepatic cells, and we have referred to the opinion of Klebs, who considered them simply rows of altered hepatic cells, and of Waldeyer, who held them to be newly formed canaliculi destined to replace the destroyed hepatic tissue. In looking over our preparations we came upon some of syphilitic cirrhosis of the liver; in these sections the number of biliary canaliculi seen in the midst of the new formation was most remarkable; they were often lined by distinct epithelium, in other cases the lumen was partially filled by an irregular mass of small cells. In specimens of ordinary cirrhosis, too, an augmentation in the number of the canaliculi is by no means uncommon, certainly to as great an extent as is represented in M. Hanot's figures. MM. Cornil and Ranvier do not deny this, but describe them as more numerous in hypertrophic cirrhosis. In the third part of their ' Manuel d'histologie Pathologique,' published last year, they devote a section to the discussion of these appearances, and inform us that there is a difference of opinion between them as to their real nature. We may hazard a guess that M. Cornil retains his already published opinion; but in the text the view for which they contend is substantially that already given by MM. Hanot, Charcot, and Gombault; namely, an inflammation of the larger ducts spreading to the smaller ones, which are thereby dilated and brought into view, their lumina being at first filled with irregular cells, and afterwards lined by definitely arranged epithelium.

Fortunately for the solution of this question the observations of MM. Kelsch and Kiener supply the missing link. They have been able to show the development of these canaliculi, and they explain their true nature, their anatomical relationships, and genesis. In their first paper they describe two examples of adenoma of the liver; the second alone interests us: it is the case of a man who was admitted with ascites, and signs which led to the diagnosis of cirrhosis of the liver, perhaps hypertrophic. He died five weeks after admission, having latterly become jaundiced.

Macroscopic Appearances.—The liver weighed 1860 grammes; it was adherent to the diaphragm; the capsule was thickened; the

surface was covered with soft yellowish granulations and little nodules from the size of a pea to that of a nut. On section the appearance was that of cirrhosis plus the tumours. The state of the external bile apparatus is not noted.

Microscopical Appearances.—The tumours were composed of cylinders or hollow tubes formed of cells like those of the normal liver; these cylinders frequently anastomosed, were bent upon themselves and often presented lateral offshoots. A delicate fibrous capsule surrounded the growth. The interstitial hepatitis was everywhere young and embryonic, but the most striking point was the extraordinary development of bile ducts in the inflammatory tissue; these canals formed a network around the atrophying lobules and were lined by a single row of cells which in the smallest ducts were very small, embryonic, and elongated in the axis of the lumen, in those somewhat larger cuboid or slightly cylindrical definitely circumscribing the central cavity, and in the largest forming a perfect cylindrical epithelium with yellow protoplasm and nuclei coloured red. In fortunate sections it was possible to trace *a continuity between these canals and the rows of hepatic cells in the lobules* they surrounded; near the point of transition the hepatic cells diminished in size, lost their yellow and granular appearance; their nuclei multiplied and took up carmine more readily, and little by little the hepatic cells passed into cylinders of small cells with round nuclei, brightly coloured, surrounded by a thin zone of protoplasm coloured reddish brown; a transverse section of such a tube showed in parts biliary epithelium in process of formation, in others perfect hepatic cells. In some places masses of irregularly sized bile-ducts were seen, and here the hepatic tissue had entirely disappeared.

In their second paper they add two other cases, the first, " slightly hypertrophic cirrhosis of the liver, jaundice, ascites and coma." The jaundice existed only six weeks before death; the ascites occurred in the last stages. The liver was of normal size, surface nodulated and the nodules covered with granulations; on section the organ was dense and coloured by bile. Microscopical examination showed newly formed embryonic tissue around the lobules, and an abundant network of biliary canaliculi. In these acini, when the degeneration was slight, they saw " trabeculæ larger than normal, sometimes tubular, composed of hepatic cells of unequal size and polynucleated. The periphery of these lobules, surrounded by cirrhosis, showed trabeculæ undergoing transformation into biliary canaliculi. The second case was one of "malarial cirrhosis of the liver, ascites, and slight jaundice." The jaundice and ascites occurred about two months before death; the liver was not enlarged; its size and shape were normal; its colour was yellow; its surface was granular; on section it was hard and elastic. The cut

surface presented the same appearances of yellow masses surrounded
by white dense connective tissue ; microscopical examination showed
the new formation to consist of zones of connective tissue circum-
scribing regions of hepatic substance and sending in processes
which passed between the trabeculæ. The liver substance was gene-
rally fatty and undergoing atrophy where the lesions were far
advanced ;] " in the morbid tissue, intercalated between the acini or
substituted for them, numerous biliary canaliculi ramified, seen both
in longitudinal and transverse section, forming a true network with
large meshes" which in their details conformed to the general
descriptions given previously. They discuss the pathogenic condi-
tions of these appearances, and point out that the true one is to be
found in the alteration of the hepatic cells themselves which become
polynucleated, lose their protoplasm, and present the aspect of
the little cuboid cells found in the new canals. They conclude
that—

" The new formation of a network of biliary canaliculi appears
to depend upon many conditions, of which the most important and
the most general is an alteration of the hepatic parenchyma cha-
racterised by nuclear proliferation of the cells and atrophy of their
protoplasm."

They draw attention to the observations of Hanot and Charcot,
and they remark that the *tendency of the new material in hyper-
trophic cirrhosis to invade the acini and surround single trabeculæ
explains the frequency of the occurrence of these newly formed
ducts in that affection ;* moreover, an inflammatory affection of the
ducts spreading along them to the acini might, in the same way,
excite these changes, but they declare, nevertheless, that these are
not efficient or pathogenic causes, as the network may be seen in
acute yellow atrophy without interstitial hepatitis or catarrh of the
bile passages.

They complete their work by certain physiological deductions ;
according to them there are two opposite evolutions possible for
the hepatic trabeculæ, both resulting in the formation of tubules ;
in adenoma the cells of a trabecula, *hypertrophied,* and multiplied,
arrange themselves in stratified layers upon a wall of connective
tissue, and a central lumen appears in the axis of the trabecula,
thus transformed into a true gland-tubule ; in interstitial hepatitis
the epithelium, *atrophied* and multiplied, arranges itself also on a
wall of connective tissue. A central lumen appears too in this
case, and the trabecula becomes a tubular duct, making part of the
excretory apparatus of the bile. In other words, a hepatic trabe-
cula transforms itself by *hypertrophy* into a gland-tubule by
atrophy into a bile-duct. Since the researches of Ewald, Hering,
and Eberth it is generally admitted that in birds, fishes, and

the amphibia the structure of the liver recalls that of a gland with branching tubes. According to Hering, the structure of the liver of a new-born human infant presents striking analogies with that of the amphibia, especially the frog, and it is possible to meet with trabeculæ presenting a distinctly tubular arrangement. In the adult human being Eberth and Biesadecki assert that the liver-cells affect a tubular arrangement, while Kölliker and Hering believe that they form a continuous mass, traversed only by the capillary plexus. All admit the continuity of the bile-ducts with the rows of hepatic cells. MM. Kelsch and Kiener consider that their observations are strongly in favour of the tubular hypothesis, which we in England always associate with the labours of Dr. Beale, and it is interesting to read over again his careful descriptions, and to look at his rather invisible, but yet truthful drawings, and compare them with this recent work. It seems to us that the problem is by no means solved at present. MM. Kelsch and Kiener ignore Chrzonszczewsky, possibly because his observations are rather difficult to reconcile with their opinions, but we may recollect that the intercellular ducts have never been demonstrated to possess any proper wall, and have been considered by many to be merely spaces between the cells.

We have looked through the second edition[1] of Dr. Murchison's 'Clinical Lectures on the Diseases of the Liver' in order to see what he says in reference to these researches, and we were disappointed to find no allusion to them whatever.

Dr. Murchison writes (p. 140)—

"Habershon has met with an inflammatory induration of the liver in which the organ became greatly enlarged, and Duckworth has described a 'hypertrophic cirrhosis.' From my own experience I have been led to believe that in a considerable proportion of cases of cirrhosis the liver is still much enlarged (very often from the presence in the liver of a considerable quantity of fat), after ascites and other symptoms of portal obstruction have set in, and that patients often die in this condition with jaundice, hæmorrhages, and symptoms of blood-poisoning, the prognosis being no better than if the liver were contracted. In this opinion I am confirmed by the independent observations of Professor Leudet, of Rouen, who observes:—'On est arrivé par l'anatomie pathologique á reconnaître que l'augmentation du volume de la glande n'était pas toujours l'indice d'une lesion récente du foie, d'un processus aigu encore curable.' This is the reason why I have brought the disease under your notice on the present occasion, but I shall have occasion to return to it in a future lecture. It

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[1] 1877.

is a matter for investigation, whether, if the patient lived long enough, the enlargement in all these cases would be followed by marked cirrhotic contraction. M. Ollivier, in fact, is of opinion that cirrhotic enlargement is a distinct affection from cirrhotic contraction. There can be no doubt, however, that it occurs under the same conditions, and gives rise to the same symptoms. It seems probable, therefore, that the same causes sometimes lead to contraction, and sometimes to enlargement of the liver."

. He then gives four causes of interstitial hepatitis :—1, congestion from abuse of alcoholic drinks ; 2, congestion from venous engorgements ; 3, syphilitic inflammation ; 4, a chill. On referring to the first edition we find that the section from which we have quoted is new, and has been inserted in the chapter on enlargements of the liver, between congestion and inflammation of the bile-ducts. Under the heading of Obstruction of the Common Duct Dr. Murchison says that this may lead to enlargement "by inducing inflammation of the biliary passages associated with more or less congestion and an overgrowth of the connective tissue," but he apparently does not recognise this as a form of cirrhosis. This book will probably form a standard text-book on liver diseases for some time to come, and we cannot help regretting the omission of all reference to these researches, and the scanty and altogether inadequate mention of hypertrophic cirrhosis.

VII.—Diseases of the Heart.[1]

In studying the history of special places and periods a clear practical view is best attained by grasping first one prominent person or leading event, around which the others range themselves, contributing causes or effects, and owing their importance, in the student's mind, to their nearness of relation to the central object. The great mass of geography and history collected by Herodotus bears entirely upon the check of the Asiatic conquest by Greece ; the failure of Democracy at Athens is always on the mind of Thucydides, every event and person is important to Macaulay according as it assists in the development of Whiggery ; and what the English Monarchy is

[1] 1. *A System of Medicine.* Edited by J. Russell Reynolds, M.D., F.R.S. Vol. IV, containing *Diseases of the Heart.* 1877.

2. *Cyclopædia of the Practice of Medicine.* Edited by Dr. H. von Ziemssen, Professor of Clinical Medicine in Munich. Vol. vi, *Diseases of the Circulatory System, &c.* English translation, edited by A. H. Buck, M.D. New York. London, 1876.

to Clarendon, that Rubens is to the Art history of Antwerp, and Rafael to that of Rome. It is history thus treated which makes a practical impression upon human life, and not records like the 'Saxon Chronicle' or 'Whitelock's Memorials,' however deep the interest of the pictures they preserve, and however graphic the details. So it is in the pathology of local diseases which we know best; the student gets clearest ideas of them by keeping in mind always the point by leading to which they ale of chief importance, or by being its consequence are valuable as signs.

In heart disease the main point to be kept always in view is dilatation or enlargement of the area occupied by the blood as a cistern, and thus brought under the influence of the contracting muscle. The only business of the heart is to discharge its load of fluid, and in proportion as it fulfils this duty its efficiency is rated. The importance of cardiac lesions is in a direct ratio to the quickness, certainty, and degree in which they induce dilatation. And again, upon the dilatation depend directly or indirectly all the consecutive derangements originating in the heart which interfere with health and life. A clear understanding of this point simplifies our views, so much so as to render prognosis in life insurance capable of arithmetical and financial estimate; and it gives a definite single aim to the treatment, prophylactic or curative. While without it mere minuteness of diagnosis will be too highly valued, and lesions will be mainly interesting to the investigator for the facility with which they are found out.

The reader of the great works whose titles head this article could not turn to better advantage the time he allots to the task than by using it in analysing and classifying the masses of facts they record, according as they bear upon dilatation. He will improve his own mind certainly; and if he is a teacher he will be able to communicate his knowledge in a form peculiarly instructive. He will find in the new volume of Dr. Reynolds' 'System' a collection of monographs of various degrees of merit; all, indeed, carrying information on their subject matter to the degree of accuracy it has attained up to the present time, but some distinguished above their fellows by an amount of original matter which will entail a reference to them upon all future handlers of their speciality. Allusion is intended to the articles by the late Dr. Sibson, whose sudden removal from us has left a blank very difficult to fill up in medicine and science. The reason for the order in which the editor has placed the treatises is not obvious. It does not follow the anatomical arrangement of the R.C.P. Nomenclature, neither is it a Natural or Physiological series, for "Endocarditis" is separated by "Carditis," "Hydro-pericardium" and "Angina Pectoris" from "Diseases of the Valves"; it is evidently not alphabetical, and from our former experience of Dr. Reynolds we may be sure it is not accidental. There might seem

to be some intention of commencing with primary lesions, but then the last article of all is on Fibrosis, after Dilatation and Fatty Degeneration, and with the latter is mixed up the utterly disconnected subject of Hypertrophy of the adipose tissue. Stranger still is the disorder in Dr. Ziemssen's Cyclopædia, where we begin with endocardial and valvular diseases, pass through the structural changes in the muscle, leave the Heart altogether for the arteries, and afterwards go fully into diseases of the veins and lymphatics, and then suddenly are brought back to Pericarditis. It is needless to say that this leads to much overlapping of material and to repetition. What is of still more importance is that it leads us to think more of the points in which diseases differ than of the points in which they agree, and to leave clinical experience a disjointed mass of raw observation rather than a harmonised interpreter of natural law.

We should advise the student of heart diseases to gain, first of all, clear ideas on the subject of dilatation, which is treated of by Dr. Gowers, assistant physician to University College Hospital, in our native work, and in the German by Dr. Leopold Schrötter, for six years clinical assistant to the well-known Professor Skoda, and frequently his representative in teaching at Vienna. Of the two, Dr. Gowers is the most practical writer, seeing the main points of each question most clearly, and dealing with them most directly. Dr. Schrötter indeed tells us very fully and correctly the various lesions and physiological conditions which are apt to result in dilatation, and allows that sometimes they do so result and sometimes they do not, leaving prognosis to the statistician and the doctrine of chances. But Dr. Gowers leads us much farther, and, in a section on what he calls the '*Mechanism*' by which dilatation is effected, aims at a conception of the way in which the morbid state is related to its causes. It is a brilliant little example of physiological reasoning, condensing into about three pages considerations which make us sure that his theory of the process is the true one, and that the gaps which he boldly leaps will be in due time safely bridged over by observation. " Dilatation of the heart is produced in every case by over-distension with blood." But there will occur to the reader a crowd of cases in which the heart is over-distended with blood frequently, habitually, and for 'considerable periods, not only without risk, but apparently with an increase of efficiency. All those whose work involves either voluntary holding of the breath, or forced bodily efforts, over-distend their auricles. Those who are not used to such efforts suffer from a stitch in the side and shortness of breath, but by perseverance they lose their stitch and get their wind. There is no reason to believe that the healthy heart of a healthy man is ever injured by bodily exertion; for the voluntary muscles give in first, and thus put a check on dangerous strains. And there are some

organic lesions also, notably contraction of the aortic orifice, which, by delaying the flow of blood, must abnormally distend the left ventricle, yet do not usually induce dilatation.   On the other hand, regurgitation even slight through the same orifice inevitably and quickly brings on the fatal change of form.   Dr. Gowers points out that the difference lies in the period in the cycle of cardiac action at which the abnormal pressure of blood occurs.   If it be during the systole, the contractile fibres are in a state to resist the pressure, and to overcome it by putting into action their reserve force.   If it be at the beginning of the diastole, it simply stretches the cardiac parietes rather quicker than usual, not to an excessive degree.   But at the end of the diastole it obliterates the interval of rest for the muscle, and thus paralyses the tissue by too continuous work.   The wearied fibre gives way quickly if the obstruction to the circulation has suddenly supervened on previous health, even though it may be slight, and more slowly, and with less functional disturbance, when the lesion is of gradual growth, in spite of the proportions it may finally assume.

The overstraining of the heart walls by blood pressure, even though they are, as above described, unable to resist it, does not immediately result in their degeneration.   If they are supplied by their blood-vessels with healthy nutriment, they follow the example of the uterus, which, as the pressure and the area within increase, takes on substance.   So that a dilated heart is always heavier than natural, and indeed the cases where the walls have not retained their previous thickness are of doubtful occurrence.   No instances are cited by Dr. Peacock, who writes on the "Weight and Size of the Heart," in Reynolds' System, or by Dr. S. Rosenstein, who has undertaken the same department in Ziemssen's Cyclopædia, of dilated hearts failing to be heavier than the average, and increased weight implies increase of tissue.   The muscular fibres must have grown in length, if not in number or breadth.   Growth usually goes beyond this, and, still pathologically following the physiological lead of the other muscular hollow viscus, it produces a thickness of parietes approaching to, equal to, or even in excess of that which is required to restore the normal proportions in the form of the vessel.   It is pleasant to think that sometimes hypertrophy may be truly compensatory, and like that of the pregnant womb be exactly equal to the occasion.   And thus we are fain to account for those numerous instances of mitral regurgitation, with or without an increased area of dulness, where the sufferers are condemned by the family doctor, rejected by army boards, preposterously overcharged by insurance offices, spurned by families with which they wish to be connected, and yet live to the full term of life, are useful in it, and sometimes enjoy it in defiance of the Cassandras.   After death, from some accidental intervening cause,

the valves are truly found incompetent, and the heart is large, but with a thickness proportioned to its size. [1]   "Hypertrophy," when it proceeds thus far, is rightly regarded by Dr. Gowers not as a disease but a cure.   A long train of evils ascribed by Dr. Hope and others to it are "in no way related to its occurrence, but are the result of the dilatation,"—nay more, they are "remotely the cause of the hypertrophy."

But the reparative process may not stop here; either the dilatation may be so great, or so continuously augmented, that the overgrowth which aims to compensate it may deform the heart out of acting shape; or perhaps the process once started goes on longer than it is wanted.

Four conditions of health are enumerated by Dr. Gowers as predisposing to hypertrophy—

(1.) General nutritive energy of the system.
(2.) Nutritive quality of the blood.
(3.) The supply of the cardiac walls of a due quantity of blood.
(4.) A full condition of rest, that is to say, an infrequent pulse, with a necessarily long diastole.

We would wish, as an additional important point in the pathology, to draw attention to the enormous local increase of capillary vessels which must take place when even a quarter of its weight is added to a heart—and a quarter is a very moderate hypertrophy.   This is a necessary collateral consequence of the sanatory conditions stated. We wish especially to draw attention to it, because in the cases where hypertrophy leads to evil, this anatomical state may reasonably be credited as the weak point.   The nutrient arteries of the heart not growing in the same proportion as the districts dependent on them, the supply is liable to be insufficient, and atrophy is the result.   Fibrous tissue, which can develop itself with a limited blood-supply, grows instead of muscle, and many of the already existing bundles of the latter are changed into molecular fat. Hence fibrosis and fatty degeneration, some patches of which are found in the substance of enlarged hearts at post-mortem examinations in almost every instance.

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[1] The due proportion of thickened walls required to do the work of a dilated area is a problem which must often have occurred to the morbid anatomist, and not much assistance towards its solution is obtained from the measurements contained in treatises on the subject. To measure in millimètres the four areas and their respective walls, takes too long, and requires too much mathematical calculation for a trustworthy report to be possible under ordinary circumstances. The readiest way would be to find the quantity of water the heart will hold, and to compare that with the weight of the solid. In an efficient heart so many ounces of flesh move so many ounces of fluid.

The nature of the change in the muscular fibre in hypertrophy has been the subject of much discussion. The general view is, according to Dr. Schrötter, that the increase in mass is produced by an increased thickness of the primitive bundles, though the amount of increase has not often been reduced to figures. Hepp's Zurich Dissertation (1853) is still quoted as authoritative, in which the normal thickness of the primitive fibres is put at $0\cdot007$ mm., and the thickness in an hypertrophied heart at $0\cdot03$; that is to say, that the latter are more than quadrupled in diameter. Dr. Gowers points out that this is proving too much, since the cardiac walls certainly do not receive that amount of augmentation. And several pathologists have failed to find this swelling in bulk, to corroborate which Dr. Gowers has directly counted the fibres in a transverse section of the wall, and finds their number in the main proportioned to its thickness. And again, in the fourth edition of Zielonko's 'Pathology of Tissues,' it is stated that the author has found the fibres of hypertrophied hearts smaller than the average of natural fibres. Professor Rindfleisch suggests that the new material may be produced by the splitting of old fibres. Each of the square cells of which they are made up contains in abnormal hearts several nuclei or centres of growth; and these under the influence of disease might revert to the fœtal or developmental state and generate a new tissue. On the whole the conclusion seems justified that overgrowth is dependent on the formation of tissue in a lower and less perfect condition, less capable of performing its duties than it ought to be.

Has not the attention of microscopists been too exclusively directed to the contractile elements in the diseased part? Dr. Robert Lee states, in his 'Memoir on the Ganglia of the Heart,' that there takes place a thickening of the nerves; and this is probably due to an increase in the connective tissue of their sheaths. The firmness of the flesh of hypertrophied hearts, and the frequency of obvious local cirrhosis, would seem also to point in the direction of the interstitial framework of the whole muscle.

Dr. Schrötter has done well to put hypertrophy and dilatation together into a single article, seeing that for all practical purposes they cannot be considered separately; still less should they be contrasted with one another, as their nomenclature tempts some students to believe is in accordance with just pathology. A permanently dilated heart which has not increased in growth beyond the standard weight is not likely to be seen by any of us; it would prove so rapidly fatal as to have no clinical importance. If the patient has lived long enough it is sure to exhibit some amount of hypertrophy. On the other hand, concentric hypertrophy, encroaching on and removing the hollow area, is of extremely doubtful occurrence. One can easily understand its rarity, on making the reflection that it cannot fail to cure itself; for the diminished supply of

blood sent out from the limited cavity would starve the tissues very quickly, and put a stop to all exaggerated nutrition.

It may be a small matter, but would it not be wise in our teaching to speak of "dilatation and hypertrophy" as a description of a given case, rather than of "hypertrophy and dilatation?" The first is the order of importance and of occurrence in respect of time, and words are not, and ought not to be, without influence on the mind. Both dilatation and hypertrophy lead equally to atrophic degeneration, and derive from thence their importance, so that practically they are parts of one and the same morbid process, as much as the papula and the scab are parts of the same morbid process in smallpox.

Degeneration may be a consequence of a general diathesis, instead of following an organic lesion; and then, it causes dilatation. And in such cases, often a much smaller amount of deformity is found in the heart than its fatal effects would have led one to anticipate. In fact, in some instances of sudden death the cavities are normal in appearance. Is it not probable that the gravity of the symptoms depends upon a *suddenness* in the supervention of the lesion? We know how through a gradually acquired habit the circulation may be carried on by a heart which in course of years has stretched and grown to a monstrous size. But a *rapid* loss of function, though much less in extent, is immediately dangerous to life. Immediately dangerous, but only temporarily so, for if time be gained reparation may to a certain extent be made, and the previous state of health restored—*cito mors venit, aut victoria læta.* We would venture to call to this point the attention of Professor Gairdner, who has contributed in Dr. Reynolds' volume a deeply interesting article on "Angina Pectoris and Sudden Death." Without dogmatising, we would suggest it as open to discussion whether the common feature uniting the diversified morbid lesions found after breast-pang, or sudden death without breast-pang, may not be a tendency to produce a paroxysmal *sudden* dilatation of the heart-walls, very different in its result from that *chronic* dilatation with which physicians are more familiar. The tracings of the pulse made by the sphygmograph during the paroxysms exhibit a diminished amplitude and impetus in the blood wave; which, though explained by Dr. Brunton as indicating increased arterial resistance,[1] will bear equally well, if not better, the interpretation of diminished force in the heart.

The slower dilatation supervenes upon its cause the less dangerous it is to health and life, and the greater likelihood there is of its being accurately balanced by the hypertrophy, to such an extent that the form of the muscle and its efficient pressure upon its contents should be preserved. And the slower the hypertrophy the

[1] 'Transactions of Clinical Society,' vol iii, p. 191.

more likely is the added tissue to continue efficient, and to avoid the risk of atrophy run by organs which have outgrown their blood-supply. A wide field, therefore, lies open for rational treatment, and it must not be trod by careless or despairing feet. We have no hesitation in saying that, in a young person afflicted with any of the causes of enlarged heart, it makes all the difference between life and death whether they follow good advice or live recklessly. It is great encouragement to hope more from treatment, when we see that in each new work on the heart it becomes more simple, that the pharmacopœial aids to it are fewer in number, and that those whose promise of giving relief are vague are omitted altogether.

The *causa causans* of the lesion is the failure of the heart to empty itself and to secure that interval of rest between the beats wherein lies its only opportunity for laying in nourishment. It beats quickly and inefficiently, and the more quickly the more in-efficiently, because the mass of blood stays too long in contact with its walls. Our object, then, should be to strengthen the contractile power of the muscles, so that it may do an extra stroke of work, and then get the repose which will strengthen it for future exertion. To this end tend all the specific remedies which are backed by the authority of these our most recent monographists, and which (their array being reduced in bulk by the discarding of pretended allies) are not too numerous to forbid quotation. In Dr. Schroetter's words—

" The first is the persistent and faithful use of *cold*. This simple remedy is fitted most wonderfully to quiet the severe activity of the heart, and is best applied by the temporary or continuous use of ice-bags laid over the chest. The second is *digitalis*, which most decidedly diminishes the frequency of the heart's action, and since it usually is only required temporarily, it is quite in place here . . . Together with the use of digitalis, *quinine* in large doses is often of great value." [1]

Dr. Gowers has also a proper confidence in lessening the blood pressure *a fronte*, by the reduction of the volume of the circulating fluid :

" This may be accomplished in more than one way. The most ready method is by the abstraction of blood by venesection or cup-ping. The relief which it affords is often immediate and striking. The ultimate effect, however, is that the volume of the blood is soon reproduced, while the heart is permanently weakened." [2]

We are not disposed to be alarmed by the last consideration. It is unlikely that a patient will be overbled in the present day, and within moderate limits the new blood which is made so transcends in

[1] Ziemssen, p. 218.
[2] ' Reynolds' System,' article on " Dilatation," p. 753.

nutritive power the old dark blood which is drawn off that we question the weakening result. We cannot, however, say the same in regard of the substitutes which Dr. Gowers proposes, " in less urgent cases purgation and diuresis." The relief from them is less, and they are liable to depress unduly the nervous system.

The general treatment aims at the same object as the specific.

" The general nutrition must be as far as possible improved. A very bracing air is useful, and gentle exercise should be taken which does not increase materially the work of the heart; food must be nutritious and easily digested. Iron is of great service, and seems to aid directly the production of the needful hypertrophy. Excited action of the heart must be calmed by avoiding the causes of excitement and by sedative medicines. Moral emotion must be avoided, and the sources of gastric disturbance guarded against or relieved: A distended stomach easily excites an attack of palpitation."

The test of the effects of these expedients is diminished frequency of pulse; and it is a satisfaction to many minds to represent these effects numerically. Dr. Milner Fothergill calculates that to a heart which is contracting 144 times per minute the period of rest is increased by one third if the pulse is reduced to 72.[1] Thus, the heart is saved a considerable portion of the work of moving its own mass, and gains one third more time for filling itself with blood for nourishment.

On the alleviation of special cardiac symptoms the English manual is much fuller than the German. Dr. Schrötter, in fact, would lead a practitioner to neglect it altogether. He refers entirely to the treatment of the primary lesion,[2] a teaching against which we feel bound to protest, as it shows an ingrained misconception of the objects of medicine. Dr. Gowers[3] has devoted four very useful pages to this subject. We would, however, take leave to suggest a few additional details as a safeguard against abuse. For example, we should be glad to know the doses of several drugs, and when he says, " headache is best relieved by posture," a hint as to what posture is recommended would be desirable. A description of the " heart-bed " and of the " reclining chair " (is it rightly named " reclining ?") might enable a country carpenter to construct them, and if the principles on which they are beneficial are explained, a patient with a taste for mechanics might suit himself with a cardiac table, a heart carriage or pony trap, or perhaps a heart saddle. It is right also to express our dissent from the advice given in Dr. Gower's last paragraph on dilatation :—" In all cases of dropsy as little fluid as possible should be taken." Yet he had just before recommended stimulating diuretics—juniper, broom, copaiba—all

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[1] ' Diseases of the Heart,' p. 4.
[2] Ziemssen, p. 219.　　　[3] Reynolds, p. 756.

likely to induce such a congestion of the kidney, as water, and water alone, can relieve.   Water has been shown, by the experiments of Boecker and others, to be a true diuretic, emptying the renal vessels of more aqueous fluid than is taken in by the mouth, augmenting both the fluids and the solids of the urine beyond the bulk ingested. We think that to follow Dr. Gowers' advice in this particular would be positively injurious to his patient.

Round the subject of dilatation there group themselves all the various lesions which lead to it.   Foremost among these is valve disease, divided in the one volume between Dr. Sibson, who takes the acute lesions under the heading of "Endocarditis," and Dr. Fagge, who has charge of the chronic; in the other cyclopædia the whole subject is united under Dr. Sigmund Rosenstein, with the heading, "Diseases of the Endocardium."   Upon Dr. Sibson's articles we are disposed to look with a hypercritical eye, for we learn by the preface that they were the cause of the long delay that has occurred in the production of the work; and, moreover, they are (alas!) the last words which will be addressed to the public by that earnest, enthusiastic worker and genial friend.   Much care has been spent upon them, the reader expects much; and it may be said with confidence that he will not be disappointed.   The entire originality of the material, the carefulness of the observations, the subtlety of thought which the analysis displays, the catholicity of the views which are expressed, the honest criticism which they contain of the opinions of other workers, render Dr. Sibson's Endocarditis and Pericarditis the most complete examples of monographs in our medical literature.

Dr. Sibson does not think it necessary to repeat again the observations of the anatomical appearances found after death in cases of endocarditis, which he says are readily available in the manuals of Rokitansky, Moxon, Payne, and others, but sketches shortly and graphically his own idea of the pathological process.   He assumes the reader to have surrendered the view once entertained, that the granulations are formed of an exudation of plastic lymph on the surface of the membrane, and to be ready to consider them as primarily swellings of the tissue itself.   Their position on the valve and the relative frequency of their occurrence in the various valves he explains by an anatomical demonstration that these are the points where the parts are most strongly pressed against one another when closed, and he concludes, therefore, that pressure is the main motive cause of endocarditis, and that the liability of each valve to inflammatory lesions is in direct proportion to the degree of compressing force to which its structure is subjected.

This is exactly what might have been expected from the behaviour of other tissues, notably those of the joints, under the action of such a morbid process as that (say) of rheumatic fever going on in

the body.   If the limbs are kept perfectly quiet from the first occur-
rence of pain the rheumatic swelling moves about, and finally moves
away altogether, without leaving a trace behind ; but should these
joints be exercised or pressed upon, the rheumatic inflammation is
apt to turn into common inflammation, to remain fixed in the
affected organ, and to leave a permanent fibrotic scar or adhesion,
and even to cause ulceration of free surfaces.   The difference between
the joints and the cardial valves lies in this, that the limbs can rest
and the heart cannot.

Still, an approach to a comparative state of rest may be made by
appropriate treatment, and Dr. Sibson puts this first in the list of
preventive and curative measures.   He says—

"The absolute rest of every joint and limb, and the soothing
application of the belladonna and chloroform liniment sprinkled on
cotton wool to the affected joints, supported by flannel applied over
the seat of pain with uniform and comfortable pressure, are the most
important measures in the treatment of acute rheumatism for the
prevention of pericarditis.   The rest and support of the affected
joints should be strictly maintained for several days after the disap-
pearance of the local inflammation, for the too early use of an affected
joint or limb, after the relief of pain or swelling, often leads to a
relapse, first attacking the joints of the over-used limb, extending to
other joints, and often producing endocarditis and pericarditis."

Next in importance, but at a long interval, he places a moderate
abstraction of blood by leeches and the use of opium, which, indeed,
might both be included under a fulfilment of the indication of tem-
pering the pressure upon the sore valves.   He does not allude to
the sedative effect upon the circulation of an even high temperature,
which, under the name of "blanketing," is so warmly insisted upon
by his colleague at St. Mary's, Dr. Chambers,[1] and which again may
be considered a carrying out of the same principle.

Death as an immediate consequence of acute endocarditis is so rare
that neither of the writers whose works are under review allude to
the subject.   In Dr. Sibson's table two of his cases of pure valvular
inflammation are marked as having been fatal (with a little figure of
a coffin), but the cause of decease is not mentioned in the text, and
may have been unconnected with the heart.   Still, there are cases
of this termination on record, and it would be useful to know
whether embolism, congestion of the lungs, or sudden enlargement
of the cardiac walls, were the results most to be feared.   It is a
question that can be decided only by systematic writers, for no man's
single experience can be extensive enough to comprise many instances
of so rare an occurrence, and he would be glad to know if his one
or two instances are accidental or representative ; and it is a practical

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question too; as is the mode of death in all diseases; for thus we learn what is specially to be guarded against in our management of the patient. It is possible that the fatal instances of endo-carditis are of a different nature from the ordinary lesion, that the process may be of a more destructive kind, ulcerative or diph-theritic, and may not be dependent upon pressure or friction, as is the endocarditis of rheumatism. If so the mode of death is probably diagnostic of the nature of the disease.

The risk of immediate death is not the most serious of the evils entailed by endocarditis. Its tendency to induce chronic or scle-rotic lesion as a scar of the acute inflammation makes it much more interesting, for thus it permanently cripples the valves and induces dilatation.

On the subject of crippled valves there is an enormous mass of information for the student, so that he is fain to use it rather as a library of reference than to hope to gain by reading a clear view of the subject, such as may serve him in good stead in view of the patient immediately before him, or in estimating the past history about which he is anxious. We will restrict ourselves here to points upon which in practice knowledge is really desired.

In the first place, what are the different effects in nature and degree of lesions of the several valves, especially as to the causa-tion of dilatation? The concurrence of opinion which might fairly have been expected on this point is not to be found. According to one of the most recent of French writers, Jaccoud, stenoses in general are more serious than regurgitations; and mitral stenosis is more so than aortic stenosis. Again, Friedrich, in the 'Handbuch der Spec. Path. und Therap.,' 1867, says, that the prognosis in obstructive is less favourable than in regurgitant affections as a rule. And Dr. Rosenstein in the volume which is now before us (page 141) rates the prognosis in aortic insufficiency as more favourable than in any other valvular disease. On the other hand Dr. Walshe estimates all the regurgitations as much more serious than the constrictions, and in particular regards aortic stenosis as admitting of a far better prognosis than aortic regurgitation. Dr. Peacock agrees with him, stating that in the former disease life may be prolonged for many years, and a large amount of health and vigour be enjoyed; whereas in aortic regurgitation it is very rare to find life long sustained. Dr. Fagge (in 'Reynolds' System,' p. 678) criticises the data upon which these opinions are founded, but con-fesses himself quite unable to reconcile their differences. He is, however, inclined to believe that the English pathologists are right, and this feeling appears in general to guide the medical referrees of our insurance offices, to whom the question is a very vital one. It would seem, then, that statistics, even when collected by the most competent observers, fail to solve the problem proposed. The

reason of their failure seems to us that from the nature of the case they do not record the real point that affects the issue. To the arithmetician a valvular lesion is a valvular lesion, and he takes no note of its degree or the effect it has in crippling the functions of the part. Now, there are valvular lesions, notably lesions of the mitral, glaring enough in the post-mortem room, and others which make a great noise in the ear of the auscultator, which yet produce no clinical symptoms, and do not bring a patient into doctors' hands; whereas lesions of that same orifice among hospital patients are quite as serious as any other valvular affection. Indeed, Dr. Walshe considers the auriculo-ventricular regurgitations as the most serious of all mechanical derangements of the circulation among those who have come under his care as a physician. Yet in a table of cases these would be reckoned together as each a mitral lesion. Would not a physiologist's way of viewing the matter lead to a truer estimate than that of a morbid anatomist or of a clinical observer? He would suggest that the different forms of injury are proportionally serious in accordance with the period of the cycle of cardiac action at which they derange it, and not at all in accordance with the amount of abnormality they present to the eye, or the loudness of the vibration by which they are clinically detected. Thus a moderately contracted aortic orifice will at the beginning of the systole cause the stream to flow slower and less forcibly through the arteries, but by the end of the stroke it is overcome by the vigorous ventricle, and the heart gets emptied of blood. The patient may be anæmic and sluggish, but is not liable to dilatation unless the anæmia is so great as to weaken the heart walls along with the other muscles. But if the contraction of the same orifice is very great, still more if it is accompanied by regurgitation even in a slight degree, the pressure is continuous, the heart-walls entirely lose their rest, and yield to distension. Thus an aortic lesion may be either the least important or the most important of all the valvular abnormalities. Again, a mitral valve may have its curtain stiffened so as to shut slowly, and present to the regurgitating stream a vibrating edge, which like a jewsharp makes a noise out of all proportion to its size, and yet by the end of the systole it shuts close enough for practical purposes. Or on the other hand it may be affected in such a way as to remain wide open throughout, and let a backstream distend the auricle to its rapid injury. Of warts, too, the mechanical impediment must vary immensely with their position.

Another burning question is what can be done by way of prophylaxis to stay acute endocarditis from becoming chronic deformity? Dr. Fagge adduces facts which tend to prove that it not rarely subsides without leaving any injurious effects behind it; in particular, that a large proportion of the cases of rheumatic inflammation of

the aortic valves in women must terminate in a restoration of normal structure. This comparative immunity from the chronic consequences which are so frequent in men can be ascribed only to the fact of the social position of the weaker sex exempting them from muscular exertion. The plain inference drawn by Dr. Fagge is, that "in either sex the way to prevent future injury, after endocarditis in rheumatism or chorea, is to keep the patients for many months, or even years, as perfectly as possible at rest; to insist on abstention from violent exercise, athletic sports and games of all kinds; to direct the choice of a light sedentary employment, and to urge the avoidance of all emotional excitement." Dr. Fagge also does not despair of the influence of drugs, and sees no reason to disbelieve that "iodide of potassium, mercury, or arsenic may be able to arrest or prevent these changes, as much as those which belong to certain skin diseases, or the chronic inflammations of parts accessible to the sight or touch. . . . . Similar principles must be applied in the endeavour to prevent forms of valvular disease which are from the first of gradual origin." To this latter clause we must demur, for the last-named lesions would seem to be commonly of degenerative nature, and to originate in causes which lower the vital powers and produce anæmia, such as alcoholic indulgence, premature old age, syphilis, imperfect nutrition, &c. A different treatment would here be needed.

The prevention by anticipation of future injury from valvular disease is a touchstone of the utility of the sphygmograph to the physician. If it fails, it is only a scientific toy. There is no doubt that it enables us to appreciate much more delicately than is possible by the *tactus eruditissimus* variations in the pulse, indicative of the degree of efficiency with which the circulation is carried. The mere fact of a diseased valve can be ascertained more easily, and as certainly, by the stethoscope, but in the tracings from the pulse the *degree* in which it affects the general circulation should be made evident. Mr. Mahomed gives in his papers in the ' Medical Times and Gazette' for 1872 some valuable illustrations of the way in which the instrument may be used in cases of this kind, to determine the degree of valvular incompetency, the amount of compensatory hypertrophy of the left ventricle, and of resistance or tension in the arteries. It is probable that more is to be learned from these phenomena, in respect both of prognosis and treatment of mechanical derangements of the circulating apparatus, than from clinical and anatomical statistics. Dr. Rosenstein seems to expect from the sphygmograph nothing beyond assistance in diagnosis, which is really the least important of its uses; as in localisation of lesions it is not strong. But in prognosis, that is to say, in estimating the value of the vital force which has to resist the disorganising force of disease, it is of great and increasing use.

The bearings of "Pericarditis" on the condition of the heart-muscle constitute its claim to a prominent position in monographs of cardiac disease. Its influence is twofold, first during its acute, and secondly, during its chronic stages. In acute pericarditis there are few thorough observations as to the immediate cause and mode of death; Dr. Sibson does not even allude to it; but "Wagner" (? Rudolf), is quoted by Dr. Bauer, in his article in Ziemssen's 'Cyclopædia,' as having found fatty change in the cardiac substance in seventeen out of thirty-five cases of this disease. The estimate is probably too low, but it affords a proof that the morbid action is not confined to the surface, and that it penetrates interstitially the more essential parts of the organ. Indeed unless it does so, and unless it paralyses the contractions of the muscular fibre, there is no sufficient reason why it should prove fatal.

The best-marked cases of myo-pericarditis answer to the description by Virchow.

"The surface of the pericardium was very rough with layers of fibrin. The substance of the heart throughout seemed flabby, pale and somewhat spotted. Beneath the whole extent of the pericardium the outermost layer had assumed a cloudy, pale-yellow appearance. This layer was from one to two lines in thickness, and within it were groups of primitive bundles in such an extreme condition of fatty metamorphosis that their internal structure could no longer be made out, even in the slightest degree. In the deeper layers this metamorphosis gradually decreased, but in no part of the muscular substance was the interior of the primitive bundles entirely free from fat-granules. Towards the surface layers of proliferating connective tissue were met with, which extended into the thickened pericardium and the visible fibrinous layer, and which were thickly strewn with numerous masses of nuclei and cells, closely crowded together and in the act of subdividing."

A muscle in condition like that must be quite unequal to its function of propelling the blood, and with this interpretation the frequency of death in pericarditis is easily understood, as also the frequency of such a degeneration of the substance as leads to atrophy, hypertrophy, and conversion into fat in cases where the immediate complete failure does not occur. "Implication of the muscular tissue," as Dr. Bauer observes, "must be considered not merely as a complication, but rather as a result of the pericarditis. It may either appear and cause death in a few days, or it may lead to a chronic wearisome illness." Under either circumstances it is the inability of the heart-wall to compress and expel its contents which is the point whither our anxieties tend. The forms of pericarditis most prone to this result are the worst; those unlikely to have such a termination are of minor moment. The grave symptoms are dis-

turbed respiration and circulation, reduction of the arterial pressure in the pulse, dyspnœa, venous congestion or cyanosis.

"It is hard to say," remarks Dr. Bauer, "in how many cases and under what circumstances complete restitutio ad integrum ensues, and in how many cases lasting changes remain. We can compare the frequency of pericarditis during life with the number of autopsies in which pathological changes of that nature are found. We can also arrive at some conclusion from appearances presented at the autopsies on the bodies of persons in whom during life pericarditis is known to have occurred, but from which they have recovered, and subsequently died from other causes."

The source of fallacy in the first mode of reckoning arises from what are commonly known as "milk-spots," which are held by M. Willigk, Louis, &c., to be results of cured pericarditis, whereas they are now traced in many instances to remoter agencies.

But there are a great number, perhaps a majority of patients, in whom though a technical renewal of the typically normal heart has not followed, yet practical health has been restored and the life not shortened or burdened. It may be suggested that a careful inquiry into the records of insurance offices would show a considerable crowd of these cases, who, from their own statement or from leech-marks on the cardiac region, are known to have had inflammation there during rheumatic fever, and yet are very good customers to the society.

Considering pericarditis in general as inflammation of a serous membrane, our first indication of treatment should be to limit the extent of the process. With this intent Dr. Sibson applied leeches, followed by cotton wool or poultice sprinkled with belladonna and chloroform liniment during the early period. He gives notes of thirty-six cases where a record of this treatment has been preserved. In twenty-nine of these there was pain in the inflamed pericardium, in seven no note of it. In twenty-four of the twenty-nine marked relief, not to the pain alone, but also to the dyspnœa and oppression in the chest, so speedly followed the bleeding, that the advantage could not but be attributed to it.

In five instances the assuagement of the pain was not appreciable, yet in all but one the action on the patient's state seemed favourable. In that one, a girl of twenty, the heart was already tumultuous on admission to the hospital, and the bleeding from one of the leech-bites could not be stayed, so that a serious loss of blood occurred, and then she caught smallpox and died. The hæmorrhage was, however, evidently more debilitating than it would have proved to a stronger frame.

We cannot think that against evidence of this sort Dr. Bauer is justified in denouncing the taking of blood as "useless and dangerous," in entirely omitting the most moderate employment of it from

his *methodus medendi,* and still less in stating that "it has been generally abandoned." He may feel quite sure it will not be generally abandoned in England and her colonies for many years to come.

The use of digitalis in acute pericarditis has not been sufficiently well known in this country of late years for us to found an opinion about it on experience; but at Munich it has been found a sovereign remedy to control the rapidity and violence of the heart's action and the high fever secondary upon the serious inflammation. The quantity of the leaves administered is from 15 to 30 grains daily, in divided doses; and it is usually found that the full influence is not developed till from 30 to 45 grains have been taken. As soon as the pulse grows smaller and quieter it must be left off. The advantage of stilling the turbulence of the heart is incalculable, and is not only quite consistent with leeching, but has the possible advantage, in reference to that treatment, of preventing the excessive hæmorrhage, of which there is a risk, alluded to just now. To recommend the application of an ice-bag over the cardiac region has a startling sound to practitioners who have been used to employ hot poultices, cotton wool, and the like. But the two treatments are not so opposite as may seem at first thought. There is no doubt that the main advantage of either lies in maintaining unchanged an artificially even temperature, independent of surrounding circumstances. This is most soothing to the nervous system, just as the strongest stimulant to the dormant powers of life is sudden alternation : witness the cold water dashed on the stillborn child and the sinking mother, and the invigorating effects of shower-baths. In order, therefore, to try fairly the German use of the ice-bag, it is necessary to apply it uninterruptedly. It may be expected to allay the pain and palpitation, and does not interfere with either leeches or digitalis.

Blisters and mercury are not without reason denounced by both systems of medicine. Opium has hardly received from either sufficient attention, in our opinion, for it certainly has a most powerful effect in diminishing the quickness of the pulse.

The list of cases in which paracentesis pericardii has been performed is as yet too small for a verdict to be given on its results. Of course it would not be proposed unless there is immediate danger to life *from the amount* of the effusion, or the certainty of an unfavourable end from a stationary *purulent* effusion. Dr. Sibson has collected the records of five patients on whom it has been done of late years, of which three died. His observations are very valuable, for his well-known accurate anatomical knowledge is a safe guide in the selection of a proper place for puncture, naturally a moot point in an emergency which occurs so seldom. Dr. Sibson does not appear to have ever witnessed it, though he records here sixty-three cases of pericarditis in his practice at St. Mary's Hospital alone, and

from his well-known interest in the subject had many other oppor-
tunities of seeing the disease in consultation and elsewhere. He
advises that the trochar should be inserted into the distended
pericardium "just above the upper edge of the sixth cartilage at the
lowest part of its curve, more than an inch within the mammary
line; and that the instrument should penetrate gently inwards with
a direction slightly downwards, so that it may advance into the col-
lection of fluid below the level of the heart, and that the liquid
should be slowly extracted by the use of a syringe or aspirator."
Dr. Bauer is more vague; he simply says he would penetrate
the thoracic wall close to the edge of the sternum in the
fourth or fifth intercostal space, laying the patient on his back
to avoid wounding the heart. This is supposing the heart to
be of normal size. But Dr. Sibson points out that it may be
enlarged, and the apex may consequently be low down, and the
dulness on percussion of the fluid extend into the epigastrium, as is
shown by a diagram he gives, page 340.[1] Under such a circum-
stance he would select a space between the left edge of the ensiform
cartilage and the right border of the seventh costal, or else he would
perforate the ensiform itself. Clearly the success of the operation
depends very much on the place of puncture, and we cannot com-
mend Dr. Bauer's off-hand manner of treating the subject, though he
seems not ignorant of the recent English experience of Mr. Teale, Mr.
Wheelhouse, and Dr. Allbutt, on which his observations are based.

"Adherent Pericardium" receives in Dr. Reynolds' 'System'
a separate article; and "Hydropericardium" is restricted by its
author, Dr. Begbie, to an effusion of fluid in the sac as a part of
general dropsy, while in "Pericarditis" we are concerned with the
acute affection only. So that the chronic collection of pus or serum,
or sero-pus, as a consequence of previous local inflammation, or the
coating of the membrane with fibrin, without adhesion, escapes
notice altogether. This is a serious omission, for though the disease
is not common, yet few pathologists have failed to see a certain
number of cases, and its treatment deserves consideration. In
Ziemssen's 'Cyclopædia' we hear about it as chronic pericarditis, but
it is somewhat swamped by the relatively greater importance of the
acute affection. In our own limited experience a collection of fluid
in the sac has not led to dilatation or hypertrophy, and we should
have been glad to have known if that were the general observation.
Its chief danger would appear to be the threatening of sudden death
from compression, a contingency in which the operation of tapping
would seem appropriate.

To the usually given physical signs of "adherent pericardium"
(viz. systolic depression in place of deficient apex beat, return shock

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[1] The reference to this diagram in the part about paracentesis is an erratum
omitted from the printed list.

over previously retracted space, increase upwards of cardiac dulness, and its immobility during inspiration and expiration) Professor Friedrich has recently[1] added another, *sudden collapse of the cervical veins during diastole,* sometimes even so marked as to deepen the shadow in the supra-clavicular fossa. So that more cases are capable of detection during life than was possible twenty years ago. But yet so often are these symptoms absent or masked by more serious evils, that it is very seldom indeed that warning can by their aid be given to a patient before degeneration of the heart muscles commences. So that practitioners in whose minds prudence predominates try and persuade all that have ever had pericarditis to live as if the membrane were adherent; and against this discipline they have many rebels; while others of a sanguine cast let all go on in a happy-go-lucky kind of way, and must in old age have several shortened lives on their consciences. The importance of adherent pericardium lies in its impediments to the contraction of the heart inducing dilatation and hypertrophy, and its seriousness in direct ratio to the rapidity with which these supervene. Is an adherent pericardium a more or a less injurious lesion than valvular degeneration, in respect of those heart walls whose integrity we have made the pivot round which all our anxieties revolve? Dr. Sibson's researches enable us to give an answer somewhat rough and ready, yet probably not far from the scientific truth. He found that the average weight in thirteen cases of valve disease with adherent pericardium was $24\frac{1}{3}$ ounces, while its weight in sixty-three cases of a like kind, in which the pericardium was not adherent, was 19 ounces or $5\frac{1}{3}$ less than the first series. It may be concluded, therefore, that in cases of valvular disease the existence of adherent pericardium tends to increase the size of the heart, but not to a great extent. Again, in 16 cases in which the valves were competent while the pericardium was adherent, in one third there was no enlargement, and in two thirds it seems to have been so slight as not to have been estimated by weight. This would appear to place the gravity of adherent pericardium between that of mitral and aortic regurgitation, somewhat worse than the first and not so bad as the last.

Dr. Reynolds is quite right in putting together in one article all the adventitious morbid products occasionally found in the heart. They are interesting to the pathologist merely by not often appearing in this situation, and so showing how well some occult influence, which it is his business to find out, defends the heart against the attacks of such foes. But to the practitioner it appears unphilosophical to make separate subjects of tubercle, cancer, hydatids, &c., in the heart and in the pericardium, and the whole together united do

not possess the clinical importance of any of the least of the already discussed lesions.

"Pneumo-pericardium" is made the subject of an article in both the 'System' and the 'Cyclopædia.' Surely this is not a disease. Directly any ulceration into a neighbouring viscus occurs, such intense pericarditis is set up that there is no room for air before death. And then, indeed, what collects is probably, as Rokitansky thought, a *post-mortem* appearance, the result of decomposition.

An evidence of this is that the serum is not churned up into froth, as it would be did the heart beat even for a few minutes in such a thick albuminous fluid.

One of the cases cited by Dr. Bauer happens to have been under the care of the present writer, and it is certan that what he speaks of pneumo-pericardium arose from the decay of chewed food which had escaped through a fistulous ulcer from the œsophagus and was not in the sac before death, or it would have been detected by percussion.

The most interesting pages in these large volumes are those which contain the thoughtful essay of Professor Gairdner on Angina Pectoris. This peculiar group of symptoms is the most deserving of study, in relation to other forms of cardiac disorder, of any which have to be considered. The paroxysm is quite *sui generis* distinguished from everything else by its agony of pain, not so remarkable in its acuteness as in its "*unbearableness*," and by its sense of impending death combined with full use of the perceptive and intellectual faculties. There is no feeling of fear, and out of deference to others the expectation of an immediate fatal termination is usually concealed by educated persons. Pain is of course a comparative symptom, and may be present in any degree, even so slight as to make Dr. Gairdner suggest as a class of allied cases "angina sine dolore": but the excessive and indescribable distress that marks the presence of the pain does not seem to be ever absent. So that there is no difficulty about the identification of the disease. Dr. Gairdner also pertinently suggests that many cases of sudden and lonely death, the wreck of a ship in mid-ocean with none but the lost crew to have marked its breaking up, are probably due to breast-pang. Several pathological changes in the centre of circulation have been found after death, of which the most frequent is fatty atrophy; but their variety, and the impossibility of tracing any direct connection between them and the symptoms, do not allow us to think that the morbid anatomy of the disease has been approached. Most observers will agree with the author in viewing the paroxysm as a neurosis, attributing its phenomena partly to vaso-motor spasm and partly to inhibitory influence transmitted through the vagus nerve from the medulla

oblongata. A confirmation of this view is found in a peculiar symptom often to be found in sufferers, namely, a loss of voluntary nerve-power without warning under moderately exhausting circumstances. A man, for example, has continued out walking a little beyond his usual luncheon hour; suddenly his legs refuse to support him, and till he can get something to eat or drink he is impotent to move. But with dietetic help he is soon right again.

Of the constitutional causes of Angina Pectoris Gout is the only one which appears to Dr. Gairdner to have evidence in its favour. Gout is unquestionable, indeed he suggests that the majority of the suddenly fatal endings of irregular and atonic gout, called "gout in the stomach," or "gout in the heart," are by angina pectoris. He connects the diathesis with its cardiac results through the degenerative changes in the heart and arteries closely associated with the retention of uric acid in the system.

The progress must depend mainly on the detection of organic changes in the circulatory apparatus. If there is reason to suppose that these are not present, or that they do not tend to increase, it is much more favourable than the patient is fain to believe. And it is satisfactory to know that invalid habits and a useless life are not required, nor indeed do they appear conducive to length of days. For proof thereof to the cases cited the following may be added:—upwards of twenty years ago an hospital physician in going round his wards was seized with breast-pang, entirely prostrating the body, but leaving the mind perfect. His colleagues sent for his wife, and when she arrived next day he was still so ill that he took a solemn leave of her. He has had two or three minor paroxysms since, as also an attack of gout, but is still practising his profession.

As to treatment during the fit Dr. Gairdner accepts the recommendations of former English physicians as to use of stimulants, and the practice is probably correct, though in point of fact sufferers do not speak of being much benefited. He refuses to endorse the disapproval of opiates expressed by Professor Niemeyer, except in cases of uræmia, dropsy, bronchial or central congestions. Both opium and chloral should, however, be given in moderate doses and stopped directly their effects are produced upon the disease, before the toxicological results follow. (One would be glad to apply as far as possible this rule to the use of all drugs in all diseases.) Of late years the nitrite of amyl in vapour has commended itself to the profession for the relief of the spasmodic contractions of the peripheral blood-vessels to which breast-pang has been attributed; and the personal experience of Dr. Madden, of Torquay, quoted in detail from 'The Practitioner' by Dr. Gairdner, is conclusive as to its efficacy. Nitrite of amyl can be carried about the person in a small stoppered bottle, and from 5 to 10 drops inhaled when an attack threatens.

Dr. Gairdner does not here allude to a remedy whose action is physiologically similar to nitrite of amyl, and which gives singular relief in breast-pang, namely, rapid friction of the external surface of the body and limbs with a hair-brush. The flush which follows its employment shows how readily the capillaries dilate, and it does not cause cerebral disturbances like amyl. The relief is very great. The action of galvanism is somewhat similar to that of the hair-brush, but very inferior, and seldom so readily applicable.

On the inter-paroxysmal treatment Dr. Gairdner's remarks are judicious. On two points perhaps we should pronounce him too trenchant; namely, on the subjects of marriage (page 585) and of continuing the usual occupations of life (page 595). He would have the patient submit, like a confirmed invalid, to the loss of these. Doubtless the stormy rapture of lust, with its shame and sorrow, and unsatisfied longings and futile repentances, is a most depressing influence; but the same cannot be said of the invigorating exercise of married love ; besides which the feeling of having some one always at hand with the needful remedies is in itself a tonic, and keeps off the demand for them. An even warmth by night is also a great safeguard against attacks. We hold that there are many cases in which marriage is decidedly to be advised, especially where there is no organic lesion to be detected. Then as to the continuance of occupations, in Dr. Gairdner's cited cases the patients have not given them up : "toiling, rejoicing, sorrowing, onward through life they go," and have not to complain of its being thereby shortened. Even amusements which "hurry the breathing," such as scrambling over glaciers, seem rather beneficial than otherwise. For it must be remembered that, not the field-labourer or navvy, but the poring sedentary scholar and the hard thinker is the most usual sufferer from angina-pectoris.

Dr. Gairdner's brilliant essay is a type of excellence of which we would fain see many disciples. Dr. Sibson's "Pericarditis" is also a model : yet how different ! Perhaps never before were such a number of minutely described cases of any one local disease collected together. In observations the author never seems to have considered his pains and labour as of any account, if a point were to be made out; and the titles and diagrams to illustrate it fairly take away one's breath. Truth was his aim, and truths of all sorts were so superlatively beautiful in his eyes, that he could see no degrees of comparison. The consequence is that information which you very much want is apt to be smothered and made to appear unimportant by an overlying mass of facts of which you are content to remain ignorant. One cannot fail to admire the perfection with which this plan is carried out. Nevertheless a question arises—Do we wish to set up the model for imitation? Is the profession to be congratulated on the best years of a valuable life having been con-

sumed in " serving tables," in proving platitudes, and slaying again
slain fallacies? Michael Angelo designed the pretty livery which
makes the halberdier of the Vatican a thing of beauty, and Julio
Romano decorated the ointment-pots of the Loretto dispensary :
but the world would have been the poorer if these πάρεργα had
impeded the execution of the Last Judgment or the Martyrdom
of St. Stephen.

VIII.—Diseases of the Urinary and Reproductive Organs.[1]

This second edition is altered from the preceding one by the
omission of that portion of the work which had to do with the
kidney, so that more room was left for the discussion of the so-
called functional derangements of the reproductive organs in both
sexes. The first edition contained little about those affecting the
female sex.

" Qui s'excuse s'accuse " is a wise and expressive if somewhat pes-
simist proverb. So the somewhat apologetic preface is not en-
couraging, and a somewhat enigmatic sentence which is its pero-
ration prepares the reader for the worst :

" To succeed in elevating the literature of the subjects herein
treated from the mire in which it has been permitted too long to
remain would have been a not unworthy triumph, to hasten the
accomplishment of this end, ample reward for the trouble which the
present undertaking has entailed."

A careful perusal of the work, however, will agreeably surprise
the reader. In spite of an inflated somewhat obscure style, many
mannerisms, and a little egotism, there is a large amount of useful
information in the book. The author has evidently read a good
deal and also can think for himself, and with comparatively few ex-
ceptions the work seems to have been written by a professional man
for professional men, not by a charlatan for the public.

We would not too severely criticise, though we can hardly agree
with his classification of functional diseases. It is confessedly
difficult to draw hard and fast lines, but the cystitis of paraplegia is
with some difficulty allocated as a functional disease of the bladder.
On that subject Dr. Black has some excellent observations. He
joins issue with Mr. Hutchinson as to the proper treatment in
cases of retention in paralysis. Mr. Hutchinson inclines to adopt
the plan of allowing the bladder first to fill and then to overflow of
itself in these cases, because cystitis constantly follows the use of
catheters. Dr. Black agrees that cystitis does frequently follow

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[1] *On the Functional Diseases of the Urinary and Reproductive Organs.* By D.
Campbell Black, M.D., L.R.C.S. Edin., &c. Second edition, revised.

catheterisation in such cases, but explains it by the depressed state of the nervous system acting and reacting on the condition of the urine and of the bladder. Neither authority seems to be aware that in cases where the catheter is frequently used, the decomposition and alteration of previously healthy urine is actually caused by the mechanical introduction on the catheter itself of germs of putrefaction, bacteria—call them what you like—and can be with almost absolute certainty prevented by the simple expedient of using hot water to purify the catheter and carbolised oil, instead of rancid oil, or questionable lard, to grease the catheter.

Retention of urine from shock is described and illustrated by a case in which we have distinct evidence that there was enough local injury, apart from shock, to account for all the symptoms:

"In this case there was considerable ecchymosis over the lower part of the abdomen; the penis, particularly the glans, presented a similar appearance of greater intensity, and the scrotum was similarly affected. The catheter was passed with ease; the urine withdrawn was perfectly normal, but the power of micturition was in perfect abeyance."

The chapter on nocturnal emissions and spermatorrhœa and their relations is rather a transcendental one. It begins with a quotation from Feuchtersleben, and culminates in the fine old crusted proverb, "Castus raro minget," which is, we hope by a typographical error only, curiously misquoted; then, through a good many pages of ill-arranged material, from seminal secretion in advanced life, and Old Parr, and Pliny on the productive climacteric to Sir A. Cooper's case of castration and Sayer's cases of phymosis, we are at last led into a very doubtful argument on the physiological relations of continence and incontinence, in which we fear continence gets the worst of it.

We are pleased to find that one of the best bits in the chapter is a long quotation from our own columns in a review of Lallemand's work in 1848:

"M. Lallemand, in our opinion, attributes far too great an importance to prolonged continence as a direct cause of spermatorrhœa. That a moderate exercise of the genital organs is the condition most favorable to the maintenance of their healthy condition and the general well-being of the economy is a fact which requires no proof; but we conceive that M. Lallemand greatly exaggerates the ill consequences which ensue on withholding such exercise."

So said our reviewer in 1848, in what Dr. Black calls a very able review; and so would we in 1877 re-echo his words, even with additional force. We believe, from a very large experience of treatment of school lads and college young men, that the whole question of the effect on health of continence has been much exaggerated.

The "sera juvenum Venus, ideoque inexhausta pubertas" is true

still; and most healthy, manly cricket-playing lads, working students
and dressers, and many young officers, really trouble their heads very
little about their generative organs.

It is the pasty-faced, slouching, smoking, and drinking lads, who
can neither look each other or any decent woman in the face, half
mad and quite stupid, for whom such books as this require to be
written. They will be pleased with arguments on the evils of con-
tinence, not the men who are to do the work of the world.

We are no advocates for prudery or concealment. Let boys be
warned of sexual temptation and sexual dangers, but let them be
told, when the unsavoury subject has been broached, that the less
they think about the state of their genital organs the better, that a
nocturnal emission now and then is not a thing to whimper about,
that the loss of mucus from a cold in the head is quite as exhaust-
ing and much more offensive, that perhaps there have been too
many bed-clothes, or the bed-room window has not been opened
enough.

Teach them to swim, box, play cricket, and speak the truth;
feed them simply, and show them that smoking, drinking, and
sweetmeats will spoil their training, and then books of this kind
will not be so much needed, even for the profession.

The chapter on "Anomalous Urethral Discharges" deals with
a subject presenting many difficulties. Dr. Black faces them as
follows:

"As in the female, in the present state of our knowledge, it is next
to impossible to distinguish between leucorrhœa and gonorrhœa
except by such surmises confessedly inexact, as general considerations
afford, so in the male it may be equally impossible to discriminate
between a case of simple urethritis and one of gonorrhœa with any
degree of well-founded assurance.

"To such an extent, indeed, is this difficulty recognised, that it is
contended by certain modern authorities that gonorrhœa is capable of
being communicated by females who are themselves perfectly free from
the disease. This, if admitted, would practically amount to the con-
clusion that there is no such disease as specific gonorrhœa, an alterna-
tive to which we cannot assent, even admitting, as we do, the extreme
difficulties of diagnosis. The relation of the two affections is not
more remarkable than that which subsists between the suppuration
of pyæmia and that of a healthy surface; yet no one will contend
that there does not exist some specific distinction, unrecognisable by
any chemical or microscopical test which we possess.

"Professor Kuss, of Strasbourg, in examining Lock cases, was in
the habit of collecting the secretion of the vagina and cervix uteri
on a glass rod, depositing it upon small pieces of glass, and examining
it microscopically.

"If the liquid exhibited pus-globules the patient was detained for
gonorrhœa; if simply vaginal cells, she was believed to be free from

the specific disease.   But this distinction is, we fear, too absolute. The *absence of pus-cells* might be presumption of a non-gonorrhœal, but their *presence*, we submit, would not necessarily prove the existence of the specific disease" (pp. 245, 246).

We may quote also a sentence on another interesting subject, to show that Dr. Black has some humour, and can speak his mind freely.   Speaking of functional diseases of the female organs causing sterility, he says—

" The position of the uterus in the pelvic cavity,—one of the most graphic sources, by the way, of professional charlatanism and of groundless feminine solicitude, has unquestionably an intimate bearing on the facility or the reverse of impregnation, though I very much doubt if many of the constitutional symptoms ascribed thereto have any relation therewith, save in the perverted brain of the too fussy brain of the gynæcologist" (p. 268).

On the whole we are disposed to regard the book as a good, well-intentioned one, which would be improved by pruning, by more care in the composition, and even in the corrections of the press.

IX.—Ferrier on the Functions of the Brain.[1]

The general bearing of Ferrier's work is, doubtless, familiar to almost all our readers, and it will hardly be expected here that we should give any detailed analysis of the contents.   The work, as a whole, may be taken as marking a distinctly new direction, which the study of nervous physiology and pathology has taken of late years, and it may be regarded as altogether remarkable that, some half dozen years ago, almost all the great results embodied here were only guessed at.   We say they were only guessed at, but it is in the highest degree to the credit of the sagacity of Hughlings Jackson that they *were* guessed at, and that the results of experiment have been to a great extent merely confirmatory of speculations which he had already given forth to the world on the basis of clinical and pathological experience.   It is very appropriate, therefore, that this work is dedicated to the distinguished physician of the London Hospital, whose researches it so wonderfully illustrates and confirms.

We shall pass over that part of Ferrier's work which discusses the structure and functions of the spinal cord and medulla oblongata. There is nothing new in these chapters, and we are not sure that much care has been exercised in putting the facts as accurately and systematically as might be.

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[1] *The Functions of the Brain.*   By David Ferrier, M.D., F.R.S.   London, 1876.   Pp. 323.

If we were to put in a few words what we consider to be the essential results of the work it would assume a form something like this: hitherto, the endeavour to arrive at the functions of the central nervous system has mostly stopped short at the corpus striatum and optic thalamus. Most were agreed as to the general functions of the spinal cord, medulla oblongata, and pons, and it was universally acknowledged that the corpus striatum is a motor ganglion, while most were inclined to believe that the optic thalamus is a sensory centre. In the work before us it is distinctly asserted that the corpus striatum is a motor and the thalamus opticus a sensory ganglion, but the author goes much further than that. He attempts to expand these ganglia, as it were, into the cerebral hemispheres. He asserts that while voluntary motion of all parts of the body is represented in a comparatively small space in the corpus striatum, it is again represented, but occupying a larger space, in certain of the cerebral convolutions; and, again, while the various sensations are represented and massed together in the thalamus opticus, they are expanded and separated in certain other of the cerebral convolutions. This seems to us to be the great advance in the attitude of observers towards the functions of the brain, and it is just that the working out of it should occupy the bulk of this book. There are, in addition, three chapters on the functions of the middle brain, including the corpora quadrigemina and cerebellum; but we confess that these chapters do not carry to us the same conviction as those on the cerebrum, and we are by no means sure that all the views enunciated in them will turn out to be correct.

From what we have said above it will be understood that, according to our author, there is a certain portion of the surface of the hemispheres which is to be looked upon as motor in function, as made up of motor centres, and it will be convenient for us to discuss certain points related to this in the first place. The determination of the function of these parts rests upon two different, but, to a certain extent, complimentary sets of experiments. It is possible, in the first place, to irritate these parts in living animals; and we can, in the second place, destroy them. Now, it may be regarded as certain that irritation by electricity of what we may call the motor region in vertebrate animals, from the frog up to the monkey, produces movements of the muscles of the opposite side of the body, and not only so, but different parts of the body are, as it were, represented in distinct and separate parts of the motor convolutions. Differences of opinion may and do exist as to the exact significance of these movements, but their production on electric irritation, first described by Fritsch and Hitzig in 1870, has been confirmed by so many competent observers that it is no longer doubtful. Various objections are taken to the view entertained by Ferrier, that the

regions concerned are true motor centres, and it may be right here
to refer to some of these.

As a possible objection, it is obvious that, in using electricity, the
current may be conducted from the surface of the brain to the deeper
parts, particularly to the corpus striatum.   This view receives some
degree of plausibility from the fact that, as has been shown by Her-
mann, by Burdon-Sanderson, and by Carville and Duret, the move-
ments are still produced if, after destruction of the grey matter of
the convolutions, the underlying white substance is irritated.   All
that these observations prove is that the fibres passing from the
grey substance are irritable, and that their irritation produces similar
movements to those which occur from irritation of the grey substance
itself.   We may, with Hitzig, admit it as possible, or perhaps pro-
bable, that when we try to irritate the intact surface it is in reality
the underlying white substance or the transition zone between the
grey and white substance which is affected by the current.

As a matter of fact it is not proved that the grey substance is
irritable, because the white substance lying beneath is so near that
it may be regarded as doubtful whether we can stimulate the former
without the latter.   But this does not in the least interfere with
the view that the grey substance is actually the seat of motor
centres ; and if the destruction of the grey substance in these regions
produces paralysis, then the existence of such centres may be regarded
as determined.   We shall return to this latter point, but we mention
it now in order to show that, though it were proved that the grey
substance as grey substance is not irritable to electric stimulation,
yet it may, none the less, be really motor in its functions.   We
are not aware that the irritability of grey substance, in any locality
to such stimulation, has ever been proved, yet no one doubts the
existence of motor centres in the grey substance, unless it be Mr.
G. H. Lewes.

As to the conduction of currents applied at the surface to the
corpus striatum, we think that this may be dismissed from considera-
tion.   When it is possible, by a comparatively mild current applied
at a given defined spot to produce definite and predictable move-
ments of (let us say) the mouth, and by moving the electrodes to an
immediately neighbouring spot to produce equally definite and pre-
dictable movements of the fingers and wrist, then we think the ob-
jection of possible conduction to the basal ganglia may be treated as
simply a bugbear.   If anything further were needed to overturn this
objection it is the fact pointed out by Ferrier, that irritation of the
island of Reil, which is immediately outside the corpus striatum,
causes no movement, " while the more distant parietal regions at the
same time react, actively and definitely, to the same stimulus."
We take it, therefore, that though Ferrier is hardly warranted in
assuming the irritability of the grey substance, yet this does not

detract from the view that it contains actual motor centres. The fact that irritation of the white fibrous substance coming from these grey regions produces movements renders it, on the other hand, exceedingly probable that these grey regions are motor centres.

It being granted that the regions of the cerebral hemispheres referred to are, in a certain sense, motor, and that irritation produces muscular movements, the question arises as to the interpretation of these movements. When Fritsch and Hitzig made their important discovery they described the motions produced as contractions of groups of muscles, different regions of the convolutions being related to different groups of muscles in the body. Ferrier, on the other hand, has always insisted that it is possible to produce not merely contractions of muscles, but very definite combined movements, which have all the appearance of purposive acts. This is, of course, a very important difference of opinion, but it is movements of the kind described by Ferrier, which would be expected *à priori* if the motor part of the convolutions is to be looked upon as a higher organisation of the corpus striatum. The difference in the results of these two sets of observations is explained by Ferrier by the fact that the German observers used galvanisation while he used faradisation. "The closing or opening shock of the galvanic current applied to the region of the brain, from which movements of the limbs are capable of being excited, causes only a sudden contraction in certain groups of muscles, but fails to call forth the definite purposive combination of muscular contractions, which is the very essence of the reaction and the key to its interpretation." There is the further objection to the galvanic current that it by and bye produces by its electrolytic action decomposition of the brain substance, as is shown by the evolution of gas. On the whole, we way say that it is hardly possible to read the details of Ferrier's experiments without being impressed with the highly organised character of the actions produced by stimulation of the surface of the brain. When we read that stimulation of a certain part of the ascending frontal convolution of the monkey produces " extension forward of the opposite arm and hand, as if to reach or touch an object in front," and of a certain other part of the same convolution, " supination and flexion of the forearm, by which the hand is raised to the mouth," we cannot but be struck with the eminently voluntary appearance of the actions, and this is confirmed when we find, for instance, that in the dog it is sometimes possible, by stimulating the region concerned in the movements of the mouth, to produce barking or growling.

The experiments hitherto referred to, in which irritation of the surface of the brain was used, although as experiments they may be regarded as tolerably complete, can hardly be taken by themselves as proving the existence of motor centres in the cortex of the brain.

Other explanations might be suggested, and the significance of the experiments may be expected to become much clearer if the check of others in which these supposed centres are destroyed be applied. And this check has been frequently used with results which appear at first sight somewhat unsatisfactory. The reasonableness of such complimentary experiments could not fail to suggest itself at once, and we find at the end of Fritsch and Hitzig's original paper that they had already entered on this line of enquiry. They give the results of experiments on two dogs, in which they removed the grey substance from the spot which they had determined to be the centre for the fore limb. They found that though the limb was not in any proper sense paralysed, yet its movements were to a certain extent interfered with. "In running the animals set down the right fore-foot in unsuitable positions, sometimes further in and sometimes further out than the other, and they readily slipped with this foot outwards, but never with the other, so that they fell to the ground. There was no movement entirely lost, but the right limb was somewhat weaker than the other." Then followed Nothnagel who destroyed small portions of the brain by injecting with a perforated needle a drop of solution of chromic acid. These experiments were made on rabbits, and the result seemed to be an affection of motion which the author refuses to call paralysis, but believes it to be "an imperfect appreciation of the situation and disposition of the extremities," which manifests itself chiefly in an imperfect use of the affected leg; it slides out very readily, and the animal allows you to put the leg in awkward positions. These lesions, it is to be remarked, disappear in from six to twelve days after the operation. It is of some consequence for what follows to remark here that Nothnagel has made experiments on the lenticular nucleus of the corpus striatum using a similar method; and he finds that the paralysis which results from destruction of this ganglion is also evanescent, disappearing two or three weeks after the operation. Experiments in which the motor centres were destroyed have also been made by Schiff, by Hermann, and by Carville and Duret, and lastly, some recent observations have been published by Goltz. To these last experiments we shall refer hereafter, but Ferrier sums up the others as well as his own at pp. 206 and 207, and the following are the principal facts.

In dogs and cats, if the motor region on one side be destroyed by excision or cauterisation, the animal shows certain well-marked motor affections. The limbs on the opposite side double up under the animal so that it falls over. There is no absolute paralysis, but the animal cannot at first walk without the limbs continually slipping from under it and causing it to fall. Gradually, however, the power of control of the limbs returns, and even in a comparatively short time there may be no apparent paralysis except on making a sudden

movement, and even this may ultimately disappear if the animal survive. In rabbits the affection of motion is even more transitory than in dogs, and comparatively soon the animals are apparently as able to move as before the operation.

Now, if these experiments were taken alone it might be very difficult to explain their true significance and to determine what light they throw on the nature of the regions of the cerebral hemispheres under consideration. There is no proper paralysis of motion, or it is only of temporary duration, and from these facts sundry conclusions have been deduced. It has been inferred by Hitzig and by Nothnagel that the phenomena indicate not a loss of power of motion, but of the muscular sense, or muscular consciousness. It is assumed that there is, apart from the sense of touch, a muscular sense, by virtue of which impressions are conveyed from the active muscles to the brain, giving information of the state of these muscles as to the amount of contraction and relative position. The condition of the animals under consideration would indicate a loss of this muscular sense, and this, as the authors expressly state, without any loss of common or tactile sensibility. But the existence of such a muscular sense, apart from tactile sensibility, is purely hypothetical, although it does exist as a part of tactile sensation, and is lost when anæsthesia is produced by destroying certain sensory centres. Our author asserts—and this part of his work presents much that is interesting—that there is not the slightest evidence for supposing that the motor nerves which convey impulses *to* the muscles are also the paths by which we derive impressions *from* the muscles as to their state of contraction. There are, however, certain facts which seem at first sight to indicate that, in the mere voluntary effort at muscular exertion, we have a kind of index of the amount of muscular exertion put forth. For instance, a patient with muscular paresis of a limb is still capable of having an impression of great muscular effort, although the limb is hardly moved, and a patient with a complete hemiplegia may express himself as conscious of putting forth great energy to move the paralysed limb, while the limb remains absolutely motionless. This looks very much as if the mere effort to produce muscular movement gave the subjective impression of muscular action. But the author explains these cases which apparently contradict his view in a very ingenious way. Taking first the case of muscular paresis, there is here with great effort a slow or small movement. But we are accustomed, in our experience, to associate a slow and difficult movement with great resistance, and so "the patient who can only move his limbs slowly and with difficulty thinks his arm is weighted with lead or some heavy substance." The case of complete paralysis may seem more difficult to explain, for here the limb is not moved at all, and no impression of movement can be

conveyed from the motionless limb. This looks as if the mere will to move gave the impression of muscular exertion, but the author accounts for this sense of muscular exertion in a way which must strike one as at once simple and convincing. For though the hemiplegic cannot move the paralysed limb, he will be found to be making powerful muscular exertion of some kind. If he be desired to close the paralysed fist, it will be found that he unconsciously closes the sound one.

" It is, however, easy to make an experiment of a simple nature, which will satisfactory account for the sense of effort, even when the unconscious contractions of the other side, such as hemiplegics make, are entirely excluded.

" If the reader will extend his right arm, and hold his forefinger in the position required for pulling the trigger of a pistol, he may without actually moving his finger, but by simply making believe, experience a consciousness of energy put forth. Here, then, is a clear case of consciousness of energy without actual contraction of the muscles either of the one hand or the other, and without any perceptible bodily strain. If the reader will again perform the experiment, and pay careful attention to the condition of his respiration, he will observe that his consciousness of effort coincides with a fixation of the muscles of his chest, and that, in proportion to the amount of energy he feels he is putting forth, he is keeping his glottis closed and actively contracting his respiratory muscles. Let him place his finger as before, and *continue breathing* all the time, and he will find that however much he may direct his attention to his finger, he will experience not the slightest trace of consciousness of effort until he has actually moved the finger itself, and then it is referred locally to the muscles in action. It is only when this essential and ever-present respiratory factor is, as it has been, overlooked, that the consciousness of effort can with any degree of plausibility be referred to the outgoing current. In the contraction of the respiratory muscles there are the necessary conditions of centripetal impressions, and these are capable of originating the general sense of effort."

Other considerations, into which our space here does not allow us to enter, still more completely indicate that not only the general sense of muscular effort, but even fine muscular discrimination, depends not on the amount of outgoing motor impulse, but on sensory impressions derived from the parts moved. Thus it is shown that differences of weight can be almost as accurately discriminated when the hand is raised by causing the muscles to contract by the galvanic current as by voluntary effort.

It would therefore appear that, apart from the ordinary paths of sensory impressions, there is no proper muscular sense; and in Hitzig's and Nothnagel's experiments, sensation being intact, the affection of motion is not to be accounted for by the loss of the

muscular sense. How, then, are we to account for this very partial interference with motion, and especially its transitoriness? Is it that these are only a small part of the motor centres? And further, when one set of centres is destroyed, can their functions flit off to some other part of the brain? Nothnagel seems to come to a conclusion somewhat like that expressed in the last of these queries when he doubts whether any strict localization of the mental functions in distinct centres of the cortex exists.

These apparently contradictory results receive very great illumination from experiments on monkeys[1] and pathological observations in man, as well as from certain other considerations. It is to be remembered that the lower we go in the animal scale the higher is the organization of the inferior nervous centres in proportion to the superior. We know that the frog is capable of performing actions by means of the spinal cord alone which higher animals require much higher parts of the nervous system to accomplish. The well-known experiment in which the decapitated frog raises its leg to scrape off a piece of mustard from its thigh, and may even use the other leg if the first one is amputated or has become tired, is an example of this. And so it is with other functions. "In the fish, the frog, and the pigeon, the removal of the hemispheres exercises little or no appreciable effect on the faculties of station and locomotion." In these low animals almost all the actions are automatic, or at least such a large proportion are so that the absence of purely voluntary movements may hardly be missed. The ordinary actions of life are for these animals organised in the centres beneath the cerebral hemispheres, which we may suppose to contain the strictly voluntary centres. But as we ascend the scale the voluntary come more and more to dominate the automatic actions. "In proportion to the degree of independence, complexity, and variety of the forms of motor activity of which the animal is capable, the more volitional and less automatic are its movements, and the longer is the period of infancy during which the animal is slowly acquiring volitional control over its limbs." "Where voluntary control is speedily acquired, or automaticity inherited or rapidly established, as in the rabbit and dog, the centres of voluntary motor acquisition may be removed without completely or permanently interfering with the powers of locomotion. Locomotion is still possible through the agency of the lower centres, in which this mode of activity is mechanically organized, and may be set in action by various forms of external or internal impulse."

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[1] It may here be remarked that Ferrier's experiments on monkeys seem to us to enhance the value of his studies to an almost incalculable extent, and to place them in a much better position than those of the German observers, who have not got beyond dogs. It is to be regretted that such a distinguished observer as Goltz seems hardly to have read, much less appreciated, the importance of Ferrier's work.

It is, therefore, most interesting to find that in monkeys destruction of the motor centres in the convolutions produces complete and probably permanent paralysis of the opposite side of the body. Here are the details of an experiment which illustrates these points extremely well :—

"The right hemisphere of a monkey had been exposed and subjected to experimentation with electrical irritation. The part exposed included the ascending parietal, ascending frontal, and posterior extremities of the frontal convolutions. The animal was allowed to recover, for the purpose of watching the effects of exposure of the brain. Next day the animal was found perfectly well. Towards the close of the day following, on which there were signs of inflammatory irritation and suppuration, it began to suffer from choreic spasms of the left angle of the mouth and left arm, which recurred repeatedly, and rapidly assumed an epileptiform character, affecting the whole of the left side of the body. Next day left hemiplegia had become established, the angle of the mouth drawn to the right, the left cheek-pouch flaccid and distended with food, which had accumulated outside the dental arch, there being also total paralysis of the left arm and partial paralysis of the left leg. On the day following the paralysis of motion was complete over the whole of the left side, and continued so till death, nine days subsequently. Tactile sensation, as well as sight, hearing, smell and taste, were retained. On post-mortem examination it was found that the exposed convolutions were completely softened, but beyond this, the rest of the hemisphere and the basal ganglia were free from organic injury.

"In this we have a clear case, first, of vital irritation producing precisely the same effect as the electrical current, and then destruction by inflammatory softening, resulting in complete paralysis of voluntary motion on the opposite side of the body, without affecting sensation."

The experiments on monkeys—while showing this marked contrast between these animals and those lower in the scale, that destruction of the motor centres in the cortex produces complete paralysis—are in one respect incomplete. As long as the animals could be kept alive after destruction of the centres, there was no recovery from the paralysis, but this period was comparatively short, and it could not be said that recovery would not ultimately have taken place. This gap is to some extent filled up by observations on man. There are a few cases on record where there was softening of the cortex, involving the homologous motor regions of the human brain, and in these the resulting paralysis has been permanent. Our author gives a summary of some of these cases, and since attention has been called to the subject our readers may have noticed the record of other cases of this kind in the journals.

From these considerations the conclusion seems more than war-

ranted, not only that there are motor centres in the cortex cerebri, but that these centres represent, as it were, the corpus striatum, in what Hughlings Jackson calls a 'higher power.' As we ascend in the animal scale these highly developed and voluntary centres come more and more to dominate the lower ones, and in the monkey and man motion seems so intimately bound up with them, that when they are removed only the most automatic and rudimentary actions can be performed. This view entirely obviates the necessity of resorting to the rather clumsy device of supposing that one part of the brain can take up vicariously the functions of another. There is a certain sense in which this may be true. Certain actions which are at first purely voluntary may become organised in the corpus striatum, and may possibly continue to be performed after the removal of the cortical centres. There are also certain functions which in a certain sense are bilateral, and which, though involving movements on both sides of the middle line, appear to be related to one of the hemispheres—such functions as that of speech. These functions may be organised in one of the cerebral hemispheres, and we know that speech is probably localised in the left, but if this centre be destroyed it is very possible that the similar region of the right hemisphere may through time acquire the power which has been lost.

The experiments of Goltz, recorded in two articles in 'Pflüger's Archiv,' the last of them published since the appearance of Ferrier's work, demand very particular attention, both from the eminence of the observer and from the fact that they appear to lead to conclusions different from those of Ferrier. We shall return to these experiments again when considering the seat of vision in the cerebral hemispheres, but in relation to motion there are some points calling for notice here. We must premise that having read these papers very carefully we cannot see that any statements can be properly founded on them as to the localisation of functions in individual parts of the hemispheres. The experiments are most valuable, as showing what motor and other functions have their place in the hemispheres, but as to the localisation of these functions we consider that nothing can be seriously asserted or denied on the basis of them. The animals used were dogs, and, in order to destroy portions of the brain, apertures were made in the skull with a trephine and a jet of water forced against and into the brain. The water burrowed among the brain substance, and if there were (as in most of the cases) two or more apertures, channels were made from one to another ploughing through the brain substance. Such a serious operation as this would, in all likelihood, nearly abolish the functions of the hemisphere concerned for a time, and would probably interfere with lower centres, and Goltz very properly rejects the symptoms occurring immediately after the operation as not due immediately to loss of

brain substance, but rather to inhibition of centres from the irritation succeeding the operation.   It is rather the lesions which remain after the animal has lived some time, and which may be regarded as permanent, that he considers important.   These latter he calls phenomena of defect (Ausfallserscheinungen) while the others are phenomena of inhibition (Hemmungserscheinungen).   In regard to the former of these it is of importance to note that a dog may have almost the whole cerebrum washed away and yet be able to walk about, move the head, eyes, &c.   The permanent phenomena are a certain tendency to slip with the foot, especially in walking on a smooth surface, and a loss of the power of using the fore paw as a hand.   This last point is of special interest in relation to the subject we have been more specially considering, as it gives a rather striking confirmation to a speculation of Ferrier's.   In commenting on the fact that after removal of the cortical motor centres in the dog the paralysis will be recover from, so far as actions which have become automatic are concerned, Ferrier proceeds to remark :

" It may be confidently asserted, and perhaps it may be one day resolved by experiment, that any special tricks of movement which a dog may have learnt would be as effectually paralysed by removal of the cortical centres as the varied and complex movements of the arm and hand of the monkey by the same lesions" (p. 215).

And now we find that Goltz experimented on some well-bred dogs, which before the operation would give either paw on demand.   After one side of the brain had been operated on, however, the animal had no longer the power, when asked, of giving the paw of the side opposite the lesion in the brain, and if both sides had been destroyed there was a permanent loss of this power altogether.

We have dwelt at considerable length on the motor functions as worked out by Ferrier, because it seems to us that this is the part which has been thoroughly mastered by the author.   It remains to us to make some remarks on the centres which he supposes to be sensory, and on certain of his observations on the cerebellum and corpora quadrigemina.

The endeavour to prove that the various senses are represented in distinct parts of the cortex is in the nature of things a much more difficult task than where movements are concerned.   Irritation of the centres is here of much less use, for though it might be expected that irritation of an auditory centre, for instance, would produce reflexly movements indicative of the reception by the animal of some unexpected sound, yet this method of procedure will not carry us far, and it is chiefly experiments in which the various parts of the cortex have been destroyed that are trusted to here.   We may therefore anticipate that the results so obtained will hardly be so complete or trustworthy as those which we have already passed under review.

It strikes us that the most convincing of these endeavours to localise sensations is that which concerns the centre for vision.

Ferrier believes that he has determined the existence of a centre for sight in the convolution which curves round the upper extremity of the fissure of Sylvius, and is called in Ecker's nomenclature the " angular gyrus" (Pli courbe). Stimulation of this region produced movements of the eye-balls, frequently associated with movements of the head to the opposite side, and very often contraction of the pupil. But these phenomena seem all referable to the reception by the animal of visual impressions which have secondarily induced these movements. Destruction of this convolution produced total blindness of the opposite eye. There is, however, one fact connected with this centre which may be of considerable consequence, especially in relation to human pathology; it is, that the blindness caused by destruction of the convolution on one side was only temporary, compensation rapidly occurring if that on the other side was intact; but destruction of the angular gyrus in both sides produced total blindness in both eyes. This is a fact of great importance as affecting human pathology. It is only immediately after a lesion has destroyed the centre on one side that blindness of the opposite eye is produced, and this is rapidly recovered from, the other angular gyrus taking up the impressions from both eyes. We should expect, therefore, in cases where there has been gradual destruction of the angular gyrus in the human subject, that blindness would not be observed at all, but where its destruction has been rapid, as by embolism, thrombosis, or injury, then a temporary blindness would be looked for.

Goltz's experiments somewhat amplify these observations of Ferrier on the loss of vision when one side of the cerebrum is destroyed, and his papers are nowhere more interesting or suggestive than when describing the results obtained in this direction. Like Ferrier, he finds that both eyes are related to each cerebral hemisphere, so that when the centre of vision has been destroyed on one side the blindness of the opposite eye is only temporary. But the other hemisphere by no means completely replaces that which has been lost. The affected eye recovers sight for some things but not for others; and it is most instructive to observe the kind of things for which vision is lost. A dog was able with the eye whose cerebral centre had been destroyed (the other or sound eye having been enucleated) to see or avoid obstacles, but it was not able to recognise a piece of flesh put right in its line of vision, nor did it show any proclivity towards a duck put into the box with it till this animal began to make the noise peculiar to it. It did not show signs of fright when a servant appeared suddenly to it dressed in a style which had previously filled it with terror; and it also showed inability to appreciate

the height of a table from the floor, or to understand the use of a chair placed near the table to assist it to descend. Goltz suggests that the explanation of this may be that the animal has to a great extent lost its sense of colour and of the position of images on the retina. It seems to us a much more likely explanation that the centre of vision in the cerebral hemispheres forms for each eye what Ferrier calls an "organic memory," and that this memory being destroyed objects formerly familiar are no longer recognised. This would seem to indicate that the "organic memory" for each eye is situated separately from that of the other in the opposite cerebral hemisphere. These observations ought to be capable of being checked by clinical observation; and we may look forward to careful observations of the state of vision in cases where it is presumed that the cerebral centre of vision has been destroyed.

The determination of a centre for hearing in the convulutions is an exceedingly difficult task, and we observe that the results come to by Goltz, in the papers alluded to, are directly contrary to some conclusions of Ferrier. The latter believes that he has localised the centre of hearing in the superior temporo-sphenoidal convolution, or the convolution which runs along the fissure of Sylvius forming its inferior boundary. It is very difficult in an animal to distinguish between a mere reflex reaction to auditory impressions and a true auditory sensation, and after the destruction of the auditory centre the former may be mistaken for the latter.

For instance, Flourens found that animals deprived of their hemispheres started when a pistol was fired close to the head, but this was merely a reflex action, and no evidence of the retention of hearing. Bearing these facts in mind, Ferrier believes that—

"When the two sets of experiments are taken together, viz. the positive reactions to electric stimulation and the absence of reaction to the usual forms of auditory stimuli, when the superior temporo‑sphenoidal convolutions were destroyed, the evidence of the localisation of the centres of hearing amounts to positive demonstration."

We shall not follow the author into the other sensory centres, those of touch, smell, and taste. It may be admitted that he has established a strong presumption that touch has its special centre in the hippocampus major and uncinate gyrus, and that he has made it probable that smell and taste are related to the tip of the temporo-sphenoidal lobe or subiculum cornu ammonis. But these cannot be looked upon as established; and it will be the part of clinical observers and pathologists to look out for facts bearing on this part of the subject.

In entering on the considerations of the functions of the corpora quadrigemina and cerebellum we come upon ground which has been

frequently under investigation, but in regard to which opinions can hardly be said to have attained to any fixity. It appears certain, to begin with, that some animals, after being deprived of their cerebral hemispheres, are able to retain their equilibrum and possess the power of locomotion, although their movements are devoid of spontaneity. Such animals also exhibit emotional phenomena; for instance, rabbits so treated may be made to give out that plaintive cry, " with which all sportsmen are familar who have gone hare or rabbit shooting." There is no doubt that the corpora quadrigemina and cerebellum are the ganglia concerned in these three functions, and clinical and pathological observations in man give a general support to this conclusion.

When we come, however, to particulars we are met with serious difficulties. No one has any doubt that the corpora quadrigemina (called in lower animals the optic lobes) are in close relation to the optic nerves, and it has been concluded that they are the centres of vision. The real centre of the sensation of sight is, however, as has been already seen, in the convolutions, and it seems probable that the intimate relation of the external organ of vision to the corpora quadrigemina, is explained by the fact that visual impressions are an important factor in the retention of equilibrium.

In regard to the cerebellum, the author brings out some most interesting facts as to the result of stimulation of this organ by electricity. Whatever be the explanation of the fact, irritation of the cerebellum produces muscular movements, to which we shall refer more particularly immediately. We observe that since the publication of Ferrier's work, Nothnagel has described the effects of irritation of the cerebellum by needles, and he also concludes that it is in a certain sense a motor ganglion, irritation producing certain movements of the head and body. In Ferrier's original experiments on rabbits it was chiefly movements of the eyes that were observed as a result of irritation of the cerebellum, but in these the head was held fixed. It appears, however, that movements of the head and limbs accompany those of the eyes, although the eyes afford the easiest index of the direction of the movements. It is in the highest degree interesting to find that irritation of different parts of the surface of the cerebellum produces movements in different directions. For instance—and here we quote from experiments on monkeys—irritation of the upper vermiform process at its posterior extremity in the middle line, causes *both eyes to move straight downwards;* irritation of the vermiform process at its anterior extremity in the middle line causes *both eyes to move directly upwards;* irritation of this process to the left of the middle line, if at its posterior extremity, causes both eyes to move downwards and to the left; and if at the anterior extremity, upwards and to the left; and so on. It seems in fact possible to patch out the surface

of the cerebellum into areas, irritation of each of which is capable of producing a distinct movement of the eyeballs, and when the animal is free, of the head and limbs. These movements, be it observed, are movements in specific directions, upwards, downwards, to one side or the other. It may be added that after the application of the electrodes, a condition of nystagmus often comes on and lasts for some time.

These experiments throw considerable light on certain facts observed in disease or injury of the cerebellum in man. In these cases nystagmus and persistent ocular deviations have been frequently found, along with disturbances of equilibrium. It has been shown also by Purkinje and Hitzig that when a galvanic current is passed through the head by placing the electrodes behind the ears, a feeling of vertigo is experienced, and external objects seem to alter their position relatively to the person experimented on. Along with this the head, body, and eyes move in particular directions at the moment of the application of the current, and these movements vary, according to the direction of the current through the head. There seems little doubt that here the cerebellum is irritated, and the experiments are a strong confirmation of Ferrier's observations.

And now, as to the explanation of these phenomena, our author has some most suggestive observations. The cerebellum is composed of automatic centres, which regulate the equilibrium of the body. When the body is rotated to the left, equilibrium will be maintained by movements towards the right, and when an attempt is made to overturn the body from before backwards, equilibrium will be maintained by antagonistic movements which move the head forwards.

"The cerebellum would, therefore, seem to be a complex arrangement of individually differentiated centres, which in associated action regulate the various muscular adjustments necessary to maintain equilibrium of the body ; each tendency to the displacement of the equilibrium round a horizontal, vertical, or intermediate axis, acting as a stimulus to the special centre, which calls into play the antagonistic or compensatory action.

"Every form of active muscular exertion must tend to overthrow the balance, and we should, therefore, expect, on the above hypothesis, that the cerebellum would be developed in proportion to the variety and complexity of the muscular activity of which the animal was capable, a relation which is fully borne out by the facts of comparative anatomy (Owen)."   (p. 109).

"The displacement of the equilibrium in any direction not only calls into play, by reflex action, the compensatory motor adjustments, but also induces conscious or voluntary efforts of a similar antagonistic or compensatory nature. Thus, a tendency to fall forward, while reflexly calling into action the muscular combinations which pull the body backwards, may also excite consciousness and cause

voluntary effort in the same direction. The same muscular adjustments which are capable of being effected by the cerebellum, are also under the control of the will, and may be carried out by the cerebral hemispheres independently of the cerebellum."

This last observation points to an explanation of the fact that lesions of the cerebellum may produce no very appreciable symptoms during life. There are, undoubtedly, many cases on record in which disease of the cerebellum has produced unsteadiness of gait, due to imperfect retention of equilibrium. But this loss of the power of retaining equilibrium may be in great part compensated by voluntary effort. Of course this will entail a great exertion, or, we may say, a great waste of voluntary effort, and movements will be accompanied by much more fatigue than when the automatic apparatus for preserving equilibrium is in action. As a matter of fact, it was observed by Weir-Mitchell, that in pigeons whose cerebellum was destroyed, and which lived some considerable time after, great fatigue was produced by active muscular exertion. On this fact Weir-Mitchell founded his theory that the cerebellum is a source of energy to other nerve centres; but, as our author remarks, it is only by sparing higher centres that it can be regarded as a source of energy.

There are many other parts of this work which we had noted for observation, such as the speculations—for they cannot be regarded as anything more—on the functions of the occipital and frontal lobes of the cerebrum, the psychological aspects of the question, &c., but the review has already reached large enough dimensions, and we have, we hope, sufficiently shown that the subjects treated of are discussed with a wonderful degree of force and perspicuity, while the experiments which form the basis of the work show an ingenuity and resource which can hardly be too highly estimated.

X.—Recent works on Evolution, Botany and Natural History.[1]

ADMITTING that the theory of evolution is that which is usually received by scientific men as the best explanation of progress in

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[1] 1. *An Examination of the Modern Theories of Automation and Evolution*. By CHARLES ELAM, M.D., pp. 163.

2. *Text-book of Structural and Physiological Botany*. By OTTO W. THOMÉ, of Cologne. Translated and edited by ALFRED W. BENNETT, M.A., F.L.S., Lecturer on Botany at St. Thomas's Hospital. 1877.

3. *Article Amphibia in the Encyclopædia Britannica*. 9th edition. By Prof. HUXLEY, F.R.S., &c.

nature, we must expect that its professors will have their vagaries and occasionally run riot, and will therefore be subject to such downfalls as Dr. Elam has the full power of giving. Evolution and Darwinism have been constantly attacked, but never before has the assault been so generous and free from abuse and the terrors of theology, as in the present instance. Half-educated naturalists, enthusiastic theologians, and some few highly educated geologists and botanists, have rather increased the value of these theories by the misapplication of facts and by careless reasoning. But the accomplished physician, whose former works have always had a great charm, attacks the Huxleyo-Tyndallian-Häckelismus on its own ground, and shows the inherent absurdity of this particular phase of evolution, and proves it to be a mere hypothesis. The following quotation will at once explain how he treats his subject:—
" An automaton endowed with free will is certainly a pleasing and interesting novelty in physical science, and Mr. Huxley deserves great credit for his ingenious invention. It would have been an intellectual treat to listen to him replying to any unfortunate opponent who had committed himself so profoundly." Dr. Elam thus seizes upon a statement, and with a little banter proceeds to demolish its value instead of indulging in scolding and insisting upon certain awful consequences. He shows that this ingenious invention, taken as a proposition, is simply suicidal, and states, " for as no one to my knowledge ever considered free will to signify anything else but the power to do as we like, the definition of man as being an automaton endowed with free will leaves him exactly where it found him ; that is, as an intelligent free agent." Further on, we find Professor Tyndall is crushed with his own words. As a philosopher, Professor Tyndall discerns in matter, the promise and potency of all terrestrial life ; that is to say, he believes in a doctrine of evolution. Dr. Elam shows that this belief may be summarised as follows :—1. That the earliest organisms were the earliest product of the interactions of ordinary inorganic matter and force. 2. That all the forms of animal and vegetable life were successively and gradually developed from the earliest and simplest organisms. 3. That the doctrine of evolution derives man in his totality from the interaction of organism and environment through countless ages past. The Professor states that the conclusions of pure intellect point this way and no other ; he sees with the eye of imagination a primitive nebular haze, gradually contracting into a molten mass, in which are latent and potential, not only all the forms of life, noble or ignoble, but the human mind itself, emotion, intellect, will, and all their phenomena . . . all our philosophy, all our poetry, all our science, and all our art—all are potential in the fires of the sun. This is an extract from the celebrated Belfast address, and most of its former critics have given it the value of an

imaginative hypothesis of a not very profound man, and have conveyed their opinion in a very decided and somewhat rude language. This is not Dr. Elam's method; he praises the ingenuity of the teacher, and then proceeds with a cruel logic to disprove his right to convey any of these assertions, except in the form of guesses. He separates the professor into the philosopher and into the man of science, and considering what the philosopher has spoken he quotes what the man of science has written, and then judges the philosophy by the science. He quotes Professor Tyndall as follows :—" Without verification a theoretical conception is a mere figment of the intellect : the region of theory lies behind the world of the senses, but the verification of theory occurs in the sensible world. To check the theory we have simply to compare the deductions from it with the facts of observations. If the deductions be in accordance with the facts, we accept the theory ; if in opposition, the theory is given up." This is, of course, true philosophy and true science ; and Dr. Elam then quotes the Professor to show that the grand theory he has accepted must be given up. Professor Tyndall writes : " If you ask me whether there exists the least evidence to prove that any form of life can be developed out of matter without demonstrable and antecedent life, my reply is, that evidence considered perfectly conclusive by many has been adduced ; and that were some of us who have pondered this question to follow a very common example and accept testimony because it falls in with our own belief, we also should *eagerly close* with the evidence referred to. But there is in the true man of science a desire stronger than the wish to have his beliefs upheld, namely, the desire to have them true." " In reply to your question they will frankly admit their inability to point to any satisfactory experimental proof that life can be developed save from demonstrable and antecedent life." Here, then, is a distinct want of accordance between philosophic theory and scientific observation. In fact, Professor Tyndall's theory is no such thing ; it is a jumble of conjectures of the vaguest possible kind, which would require very much explanation to give them any scientific value. Häckel is pounced upon in the same logical manner for having inserted in our genealogical tree a form of animal which he calls *Chordonia,* which develop themselves from the Annelida by the formation of a spinal marrow and a chorda dorsalis.

The details of the structure of these interesting animals are given very systematically, and it is shown how they became the parents of the nearest now living genera, the Ascidians. Unfortunately, Häckel does not even profess to have any evidence to produce that such animals ever existed ; there is no living representative of them, there is no fossil evidence of their early existence. They are, of course, required to fill up a gap in Professor Häckel's argument, but on applying the above Tyndallian statement, "without verification, a

theoretic conception is a mere figment of the intellect" to Professor Häckel's dictum, he is shown to be fertile in invention, false to science, and perversely unreasonable.

In fact, this proceeding of Häckel's is most dangerous to science, and really may be said to be impudent. It is in this same spirit that Häckel pronounces upon man's pedigree with the most unhesitating confidence, and Dr. Elam shows that this confidence is folly. Dogmatism and folly are twin sisters and support the Professor's coat of arms. Dr. Elam shows us that, according to Häckel, the Monera were formed in the Laurentian epoch by spontaneous generation from inorganic matter although Tyndall and Huxley have shown that spontaneous generation cannot be; but we are comforted by the assurance that the acceptance of one of the Monera as our earliest ancestor rests *on the most weighty grounds.* Of course, Amœba came next. Then came ciliated animalcules, which preceded a group *Gastræa,* but these are imaginary, and like *Chordonias,* are inferred to have existed. Then comes the worms, and finally, these imaginary *Chordonias,* which are, *undoubtedly,* the progenitors of all the Vertebrata. When we get to the fifteenth stage we come to the *Protamniota* or the general stem of the Mammalia, Reptiles, and Birds. Häckel writes, " I do not suppose any one is in a position to say what these were like, but they are *proved* to have existed, because they were necessary forerunners of the pro-Mammalia."

After passing over some other steps we are gravely told there are the Anthropoids or man-like apes, represented by the modern Orang, Gibbon, Gorilla, and Chimpanzee, amongst which, however, we are not to look for the direct ancestors of man, but amongst the unknown extinct apes of the Miocene; then out of these came dumb ape-men, an unknown race, the nearest modern representation of which are cretins and idiots. They *must have* lived as a necessary transition to the Homines who develope dthemselves from the last class by the gradual conversion of brute howlings into articulate speech. To all this Dr. Elam adds with great force the following quotation, which is eminently instructive regarding the exaltation of the Häckelian intellect. This opinion :

" The direct descent of man from ape-like ancestors, is never held by thoughtful supporters of the descent theory, though often by their *thoughtless* opponent. Our ape-like ancestors are long since extinct. Perchance their fossil remains may some time be found in the tertiary deposits of southern Asia or Africa. They must, nevertheless, be ranked amongst the tailless catarhine anthropoid apes."

It is greatly to the credit of our author that he does not burst, as most reasonable beings feel disposed to, do into a diatribe ; it is

typical of his style, that this should not be done, and he contents himself by stating :

> " Such a scheme of progression has no existence in nature. There is no evidence of it in existing forms of life, and there is no indicating of it in fossil remains. There is no possibility of such a progression, even as a matter of theory in accordance with the recognised laws of morplology—that is, if morpheology be a science at all, or anything beyond an incoherent aggregation of irrelevant and unconnected details of structure."

One of the great advantages of being an uncompromising evolutionist of the dominant school is, that you will be well backed up by your fellow-believers. Thus it was much to the advantage of Mr. Herbert Spencer that he took up evolution. He has written much, and in a style beautiful in the eyes of those who enjoy long words and involved sentences. Science owes him one doubtful botanical fact, but his volumes increase year by year. Not an original investigator of nature, he is an original thinker, as the term is, that is to say, he puts odd constructions on the discoveries of others and evolves evolutionary dogmas from his inward self. It is the fashion to admire Herbert Spencer, to speak of his works with subdued reverence, and generally for the very good reasons involved in the words "Omne ignotum pro magnifico." But although ordinary mortals, who like proofs better than inferences, may think with Dr. Elam that many of Mr. Herbert Spencer's "irresistible inferences" are "unverified assumptions," still he is loved and honored by the distinguished professor at the Royal Institution, who calls him tne "Apostle of the Understanding." Let us see what this apostle tells us about the beginning. He tells us that "organisms are highly differentiated" portions of the matter forming the earth's crust and its gaseous envelope, and that organisation consists principally in "the formation of an aggregate by the continued incorporation of matter previously spread through a wider space;" and also that this formation depends upon "an integration of matter and concomitant dissipation of motion, during which the matter passes from an indefinite, incoherent homogeneity to a definite coherent heterogeneity, and during which the retained motion undergoes a parallel transformation." These quotations from the "first principles" influence the understanding according to the belief we may have in this "apostle." The school which likes the inferential, rejoices at the hollow sound they convey; but the independent in thought are apt to consider that these grandiloquent sentences will apply to the manufacture of a snowball quite as well as to that of an Amœba, in fact rather better. The apostle of the understanding is rather severely taken to task by our author, who finds endless holes in his armour. Thus, after summing up the

opinion of Tyndall and Darwin regarding their development of
evolution, Dr. Elam writes as follows :—" This, although meagre and
bare, is, I believe, a tolerably faithful outline of a system which is
now *known* to afford the only possible solution of the mystery of the
universe, a conclusion the grounds of which *will never be shaken,*"
a doctrine not founded " on the basis of vain conjecture, but of
positive knowledge."   (The italics represent the words of the dis-
tinguished physicist and naturalist.)   This is contrasted with the
doctrine of special creation by Mr. Herbert Spencer, whose com-
parison concludes thus :—" The belief which we find thus question-
able, both as being a primitive belief and as being a belief belonging
to an almost extinct family, is a belief that is not countenanced by
a single fact.   No one ever saw a special creation ; no one ever
found proof of an indirect kind that a special creation had taken
place.   It is significant, as Dr. Hooker remarks, that naturalists,
who suppose new species to be miraculously originated, habitually
suppose the origination to occur in some region remote from human
observation."   " If this," writes Dr. Elam, " be intended for argument,
it is certainly double-edged.   Did any one ever see an organic
evolution ? or did any one ever see proof of such evolution having
taken place ?   The answer must be, *No !* however circuitous and
veiled it may be.   In the remaining allegations there is an uncon-
scious and childlike innocence that almost disarms criticism.   The
system that demands ten or a hundred thousand generations for the
development of the distinguishing characters of a single species, and
a world so different from its present state that not even a trace of
its existence remains, can scarcely object logically or consistently to
the relegation of certain phenomena to a " region remote," whether
in time or space.   And with all this, those who do not or cannot
accept this evolution doctrine are denied the possession of the very
faculties of thought and belief.   To any one who says he *thinks* the
universe was created, Mr. Herbert Spencer replies, " No ! you do not
think so, for such a doctrine is not thinkable."   And to those who
say they believe in a Creator and creation, Mr. Spencer replies,
" No, you do not believe, you only believe you believe."   Surely
this is the very Dundrearyism of philosophy."

In continuation, Dr. Elam declines to discuss anything but
evolution, as that is his point of attack, and he wishes to know
whether evolution is true or not.   He writes : " It is not altogether
easy to approach this question so as to obtain a decisive answer.
If we treat it as a scientific inquiry, and ask for some confirmatory
evidence, we are told, almost plaintively, that the strength of the
doctrine of evolution consists not in experimental demonstration
(Tyndall).   If we further inquire how it is to be approached, and in
what its strength does consist, we fail to get any definite answer,
except some vague statement as to "its general harmony with

scientific thought." Indeed, the attitude of evolution is entirely exceptional. It seems to be taken for granted that the doctrine possesses some esoteric and mysterious principle of utility and credibility which makes it independent of any support from science or certain (exact) knowledge."

Dr. Elam is very happy in his explanation of how the "constructive philosophy" termed evolutionism is built up, and how the studies are made from conjecture to certainty in its interests. He notices how Mr. Spencer, having seen *reason to suppose* such and such things, without any further botheration, as the unscientific might call it, treats the suppositions as ascertained facts, and proceeds to build up another, as if they were realities. He notices that Tyndall "*expects to find*," on philosophic grounds, such and such physical conditions, and then commences an introduction from the same with the phrase, "the relation of physics to consciousness *being thus* invariable," &c. "Such," writes our author, "being the received method of evolving science out of personal consciousness at the present day, it ceases to be subject for surprise that so many volumes of portentious dimensions should have appeared containing so little absolute addition to our certain knowledge of nature."

In considering the great difficulty of evolution, the impossibility of the synthesis of organic matter—a subject rather ignored by Professor Huxley—Dr. Elam is cruelly sharp upon Mr. Herbert Spencer, who has felt that it must be met with some form of words at least, and has written : "The chasm between the inorganic and the organic is being filled up. On the one hand, some four or five thousand compounds, once regarded as exclusively organic, have now been produced artificially from inorganic matter, and chemists *do not doubt their ability* so to produce the highest forms of organic matter. On the other hand, the microscope has traced down organisms to simpler and simpler forms, until, in the Protogenes of Professor Häckel there has been reached a type distinguishable from a fragment of albumen only by its purely granular character." Dr. Elam notes that it seems incredible that this should be intended for serious argument, and he shows the untruth of the statement, suggesting, moreover, that not one grain of albumen has ever been made. After explaining how Professor Huxley elaborates his notions on the physical theory of life, in which the idea of vital energy is abolished and asserted to be identical with ordinary physical and chemical energies, our author very happily seizes upon the weak point in the Professor's argument about the differences of dead and living protoplasm. Huxley is driven, then, to assert that the protoplasm without life is subjected to *subtle influences;* and when he is required to state that there is any difference in the molecular or chemical constitution of dead and living protoplasm he pronounces the idea "frivolous," unless Dr. Elam misapprehended

" the meaning of the writer's (Huxley) rather obscure and perhaps quaquaversal expressions." Subtle influences and automata endowed with free will are singular evolutions, but " evolution is forbidden to be judged by any ordinary standard ; it has privileges, a language, and an inviolability all its own, and those who think or believe otherwise do not, as we have before seen, think or believe at all, but only think they think, and believe they believe." After a very able exposition of the fallacy of the doctrine of the " evanescence of evil " under the evolution hypothesis, Dr. Elam requires the withdrawal of the Darwinian notions on the following plea (in which his own words are mainly used) : " In the ' Origin of Species ' we are more than once told that it would be fatal to the theory if the discovery were made of characters or structures which could not be accounted for by numerous successive slight modifications ;" but in the ' Descent of Man ' there is the following passage : " No doubt man, as well as every other animal, presents structures which, as far as we can judge with our little knowledge, are not now of service to him, nor have been so during any former period of his existence. Such structures *cannot be accounted for by any form of selection,* or by the intended effects of the use and disuse of parts." Immediately after this passage Mr. Darwin refers to their production by *unknown causes,* which, obviously, like Professor Huxley's *subtle influences* as a source of life phenomena, involves a relinquishment of the entire position. The conclusions which necessarily follow from the foregoing observations may be briefly summed up in one syllologism, embracing not only natural selection but also the larger theme of organic evolution generally. Without verification a theoretic conception is a mere figment of the intellect (Tyndall).

But the theory of organic evolution is an unverified theoretic conception (that is to say, Huxley states that the only way in which such an hypothesis can be proved to be true is by observation and experiment upon existing forms of life).

Therefore organic evolution is a mere figment of the intellect.

This is the candid expression of a thoughtful man of high culture, and as we live in an age of opposition to popes of all kinds it merits much attention. There is no doubt that evolution has run mad under the hands of its very enthusiastic developers, and that an hypothesis which explains things better than any other has been assumed to be of the value of a theory—of a fact. All this must be kept in mind whilst reading this interesting book, and it must be remembered that a theory must stand or fall upon its intrinsic merits, and that it is not just or useful to say that such and such an hypothesis must be left alone because all others relating to the subject are unreasonable, or fail in their applicability.

Nevertheless, at the present age of zoology and comparative anatomy, studied, as these sciences are, by the light of embryology,

there must be a theory upon which to hang facts and satisfy the longing for causation. The Palæontologist insists upon including his ideas in some theory which will render his facts of subjective value, and requires that the one particular hypothesis which relates to repeated acts of special creation shall be struck out. He claims t he lesson taught by geology and the continuity of structural detail through long lines of animals and plants as evidence of the continuous progression of organic forms, and as a proof of the *probability* of an evolution. The last authoritative suggestion, emanating from the President of the Geological Society, requires something over and above evolution as ordinarily taught. He may be thus quoted: "It (the mind) is, moreover, dissatisfied with the belief that all the wonderful art in nature, the limited direction of variability, the parallelism of form, ornament, and physiology, in contemporaneous and successive groups of fossils, sometimes so widely separated zoologically, are due to the action of physical changes and heredity alone. It is true that the physical change is not fortuitous, but relates to the inevitable, and thus its influence on life is part of a great philosophy; but is that source of the action of the mysterious energy on matter which we call life, simply passive and only alterable by external conditions? According to the prevailing theory, if all the external conditions remain the same, the individuals of a species, or the species of a genus, will retain their classificatory character; but if change takes place in the physical condition, or if alterations occur in the struggle for existence, then the variability will bear a relation to the intensity of the opposing forces. Extinction, or the removal of the fitness, results, and this is accompanied by a loss of specific identity. Is this all the truth? Is there not some positive energy in living things which, if uncontrolled and uninfluenced by externals, will produce progressive change." "It was not competition or the influence of external conditions alone that enabled the structures of the marsupial to meet the habits and the method of life of the quadrumana, bats, insectivora, rodentia, herbivora, carnivora, and even of the pachydermata, but the operation of a law of variation in definite directions, potential in the organism, and irrelative of physical conditions." Clearly there is a movement to get out of the dogmas of the so-called advanced school—a school which unfortunately cannot distinguish between the proximate and the ultimate, and is ever ready to move out of its path to attack those opinions which are quite beyond its aspirations, and which have been the guides of the pathway of the men who have really and effectually elevated humanity.

That the hypothesis of evolution is highly probable, there is no doubt; that it can be proved from demonstration is not possible; and hence it is advisable for the chemist to wait until he can

synthesize albumen, the physicist to try successfully to combine energies and forces so as to turn dead protoplasm into living protoplasm, and the zoologist to show the transition of one species to another, before they deal with the subject of the great first cause that at any rate did create energy and moving molecules—not more incomprehensible than the moral government of the world.

2. This is the text-book which is used in many of the German schools, where it is very popular on account of its elaborate yet condensed treatment of morphological and histological botany, and doubtless also an account of the numerous (more than 500) woodcuts which embellish the work. But why should this closely-written manual be introduced into England in opposition to the manuals of Masters, Bentley, and others. The answer appears to be, firstly, that either Dr. Bennett or a Rev. Alexander Irving, of Wellington College, was not aware that a work of this same scope was to be found in the English language! We are informed, moreover, in the preface, that " in editing the present work, the object especially kept in view has been to make it useful to candidates preparing for the Science Examination conducted by the Educational Department at South Kensington, or for those of the University of London." The editor believes that this book will suffice for the examination as high as the second Bachelor of Science pass examination, but then a better will be required for the honours. He advises for the highest examination such books as Sachs's, Le Maout's and Decaines'. The German text of Thomé, has been, for the most part, translated, but the scheme on the classification of the flowering plants has been rewritten, and the orders arranged in accordance with Hooker and Bentham. The two chapters which relate to geological and geographical theories have been cut down, and the writer has taken pains to avoid, " as far as possible, the putting forward of geological or geographical theories," as demonstrated truths. It will be observed, that the editor desires to teach well and to do no harm, and especially to keep the young, from the evil counsels of those dreadful geologists. There is no doubt if this book could be mastered, for it is ably and conscientiously translated, any examination could be passed and the mind left settled in comfort, the old skeletons being carefully locked up in nice little closets. But is the passing a South Kensington or London University examination the ultimate expression of the botanical intellect? Suppose that the student had passed gloriously in Thomé. What then? probably he or she, as the case may be, will turn out priggish, mild, unoriginal and learned, or will be content with the success of " cram," and will botanize no more. It is a remarkable fact that ever since these examinations have been so much in vogue, and so many well-educated botanists have been sent out into the world, England has not

celebrated for research into botanical histology and physiology. In fact, these parts of the science are nearly extinct with us, and they are kept alive by Darwin, Burdon-Sanderson, Masters, and one or two others, all of whom, by the bye, got their primary instruction from British writers, such as Lindley, Forbes, and Henfrey, and men of that grand stamp. Everything in the present dominant botanical school must be foreign, and when English workers find that their labour is in vain, and that their research is absolutely ignored by such men as Thomé, it is not likely that they will persist. Now suppose our good English text-books are compared with this work of Dr. Bennett's is the comparison to their disadvantage or not? Most certainly not, and therefore the Rev. Alexander Irving, of Wellington College, doubtless a very able man, had better learn a little more of the work of his own country- men before he says that there is "no work of the same scope" as that now under consideration in the English language.

In the introduction to the book, there is an interesting history of the progress of botanical science, and of course, from the German point of view, but so much reduced that the translator adds a note introducing some English names, but strangely forgets Henfrey, Masters, and the host of our microscopic botanists. Thoroughly German, as the book is, we were not prepared for the following:

"But when we descend from the higher animals and plants to the less perfect organisms belonging to these two kingdoms of nature we come eventually on the boundaries of each to organisms so small and so imperfectly developed that it is scarcely possible to pro- nounce an opinion whether they are of animal or vegetable nature."

This is bad teaching. Take one of these organisms such as Volvox globator, it is not the *imperfection* which makes it impossible of classification or the minuteness. Consider Actinophrys with its won- derful life-history, or the wonderful moving Naviculæ; it is not the minuteness, and certainly it is not the imperfection which has any- thing to do with the classification. There is no imperfection. The development is perfect, and even in Amœba, there is the nucleus, contractile vesicle, endosarc, entosarc, and a definite life-cycle. Why does Herr Thomé not assert at once that the division of organic nature into the vegetable and animal is arbitrary, and that what was taught in England thirty years ago, and what Germans have imitated of late is true, namely, that in the simplest forms of living things there is perfection, and that there is a group to which the term animal or vegetable does not apply. There are several instances of that unscientific inexactitude which characterises these examination- passing books to be found in the early pages of Thomé's book, matters of small importance, but leading to slip-shod work. Take

one or two, " If a small quantity of the green mould which com-
moᴜly appears on articles of food that have been kept for a few
days is examined." Such articles may be kept for ever in some
places and decompose or dry up, and yet no penicillum glaucum
form. In treating of nuclei there is the following :

" In the living condition (cell, we suppose, is meant) they are very
difficult to recognise, but as far as we know at present, they are
larger than when dead, and of variable irregularly jagged form. They
creep about in the protoplasm in which they are embedded after the
manner of an amœba, and certainly take the largest share in the
continuous shifting and transformation of the bands of protoplasm
which start from them."

Firstly, did Dr. Bennett ever see amœba in movement; if he did,
he will have observed something utterly unlike any nuclear faint
undulation and passive movement; and secondly, is it a fact that
most nuclei are so irregular in shape; and lastly, it is a fact that
the nuclei has energies by which it moves anything. In the
page from which the above quotation is taken, there are some
figures which are very remarkable; a Coscinodiscus and a stellate cell
are interesting. If the student, after passing the tremendous ordeal
of South Kensington and earning his teacher a microscopic gratuity,
were to see a real Coscinodiscus and a real stellate cell, he would be
much surprised. Equally funny and inexact are the figure of Euas-
trum *crux-Melitensis* (which is certainly a queer way of spelling the
latin for *Maltese cross*)—of the wood cell of the Scotch fir—of a spiral
cell of a cactus—of conjugation and spore formation—of intercellular
substances—of pediastrum, and again of stellate parenchyma. The
figures of scalariform vessels, dotted cells, and of a leaf section of
Cycas, and of hairs, are like the rest, diagrams and bad woodcuts, and
therefore in every way objectionable. They cannot bear comparison
with the drawings in our standard British books.

The chapters on the external forms of plants are very good, and
even painfully minute, for even a carrot and a radish are figured, and
there is no excuse for the student not being thoroughly *au fait*
with the terminology. The leaves and their shapes are very well
explained, and the greater number of the figures of them are
admirable; and the same remark applies to the flower and fruit.
Some excellent descriptions of the lower plants follow. Good
chapters on physiological botany and the special type of vegetable
life, the development of Torulais, is explained according to Professor
Huxley, but from whom he derived his information is not men-
tioned.

In the chapter on what is called palæo-phytology the naughty
geological theories are considered, and we are told that water has
acted by *mighty convulsions,* and that the Cambrian system contains

but few remains of organic life. Then it is stated that in the Devonian period the variety and luxuriance of vegetable forms far surpassed our existing vegetation. These extraordinary statements are supplemented by a miserable description of the Carboniferous vegetation, and a worse of the wonderful development of plantæ in the Trias. The writer appears to ignore the upper cretaceous flora, and indeed to know little of the subject which has excited the geological world for a long time past, namely, the wonderful persistence of plant types during changes in the fauna and the physical geography. If this is the way geology is treated by Thomé, the learners will not be troubled with anything that demanded the criticism noticed in the commencement of this notice. In the chapter on Botanical Geography we turn naturally to Australia to see how the author treats the strange isolation of the floras of the southeast and south-west, and the African affinities of the last and the Asiatic affinities of the northern flora. No information is given. If this is the kind of book which is to rear the future official botanists a good time is coming for the British writer. Dr. Bennett is a hard working, able botanist, and could write an admirable manual, and one which would meet the real requirements of the day. It is a pity that he does not do so; and he must be aware that translations, however well and conscientiously performed, do not bring much credit.

3. If any proof were required to demonstrate that anatomy has progressed wonderfully as an exact and comparative science during the last ten years, the admirable essay of Professor Huxley's would afford it. There are few encyclopædiacal articles which may be read, or rather closely studied by those medical men who have had a modern training with greater satisfaction, and indeed pleasure, than this elaborate description of a group comparatively unknown on account of its having been jumbled with the reptiles. Formerly the term amphibia was very widely used, and in as unphilosophical a manner as the division of the vertebrate into warm and cold-blooded animals. It was made to include snakes, and even some fishes; but the discovery of the fossil remains of huge vertebrates with frog-like affinities rendered the careful study of the osteology of the Batrachia necessary for purposes of comparison, and this led to the admirable work of so many British and foreign naturalists on the embryology of the groups. Not to be grouped with mammals, birds, true reptiles, or with fish, the amphibia present in their own principal divisions a singular separateness which impedes a positive definition of the class. As a class, they are intermediate between the fish-like (Ichthyopsida) and the sauran-like (Sauropsida), and are distinguished very sharply by having the visceral arches of the embryo developed into gills, which temporarily or permanently

perform the respiratory functions. In the amphibia, there is no trace of an amnion, and the basi-occipital region of the skull is either incompletely or not at all specified. They differ from the Sauropsida in having two occipital condyles. From the fish-like they may be distinguished by the characters of the locomotive apparatus only. When they possess median fins and limbs these never present fin rays; and the limbs exhibit in full development the type of structure which obtains among the Ganoids and Mammals, and differ widely from the fins of any other fish at present known. Even among the long extinct amphibia of the Carboniferous spot this difference prevails. But the lowest of the amphibia approach the Ganoid fish and the Dipnoi, and they present various approximations to the Marsupibranchia.

As a class, the amphibia may be divided into four families, the Urodela, the Anura, the Peromela, and the Labyrinthodontia; and these are of course sub-divided. The Urodela have branchiæ persistent throughout life—the old-fashioned Perennibranchiate, and these are divided into two groups, the Siren being the type of one, and the Proteus of the other. But even here the extraordinary biological fact of one group having pelvic arches and limbs, and the other being deficient in them, explains how slight is the cohesion of the whole zoologically. Two other groups of the Urodela have the branchiæ caducous; in one the gill clefts are persistent (Amphioma and Menopoma), and in the other these structures are closed in the adult condition (salamanders).

The Anura—all of which have, like the Urodela, a horny beak in the young condition—are sub-divided on the principle that the phases through which the frog passes in the course of its development show that the Anura, which are devoid of a lymphatic cavity, are of a more embryonic character than those which possess one. The abnormal habit is so evidently adaptive that it can hardly be regarded as a safe basis for classification. " Even *Rana temporaria*, at a year old, will climb up the vertical side of a glass vessel, flattening out the ends of its toes and applying its belly against the surface of the glass like a tree frog." So the Anura are sub-divided into two divisions, those with the tympanic cavity with its Eustachian tube present or absent; when present, the oval apertures of the tube are separated, and the pterygoid bones do not furnish a floor to them; and in the other division the Eustachian tubes have a common median aperture in the mouth and the pterygoid bones extend beneath, and form a floor to them. The toads are types of the first, and the Pipa of the last.

The Peromela are recognised by their snake-like bodies, and are totally devoid of limb arches. In most the integument is provided with transverse rows of embedded cycloid scales, but there are no pectoral plates. The vertebræ are amphicœlous, and the

hyoidean arch is attached neither to the suspensorium nor to the skull; it is followed by several slender hoops, like branchial arches. The young have branchial clefts with rudimentary branchial filaments. The well-known genus Cœcilia may be called as the type. Next come the extinct Labyrinthodontia, so named from the elaborate intricacy of the markings on the transverse section of the teeth. For the most part they resembled the Urodela in the proportions of the tail and limbs to the body, but some as Ophiderpeton, were serpentiform, and apparently without legs. They had digits to the hand and foot, and sculptured pectoral plates, besides small armour plates and an elegant grooving and gyration of ornament on the skull. The Labyrinthodont skull had great analogies with that of Peromela; and considering what limb bones, vast jaws and teeth, and huge bodies they had, it is certainly obvious that the amphibia were fully developed during the later Palæozoic and earlier Mesozoic ages.

The biological articles in this edition are most satisfactory.

Bibliographical Record.

Public Health.[1]—The returns of the Registrar-General for the past year have just been published, and it is satisfactory to find from them that the state of " public health " is yearly improving. It is evident that this result is entirely owing to recent legislation, and to hygienic knowledge disseminated by various means throughout the length and breadth of the land ; for we observe on all sides, with very few exceptions, towns and districts undergoing changes of some sort—old houses are being demolished to give place to more improved dwellings, narrow streets are being widened to increase, amongst other advantages, that of ventilation ; sewerage, drainage, and water supply are being improved, and trees, useful and ornamental, are being planted in our populous towns ; and though these changes are necessarily slow, yet when they are effected they will be sure to be attended even with a still further improvement in the state of public health. There is no greater evidence of the beneficial results of applied sanitary laws to urban and rural populations than the fact that the deaths from fevers during the short space of six years have been gradually reduced from 79 in 1870 to 43 in 1876 per 100,000 living, and, moreover, this includes a diminution in the deaths from enteric fever, which is at present one of the most fatal of the diseases of the zymotic class.

As the conditions which influence the state of public health are as varied as the occupations, tastes, and habits of the people, so are sickness and mortality influenced, and just in proportion as knowledge, even of simple sanitary laws, are diffused and understood, so will be the health rate. Officers of health and other sanitary authorities have, however, much to contend against in the performance of their duties, and it is frequently no easy matter to overcome, without the aid of the law, prejudices and ignorance ; for instance, in a case where the water supply was found to be un-

[1] 1. *On Personal Care of Health.* By E. A. PARKES.
2. *Public Health.* By E. A. PARKES, M.D., F.R.S.   Revised by W. AITKEN, F.R.S. London.

doubtedly contaminated with sewage, strong opposition was offered to its improvement on the ground that as the water had been in use for some time, no case of sickness was directly traceable to it.

Besides, we all know of the violent opposition occasionally offered to the Vaccination and other Acts of no less utility; and we can account for such opposition only on the score of sheer ignorance which, it is hoped, will vanish sooner or later as people become better informed.

With this object lectures on state medicine are now given in most of our universities, as well as popular illustrated lectures on hygiene, in the various towns in the United Kingdom, in addition to an immense amount of practical information conveyed by the public journals, especially those devoted to the numerous departments of public health and sanitary science. We look forward, however, principally to the results of careful observation and continued experiments with the view of further finding the best means of preserving health and happiness; and here we must deplore that the State holds out so little inducements to the unselfish scientific labourers, to whom this country in particular owes so much of her greatness.

These considerations lead us naturally to feel the loss of one of the most earnest investigators and one of the greatest public benefactors of his age—the founder of hygiene—the late Dr. Parkes, whose unselfish life was principally spent in endeavouring to find out under all conditions nature's secrets, and by means of experiments and close observations, deducing and framing from them rules and regulations for improving the condition of the whole human race.

We shall here briefly refer to some of his last writings, in which he has also indicated lines of research for future investigators. During the course of his fatal illness he found time to write a small manual 'On the Personal Care of Health' for the Society for the Propagation of the Gospel, and after his death a short treatise of a more general character was found in manuscript amongst his papers, both of which are now published.

That of the last, entitled 'Public Health,' has, we are informed in the preface, been revised by his friend and colleague at Netley, Dr. Aitken, who has very wisely made no material alteration or addition beyond writing a table of contents.

These two little books taken together contain in a condensed form a vast amount of information, conveyed in plain, intelligible language, and should be regarded by the public, as no doubt they eventually will be, as legacies of no mean value.

The manual 'On the Personal Care of Health' is, as its title indicates, addressed to individuals, pointing out how they are to

manage and keep the precious gift of health during puberty, man or womanhood, and old age; because, as the author truthfully remarks,

"There is, so to speak, an individual or personal hygiene which must also be brought into action, and without which half the work must remain undone, and the burden of sickness and suffering be but half removed."

The chapters on the management of the body during the periods of growth and manhood are especially useful, and the author impresses in forcible language how errors in diet and regimen are to be avoided, and how life is to be prolonged. He also shows clearly the comparative value of animal and farinaceous foods, and that the poor man makes a great mistake in paying a high price for beef or mutton, which he cannot well afford, when he could obtain the same amount of nourishment in oatmeal, maize, &c., for less than one fourth of what he now pays for animal food.

This is proved by chemical knowledge, and the well-known examples of the hardy races that used to inhabit the North of England and Scotland, the splendid races of Northern India, and the ancient Roman soldier and gladiator, all of whom were principally vegetarians.

His advice about alcohol should not be lost sight of, for he advises every young man and woman to be a total abstainer, since it is proved to be of no use in health, and may be injurious; and Dr. Richardson also asserts that it is positively hurtful.

With such evidence against alcohol it is no wonder that there is at present such a wide-spread feeling in favour of temperance.

What a marked difference there is now in the mortality in India, compared with the beginning of the century, during the Mahratta war for instance, when the annual death rate of our soldiers was 101 per 1000; and when one reads in the Wellington despatches of the period such orders as "Urge the gentlemen there to send forward 3000 or 4000 gallons of arrack," no surprise need be expressed at the high rate of mortality in those times. In fact, to quote Dr. Parkes' own words, "The immense disease-making of intemperance is appalling" even now.

In the last chapter he asks the question, "Will men follow the rules of health?" He answers it by asking—

"May we not receive it as a sure principle that when men are once convinced that a certain course will bring them a material good they will eventually pursue that course, and if once the principles of health can become implanted and taught to each generation, the tendency to follow the guidance of these principles will grow by transmission and inheritance?"

In conclusion, he holds out great hopes that men will follow those

rules, and by education, moral, intellectual, and physical, the health will be improved in proportion.

This little manual, which should be read and remembered by everybody, ends with a few simple sanitary hints for working men, as regards ventilation, cleanliness, water supply, food and drinks.

His work on 'Public Health' is, as we have said, of a more general character, and gives an outline of the various health acts now in force in England, and also of the points which are engaging or should engage the attention of our legislators and medical officers of health. He begins by showing the importance of forests, and the necessity of drainage in preventing ague and dysentery, both of which were common in certain parts of England at one time, and he regrets that there is at present no complete "Land Drainage Act." The importance of healthy sites for towns and villages is then referred to; but it frequently happens that other considerations than those of health, such as the conveniences for trade, vicinity of water supply, and strategical reasons become paramount.

Of the various kinds of roadway recommended for streets, Dr. Parkes, on the score of health, is in favour of wood and asphalt laid in a peculiar way; and now that such pavement is becoming general, it will be interesting whether and in what way it will influence public health in our large towns. We hear a great deal occasionally about the rights of citizens, the liberty of the people, &c., but there can be no question that the state in the broadest sense as possessing the highest degree of intelligence, and being the representative of the people, is quite justified in interfering with private enterprise and dishonest speculation.

Why should there not be Government supervision, for instance, in the building and arrangement of houses in town and country, so as to insure good, substantial, and well-ventilated houses being built? Surely it is ultimately to the advantage of the owner to have his houses fulfiling every sanitary condition which is not necessarily incompatible with architectural design; they would then be diligently sought after; but unfortunately by far the largest class of people have really no choice in the matter, and often must take whatever sort they can find, hence the necessity of some legislation.

Practical and useful hints are given as to the purity and amount of the water supply to towns, the various systems of conservancy, &c., and also as regards the various kinds of food and its adulterations. Alcohol and its effects on the masses of the people are again noticed at length, but as the State looks on its sale, and as it really is, a very important source of revenue, there will be great difficulty and opposition in restricting its use. However, he writes: "A remedy ought to be and must be found for this state of things or

else sanitary legislation will still present the absurd spectacle of raising up with one hand what it is smiting down with the other."

His opinions should be valuable on one of the great problems of the age, viz. the prevention of venereal diseases, and under this head he states that in some military and naval stations where the "Contagious Diseases Acts" are carried out the effects have been to lessen primary syphilis by one half, and to abate its virulence, and also that the Acts have a beneficial influence on the women, not only in curing but reclaiming them.

There is no doubt, if such be the case, that the Acts are beneficial; but, on the other hand, a very considerable number of persons deny that they have been so productive of good as has been stated, and moreover contend that prostitution and venereal diseases in general have very much increased in consequence of the Acts. The Acts are possibly wrong in principle, one-sided, as they do not apply to men as well as to women, and sometimes shamefully abused and frequently imperfectly carried out; but then a great and terrible contagious disease is increasing in our midst, and some measures to prevent its spreading must be adopted. What a pity it is that the opponents of these Acts do not give a little of their time, money, and talents towards improving the morals of the people, and bringing up the young in the way they should go, for we fear that there is no hope that prostitution will be lessened until men and women are made better.

A wide-spread movement is taking place in England for the total and unconditional repeal of the "Contagious Diseases Acts," and a similar movement has extended to the Continent, especially to France and Switzerland.

One of the best accounts that we have seen of prostitution considered in its relation to health, morality, and the laws of the various nations, is that of Dr. Mireur, Médecin-inspecteur du Dispensaire de Salubrité Publique of Marseilles.

In conclusion, we earnestly recommend the perusal of these two little works of Dr. Parkes, especially to our legislators, and all others who may wish to benefit themselves as well as their fellow men and women.

Vivisection.[1]—The professed object of Mr. Macilwain is to criticise the evidence given before the Royal Commission "on the practice of subjecting living animals to experiments for scientific purposes." At the same time it is obvious throughout that Mr. Macilwain has a strong bias towards the views of certain associa-

[1] 1. *Report of the Royal Commission on the Practice of subjecting Live Animals to Experiments for Scientific Purposes.* London, 1876.
2. *Vivisection.* By George Macilwain, F.R.C.S. London, 1877.

tions, or self-styled societies of anti-vivisectionists. The author's set purpose, however, is to show that, even where experiments seem to have been justified, a more certain result might have been arrived at by the slower process of awaiting pathological changes. But, apart from this, an animus reveals itself by the selection of certain cruel experiments recorded in the appendix, with the too evident object of arousing sensational excitement. The sting of this appendix is, however, removed by the perusal of the evidence contained in the Parliamentary Blue Book. For if there be any one fact brought out therein more prominently than another, it is that such atrocities as are described are not met with within the limits of the four seas, and are without parallel in the present-day investigations. In an experiment by M. Bouillaud, an opening was made into the forehead of a young dog, and a red-hot iron forced into each of the anterior lobes of the brain, &c., &c.[1] After some days, M. Bouillaud added, " I was obliged to kill it, as its irrepressible cries disturbed the whole neighbourhood." M. Brochel, " after inspiring strong aversion in a dog by plaguing it, and inflicting pain on it, first put out its eyes, and then destroyed its hearing by piercing the drum of the ear, and filling up the cavity with wax." This, Mr. Macilwain adds, was done to discover whether the animal would evince the same aversion as before. Majendie opened the body of a bitch with young to know if the mother, on seeing them in her dying state, would show parental feeling, which it appears she did by applying her tongue to them !

Mr. Macilwain fears that such narratives may divert the attention from the scientific bearing of the subject. The fear, we admit is by no means groundless, and furnishes the additional reason that it might have been wisdom not to have revived them. We have referred to them here in order to show the unwisdom of the course adopted in the reiteration of such painful narratives, since the non-professional mind fails to distinguish these horrors from the comparatively painless and wholly justifiable experiments upon living animals, made with the object and intention of conferring immediate benefit upon suffering humanity. Some benevolent but misguided people cannot bring to the consideration of this subject a calm judgment ; they allow their feelings and emotions to carry them off on the hue and cry of mere emotional sentimentality, confounding with barbarous vivisections, indispensable and cautiously conducted experimentation upon animals. That we are not overstating this view of the matter, let bear witness the balderdash that is circulated in handbills and broad sheets by the so-called anti-vivisectionists ! In these appeals, for which the sanction of religion and morality is pervertedly sought, there is a persistent and wilful misrepresentation in the use of the word VIVISECTION.

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[1] We forbear to quote the whole paragraph.

How this word has been misused may further be learned by a study of the evidence given before the Commission. The names of the greater proportion of the witnesses, taken together with the opinions expressed by them with regard to the intent and the modes of their experiments, afford sufficient guarantee that they were not disposed to be parties to wanton cruelty, or the infliction of needless suffering. The restrictions these witnesses would place upon experiments upon animals would preclude the infliction of torture, whilst they reluctantly assert the superior claims of human suffering for ultimate relief through those means.

Mr. Macilwain lends the weight of his professional reputation, and all the force of his pen, to the cause of a mere sentimentality, which puts forth the *petitio principii,* that life was never given to be experimented upon; that to experiment upon animals is to outweigh by the strong, the right of the defenceless; that it is the abandonment of mercy by those who expect mercy; that—

"Restriction sanctions the principle of vivisection, which is the admission of torture, under the inhuman arguments that the claims of humanity demand the sacrifice of living, quivering, mutilated animals: this necessarily suggests the hypothesis that mankind should stand prepared to be dissected alive for the superior rights of organizations next in ascending gradations, rising to the loftiest of created beings."

With such illogical and fanciful notions before him well might Mr. Macilwain fear lest "such narratives may divert the attention from the scientific bearing of the question." We opine that Mr. Macilwain is himself in this predicament when he takes upon himself to become the advocate and champion of those who boldly assert that experiments upon animals are wholly unnecessary, and who, in the face of the testimony of men of highest renown in science, unhesitatingly deny the accuracy of the statements on which are based the opinion that such experiments are justified by the benefits bestowed upon mankind. In asserting roundly that this mode of research is altogether fallacious, and a source of serious practical error, Mr. Macilwain seems to forget that he sets his word against that of such members of his own profession as Sir Thomas Watson, Sir George Burrows, Sir James Paget, Professors Sharpey, Humphry, Taylor, Rolleston, and others equally distinguished for endowments of head and heart.

The sum of Mr. Macilwain's counterblast amounts to this, that all the witnesses who favour experimentation are in error, whilst he and those who think with him are in the right. He supports this conclusion by long dissertations conveying his own views of the pathology of certain affections—dissertations the connection of which with the matter in hand we fail to see, or in which we can only detect a very remote or indirect relation thereto. Mr. Macil-

wain must pardon us if, without consenting to yield to him the palm of benevolence, we decline to concur in his special pleading, which would throw upon a large section of the profession the slur of wanton cruelty and indifference to suffering.

As corrective of the positive and one-sided conclusions of "anti-vivisectionists" we cite a few passages from the Report of the Royal Commissioners. It should be borne in mind that among these seven gentlemen are two of the most distinguished ornaments of physiological and medical science.

"The conviction has been arrived at," the reporters observe, "that no teaching of physical science is complete unless illustrated by practical instruction. Physiology in particular is now for the first time assuming the position of a separate science." The number of persons, however, systematically engaged in the performance of experiments in physiological laboratories does not appear to be more than from fifteen to twenty at the utmost, and in the hands of these the experiments are performed with every human precaution and means for the diminution or entire suppression of pain and suffering. The use of anæsthetics has enabled the experimenters to attain this most desired end. Previous to the discovery of anæsthetic agents there is no doubt but that much torture was inflicted upon animals, and at times even upon men; but this cannot be predicated of modern experiments, in which the induction of insensibility is the almost universal rule. Medicine, the reporters continue, rests upon the triple basis of clinical observations and pathological and physiological research; and they remark that experiments upon animals and men also have been coeval with the commencement of medical science. It is further shown in the report that while instances of cruelty in past time may be adduced, that yet a general sentiment of humanity pervades all classes of society, and has been strikingly manifested in the statements of the witnesses examined by the Commission. There is a general concurrence in the evidence of lecturers and teachers of medical schools that no infliction of pain not absolutely necessary is tolerated by the students, who would instantly resent either careless or deliberate cruelty if perpetrated, or even indifference to the sufferings of the subjects of experiment. The Secretary of the Society for the Prevention of Cruelty to Animals readily acknowledges that he does not know a single case of wanton cruelty, and that in general the English physiologists use anæsthetics where they can do so with safety to the experiment.

The Commissioners classify experiments upon animals under three different heads, viz. :

"(a) Operations.—These are performed for the purpose of examining, either for original research or for demonstration to students, the processes of life.

" (*b*) The administration of poisonous or dangerous drugs.—For the purpose of exhibiting the effect, or of discovering the cure, or for the purpose of assisting legal investigations.

" (*c*) The production of disease.—For the purpose of observing its progress, and discovering the means of preventing, mitigating, or curing the effects of the same or similar diseases in men or animals."

Abundant illustrative instances will doubtless occur to our readers under these several headings. Many of these are referred to by the reporters, *e. g.* Sir Charles Bell's discoveries, Harvey's experiments, Jenner's researches, &c., &c. One very forcible illustration is quoted :—

" Who," says Helmholtz, " when Galvani touched the muscles of a frog with different metals, and noticed their contraction, could have dreamt that all Europe would be traversed with wires flashing intelligence from Madrid to St. Petersburgh with the speed of lightning ?

" Had these investigations and experiments been abandoned on the ground that they promised no immediate practical result we should be ignorant of the most important and most interesting links between the various forces of nature. Whoever, in the pursuit of science, seeks after immediate practical utility may rest assured that he will generally seek in vain."

Mr. Macilwain makes this very apposite observation :—" I believe there is nothing which so successfully elicits the credulity of the public as bold assertions, especially if they have the advantage of not being easily intelligible. The very boldness of the assertion seems to produce a kind of senseless astonishment, and people fancy they must be true which they have not the power to examine." This ready credulity, as we all well know, is at the bottom of the success of the endless forms of quackery, and it is none the less applicable to the matter before us. Charges of wanton and cold-blooded cruelty are freely handed about, and instead of a deliberate judicial inquiry we are treated to the intemperate raving of an ill-regulated benevolence, and invited to an anti-vivisection crusade through sensational placards by which the walls of the metropolis are disfigured. At this moment there may be seen on advertising boards, &c., the foulest misrepresentation and exaggerations in the shape of huge drawings of " live " rabbits and " live " dogs, under the instruments of the experimentalist, and headed, " This is VIVISECTION !! " Most sincerely it is to be hoped that the author of the criticisms on the evidence contained in the Blue Book is not associated with the perpetrators of such scandalous and libellous outrage upon the members of a profession that has ever been foremost in all the benevolent movements of the past and present time, and to whom, despite the odium thus sought to be cast upon them, the distressed and the suffering look up for comfort and alleviation.

Turner's Introduction to Anatomy.[1]—The appearance of this second part of Professor Turner's 'Introduction to Human Anatomy' will be welcomed by every student of the science. It completes the work as originally planned for the new edition of the 'Encyclopædia Britannica,' now in course of publication. It presents the reader with an admirable outline of human anatomy, and is, in the strict sense of the word, an introduction to the science; as such, therefore, it is not the complete treatise on descriptive and regional anatomy, nor a handbook for the dissecting-room, such as a medical student must possess himself of, in order to qualify himself for examinations and for the exigencies of practice. Nevertheless, it is a volume that every student ought to have and to make himself master of, as preliminary and auxiliary to the more complete works which he must study.

Professor Turner has contrived to include, in a comparatively small compass, no mere superficial amount of descriptive anatomy, but a very substantial portion; this he has effected by terseness and lucidity of style, bringing to his aid the use of numerous diagrams, commendable as being for the most part original, and not mere copies of well-known and well-worn engravings.

This second part is occupied by chapters on the vascular system, on the larynx, on the respiratory system, on the organs of digestion, on the urinary and reproductive systems, and on the placenta. These chapters are supplemented by others descriptive of the general and minute structure of the different organs and tissues in which the author's own researches are brought under notice.

No individual can lay claim to greater fitness to produce an anatomical treatise than the author, wholly engaged as he is in the teaching of anatomy in the largest medical school in the United Kingdom, and also an assiduous worker in and contributor to anatomical science; consequently his readers will have the full conviction that he is a master of his subject, their only regret being, possibly, that he has not presented them with a more complete account of it. The scientific world would be, indeed, deeply indebted to Professor Turner for a more comprehensive treatise on anatomy. There is a scope for one constructed on a more enlarged plan than any anatomical work we have in this country, wherein anatomy should be presented to us not as a mere portion of technical knowledge addressed to medical students, but as a portion of biological science, wherein human structure should be viewed in relation to animal structure at large, and human anatomy treated not as a dislocated fragment of comparative anatomy.

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[1] *An Introduction to Human Anatomy.* By W. Turner, M.B. Part II, 1877. Edinburgh, pp. 504.

Reports on Leprosy.[1]—Since the subject of leprosy was last discussed at some length in the pages of this Journal, there have been several contributions to it, with an outline of the contents of which we should like to keep our readers acquainted.

Passing by an elaborate essay on the history of leprosy by Dr. Munro, which is appearing in the 'Edinburgh Medical Journal,' but which is not yet completed, the longest and most important reports are those whose titles appear at the foot of the page.

Of Dr. Vandyke Carter's two reports, the second, which we are glad to see was printed at the expense of the chiefs of Kattiawar, gives a detailed account of his tour through that province of Western India. This volume is essentially a supplement to his first report.

In both papers Dr. Carter shows indefatigable industry; in the first he gives clinical details of some cases running a comparatively acute course, which he observed in Bombay, and in the second a short history of each case which he examined in his tour through Kattiawar.

He thus sums up his results :—1. It has been shown that leprosy is frequent, sometimes very frequent in the part of Kattiawar under review, and to an extent scarcely anticipated. 2. The malady is not, however, uniformly distributed over the land, being most prevalent in the populous coast districts and westward; and its mode of dissemination is everywhere highly suggestive of communication by human agency. 3. So large a proportion too is recent disease, that the influence seems inevitable of an actual overspreading as it were of the pest; and besides the form of the disease is often severe. 4. There is absolutely no public provision for the leprous sick and poor. His main suggestion is, that a convenient refuge be offered to all vagrant lepers, whose further wanderings should then be interdicted, and that a similar asylum be open to the poorest class of peasantry whose compliance with the wishes of the state is to be insisted on; thirdly, that those who are willing to provide separate maintenance for their leprous sick, must insure the isolation proposed to be an efficient one. In short, he practically repeats his recommendation of compulsory segregation.

We have formerly observed that Dr. Carter, on his visit to Nor-

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[1] 1. *Reports on Leprosy.* (Second Series.) By H. Vandyke Carter, M.D. Published under the sanction of H.M. Secretary of State for India. London. 1876.

2. *Modern Indian Leprosy.* Being the report of a tour in Kattiawar. (Printed at the expense of the Chiefs of Kattiawar.) Bombay, 1876.

3. *Report on Leprosy in the North-western Provinces.* By C. Planck, Sanitary Commissioner N.W.P. October, 1876.

4. *Leprosy in India.* A Report by T. R. Lewis, M.B., and D. D. Cunningham, M.B., Special Assistants to the Sanitary Commission. Calcutta, 1877.

way, acquired a bias in favour of contagion, and doubts as to the extent of the influence of heredity.

Dr. Carter now says that individually he is unable to furnish any demonstration of the accuracy of the view of contagion; still he considers that he has furnished new collateral evidence of the transmission by man, and entertains the hope that perhaps some day affirmative facts respecting contagion will be elicited. In like manner, he says that heredity, as the exclusive agent in the propagation of this disease, hardly having been maintained by observation in Kattiawar, does not seem entitled to the position once occupied by it. As to the ætiology of the disease, Dr. Carter indicates a not very probable mode in which the system may be contaminated with leprous poison, the use of *chass* or whey, as sour or impure milk is known to be a suitable medium for the retention and conveyance of animal poisons. He seems to regard disordered digestion as one of the causes of leprosy. Dr. Carter is indisposed to believe that leprosy ever arises in a purely spontaneous manner.

Dr. Planck, the Sanitary Commissioner of the N.W. Provinces of Bengal, has furnished a very interesting report on the disease within that large area, within which he says that probably considerably more than 10,099 persons (the return given by the Census) are afflicted with leprosy. The following are his main conclusions: Leprosy in the north-west is known in all parts of it, although best known in Kumaon, Ghurwal, and Banda. It is so diffused that it is difficult to entertain the idea that local conditions have any influence as a cause of disease. But these, he admits, require to be studied more carefully. It is a disease of man specially as distinguished from woman, not peculiar to persons of any employment or religion, affecting in about equal proportions the well-to-do and the poor. It results, however, in reducing to beggary at least one third of those attacked.

As a rule leprosy assumes outward and hurtful appearance at the age of from twenty to fifty years, or after children have been born to those afflicted; lepers, or those who are to suffer from it, being as prolific as mankind in general, and living as long.

Its attacks result so little from contagion that only about 1 per cent. of 855 cases of cohabitation between a leprous husband and healthy wife, or leprous wife with healthy husband, resulted in showing leprosy of both husband and wife. It is probable that in some instances of leprosy of both man and wife there had been intermarriage of parties in both of whom leprosy was hereditary. The people do not practically believe in contagion. Leprosy is essentially a hereditary disease, as shown by the testimony of about 20 per cent. of the persons examined, and probably in far greater proportion, if the whole histories could have been made out.

There seem undoubtedly to be cases which arise in persons

remote from contagion and who belong to healthy families. The circumstances of these *de novo* cases require further study. Any measure of sequestration of lepers with a view to the eradication of the disease, such as is understood to be in force in Norway, is not likely to attain its object in India. Seeing that in the great majority of cases the disease has been transmitted to the coming generation before any accurate knowledge of its existence in the constitution can be attained to, it is not possible to sequestrate an apparently healthy man because his grandfather or father have been lepers. While Dr. Planck's report is somewhat general, and does not deal in the minute examination of cases, and embraces a vast extent of country, that of Drs. Cunningham and Lewis, after giving some statistics of the general prevalence of the disease in India and a valuable map of its distribution, confines itself chiefly to the small area of Kumaon in the Himalayas, and to a study of leprosy as it occurs in the leper asylum of that district. Their general convictions, though professedly not final, are these:

According to the census returns there are some 99,000 lepers under British rule, yielding a proportion of 54 lepers to every 1000 of the entire population, or 1 leper to every 1845 persons; but in some districts the proportion is vastly greater, there being in them as many as 1 leper to 384 persons.

This was about the proportion in the district of Kumaon, of the leper asylum of which district the 80 lepers were subjected to the closest scrutiny. Forty-nine proved to be cases in which anæsthesia was the predominating feature, twelve in which tubercles of the skin were most marked; in 15 cases those two conditions were so equally evident that they may be classed as mixed, and in four cases eruptions were the most pronounced feature.

The average age at which the onset of the disease was observed, was found to be between twenty-three and twenty-four years. There was, however, a range from three to sixty. The average duration of the disease was nearly fourteen years. The anæsthetic cases were the most chronic, the tubercular being shorter by nearly six years. The history of the asylums gave no support to the doctrine that leprosy is a contagious disease, but strong evidence to the contrary.

With reference to the possible influence of heredity in the propagation of leprosy, the facts elicited give forth no uncertain sound. Taking into consideration the prominent part undoubtedly played by heredity, and the fact that the disease but seldom manifests itself until after puberty, it is evident that any attempts at stamping it out by the segregation of leprous persons would prove wholly impracticable; for it would be necessary to segregate not only those suffering from the developed disease, but those also who were hereditarily disposed to it. How and by whom could the predisposition be determined?

There appears to be no dread of the amount of leprosy increasing in Kumaon, so far at least as it is increased by heredity, and this partly because the disease induces a tendency to sterility, and partly because the mortality among children of lepers appears to be abnormally high.

Before leaving Kumaon, we may observe that the authors of the report are inclined to attribute the excessive prevalence of leprosy partly to an influx from Nepal. Leprosy appears to be very common in Nepal, and also in the Trans-Himalayan regions and in the whole of the central area, where Schuyler recently describes it in the more western portions which he visited. Lepers are obliged to live by themselves, but he saw numbers of them near the Gate of Samarkand.[1] About China, Wong[2] reports that the great seat of leprosy is in the provinces of Canton and Fuking. There are fewer in the interior and in the northern provinces. In Canton there are believed to be over 10,000 lepers. Yet the people in the south are better off than those in the north, and eat more butchers' meat. The poor probably suffer most frequently, but the richer also suffer from it. Heredity is universally believed in. All this is interesting, for Chinese labourers carry leprosy with them to our colonies, as do the Indian coolies.

The question how far leprosy is indigenous, and how far introduced in some of the islands of the Pacific, and the behaviour of leprosy where introduced, are subjects on which we hope some light will soon be shed ; as yet little has been done.

Mr. Hogg, of Sydney, has written, November 5th, 1875, to the College of Physicians, that leprosy is unknown among the aborgines of New South Wales, but that leprosy has been spread beyond the colonial population ; that it is not uncommon in the Malenican Islands, where it is probably indigenous.

To our scanty information about Africa, where, however, we believe there is much leprosy, besides at the Cape and in Abyssinia, Mr. Cameron[3] adds a notice of a leprous district or village west of Lake Tanganyika. The leprous village is kept as much apart from the rest of the population as possible.

Besides visiting its sites near Jerusalem, Dr. Carter has given us, chiefly in his first report, an account of leprosy as he saw it in the islands of the Levant, where it still prevails in Scio, and especially in Crete; but we do not know that he has gathered anything throwing new light on the disease.

With its tendency to linger on in islands, it is not surprising that leprosy is still to be found in Sicily and in some of the Lipari

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[1] 'Turkestan,' by E. Schuyler, vol. i, p. 147.
[2] Abstract in Virchow and Hirsch, 'Jahresbericht,' Band i, Abth. 2, Berlin, 1876, s. 431.
[3] 'Across Africa,' vol. ii, p. 90.

islands Profeta[1] had seen 114 cases of leprosy, 80 men and 34 women. He says that there are only two out of the seven provinces of Sicily that furnished no leprosy; that leprosy does not prevail so much on the coast as inland; that it by no means attacks the poorest people only; that malaria is out of the question as a cause of it; that in three fourths of the cases heredity was proved; while he never met with any fact favouring the idea of contagion.

From St. Remo in the Rivièra we have a report one year more recent than Dr. Carter's visit to it. Mr. Thaon, of Nice,[2] describing a case of anæsthetic leprosy which is more frequent than the tubercular, says that the disease is dying out; that at St. Remo they used to have forty lepers in the asylum, sent from various places along the coast, and that now they have only six; and the managers have resolved to admit cases of ordinary skin disease into the leper hospital. It may be remembered that when the leper hospitals were closed in France it was found that there were few or no lepers remaining in them, their places having been taken by beggars and by patients suffering from skin diseases. This was also the case at Exeter as late as 1835. Thus, history repeats itself.

To this short summary we fear it must be added, that leprosy has of late years been more frequently observed in Europeans who have visited the East or West Indies. Distressing cases of this kind are every now and then appearing in London and in large continental cities. Some such cases have also occurred in New York in persons who have not visited the tropics. Unfortunately, in almost all these cases there is no satisfactory explanation to be had. In some there is a very strong suspicion of heredity, but there is an extreme unwillingness on the part of European patients to acknowledge it. It is not a little remarkable that we as yet have heard of no cases of leprosy among European soldiers in India. Its occurrence in officers is better known.

Reviewing the facts and opinions that have now been brought together, can it be said that they throw much light on the subject? We fear it cannot be said that they do.

As to the etiology of the disease, it would appear that diet, occupation, habit of life, social condition, have wonderfully little influence. One point only has been brought into strong relief in India—the extreme localisation of the disease. Possibly, some leprosy-inducing conditions may be detected in the specially affected localities. This would be an important aid to our ætiology. The evidence in favour of heredity is gaining strength. The fact of the early development of leprosy being exceptional, points to the

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[1] Abstract in Virchow and Hirsch, loc. cit.
[2] 'Nice Médical,' No. 3, 1876.

uselessness of compulsory segregation with the view of preventing hereditary transmission. The age at which the tendency to procreate is greatest is usually anterior to that at which the signs of leprosy are detected. The evidence, on the whole, leans towards the comparative sterility of lepers. There is no evidence of the contagiousness of the disease; most evidence seems to point the other way. Some few believe in a connection with syphilis.

On the treatment of the disease nothing new can be said. It is sometimes retarded, nay, sometimes arrested, by improved hygienic arrangements, by careful local treatment, by the use of alteratives or tonics, but there is no specific; and the hopes raised in the minds of some respecting the special efficacy of Gurjun oil have not been realised.

The 'Oriental Sore.'[1]—The authors commence by giving a large amount of statistics respecting the prevalence of sores and ulcers in various parts of India. But the classification in the tables which furnish the statistics is so rude and imperfect, that an examination of these returns does not lead to any conclusion of importance, bearing either on the Delhi sore as it is usually called, or on other forms of tropical ulcers and sores.

An accurate history of the various forms of ulcers which occur in different parts of India would be valuable for comparison with the ulcers that prevail in Cochin China, in the Mozambique, in New Caledonia, in Senegal, Guiana, in Fiji, and in many other tropical countries, some of them having analogies with the Delhi boil, and others running almost into the frambœsia or yaws.

But these statistics are of little use for any such purpose. The following is a summary of the results at which the authors have arrived respecting the Delhi boil, the special subject of their investigation :

There is no evidence of any parasitic agency in the production of the disease; and it appears probable that the deleterious effects are due to the chemical constituents of the drinking water of Delhi, which is of extreme hardness and is impregnated largely with salts.

With regard to the nature of the Delhi boil, they have no hesitation in saying that the disease is in no way distinguishable from one or other of the various forms of lupus. It may, however, be modified from its European prototype,[2] as is suggested by its local distribution, and they would, therefore, recommend that the disease should be called *Lupus endemicus*. Although most of the micro-

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[1] *The 'Oriental Sore' as observed in India.* A Report by T. R. LEWIS, M.B., and D. D. CUNNINGHAM, M.B., Special Assistants to the Sanitary Commission. Calcutta, 1877.

[2] Tilbury Fox thinks it may be a furunculus modified by locality, and Dr. Carter insists strongly on its parasitic character.

scopic appearances they have seen, are common to many cutaneous affections, it seems to be practically on them that they so confidently base their conclusions; and they have also been influenced by consulting the most recent works descriptive of the forms of disease known under the head of lupus. But some of these forms differ much in their nature from the ordinary lupus, so much so that their classification can scarcely be regarded as settled.

Drs. Lewis and Cunningham appear to have been hasty in their generalisations, founded chiefly on book knowledge, and we are not inclined to agree with them in regarding the Delhi boil as a form of lupus.

We are not aware that any form of lupus is especially prevalent in particular places at particular seasons; whereas this is a characteristic of what has been called the Oriental Sore.

Common lupus is a disease chiefly of the young. It very rarely commences after the age of twenty. The Delhi boil attacks at any age, not, indeed, sparing the young. Lupus has been considered in Europe sometimes to have a connection with syphilis or with scrofula. This is never the case with the Delhi boil.

Every described form of lupus, even the erythematous, the most diffused one, is more chronic than the Indian sore. Lupus is much more destructive of tissues and is much more difficult to cure. Its ordinary forms attack specially the nose, and ears, and cheeks; probably not oftener than once in five cases the extremities, while the sore appears as readily on the arms or hands or legs, as on the face. Such are some of the obvious differences.

Messrs. Lewis and Cunningham are inclined to attribute the disease to the Delhi water; but considering that an affection closely similar, if not identical, prevails in a variety of places differing much from each other in soil, climate, and water, such as Biskara, Crete, Aleppo, Bagdad, and Scinde, we can scarcely be convinced that the inhabitants of all these places suffer through the water, simply by their general assertion, that in many of these places the water is notoriously brackish, nor do calcareous waters elsewhere produce boils.

We cannot say that this report appears to us to prove either that the Delhi sore is lupus, or that its main cause is the badness of the Delhi water; nor does it add much to the accounts of the disease collected by Drs. Tilbury Fox and Farquhar, materially aided by Dr. Carter, and illustrated by his drawings.

We would close this notice by calling attention to one or two facts respecting the occurrence of the sore in the lower animals, not indeed novel, but which are worthy of further investigation.

The dogs in Delhi appear to get the sore occasionally, but almost exclusively on the nose, as is also the case in Algiers.

The historians of the Biskara bouton, describe a disease in horses, which they believe to be a form of the bouton. A similar disease is well-known in horses in the East, and we believe in parts of

India where the Scinde or Delhi sore is unknown. The disease is characterised by large fleshy vegetations on the trunk and the extremities, and it is worth observing that the French have described in Algiers one form of the bouton which, from its strawberry-like granulations, resembles yaws.

It is commonly said that one attack of the Indian boil gives immunity against subsequent ones in man. This is certainly not the case with the disease in horses now alluded to. Though, with care a healing of the sores may usually be effected, they are tolerably sure to occur next rainy season.

The Vivisection Question Popularly Discussed.[1]—To Dr. Dickson the thanks of all interested in the question of vivisection are due for his excellent translation of the recent pamphlet on this subject by Dr. Hermann, the well-known physiologist of Zurich. This latter work has special interest in its being a contribution from the hands of an intelligent foreigner, and of one who writes rather from a strictly scientific than from a medical and professional stand-point. During the recent agitation in this country, the attack of the anti-vivisectionists was, in consequence of the conditions of physiological teaching here, directed mainly against the medical profession, and the defence consisted for the most part in arguments concerning the relation of physiological experiments to practical medicine and the treatment of human suffering and disease. Both Dr. Hermann and Dr. Dickson take their readers out of this region of utility and attempt to justify vivisection on the ground of the claims of just science. The translator holds that " before we can take a right view of any question relating to science, we must recognise that she must be pursued not merely where we see a prospect of some special application of knowledge, but even where we have nothing to encourage us with general faith in the elevating power of all knowledge." Dr. Hermann, himself, speaks plainly on this point, and gives it as his opinion that " the advancement of our knowledge and not practical utility to medicine, is the true and straightforward object of all vivisection. We fail not, however, to find in the course of the argument full recognition of the utility of vivisection to medicine, all branches of which, it is stated, avail themselves directly of experiments on living animals, and do so with infinite advantage.

Dr. Hermann argues that experiment is an essential condition of the advance of the natural sciences. Though it is not every physiological experiment that necessitates interference with life, or even injury to the creature, still the numberless minute details as well as the fundamental facts of modern physiology can be

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[1] *The Vivisection Question Popularly Discussed.* By Dr. L. HERMANN. Translated and edited by Dr. ARCHIBALD DICKSON. Pp. 59.

apprehended only by vivisection. Physiologists, it is asserted, are still at work on the further examination of the mechanism of the circulation, and of the influence of the nervous system on the maintenance and regulation of this arrangement, an influence which would never have been understood or even noticed without numerous vivisections. The anti-vivisectionists who deny all merit to research by means of experiments on animals are assured by those who grant that vivisection, though no longer needed, has done service. Scientific vivisection, Dr. Hermann contends, is as justifiable as the killing of animals for food, the destruction of noxious animals, the mutilation of animals for cattle-breeders' purposes, or even for mere fancy, and their torture and destruction through love of amusement. It cannot be proved that science has been guilty of various excesses in cruelty. In the vast majority of vivisections the animal is, by very efficient anæsthetics, wholly spared suffering. It may be assumed that the sensations of pain diminish in intensity as we descend in the scale of animals, and it may be doubted whether all animal organisms are endowed with the power of feeling pain. There is, moreover, the psychological consideration that in most animals there can be no expectation of pain or anxiety with regard to future suffering.

We meet in this pamphlet with vigorous opposition to the proposal of abolishing vivisection as a means of instruction, and of banishing it from the lecture room. In the fears of Dr. Hampton and other British teachers, Dr. Hermann can see nothing but bad and ill-considered testimony to the character of students on this side the channel. It would be impossible, it is held, in academical education—*not* in primary schools—to banish experiments on living animals from physiological instruction without degrading it. The following brief extracts will indicate clearly the author's views on this point :

"The student that has never, by vivisection, looked into the innermost machinary of animal life, will always remain a bungler in medicine."

"The experiments concerned (in lectures) are just of as remarkable utility, perhaps even of more utility, than the individual experiments of actual research."

Dr. Hermann objects as strongly to the proposals that vivisection should be confined to recognised physiological laboratories, and that only teachers of physiology should have the right of performing such experiments.

Dr. Hermann concludes with an expression of his views on the agitation against vivisection in this country, and with an elaborate criticism of the different sections of the recent Act. These remarks show that the author possesses full knowledge of our political and moral conditions, so that the force of his judgment is not weakened,

as is so often the case with foreign critics, by a grotesque and self-complacent ignorance. The author's criticism, though vigorous and unsparing, is not, in our opinion, unjust; and in his defence of science, he seems to recognise fully the equal claims of justice and humanity.

The Treatment of Spina Bifida.[1]—Dr. James Morton holds with the majority of surgeons that injection is the most promising mode of arriving at the radical cure of spina bifida, and in accord with Velpeau and with Brainard of Chicago, regards iodine as the most suitable active agent for the injected fluid. Novelty, however, is claimed for the method described in this book, as the author uses as an injection, not a simple solution of iodine or a combination of iodine and iodide of potassium, but a fluid called iodo-glycerine solution, so named from its components, which are ten grains of iodine and thirty grains of iodide of potassium, dissolved in an ounce of glycerine. It was thought that as this fluid is less diffusible than either a spirituous or watery solution, it would be found less likely to permeate the cerebro-spinal fluid with rapidity, and so to cause shock or bring on convulsions. The injection of the iodo-glycerine, solution in order to be successful, must be practised under certain precautions, the most important of which is, in the opinion of Dr. Morton, the prevention of the continuous loss of the subarachnoid or cerebro-spinal fluid.

The results of this method, as shown in this book by the reports of fifteen cases treated by the author and by other surgeons, appear to be most satisfactory and certainly far surpass those obtained by any previous plan of treatment. Of the seven cases treated by Brainard before the publication of his paper in 1861, in these only was there a permanent and complete recovery. Dr. Morton states that of the fifteen cases treated by his method, twelve were successful, and three fatal, and that all his own lumber cases have hitherto been fortunate. In the operative treatment of spina bifida some care must of course be taken in the selection of cases. Some cases, as the author points out, are so complicated by other defects, as paralysis, hydrocephalus, &c., as to be hopeless. In subjects who have no paralysis either of limbs or sphenatus, and no deformity of importance, and who, apart from the presence of the tumour constituting a spina bifida, ought to be sound, this new method of treatment may, according to Dr. Moore, be undertaken and recommended, in lumbar cases at least, with very little fear of an unfavorable result.

It is to be regretted that those engaged in the preparation of this useful and, in other respects very creditable book, have received so little help from the draughtsman.

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[1] *The Treatment of Spina Bifida by a New Method.* By JAMES MORTON, M.D., Professor of Materia Medica, Anderson's University, and Surgeon and Clinical Lecturer on Surgery in the Glasgow Royal Infirmary. Glasgow, 1877.

Gant on Bladder Diseases.[1]—This book consists of Mr. Gant's monograph on irritable bladder, with a few chapters from the same author's 'Science and Practice of Surgery' added to it, so that it has now become a work on urinary diseases in general.   Mr. Gant gives as his reason for thus enlarging his former treatise that at the present time, and in fact, since Mr. Coulson's book on diseases of the bladder and prostate has been out of print, now ten years ago, no standard work on this subject has existed in British medical literature, and that during that time the state of pathology and surgery in this department has been " greatly altered and extended."   Mr. Gant seems strangely to have forgotten the existence of the works of Sir Henry Thompson.

The work commences with an anatomical introduction illustrated by some borrowed drawings.   As, however, this part contains nothing that cannot just as well be found in an ordinary text-book of anatomy, it requires no notice.   The first chapter is devoted to functional disorders.   The exact definition of functional disease is never easy, most men of science, we believe, considering the term merely a cloak for our ignorance concerning the structural change of which the altered function is a symptom.   We are not much aided by the opening sentences of this chapter: " I scarcely need observe that pathology recognizes two forms of disease—derangements of the function and alterations of structure, both forms of deviation being estimated by comparison with a presumed standard of health.   Alterations of structure, including those of physical character and chemical composition, are together represented by pathological anatomy, while pathology proper is thus restricted to disorders of function.   Such, then, is the general nature of pathology, and such its more limited and usual signification."   We find the author includes paralysis, engorgement and overflow of the bladder, and retention of urine under " functional disorders ;" and the treatment of these various conditions is briefly indicated, so briefly that we fear the reader would derive but little benefit from it.   More than one-third of the book is composed of the chapters taken from the ' Science and Practice of Surgery,' and these we need not notice.   The subjects included will be found in every student's text-book, treated of as fully as they are here, and much in the same way.   The last 130 pages—nearly another third of the work—is devoted to urinary diseases, deposits, and calculi.   Mr. Gant employs throughout the old notation in giving the formulæ of the various chemical components of the urine.   As a sample of how

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[1] *Diseases of the Bladder, Prostate Gland, and Urethra; including a Practical View of Urinary Diseases, Deposits, and Calculi ; being the Fourth Edition of the ' Irritable Bladder,' revised and much enlarged.*   By FREDERICK JAMES GANT, F.R.C.S.   London, 1876.

that part of the work is executed we will give a brief extract of the
information to be found concerning albumen in the urine.   A table
of the diseases with which it is associated is first given, commencing,
of course, with Bright's disease, acute and chronic.   Then follows
a long list of ailments, including subacute rheumatism, diarrhœa,
bronchitis, peritonitis, and paralysis.   Beyond the enumeration
of these diseases in the table nothing is said as to the conditions
under which albuminuria may coexist with them,   Bright's disease
is, however, treated of at greater length.   We are informed that
structural changes in the kidney are the immediate cause of the
symptoms.   These changes are congestion and its consequences,
which has been named by Dr. George Johnson acute desquamative
nephritis ; degeneration—fatty or waxy, which has been named
chronic non-desquamative nephritis ; and lastly, "partial absorption
and contraction, producing a small, firm, remnant kidney, having
an irregular puckered surface and granular aspect when the adherent
capsule is withdrawn—granular degeneration."   This " condition
might be termed the atrophied or remnant kidney."   These are all
included under the name Bright's disease ; and Mr. Gant seems to
consider it an open question whether they are all stages of one
disease or independent forms.   The symptoms, he tells us, are
essentially the same.   " The blood and urine have, in respect to
each of their prominent constituents—albumen and urea—changed
places.   While, therefore, the nutrition of the body is undermined
by the constant abstraction of the one, the system, through the
blood, is poisoned by the retention of the other."   The symptoms
of uræmia and of " febrile oppression " (whatever they may be) set
in.   The urea in the blood causes meningitis, pleurisy, irritation of
the gastro-intestinal mucous membrane, &c.   The retention of
water in the blood causes dropsy.   Then comes a paragraph passing
the wit of man to understand.   " The essential symptoms of
Bright's disease of the kidneys are connected also with the altera-
tions of structure which occur in consequence, or perhaps inde-
pendently, of congestion ; but the albumen returns to the blood,
and the water, about proportionately, to the urine, the urea and
other solid urinary constituents being still retained in a progres-
sively greater proportion in the blood, thus reducing the urine to
mere discharge of water, and often in greater quantity."   This is a
fair summary of what is said of Bright's disease, and anything more
useless, inaccurate, and unscientific, it is difficult to conceive.   Under
physical characters of the urine in Bright's disease we are told
that the colour is smoky brown, quantity small, and specific gravity
low, but subsequently the urine becomes pale and opalescent, specific
gravity falls perhaps to 1004, there is much less albumen, and the
quantity is greatly increased.   No doubt the first part is true of
acute Bright's disease, and the last of some form of the chronic

disease ; but with our present knowledge what can be the possible use of such muddled scraps of information ? Any well-educated fourth-year student possesses more accurate and extensive informa- tion. After five lines on casts the author goes on to chemical tests, under which we find the usual reactions briefly given. The well- known fallacy caused by the addition of a single drop of nitric acid to albuminous urine before boiling is explained by Mr. Gant by supposing that a nitrate of albumen is formed which is uncoagulable by heat, and he states that the condition can only occur when " just so much acid be added to the albuminous urine as shall combine with all the albumen present and form this nitrate. Nitrate of albumen being insoluble in nitric acid appears when more acid is added." We suppose this is intended to be a representation of the views of Dr. Bence Jones, although his name is not mentioned. Now, we believe that Dr. Jones's explanation of this condition was that the nitrate of albumen which he supposed to be formed was soluble in dilute nitric acid, but insoluble in a solution of moderate strength. The accident of adding the exact equivalent of nitric acid to form a nitrate of albumen evidently could not occur once in a thousand times : whereas this peculiar reaction can be obtained with the greatest ease in any highly albuminous urine. Dr. Beale's explanation that it is due to the liberation of phosphoric acid is not mentioned.

We believe the small portion that we have thus examined forms a fair sample of the whole of this section of Mr. Gant's book, and, we fear, of a great deal of the other chapters as well. We cannot see what purpose the book is to serve. A great part is avowedly taken from a text-book intended for students, and is consequently, we should think, hardly adapted to a special treatise on a single group of diseases. The work is essentially theoretical in character, scarcely a case being mentioned from one end to the other. We can but look upon it as an encumberance to medical literature, and we think, if Mr. Gant is well advised he will let the book rest where it is, supposing the present edition sells out, or will re-write it, introducing more original matter and more clinical illustration, which we feel sure his ample experience would enable him to do.

Hilton on Rest and Pain.[1]—The first edition of this well-known work appeared in 1863, and was soon out of print and Mr. Hilton, tells us that it was his intention at that time to have " enlarged the volume by additional matter derived from other and different surgical diseases, but all having the same purpose in view—namely, to show

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[1] *On Rest and Pain.* By JOHN HILTON, F.R.S., F.R.C.S., Surgeon-Extra- ordinary to Her Majesty the Queen, Consulting Surgeon to Guy's Hospital, &c. &c. Edited by W. H. JACOBSON, F.R.C.S., Assistant-Surgeon to Guy's Hospital. Second Edition. London, 1877.

how largely we are indebted for our professional success in surgical practice to the recuperative power of Nature ;" but insuperable difficulties presented themselves, and at last it has been allowed to appear again without any material change or addition.   Mr. Jacobson has not altered the original text to any extent, but has confined himself chiefly to the addition of a few interesting notes and some new drawings.   His work is, however, admirably done, and we have no doubt this present edition will meet with a success equal to that of the first. There can be no doubt that the value of rest in the treatment of disease is more fully recognised at the present time than it was fourteen years ago, and that all surgeons are coming more and more to trust to the vis medicatrix naturæ, and to recognise more fully the fact, that in the vast majority of surgical cases there is a natural tendency to cure which cannot be increased by any artificial means, but which can be greatly helped by removing obstructing causes and warding off complications.   This is observable, not only in surgery, but in medicine also, the physician as well as the surgeon having come to recognise how little he can really do beyond aiding nature in the cure she is effecting.

The term "rest" as employed by Mr. Hilton, has a rather wide significance.   He does not mean only mechanical but physiological rest—not only absence of movement, but cessation of function. The use of the word may at times seem strained, as when he speaks of the object of the operation for hernia being to give rest to the strangulated gut, and the opening of an abscess giving rest to its walls, but it is always quite clear what he means by it.   In the majority of cases the treatment by rest involves letting the patient alone as much as possible, the avoidance of violent counter-irritants and such remedies, and one of the most valuable features of the book is the protest it enters against "meddlesome surgery."

The value of pain from a diagnostic point of view occupies a large portion of the work, and here Mr. Hilton does invaluable service by pointing out the vast importance of an accurate knowledge of anatomy, showing that the distribution of the cutaneous nerves, the nervous supply of joints and muscles, the communications between one nerve and another, although so often looked upon by the student, and even by the practitioner, as merely useless details, often serve as the only means of guiding the surgeon to a correct diagnosis.

The work is so well known that it is needless for us to review it at any length.   Although the text is scarcely altered in the present edition, and consequently here and there the phraseology may seem antiquated to the modern student, and possibly be at variance with recent pathological doctrines, the greater part of the work can never grow old, being founded on clinical observation and the application of the undisputed facts of anatomy to actual practice. It is a work

which every one should read, and when we say this we are not, as it were, setting a task to the reader, for the style of the writing is such that it reads as pleasantly as a novel, the interest being constantly kept up by the descriptions of actual cases from the author's practice, graphically told, always instructive, and often amusing.

Cleland on Dissection.[1]—Dr. Cleland, in the preface, thus explains the object of this small work—" The following pages have not been written with the view of interfering with any works of anatomical demonstrations or systemic anatomy, already in existence, but are intended to supplement such books. The student ought to study the " subject " in the dissecting-room and his books at home, and he ought never to be encouraged in the too common error of looking on his dissections as mere illustrations for the statements of the text-book. Yet it is necessary that he should be guided in his dissections, both that he may make them in such a manner as to display the anatomy to the greatest advantage, and that he may recognise the structures by the names by which they are known. To these two purposes of guidance these pages are exclusively devoted. . . . . The author's effort has been, by the omission of all description, to give to the student who seeks to learn, scalpel in hand, a fuller assistance in the practical difficulties which he is likely to meet with than could be afforded in a work devoted to description either in the systemic form of arrangement or that of demonstrations. It is expected of the student that, before coming to the dissecting-room, he should glance over, each evening, a portion of work in this book, and consult his descriptive manual sufficiently to have an intelligent idea of what he is to exhibit on the subject next day. Taking with him this book to the dissecting-room, he will with its aid cultivate his manipulative powers and his observation ; and on his return home he will recur to his text-book, and find how far his own observations agree with those of more experienced men." We may at once state that the directions are clear and accurate, and in every way admirably adapted to the purpose for which they are intended, but it may be doubted whether the system of teaching advocated by Dr. Cleland is that calculated to enable the student most readily to acquire a sound knowledge of anatomy. Every teacher of anatomy will agree with the author when he says that the student should study the " subject " in the dissecting-room ; but it is not quite so clear that he should use his books only at home. Suppose a student to follow Dr. Cleland's plan, what would be the result ? Having

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[1] *A Directory for the Dissection of the Human Body.* By JOHN CLELAND, M.D., F.R.S., Professor of Anatomy and Physiology in Queen's University, London, 1876.

obtained, by a process of pure cram, an indistinct notion of the
region he is about to dissect, he would proceed with no further aid
than is afforded by these directions, perfectly free from description,
to attempt the dissection of his part.   In all probability he would
commit many errors, cut away many structures, and fail to observe
many important details.   He is then advised to make notes of his
imperfect observations and to go home and see how far the de-
scriptive works agree with him.   Supposing he finds that his
observations are greatly at variance with those of the authors of
his text-books, what is he to do?   In all probability it will be
impossible for him to refer again to his part as in the process of
dissection he will have cut away many structures and displaced
others.   He must, therefore, be content to accept the statements
of the text-book in opposition to his own ideas, or to adhere to
his own views in spite of the immense probability that they are
inaccurate.   The one would involve the acquisition of anatomical
knowledge by a process of pure cram, and the other a degree of
self-sufficiency, which we hope is rare amongst students of anatomy.
It is possible the student might be able, by studying the dissections
of his fellows, to correct his errors of observations of the day
before ; but this would, we fear, be more conducive to wandering
about and gossiping, the two greatest snares of the student in the
dissecting-room, than to diligent study.   Moreover, we firmly
believe that the student derives immense benefit from reading the
description with the part actually before him ; not merely using
the dissection as an illustration for the statements of the text-book,
but actually verifying every statement as he reads it, and then, if
he is unable to agree, make a note to that effect in the margin of
his book.   After he has thus read over the dissection a few times
with the part actually before him, he is able to derive some benefit
from the process of reading at home, as the picture of the dissection
will rise before his mind's eye.   It has taken hundreds of years,
and the dissection of thousands of bodies, to arrive at the present
accuracy of detail which characterises our best manuals of dissec-
tion, and how can we expect a student to attain the same in the
comparatively short time allotted to the study of practical anatomy,
unless he avail himself to the fullest possible extent of the labours
of those who have gone before him ?

We believe, therefore, that although theoretically it is very ad-
vantageous for a student to cultivate his powers of observation by
attempting to acquire a knowledge of all the minute details of
anatomy by a process of discovery, practically he would fail to
observe a vast number of important facts, which he could only
acquire afterwards by artificially learning them from books or
plates, and that his knowledge of anatomy would lose more than
his faculty of observation would gain.

In his advice to dissectors Dr. Cleland does not mention the great advantage a student derives from making drawings of his own dissections. However rude and inartistic they may be, nothing serves so well to recall dissections to the mind as drawings actually made by the student from his own parts. The accurate and prolonged study of the part, required to make even a rude sketch, impresses it upon the mind in a way that nothing else can. Nor do we think the author sufficiently impresses upon the reader the necessity of cleaning a part perfectly. The picture left upon the mind's eye by a perfectly clean dissection is sharp and distinct, like that of a view seen on a bright clear day, whilst that left by one in which the various structures are half concealed by fragments of fat and fascia, although the outline of every one may be visible, is like the impression left by a view seen through a haze or mist.

The author states that his object has been to give a "fuller assistance in the practical difficulties which the student is likely to meet with, than could be afforded in a work devoted to description, either in the systemic form of arrangement or that of demonstrations." We do not find, however, that the directions here given are fuller than those to be found in at least one well-known work in the form of demonstrations. Yet they are so good, clear, and accurate, that we regret that our firm belief in the value of reading descriptive anatomy in presence of the dissection itself prevents our recommending this work to the student.

Heath's Operative Surgery.[1]—There is probably in this country no man alive at present who has a better right than Mr. Heath to take the responsibility of publishing a work of this character.

For nearly twenty years he taught operative surgery, or at least a selection of the operations of surgery on the dead body. He has also for many years operated frequently, brilliantly and successfully on the living, and has contributed his share to current surgical literature.

Now that he finds his position in his hospital is elevated above the pleasant drudgery of teaching relays of students to operate on the dead body, which, after all, *is* a little apt to pall, after the repetition of many sessions, he is leaving as a legacy to his successors and the profession an expression of his opinion as to the best procedure for many of the capital operations of surgery.

Here we may remark that perhaps it is as well that by the arrangements of medical schools the teachers of operative surgery should be often changed, for if not, the tendency is that the wearied teacher, tired of his routine drill, either becomes a lifeless

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[1] *A Course of Operative Surgery, with Plates drawn from Nature,* by M. LEVEILLE, and coloured by hand under his direction. By CHRISTOPHER HEATH. F.R.C.S. London. Parts I, II, III and IV, 1876-7.

machine giving the same old story without zest or life, or, still more dangerous to the pupils, seeks variety for himself by making his puzzled scholars' practice operations new, strange, and varied. Though the student of history may succeed in unearthing, perhaps, twenty-five different methods of amputating at the hip joint, there is really only one best method of taking off a finger, and this, drudgery though it may seem, must be shown year after year, day after day, till the class know it.

It is more than probable that Mr. Heath's pictures are of those operations that he himself has found most valuable in practice and most easy to teach.

We find, in his first part, that the first plate contains some clear diagrammatic pictures of operations on the eyeball and eyelids, Graefe's operation for cataract being the one figured. The remaining three plates of this part contain the ligatures of the arteries of the upper extremity and the operation of excision of the mamma. The ligatures are admirably described; the pictures are so clear and well dissected as to be misleading to the young surgeon, who wants to know what he will really see, though instructive to the student, who merely wants to cram and impress his memory through the eye.

The incision for ligature of subclavian will be too small in many cases if it is made only to extend from sterno-mastoid to trapezius; it should often overlap the edge of each of these muscles, and sometimes be supplemented by a small incision along the posterior border of the sterno-mastoid.

One dictum in the operation of excision of the mamma we must dissent from most emphatically. Mr. Heath says, " when diseased axillary glands exist in connection with mammary disease they are close beneath the pectoral muscle, and not in the neighbourhood of the axillary vessels, unless the disease is very extensive" (p. 25).

We have found the opposite of this often, and believe that in every case the surgeon must be prepared to face the difficulty of removing glands in *close* contact with the axillary vessels, and that to do this safely and thoroughly he may often have to divide a large portion of the sternal division of the great pectoral muscle.

Part II contains ligatures of head and neck; the carotid is admirably described, short, precise, and accurate. Mr. Heath brings out the doctrine, only lately fairly recognised, that the whole course of the common carotid is overlapped by the edge of the sterno-mastoid, and as a consequence of this doctrine he makes his incision along the edge of that muscle.

In describing the operation for ligature of posterior tibial artery Mr. Heath selects the one along the edge of the tibia, and does not allude to the method between the heads of gastrocnemius.

Tracheotomy is given in Part III. Mr. Heath prefers the upper

operation (above the isthmus) as easier and more satisfactory—easier certainly, more satisfactory for the surgeon, but, we believe, in many cases, less safe for the patient.

The operations on jaws teach the student much, the surgeon little. For the upper jaw Mr. Heath, rightly we believe, recommends the central lip incision to be carried upwards as far as necessary.

Lithotomy and colotomy are beautifully drawn and excellently described, as also are castration, amputation of penis, and operations for phymosis; the latter are about the most perfect diagrams of any operations we have ever seen. The remainder of Part IV contains the amputations of the upper extremity; skin flaps, with circular division of the muscles, seems to be the key-note of this excellent description.

The work is an admirable one. The illustrations are beautifully executed and lavishly coloured. It does not profess to give historical details nor describe varieties of operations, but gives what it proposes to give—a detailed account of what Mr. Heath considers a good series of operations on the dead or living body.

Boudant on Mineral Waters of Mont-Dore.[1]—This is an excellent book and a valuable contribution to our knowledge of the treatment of pulmonic affections by mineral waters aided by mountain air. Although the French had long sent that class of affections to altitudes such as Eaux-Bonnes 2300 feet, or Mont-Dore 3300 feet above the sea, and the Spaniards had sent their cases to Panticosa, a height of 5000 feet, yet the proposal to treat pulmonic patients at Görbersdorf, and still more at Davos, that is, at heights of from 1700 to 5000 feet, came on the English and Germans, especially on the latter, as an entire surprise. The special value of Dr. Boudant's work consists in its being the result of twenty years' clinical intelligent observation, and in his cases being given in detail. Although various other ailments are treated at Mont Dore besides pulmonary ones, yet their predominance is borne witness to by the fact, that of the clinical observations 300 pages are devoted to the air passages, and only sixty to other subjects.

Dr. Boudant is naturally influenced by the current medical opinions of France, and he attrbutes the virtues of his waters to their operation on the various diatheses. Those on which their influence is most marked are the arthritic, the dartrous, the scrofulous, the secondary syphilides; it is less marked in the tubercular diathesis; it modifies chloro-anæmic and lymphatic diatheses, although Dr. Boudant does not admit that these diatheses are independent

entities as the others are. Some of his remarks on phthisis are well worth quoting. Phthisis consequent on pneumonia is scarcely ever preceded by hæmoptysis or laryngeal phthisis.

When cases of pneumonic phthisis are not complicated by tubercular diathesis, the waters of Mont-Dore are very efficacious, while in true tubercular disease it is only occasionally and exceptionally that they are of use. The caseous form of pneumonic phthisis is the most dangerous and also the most common.

When the waters do not cure, they at least have the advantage of calming the cough, of dispelling the evening febrile exacerbation, of stopping the morning perspiration, of favoring the conditions of nutrition, and of opposing colliquative action. They very often retard the progress of a case, which is obviously a clear gain, especially in those who are approaching to middle age, when there is some hope of the injurious constitutional action ceasing.

The conclusions respecting asthma seem to us judicious. The waters of Mont-Dore are very useful in nervous asthma if the case comes early under treatment, or is connected with an asthmatic herpetic or hæmorrhoidal diathises. They are very advantageous in bronchial or catarrhal asthma.

All emphysematous cases are greatly soothed, and some of them are cured, especially if the patients are young and the malady is recent.

'It is more difficult to procure absorption of any œdema of the lung; indeed it is rare, but the health of the patients improves, and they are able to struggle against it longer.

Cardiac asthma requires very careful treatment, but with certain precautions some benefit is derived; if some few are cured, the dyspnœa must have been dependent upon a diathesis which has been counteracted. Dyspeptic asthma is usually cured, but of course precautions are required to prevent a relapse.

We have not space to offer any opinions of Dr. Boudant on the treatment of neuralgia, rheumatism, and gout, but we observe that he gives some detailed cures of very obstinate cases of diarrhœa at Mont-Dore.

The work contains a full account of Mont-Dore and its climate, of its several sources, and of the various arrangements for baths and drinks, embracing the pulverisation of the water, the inhalation of its vapours, gargling the throat, and nasal irrigations.

Study on the Definite Results of Amputation.[1]—This pamphlet contains much information, compiled from various sources, mainly French, on certain points of interest connected with the subject of amputations. Dr. Viard deals in the first place with the question

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[1] *Etude sur des Résultats Definitifs des Amputations.* Par H. VIARD. J. B. BAILLIÉRE ET FILS. Paris, 1877. Pp. 114.

of the influence of diatheses and pathological and physiological conditions amputations and other surgical traumatisms.   Here, as in other parts of this work, we meet with full proof that the author has studied with much industry the surgical literature of his own country.   No allusion, however, is to be met with to the work that has been done in this direction by English surgeons, especially by Sir James Paget and Mr. Callender.   The second chapter is devoted to the subjects of anæsthesia and the prevention of hæmorrhage during amputations, and the concluding portions of the work deal with the comparative merits of the different methods of dressing.

The flap operation, M. Viard thinks, gives the best permanent results, but has no influence in favoring or preventing the development of secondary conicity in the stump.   Cicatrization of the stump is believed to take place more rapidly after amputations for injury than after amputations for disease.   Antiseptic dressings ought to be preferred because they favour immediate union; they have no influence, however, on the remote results of the amputation.

These conclusions have been based on fifty cases of amputation, brief records of which are given at the end of the pamphlet.

Lectures on the Physical Diagnosis of Diseases of the Heart.—We owe Dr. Sansom many apologies for not having accorded an earlier notice to his really valuable little book.   When authors strive one against the other to be diffuse and obscure, and try to hide what they have to teach beneath a huge and cumbrous structure of words—when writers on heart diseases find a thousand octavo pages too small a space into which to compress their subject—and when a monograph on pericarditis exceeds in size the books which taught our fathers the whole science of medicine, it is refreshing to encounter a work which is concise, clear, and to the point.   Dr. Sansom writes as only a man who is a thorough master of his subject can write, and he gives abundant proof that his reading has been as extensive as his practical experience.   He rides no hobbies and airs no crotchets, but is content to give us as shortly as possible all the well-established facts connected with the physical diagnosis of heart disease.

We are glad to observe that Dr. Sansom teaches his students to rely largely upon their unaided faculties, and that he wisely commences by devoting a chapter to the more general and less special of the symptoms which sufferers from heart disease present.   The value of pain, palpitation, dropsy, dyspnœa, cyanotic complexion, hoarseness, &c., are all reviewed, and the student is warned that he is not at once to rush to his stethoscope for the sake of exactly

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[1] *Lectures on the Physical Diagnosis of Diseases of the Heart.*   By Arthur Ernest Sansom, M.D.   London, 1876.

diagnosing the seat of a trouble, without attending to the complaints which a patient makes, and which, although they are the real cause of his seeking medical advice, have often very little apparent connection with his pathological condition.

The whole subject of physical examination in heart disease is treated at length, and although we do not notice any new facts, we commend the admirable judgment with which the facts given have been selected, and the clearness with which they are set forth.

The chapter on auscultation has about it a highly practical tone, and we are glad to see that the author has adopted the graphic method of representing murmurs which we owe to Dr. Gairdner. Dr. Sansom's rules for forming a diagnosis in difficult cases are worthy quoting :

"In the first place, do not be content to write in your notes 'rhythm of the heart irregular and tumultuous,' but let there be order in your record of such irregularity, and system in your treatment of the seeming chaos. Record all the signs which you have observed previously to those derived from auscultation. Describe the sounds, normal and abnormal, heard over the situations of each of the orifices. Note first the characters of first sound and second sound at the aortic cartilage. Reduce these to diagrammatic form indicating murmurs where present. Repeat the process at the pulmonary, tricuspid, and mitral areas successively. Compare the observations and diagrams only after they have been completed, and then fill in the lines of conduction of normal and abnormal sounds. Do not hasten your conclusions, but obtain all the evidence before you give your verdict."

The Electric Bath.—The medical world would be glad of some real information concerning a therapeutic measure of which it has heard much. It is not to be found in the present volume, which is more remarkable for its assertions than its scientific reasoning. When a patient is immersed in a tub of water, which is part of an electric circuit, what results, other than those attributable to the water and the warmth, are likely to be obtained ? Does the electricity prefer to take the highly resisting course of the patient's body, or the path of slight resistance offered by the water ? These are among the questions we want answered before serious attention is invited to the electric bath, and Dr. Schweig certainly does not answer them satisfactorily. The bath seems to be almost a panacea, and is said to cure rheumatism, cholera and paralysis, and, of course, nervous exhaustion and impotence. This work, however, is more likely to decrease than increase the belief of the profession in a therapeutic agent, which has had the misfortune to be introduced by persons with more zeal than judgment.

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1 *The Electric Bath.* By GEORGE SCHWEIG, M.D. New York, 1877.

Bristowe on the Theory and Practice of Medicine.[1]—The work
before us is an 8vo volume of 1100 pages, and has been written
for students and junior practitioners. It is intended to give, in a
moderate compass, the results of the author's reading and ex-
perience. The first 120 pages are devoted to general pathology,
and contain a well·written exposition of its doctrines. Here and
there a statement may be met with, to which some exception may
be taken, as, for example, that on page 41, affirming the probable
existence in the blood-vessels of a distinct power of active dilata-
tion. But such instances are rare.

The rest of the book is given up to special pathology, nearly the
whole field of which is embraced. As this is too wide for minute
culture by a single labourer, or for minute description in a single
volume, there is inevitably some inequality in the completeness of
its different parts. Some are full and carefully elaborated; others
are mere summaries.

Dr. Bristowe defends himself, by anticipation, against the com-·
ment that his directions as to treatment are less ample and specific
than is desirable, and we agree with him that the inculcation of
sound general principles is of more importance than an enumeration
of therapeutic details. But in some cases, it would have been
satisfactory to have had a more positive expression of opinion
as to the value of certain medicines and modes of practice.

Thus, as regards the management of cases of pneumonia, we are
left in doubt whether or not Dr. Bristowe approves of the early
employment of alcoholic stimulants. We presume that he does not,
as their administration receives no distinct sanction, and as he gives
a qualified recommendation of bleeding from the arm. On the
same subject, he mentions that, for the purpose of reducing tempera-
ture, recourse may possibly be had with advantage to veratria,
digitalis, or aconite.

Holding as we do, with Juergensen, that the great risk in
pneumonia is from failure of cardiac power, we doubt much the
admissibility of the first or last of these remedies. Dr. Bristowe
maintains the old doctrine as to the relationship between miliary
tubercle and caseous matter, and regards them as stages of the
same morbid process. He considers that caseous pneumonia, or the
ordinary form of phthisis, has a right to the name of tubercular.
We do not share this opinion, but believe that phthisis may
exist without tuberculosis, either existent, or antecedent. Hence
his chapter on tubercular disease of the lungs fails to satisfy us,
not giving, as we think, an adequate idea of the weight of evidence
on the question.

A *Treatise on the Theory and Practice of Medicine.* By John Syer Bristowe,
M.D., &c. London, 1876.

The section on Bright's disease of the kidney is too short and restricted to permit the author to do justice to his subject, some eight or nine pages only being allotted to it. The disease is considered as simply inflammatory, and as the result of general acute or chronic nephritis. The fact that it is often the result of a degenerative change, and of gradual origin is mentioned incidentally. Descending to smaller matters, we note that Dr. Bristowe invariably uses the word aphtha, instead of aphthæ. As far as we know, the word can be only legitimately used in the plural.

Having pointed out a few shortcomings, which may be easily made good in the next edition, we have pleasure in stating that the book, on the whole, is an excellent one. Many of the articles are admirably written, as those on diphtheria, thrombosis and embolism, thoracic aneurism, and the introductory remarks on diseases of the heart. They are full, clear, and terse.

The division containing the diseases of the nervous system is one of the best in the work. Here the student will find information which he would seek without success in any other English treatise, except the translation of Ziemssen's ' Cyclopædia.'

The author has laid under contribution, and largely profited by the writings of Duchenne, Charcot, Jackson, Clarke, and other modern workers in this department of medicine.

The sections on sclerosis, lateral and disseminated, locomotor ataxy, glosso-labio-laryngeal palsy, infantile spinal paralysis, and progressive muscular atrophy, contain a carefully written abstract of all that has been established respecting these interesting, but, until lately, unstudied maladies.

We have said sufficient to show that we estimate Dr. Bristowe's treatise highly as an introduction to the study of pathology.

Sea Air and Sea Bathing.[1]—This little work, by Dr. Parsons, contains many useful hints for those who are seeking for recreation and health by the sea. The author first traces the various stages of action of sea water on the body from the first coldness and shock, on immersion, to the glow of reaction. He then proceeds to a chapter of general directions for bathing by different classes of bathers. The best time of the year for sea bathing in England is, he says, between the first day of June and the last day of September. The most suitable time of the day for sea bathing, is about two or three hours after breakfast, when the morning meal is digested, and the system is beginning to feel the effects of the conversion of food into fuel. No person should bathe more frequently than once in the day, and a daily repetition of the open sea bath is

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[1] *Sea Air and Sea Bathing ; their Influence on Health. A Practical Guide for the use of Visitors at the Seaside.* By CHARLES PARSONS, M.D., Honorary Surgeon to the Dover Convalescent Home.

not suitable for all persons. For the majority of persons bathing on alternate days will be found amply sufficient, and quite as much as the average seaside visitor can sustain. Some very correct and useful hints are given as to the way to bathe. It is recommended that all who can make up their minds should go into the sea with a plunge. Those who cannot do this, should go in rapidly and duck as quickly as possible under the first advancing wave. The author most justly states, that bathers who go into the water timorously, knee-deep, and stand hesitating before they proceed further, with their teeth chattering and their bodies cold, had better not practise bathing at all, unless they can amend their ways. Such persons, after the bath, suffer from congestive headache, and sometimes from sickness; no good reaction, but it may be that a long sensation of chilliness and depression, follows. We agree entirely with this advice, and we would push it a little further. It not unfrequently happens that when young children are first taken to bathe, they are undressed and put into the water while in a state of great fear. They resist, and then, by persuasion or force, are made to take half a bathe. The effect is often most injurious. When a young child for the first time is brought to the sea, he should be taken into it as quickly as possible, without exciting his or her fears. If fears actually arise and the little bather become at all nervous and faint, he should not be taken at that time into the sea at all, but should be quickly dressed again, and allowed to run to his play without either being scolded or teased. In time, by familiarity with the sea, and by observation of other bathers the fear wears off, and the bathing is accomplished satisfactorily.

To this bit of practical advice, we would add another bit, viz. never, in excess of zeal for teaching the healthy plunge, take the young bather by surprise and throw him into the sea. This is a most dangerous practice, both to mind and body. In a future edition of Dr. Parsons' book we would suggest to him to add a short paragraph or two on this subject of bathing by the young and uninitiated.

Some instruction is given about the necessity of keeping up muscular exercise while in the open sea, and as to the state of the body in respect of its temperature. It is very correctly taught that when the body is warm from exercise, and is not reduced by excessive exercise or other reducing cause, there is no harm from bathing, rather the contrary; but that no one who is perspiring, and at the same time wearied, should enter the cold sea bath. Exercise before and after the bath is, nevertheless, in all cases good, provided that it be moderate. Prolonged bathing in the sea at one time is very practically and clearly denounced. The bather should not stay in the sea until he is beginning to feel cold. For those in health, a bath of from five to six minutes is, as a rule, sufficient;

those who are weak should not exceed four minutes, and the really invalided should be content with two or three dips and out again. We second heartily the advice here given, as well as the further advice, that good swimmers should not exceed a quarter of an hour at a time in the sea, and should not, on any pretence of seeking health, bathe more than once in the twenty-four hours.

There are some ailments which follow at times the use of the sea bath, such as bilious attacks, constipation, diarrhœa, irritation of the skin, boils, nettle-rash, congestive headache, faintness, and vomiting, and in women, catamenial discharge. To these induced ailments Dr. Parsons draws the attention of the reader in a special chapter. His direction here are explicit, and at the same time brief. If we have a fault to find, it is in the recommendation for treatment of those who suffer from congestive headache of a severe kind, with deficient reaction, after bathing. In such cases the author recommends, when the collapse is alarming, as in rare instances it is, a warm bath and friction, preceded by a glass of hot brandy-and-water. We demur to the hot brandy-and-water in a case where there is failure of power from congested nervous centres. It is bad physiology—it is worse practice. A draught of hot-milk-and water or of soup is the proper addition here to the warm bath, and friction. The great paralyser of nervous function, alcohol, is simply so much added evil.

On the whole, we commend heartily Dr. Parsons' book. It is a good book for professional men to put into the hands of those whom they may send to the seaside. It is plain, practical, and sincere.

The Nurse's Companion.[1]—This is a small and, on the whole, pretty sensibly arranged compilation. It will, no doubt, be found very useful by beginners or unskilled nurses. The directions are for the most part given in clear language, and it is seldom that any room occurs for misinterpretation. It strikes us as singular, however, that scarcely a word is said respecting the best methods of administering medicines to children, nor reference made to the peculiarities of sick-child nursing, as distinguished from the management of infants which properly belongs to monthly nursing. The author will, no doubt, in a future edition, make his little work more complete on this and several other points which call for fuller treatment. What he has given the reader is in the main so good that we wish there were more of it. Some portions of the book can hardly be said to consist of teaching for nurses, and, however valuable the information given may be, it would more appropriately belong to a work on domestic medicine. For example, some of the

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[1] *The Nurse's Companion, a Manual of General and Monthly Nursing.* By C. J. CULLINGWORTH. London, 1876.

chapters on "Nursing during Pregnancy, Labour, and the Puerperal
State," have reference to points concerning which a medical man is
pretty sure to be consulted long before any skilled nurse is in
demand; *e.g.* hæmorrhage, or discharge of blood during pregnancy,
and the precautions to be taken against miscarriage.   Herein, as in
some other parts of the work, the author has, no doubt with most
praiseworthy motives, somewhat confused the respective provinces
of the medical attendant and the nurse, and he consequently
occasionally lectures over the heads of those for whom the work is
ostensibly intended.   Fortunately, the information given is trust-
worthy, and it is best that a high standard should pervade the
pages of a work of this kind.   Some very sensible, and ordinarily
much needed, advice is given as to the behaviour of a nurse in the
lying-in chamber; and altogether the very full directions given for
the management of mother and child are highly to be commended.
This department of practice is evidently familiar to the author, and
monthly nurses cannot have a better guide.   We hope to see
another edition of the work, and would suggest that it might
profitably be amplified in several respects.   It strikes us that a
glossary of medical terms would prove very useful to the class for
whom the book is intended.

History of Asiatic Cholera.[1]—Notwithstanding all that has been
said and written on the subject of Asiatic cholera, Mr. Macnamara's
work will be read with interest by all those who desire to make
themselves thoroughly acquainted with the history of the myste-
rious disease of which his pages treat.   The author makes no pre-
tension to the discovery of any special remedy for cholera, nor,
indeed, does he lay down any rules of treatment at all; and, with
reference to what may be called the theory of the malady, his views
are advanced with due caution and upon a full consideration of all
the facts bearing upon each matter discussed.   His work is, in fact,
only what it professes to be, a history of Asiatic cholera, and as
such it possesses very high merits, being the result of great labour,
of much personal experience, and of mature judgment.

In order that there may be no mistake as to the disease described,
Mr. Macnamara desires it should be clearly understood that by
"cholera Asiatica" he means an affection of a most fatal nature,
generally destroying about 50 per cent. of its victims, and cha-
racterised by a train of well-known symptoms, which we need not,
therefore, specify in detail.   This preliminary caution is, no doubt,
given by Mr. Macnamara in order to exclude from the category of
true cholera all those allied maladies which, although resembling it
in some of its features, are really of a much milder type and of a

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[1] *A History of Asiatic Cholera.*   By C. MACNAMARA, F.C.U., Surgeon to the
Westminster Hospital.   pp. 472.   London, 1876.

totally different nature. It is well known that some authors have regarded the bowel-complaints usually prevalent in the summer in our own and other temperate climates as forms of cholera, differing from the Asiatic only in intensity and capable of passing into it if neglected, but it is evident that Mr. Macnamara holds no such opinion.

The origin of all diseases is obscure, and cholera is no exception to this general statement; and although Mr. Macnamara displays much research in tracing the earliest records of its appearance, he admits that the ancient descriptions of the disease are but few and vague. But he strongly maintains that cholera had its cradle in the East Indies, and especially in British India, and that all the outbreaks which have appeared in Europe, the New World, and elsewhere, are traceable to this primary source. He produces abundant evidence, with much of which the medical profession is already acquainted, to prove that cholera prevailed in British India at the close of the last century and at the commencement of the present, the most remarkable epidemic having occurred in 1817. From this time, as is well known, the disease has travelled in various directions, and it made its appearance in our own country in 1831 and 1832.

Mr. Macnamara takes great pains in proving the limitation of the disease to India in the first instance, and in showing how it was subsequently propagated to other regions. He seems to be clearly of opinion that as far, at least, as our own country is concerned, the malady was unknown until the years just mentioned, and that the descriptions of authors who hold a different view are probably to be discarded as based on insufficient grounds.

He argues that the immunity of England and of Europe generally from cholera before the second quarter of the present century is explicable by the difficulty, formerly existing, of the voyage from Europe to the East, the long sea passage round the Cape being calculated to destroy any germs of infection, if any such were likely to be transmitted from the Indian settlements. But in 1830 a steamer for the first time sailed from Bombay up the Arabian Gulf to Suez; in 1834 it was reported by a committee of the House of Commons that it was practicable to carry on steam communication between Suez and Bombay during the north-east monsoon; and it is well known that, after a series of trials, a regular communication by steam was instituted and still exists by this route between India and Europe. But Mr. Macnamara also adverts to the international communications formerly existing by land, or at least along the western shores of India and the Persian Gulf, and he shows that this mode of travelling was once so tedious that disease was not often transmitted from one country to another. Besides this route from India he mentions another by land to several cities in China.

Mr. Macnamara then explains in detail the bearing which such observations have upon the history of Asiatic cholera; and he maintains that the facts elicited by studying the operations of trade between India and other countries account for the non-appearance of the disease in Europe before the year 1832. Admitting that cholera existed in India before that date, and that it is communicable by means of the excretions of people affected by it to healthy persons, it is alleged that the disease was not communicated, because the channel of direct intercourse between the nations of India and foreigners was of an extremely restricted nature, and for many centuries was absolutely almost impracticable. It is hardly conceivable, the author argues, that cholera, or any such disease, could be carried from Hindostan to England by men passing over a long sea voyage of several months' duration; but now that regular steam communication takes place from India, both into the Red Sea and into the Persian Gulf, the propagation of the disease can easily be accounted for.

It is, of course, quite impossible for us to follow Mr. Macnamara into the multitudinous details with which his pages abound, or to trace with him the numerous channels by which cholera was propagated to Europe and to America, but we may observe that, with regard to its invasion of England, it is proved that it came to this country from the continent of Europe in the year 1831. In 1829 it began to travel from India northward, and appeared on the Russian frontier; in 1830 it broke out in Persia and in Russia; in 1831 it travelled through Asia Minor, Egypt, Constantinople, Bulgaria, and Germany, and, as will be remembered, it appeared about the end of October at Sunderland, being supposed to have been imported from Hamburg. In March, 1832, it broke out in Paris, and with such extraordinary malignity that of the first ninety-eight cases admitted into the Hôtel Dieu no less than ninety-six were fatal, and in eighteen days 7000 persons had died of the disease. In this same year the disease visited England in an epidemic form. Up to the date now mentioned, America had been free from Asiatic cholera, the progress of the disease having apparently been checked by the intervention of the Atlantic Ocean. On the 8th of June, however, the disease broke out at Quebec, and two days afterwards at Montreal, and the visitation is readily explained by the fact that a vessel had sailed from Dublin, then affected with cholera, in the previous April, having 173 emigrants on board, of whom 42 died of cholera during the voyage. The remainder were allowed to land a few miles from Quebec, and no rigid measures were taken to prevent intercourse between them and the city. But, besides this vessel, it appears that several others arrived at Quebec with cholera patients on board, and large numbers of emigrants from cholera-infected quarters arrived upon the St. Lawrence, and thence were

distributed throughout the province. " This was the beginning of cholera in America," writes an American physician, quoted by Mr. Macnamara, and the magnitude of the results as affecting a great part of the New World constitutes a sufficient excuse for this brief allusion to the first appearance of cholera at Quebec.

Although, as we have observed, we are unable to follow Mr. Macnamara through the different threads of his historical researches, it will be gathered from the above remarks that he has a definite idea as to the propagation of the disease, although he has no explanation to offer as to its original cause. He seems to disbelieve the possibility of its development *de novo* from any known and recognised sources, and he assumes its original and endemic existence in India from all antiquity merely as an established and incontrovertible fact. If India had remained isolated from the rest of the world by the interposition of seas, and deserts, and mountains, and if steam had not instituted rapid and regular communication for the purposes of commerce, cholera might have remained a stranger to Europe and to America; but as it is, the germs of the disease are carried from one port to another by persons, by clothing, and by merchandise, and by the influence of drinking-water, and hence there are but few nations of the earth which have remained free from its epidemic visitations. For many mysterious or capricious features exhibited by cholera no explanation can be offered, such as its unequal and very partial distribution, its absence for some periods, its presence at others, the immunity of some localities which might seem most open to its attacks, and the invasion of other regions which, it would be supposed, were exempted. Still, with all the difficulties of the subject, some general principles may be laid down to guide the inquirer and the sanitarian in their researches into this dreadful malady, and in reference to preventive medicine much has, no doubt, been already done in warding off many threatened invasions.

We cannot, perhaps, pass a higher eulogium on Mr. Macnamara's book than to state that he has had to grapple with great difficulties and has overcome some of them, and that even those who do not agree in his conclusions will have ample reason to admire the industry and zeal which he has displayed in massing together a multitude of facts and arranging them in admirable order.

Theory of Medical Science.[1]—We have read this little volume with some care, but are unable to understand the object of its publication. That Dr. Dunham is dissatisfied with the present condition of medical science is evident from the following passage at

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[1] *Theory of Medical Science. The Doctrine of an Inherent Power in Medicine a Fallacy.* By WILLIAM DUNHAM, M.D. pp. 150. Boston, 1876.

p. 102 :—" I have long maintained," he says, " a great contempt for our medical philosophy, but it is practically wise not to condemn a principle without superseding it with a better substitute." But he does not tell us what our " medical philosophy " is, and we are ourselves ignorant upon the subject.   He is right, however, in not condemning a "principle," whatever it may be, without superseding it with a better (?) substitute, but we fail to find that Dr. Dunham has provided or suggested any intelligible substitute for the shadowy " philosophy " or " principle " for which he has so great a contempt.   We are glad to find that Dr. Dunham regards the homœopathic theory with the same contempt as that which he bestows on " our medical philosophy."   The doctrine of an inherent power in medicine (or, as Dr. Dunham seems to interpret the term, in drugs) may be, and probably is, a fallacy, but we are not aware that any rational persons hold such a doctrine, and its refutation, therefore, is a work of supererogation.   We can find nothing in Dr. Dunham's pages but a jargon of words without any definite meaning, and put together, we may add, without much regard, in many instances, to grammatical accuracy.

Tables of Materia Medica.[1]—This book belongs to the class of well-intentioned productions, intended to facilitate the acquisition by students of the modicum of knowledge required by the medical examining bodies.   We quite sympathise with the author in " the weary mechanical work involved in the preparation of these tables," and may be allowed to express the regret that the amount of time and work expended upon them had not been applied by Dr. Brunton to the investigation and elucidation of some of the innumerable questions in the therapeutics of which we are profoundly ignorant.

However, we doubt not that students will duly appreciate the book, which is a sort of condensed summary of the principal facts in materia medica, arranged in a tabular form and stated with extreme brevity.   It is, in fact, just a " cram-book," defying perusal, and, in our opinion, entailing more labour on a student really desirous of learning materia medica than the diligent study of the ordinary text-books on the subject, which have some context to make their perusal practicable and possibly interesting—which present some flesh on the bones, and not the dry bones only for digestion.   Of the many columns of the tables before us, those relating to the therapeutical action and uses of the drugs are the least satisfactory.   The notes on these matters are crude and superficial, faults due partly to the brevity demanded in the tabulation of the facts.   But they present also a lamentable commentary on our

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[1] *Tables of Materia Medica.   A Companion to the Materia Medica Museum.* By T. LAUDER BRUNTON, M.D., F.R.S.

therapeutical knowledge by their vagueness, the contrariety of their statements, and the looseness of terminology.

However, our standpoint in estimating the value of this treatise is wholly different from that of the student, who, the more the pity, is driven, by the accepted policy of medical examinations, to cram his head with hosts of facts and statements, which he has to learn to forget when admitted into the ranks of the profession, before he can be an intelligent, rational practitioner of medicine.

Routh on Infant Feeding.[1]—The Hippocratic aphorism adopted by the Royal College of Physicians of London, and cited in all the documents put forth by that learned body, viz. "that life is short, but art long," does not, judging from the work before us, seem to sufficiently impress itself on all its members. Now, we are not about to find fault with the matter contained in this book, which is well enough in its way ; nor with the style, except so far as to say it might be better ; but we do object to the compilation of books of several hundred pages on subjects which may be sufficiently treated in a tithe of the number. Here we have a volume of above five hundred pages on infant feeding, a subject on which a skilled writer could say all that is of importance in far less compass. To make up the quantity of matter requisite to fill these many pages, a vast deal of collateral information has been collected, information culled from mortality statistics, from physiological and therapeutical treatises, from the Materia Medica, from works on children's diseases and their treatment, and from books on diet and regimen.

We admit that we cannot affirm that the whole of the varied topics dealt with in this volume are not legitimately covered by the the full title of the work ; but, at the same time, we are of opinion that even in this case they need not to have been treated with so much prolixity. The book is certainly not addressed to medical men, but rather to the public ; and it may be contended that the public want all this miscellaneous information, and have shown their appreciation of it by the purchase of two previous editions. But if so, we would reply that there is much in it needlessly written, as far as the wants of non-professional people are concerned, and that many medical details might well have been omitted. Moreover, we question whether among its many purchasers it has had many readers—many who have followed the author through his discursive pages. However, there are those with whom time is long and occupation scant, and such of those as possess themselves of the volume will find in its fulness of matter much worthy their attention and study.

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[1] *Infant Feeding and its Influence on Life, or the Causes and Prevention of Infant Mortality.* By C. H. F. ROUTH, M.D. Third edition. London, 1876.

Austin Flint's Manual of Percussion and Auscultation.[1]—A work of this kind, addressed to students, written by a highly experienced physician and teacher, calls for no extended notice in the shape of a review. Its purpose is sufficiently represented in the title; whilst, on the other hand, the competency of Dr. Austin Flint to teach percussion and auscultation will be admitted by every one acquainted with the recent literature of chest diseases. Indeed, the diagnosis of these diseases has been a special study with him, and his position as lecturer and physician of the large Bellevue Hospital, New York, has afforded him a wide field for observation.

As a matter of course, the author has little novel to teach on the oft-written subject of auscultation and percussion. Yet he can refer to some personal research and opinions, as, for instance, to the recognition and naming of broncho-vesicular respiration, a sign of a certain degree of solidifying disease of the lung, consisting of a combination of the vesicular and tubular quality in the inspiratory sound. So again, he claims credit for pointing out the distinctive characters of the cavernous respiration from bronchial or broncho-vesicular.

His manner of teaching is full and precise; and he has advisedly abstained from troubling his readers with disputed questions respecting the mechanism of signs, "taking the ground that our knowledge of the significance of signs rests solely on the constancy of their connection with the physical conditions which they represent," relating immediately, not to diseases, but to the physical conditions incident thereto. Moreover, he urges, that exclusive reliance is not to be placed on physical signs, but these are always to be taken in connection with pathological laws, the history and the symptoms, otherwise they become sources of error.

In conclusion, we can confidently recommend this treatise to all who would learn auscultation and percussion, and rightly value these modes of exploration of disease. If we may venture on noting any deficiency in the work, it would be by remarking on the entire absence of diagrams, which serve greatly to illustrate verbal teaching, more especially in making clear the nature of cardiac sounds.

Bennet on Nutrition.[2]—It is some years since Dr. Bennet produced this work, which has now reached a second edition; the delay, however, in its re-issue being due, as he tells us, to personal

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[1] *A Manual of Percussion and Auscultation of the Physical Diagnosis of Diseases of the Lungs and Heart, and of Thoracic Aneurism.* By AUSTIN FLINT, M.D. London, 1876.

[2] *Nutrition in Health and Disease. A Contribution to Hygiene and to Clinical Medicine.* By JAMES HENRY BENNET, M.D. Second edition. London, 1876.

circumstances. His purpose in writing it was to impress on the profession and the public the fact that the imperfect performance of the digestive and nutritive functions leads, slowly but surely, to ill-health, to disease, and to death.

The text was a good one, though not new, nor discoursed upon for the first time. Nevertheless, it is one that can well bear iteration and illustration, and, in the hands of Dr. Bennet, has obtained very adequate and satisfactory treatment. We may, therefore, wish this new edition the same popularity and ready sale that attended the former one, for if read, marked, and inwardly digested, it cannot fail to be of service to the public. To professional men, instructed in physiology and dietetics, it will present no novel teaching. Yet, as a record of the author's long observation and experience, it will have a value even to them as confirmatory evidence of some important truths recognised in practical medicine.

On one point, indeed, Dr. Bennet believes he differs from generally accepted views. He considers too much importance has been attached to the differential diagnosis of the several morbid salts found in the urine as a result of disordered digestion and nutrition. He thinks that the presence of uric acid and of lithates in the urine, in such abnormal quantities as to constitute a deposit, is very much more frequently the result of defective digestion than of defective metamorphosis of tissue, especially of dyspeptic individuals. The like opinion he holds with regard to oxalate of lime deposit, which, also, he cannot recognise as indicative of a peculiar diathesis. But if the author thinks little of the differential diagnosis of urinary sediments, he has the most profound conviction of the importance of examination of the urine as a guide to diagnosis, particularly to that of imperfect digestion, believing it to afford the most delicate and best test of the condition of the digestive and nutritive functions, and thereby also the safest and most tangible guide in the dietetic, hygienic and medicinal treatment of patients. As a consequence of this conviction, Dr. Bennet is very precise as to the method of examining the urine, particularly so with regard to the time after food at which the examination is made.

In these days of promiscuous and unrelenting onslaught on all alcoholic drinks, it is well to find an author point out that all the ills which afflict humanity are not the product of intoxicating beverages, but that in no small proportion over-indulgence in solid food is chargeable for them. Or, as Dr. Bennet writes, "the fact is probable that in the middle and upper classes of society more human beings suffer in health and strength from taking too much food than from taking too little;" and "over-feeding is a much more baneful error than is generally supposed."

The author's remarks on "food requirements in ill-health" are marked by sound sense and the fruit of experience. He gives a

very necessary warning against the too prevalent custom of cram-
ming the sick with the view of supporting strength, and utters a
protest against the exaggerated notions in vogue as to the vast
superiority of animal food, and points out that density of structure
in aliments is a more important condition to be kept in view than
merely their animal or vegetable nature.   In his observations on
alcohol he raises the question of its value as food, but does not enter
into the controversy that has raged on the matter.   Whilst judi-
ciously pointing out the errors and abuses in the use of alcohol as a
dietetic for the sick, he does not fail to recognise its occasional
utility.   But his general deduction is, " that, as a rule, the con-
firmed dyspeptic should be all but a water-drinker, until he have
recovered his health."

What is now written must suffice for a notice of this work.   It
abounds in sound instruction and advice, and cannot fail to benefit
the non-professional reader, for whom we assume it to be primarily
intended.

Bucknill on American Asylums.[1]—These notes previously ap-
peared in the pages of a contemporary, and called for sharp dis-
cussion and severe comments in the United States of America, the
asylums of which constituted their subject-matter.   In the opinion
of those whose feelings are not aroused by disagreeable criticism
these notes will possess a high value.   They come from a physician
than whom none could be better qualified to form an opinion as to
the structural arrangements, and the management of asylums, and the
treatment of the insane.   If he have found fault with the institu-
tions for the insane in the United States and in Canada, it has been
in no carping spirit but in fairness and honesty, and his censures
should be received with respect and attention—not resented as in-
flicting an injury.   No unprejudiced reader of the notes can come
to any other conclusion than that American asylums are far from
the complete and satisfactory condition which their own reports
would suggest; that much is wanting in the arrangements made for
the comfort and convenience of their inmates, and that greatly
more restraint is resorted to than is really needed.   Moreover, the
lunacy laws in several of the States are most defective and unsatis-
factory, and a great responsibility rests upon some of the
State legislatures for the many abuses and defects Dr. Bucknill
has pointed out, and which have been referred to by other writers.
In fact, Dr. Bucknill appears disposed to attribute many of the
faults he observed to defective legislation and administration, and to
the absence of a healthy public opinion as to the requirements of the

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[1] *Notes on Asylums for the Insane in America.* By JOHN C. BUCKNILL, M.D.,
F.R.S. London, 1876.

insane. With the generally miserable condition of asylums in Lower Canada we have been long acquainted, and can but wish that the governmental influence of the parent country may be brought to bear on this important colony, in favour of placing Canadian asylums on a proper footing and of making them more in accordance with our own institutions in structural arrangements, management, and treatment.

We should not fail to observe that, whilst noting faults in structure and management, Dr. Bucknill rightly awards all praise to the asylum superintendents of America for their skill, zeal, and humanity in their responsible position.

Bull's Hints to Mothers.[1]—The former of these two works comes before us as the twenty-fifth edition; the latter, also, has passed through many editions, but the number is not stated. However, it is clear enough that these two volumes are highly appreciated by the public, for whose benefit they were written. And from our knowledge of their contents, and of the manner in which these are handled, we consider that the popularity they have achieved is well deserved.

The volume on the maternal management of children is a sequel to ' Hints to Mothers,' and without doubt owes its origin to the success attending the publication of the latter. The two cover much of the same ground, and consequently we find many repetitions of the same statements in both. They, however, appear as independent works, to be separately purchased, and consequently no objection need be taken against this circumstance.

The manner in which the incidents of pregnancy and labour are treated is most unobjectionable. It awakens no prurient curiosity, and is suggestive of nothing to which the most sensitive woman can object. This volume was most unwarrantably referred to in a recent trial—where a scandalous book, professedly put forth to convey legitimate information to women on some kindred subjects, was the object of prosecution—as a work belonging to the same category, and sinning in the same direction. But the proceeding had not the least justification furnished by the contents of the present treatise.

It is a further commendation of both volumes that they do not encourage their readers with the notion that they can afford to do without the aid of medical men; that armed with the books, they can alone undertake the treatment of the diseases written about. As a further praiseworthy feature, it may be stated that the style is simple and clear, and scientific and inflated verbiage alike avoided. It is with much satisfaction we recommend these two volumes to those of the public for whose information they have been written.

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[1] 1. *Hints to Mothers for the Management of Health during the period of Pregnancy and in the Lying-in Room.* By Thomas Bull, M.D. New edition, thoroughly revised by R. W. Parker.

2. *The Maternal Management of Children in Health and Disease.* By Thomas Bull, M.D. New edition, thoroughly revised by R. W. Parker.

Lawson on Sciatica.—We noticed favorably, in a previous number of this ' Review,' the first edition of this work; what we have now before us is professedly a second edition, although, in fact, nothing but a re-issue of the former one, with a new title-page and a concluding chapter, of a miscellaneous character with regard to its contents.   This is not what the public look for when invited to purchase a new edition; and surely Dr. Lawson might have revised what he had first written, and have corrected certain references which now appear incorrect.   Moreover, instead of writing Part XI as a supplementary chapter, he should have incorporated most of it in the body of his work.   His subject, indeed, was a limited one in the first instance, viz. the advocacy of hypodermic injections of morphia as the true treatment of sciatica, and admitted of no more extended consideration than he had given it.   And whatever addition was made to our knowledge of the pathology and treatment of the disease was made in the original edition, which, therefore, for all practical purposes is of equal value with this so-called second one.   The new matter found in Part XI consists in a statement of the author's continued confidence in the treatment recommended; a reference to the effects of morphia injections when bleeding follows puncture of a vein; to the not infrequent swelling up of the lymphatic glands; and to the occasional curious disturbance of vision by the morphia injection.   There is also a brief discussion of the relative effects of morphia and atropia, from which we learn that the author agrees with Dr. Harley, that the two drugs are not " in the least way antagonistic in their general effects on the body." Of injections of atropia he does not approve; they are attended by unpleasant consequences and are far inferior in the relief afforded to morphia.   As to subcutaneous injections of cold water, advocated by Lebert and others, he does not believe in them.

Dictionary of Medicine and Surgery, by Jaccoud.[2]—The principal articles contained in this comprehensive cyclopædia of medicine (which has, to the great credit of its editor and publishers, been produced so uninterruptedly, notwithstanding its magnitude and the number of writers employed on it) are—on the structural anatomy, physiology and pathology of mucous membranes, of muscles, and of nerves, by J. Straus, Math. Duval, Labadie-Lagrave, Le Dentu and Poïnsot.   Hallopeau treats of neuralgia, and A. Luton of neuroses.   Dieulafoy writes of death in its physiological aspects, and A. Tardieu and Laugier of death in its medico-legal bearings; other noteworthy articles are contributed by Hirtz on

[1] *Sciatica, Lumbago, and Brachialgia; their Nature and Treatment.* By HENRY LAWSON, M.D.   Second edition.   London, 1877.
[2] *Nouveau Dictionnaire de Médecine et de Chirurgie pratiques, illustré de figures intercalées dans le texte.*   Directeur de la redaction, le Docteur JACCOUD. Tome xxiii.   Mol-Nev.

narcotics; by J. Simon, on thrush; by Verneau, on monstrosities; by A. Tardieu and Martineau, on glanders and farcy in the lower animals and in man. There are several minor articles on the natural history and therapeutical properties of the myrtaceæ; on myrrh, monesia, mucilages, mustard, &c.; and others again on nævus, neoplasm, moxa, and molluscum. The experienced pen of M. Hardy is employed on the articles on cutaneous diseases.

As we have observed before, this dictionary of medicine and surgery reflects the position of those sciences rather from a French point of view than from one embracing a survey of the state of knowledge existing in the several principal countries of the world in which they have been long and assiduously cultivated. This purely French handling of the subjects is much more observable in some articles than in others. The impression left on the minds of the reader of the volume now before us, and, indeed, of other French medical and scientific works, is that in France the languages of other nations of importance are not widely cultivated, and consequently that the literature of those countries is unknown, or cannot be used, and that an unfortunate indifference to it is promoted to the national prejudice and the injury of science.

After allowing for this defect of a too limited and peculiarly national representation of the state of medical science, it is only just to say that this dictionary stands foremost among the publications of the day for its fulness and value.

Swain's Manual for Emergencies.[1]—Mr. Swain's book on emergencies is so well known to the profession, and has been so well received, that it suffices to remark the appearance of a new edition and to note the chief additions made; what these additions are is thus stated in the preface:—" In the chapter on 'Injuries of the Eye' I have given the symptoms and treatment of acute diseases of that organ, whilst to that on 'Emergencies connected with Parturition,' amongst other things, the signs of pregnancy have been added. The last chapter, on 'Apparatus and Dressings' will be found much fuller, and to contain an account of some of the most recent improvements in surgical apparatus."

In future issues of his work, which are sure to be called for, it will be desirable for Mr. Swain to weigh well the additions to be made to the matters treated, so as not to overstep the precise object signified by its title, a manual for "emergencies," and so deprive the treatise of its distinctive character.

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[1] *Surgical Emergencies, together with the Emergencies attendant on Parturition, and the Treatment of Poisoning. A Manual for the use of General Practitioners.* By W. P. SWAIN, F.R.C.S. London, 1876.

Transactions of Bombay Medical Society.[1]—The extent of work now carried on in all parts of the world by intelligent practitioners to stem the progress of disease and obviate its consequences, and to devise and improve sanitary means and therapeutical agents to accomplish those ends, we may fain hope will, sooner or later, meet with its reward. In every city where a group of medical men are found we find them forming themselves into associations to promote medical science. The volume before us is the outcome of the labours of the members of the Medical and Physical Society of Bombay, a society which, within our knowledge, has existed above twenty years, and has put forth a large number of volumes, by which no inconsiderable additions have been made to our stock of knowledge.

As a volume proceeding from medical men actively engaged in their profession, and most of them connected with the public service as military or civil medical officers, we have in it, as might be anticipated, a series of papers characterised rather by their practical, than their theoretical matter; and, bearing in mind the land of its origin, we are prepared to find that cholera constitutes its *piece de résistance*. Unhappily, however, although cholera occupies the foremost place in Indian publications, among topics for inquiry and discussion, we discover no positive advance in the right understanding of its pathology; no approach to a rational and successful treatment.

This fact Civil surgeon G. Bainbridge refers to in his paper on the "Cholera Epidemic of 1875," in Dhulia. In his opinion we have no real evidence that the various therapeutical agents resorted to are of any value in arresting or curing the disease; and he advocates "a reconsideration by the medical authorities of the therapeutics of cholera," by a sort of test process of the value of the several plans of treatment recommended, by carefully putting them into practice, in the hands of different physicians, who should report on their action and results.

Dr. W. G. Hunter, first physician of the Jamsetjee Jejeebhoy Hospital, was led by the appeal of Surgeon A. R. Hall, in the 'Practitioner' (July, 1875), although not a believer in the theory of contracted pulmonary arterioles, to try the sedative treatment recommended by chloral hydrate. "Thirty-two cases were accordingly treated by hypodermic injections of chloral hydrate, in strict accordance with the plan laid down by Mr. Hall. The results were so disastrous—viz. 53·1 per cent., or, roughly speaking, 20 per cent., more than by the stimulant plan—that I did not feel justified in further pursuing this treatment, and abandoned it. Apart from

these unsatisfactory results, a farther danger was introduced by this method of treatment, which in a hot climate should not be lost sight of. A patient who had been discharged as cured was brought back to hospital three or four days afterwards, suffering from traumatic tetanus (which ended fatally), the result of the punctures by the hypodermic syringe."

Dr. Henry Cook, a hospital colleague of Dr. Hunter, contributes a paper advocating the use of belladonna as a remedy for salivation. After giving his clinical experience, he embarks on a description of its modus operandi, which will repay examination. He follows with another paper, of physiological interest, "On the Diminished Elimination of Urea in Cases of Hepatic Abscess."

Among other instructive articles may be named Surgeon G. Bainbridge's "Experiences in Lithotomy and Lithotrity," the "Report on the Plague in Turkish Arabia," by Surgeon-Major Colvill; a "Medico-Topographical Report of Muscat," by Surgeon G. F. Peters, M.B.; and a "Report of a Journey from Bushire to Kazeroon and back," by Surgeon George Waters. We must note, in addition, among the valuable contents of the volume, the "Medical Report of the Jamsetjee Jejeebhoy Hospital," by S. Hunter, and the "Annual Report of the Civil Hospital, Aden," by Dr. Nolan. An appendix follows, containing records and jottings of cases by several writers; and bound up with the volume are the "Abstract Proceedings" of the society in session monthly, recording briefly the papers read before it, and the discussions thereupon.

The society needs no praise from us: it has long established its reputation as an active agency for advancing medical knowledge. and its volumes of 'Transactions' should be found in every medical library of reference.

Philadelphia Pathological Transactions.[1]—The production of this considerable volume of above 200 pages as the record of the work done within a period of eighteen months by the Pathological Society of Philadelphia, and the long array of names of members who have contributed specimens and notes of cases, indicate much activity and industry in clinical observation, and an ardent desire to extend the boundaries of medical science. As a series of brief notes of morbid specimens, severally accompanied by a short notice of symptoms preceding death, the volume cannot be, in the strict sense of the word, reviewed, or have its principal features pourtrayed by extracts from its pages; but it, nevertheless, commends itself to those who are pursuing inquiries in pathology and morbid anatomy, and reflects great credit on the society which has issued it. Some few woodcuts are introduced in illustration of cases recorded.

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[1] *Transactions of the Pathological Society of Philadelphia.* Vol. v. Edited by JAMES TYSON, M.D. Philadelphia, 1876.

Researches on the Action and Sounds of the Heart.[1]—We are informed that this little book is a re-issue of certain papers and treatises which have appeared in various forms in previous years. The author states that the views advanced are based on a series of carefully conducted experiments on turtles, performed for several successive summers in Canada, during the highest temperature of the season, where every opportunity was afforded of listening to the sounds of the heart. The hot season was selected because at that time the circulation in these reptiles most resembled that of warm-blooded animals. The author, guided by these researches, arrives at a totally different conclusion, as to the causes of the sounds of the heart, from those drawn by most modern writers in this depart-ment of physiology, and he maintains that the first sound of the heart is produced by the contraction of the left ventricle and the impulse of the aorta, and that the second sound is caused by the contraction of the auricles. Thus, it will be seen that Dr. Paton rejects the views advanced by Dr. Hope, Dr. Williams, and others, as to the cause of the second sound of the heart, and reverts to the explanation offered by Laennec, and that he believes the first sound to be really due to the ventricular systole and the shutting of the aortic valves. Admitting the great difficulty of determining accurately the succession, either of the actions or of the sounds of the heart, it must be allowed that the observation of the phenomena, occurring in the heart of the living turtle, is a novel and interesting mode of investigation, the slowness of the pulse in these animals affording more time for arriving at precise data than would be allowed in the case of more highly organised beings. We must observe, however, that, whatever may be the merits of Dr. Paton's investigations, they are very carelessly recorded in the work now before us, and, in the discussion of a subject avowedly of great intricacy and difficulty, where precision of language is indispensable, it is to be regretted that the composition of the sentences is so faulty that many of them are nearly unintelligible.

Practical Guide to the History and Treatment of Diphtheria.[2]—This little treatise is one of a series of articles in an Italian dictionary, the *Biblioteca Economica di Medicina Pratica*, and the author is well qualified to write upon the subject, not only from the care he has bestowed on the literary examination of works re-lating to diphtheria, but from his own practical experience in the

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[1] *Researches on the Action and Sounds of the Heart.* By GEORGE PATON, M.D. Pp. 105. London.

[2] *Guida Pratica alla Conoscenza e alla Cura della Difterite.* Per il Dottore GIOVANNI FARALLI, di Firenzi.

(Practical Guide to the History and Treatment of Diphtheria. By Dr. FARALLI, of Florence.) Pp. 163. Florence, 1876.

study of the disease, which, as is well known, has visited Florence of late years with great and indeed appalling severity. In the present work, which consists of a preface and eleven chapters, Dr. Faralli treats successively of the definition of the word diphtheria, and of the history of the disease from ancient periods down to the present day; of the etiology of the malady; of its symptomatology, both when it attacks the larynx, and is called *croup,* and when it affects other parts of the body; of its sequelæ, such as paralysis, and other nervous disorders; of its diagnosis and prognosis; of its treatment, both medical and surgical; of its pathological anatomy; and of its pathogenesis and its nature.

From the numerous points of investigation thus presented, it is only possible to refer to a few of the conclusions at which Dr. Faralli has arrived; but we may mention generally that he agrees in the main with the views entertained by Bretonneau and other French writers as to the nature, the diagnosis, and treatment of the disease. He has, however, also examined the German literature of the subject, and he gives due weight to the opinion which has been offered as to the parasitic nature of the malady. He is evidently inclined to believe that diphtheria and the so-called *croup* are merely modifications of one and the same disease, or, in other words, and to state the matter more accurately, that, excluding laryngismus stridulus and the inflammations of the larynx, what is usually called *croup* or *membranous croup,* is only the laryngeal or laryngo-tracheal form of diphtheria. In his chapter on laryngeal diphtheria, therefore, he treats this affection as synonymous with croup. As to the nature of the disease, he regards it as partly miasmatous and partly contagious, and although its contagious character has been fully demonstrated, he believes that it very often developes itself spontaneously, and is then propagated in a truly epidemic form, and such is the power of the poison that it appears to be but little affected by the common conditions of seasons or climate, or the social position of the patients. Some authors, whom he quotes, attach much importance to hygienic influences, but others, on the contrary, deny the existence of any relations between the salubrity of places or families and the development or diffusion of the disease. With regard to the treatment of diphtheria, local applications and general hygienic appliances are recommended, and tracheotomy is admitted to have been the means of saving many cases which, without its adoption, would in all probability have succumbed.

Lectures on the Diseases of the Nervous System.[1]—This volume will be highly prized by the members of the Sydenham

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[1] *Lectures on the Diseases of the Nervous System, delivered at La Salpêtrière.* By J. M. Charcot. Translated by George Sigerson, M.D. Sydenham Society. London, 1877.

Society. M. Charcot's name ranks among the very foremost of those who have advanced the knowledge of nerve-pathology. The work he has done is marked by great accuracy and close observation, and by great acumen in interpreting facts and drawing inferences. He has had, moreover, at the Saltpêtrière Hospital, a most extensive field of research afforded him. The use he has made of it has speedily brought him a world-wide reputation.

We are consequently quite prepared to learn, as stated in the translator's preface, that, "although but recently published, the lectures of Professor Charcot on diseases of the nervous system have already taken a place amongst the classic works of medical literature, and been translated into several Continental languages."

The present volume consists of thirteen lectures, devoted to the consideration of disorders of nutrition consequent on lesions of the nerves and of the spinal cord and brain, paralysis agitans, disseminated sclerosis, apoplectiform seizures in disseminated sclerosis, hysterical hemiaæsthesia, ovarian hyperæsthesia, hysterical contracture, and hystero-epilepsy. Some woodcuts are intercalated in the text by way of illustrating facts or hypothesis, and there are foot-notes introduced by the translator, and also by Dr. Bourneville, the able reporter of Charcot's lectures in 'Le Progrès Médical,' of Paris.

Clinical Society.[1]—Apart from the lists of officers and members prefixed, the present volume, issued by the Clinical Society of London, is entirely made up of "communications" submitted to the meetings. On the whole, the surgical members have produced the most papers.

The subjects brought before the society have been of the most miscellaneous character, but scarcely any of them afford material for the advancement of therapeutical knowledge. The skill of the surgeon in cutting short and removing disease, and in processes of repair, is well represented; but the skill of the physician in dealing with disease by therapeutical agents is scarcely brought under notice. The great majority of the medical communications are simply records of cases viewed from a pathological stand-point, and might have been equally well addressed to the Pathological Society.

Judging from this and preceding volumes of the society—which, by the way, we highly esteem for what they do contain—there seem small prospects that the Clinical Society will, as some of its original distinguished members hoped, contribute very appreciably to the improvement of our knowledge of the treatment of disease.

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[1] *Transactions of the Clinical Society of London.* Vol. ix. London, 1876.

Original Communications.

I.—Invalid Criminals and their Diseases. By Dav. Nicolson,
M.D., late Senior Medical Officer, H.M. Prison, Portsmouth.

The standing army of convicts in England numbers some eight
or nine thousand men and about twelve hundred females, and
they are by no means an uninteresting group of human beings.
If convicts are at once the scum and the dregs of society, they are,
too, the crack or master-hands, the *élite*, I might say, of the
criminal circle, and as such, they possess a collective and a *real*
interest not at all akin to that which attaches to the " consummate
villain " of the three-volume heroic. It would be a curious point,
for instance, to work out the *underlying cause* of criminality in the
various members of this group of social rebels—what proportion
of their defection from the moral standard is due to original
mental inferiority ; what to physical defect or disease ; what to
positive misguidance and neglect in early life ; what, later in life,
to the demoralizing influence of intemperance and debased social
surroundings, or to the mere pressure of misfortune and want.
Looking at crime as a possible effect having relation to one or
more such causes, or as a result of certain antecedent conditions,
whether personal or social, it is not difficult to conceive that
something of the nature of kinship exists between minds which
are undoubtedly insane, on the one hand, and minds which are
merely criminal on the other. In both cases a deviation from
a given standard is implied, but in the one case, the area (however
arbitrary) of responsibility is departed from ; in the other, it is
not : and the issues and subsequent treatment are therefore, and
justly, widely different in the two cases. So that, whatever
alliance may be conceivable between crime and insanity in the
matter of *causation*, or, indeed, as to the actual conditions in
some cases, I would not be understood to say that the basis upon

which crime stands is by any means identical with that upon which
insanity stands. In short, criminals are not lunatics. But to
approach our subject. The coexistence, in the same individual,
of mental peculiarity or infirmity with bodily imperfection or
deformity has often been remarked and demonstrated. There is,
for example, a self-assertion, a bumptious arrogance, a vanity and
irritability, and a guerulousness in some very diminutive or hump-
backed men which one passes over as little more than a pardonable
weakness hardly worth noticing under the circumstances. But an
exactly corresponding display of character[1] in a man of large and
proportionate *physique* would be so much out of keeping with
what we would naturally expect, that we could scarcely avoid
looking upon it as an indication of mental derangement. We
have come to accept a certain self-importance and crabbedness of
disposition as not unnatural accompaniments of physical dwarfish-
ness; and happy are we as students of human nature if we have
schooled ourselves to accept in the same light the petulance and
peevishness of our dyspeptic friends and patients. And who is
there that will deny such a companionship of mental and bodily
condition as that here indicated ?

Again, no one doubts that some attacks of insanity are distinctly
due to physical causes in the way of bodily suffering and disease;
and although the same prominence has never been claimed for the
operation of bodily infirmity in the causation of criminal acts,
I may state my conviction that the defective physical condition
of criminals has in many cases much to do with the offences and
misdemeanours committed by them. It was not with the intention
of working out this point that this paper was written; but I
mention it as worth keeping in mind in connection with the
subject of disease among criminals. Bodily disease in the criminal,
when at large, may be taken as acting for the most part in two
ways : either indirectly through its unhealthy influence upon the
mind, which it tinctures with a morbid disaffection for the in-
dividual's circumstances and surroundings ; or more directly by
incapacitating the individual for turning anything in the way of an
honest livelihood for himself. Nothing struck me more in my
work, as assistant medical officer, among the invalid convicts at
Woking Prison than the extensiveness with which disease mani-
fested itself, not in one but in many, of the bodily organs, and
its extreme chronicity. The full tenaciousness of such lives was

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[1] The "bullying" of the "big" man bears a different interpretation, and
arises from a feeling of (at least) physical superiority. The bumptiousness and
querulousness of the little and dwarfish individual are due partly to self-con-
sciousness of physical inferiority, and partly to a fear and suspicion that his
(possibly legitimate) social and intellectual importance is estimated (especially
by strangers) by the size of his body.

revealed only after death, when a healthly organ would be sought
for in vain, and when each organ seemed more degenerate and
corrupt than the other. In his last report (for the year 1875)
Dr. Campbell, the Senior Medical Officer of that establishment,
says—" Owing to the varied and complicated nature of the dis-
eases observed in many of the invalids received here, it is some-
times difficult to assign them a proper place in the nomenclature,
two or more maladies frequently coexisting in a well-marked form
in the same individual. A large proportion appear to belong to
the habitual criminal class, or the greater number have previous
convictions recorded against them ; indeed, the invalid class may be
looked upon in a great degree as the dregs of the different convict
prisons, with constitutions impaired by a long course of profligacy
and vice."

The "invaliding" of prisoners is, a provision of the English
onvict system, by which the more established and chronic cases of
disease or ill-health are transferred (or " invalided " to special
hospital prisons in some healthy locality, where they may have a
better chance of recovery. By this means the labour prisons are
kept apart as far as possible for the able-bodied and healthy. I
may explain that the first nine months of penal servitude are
passed in what is called " separate confinement," and that at the
end of that period the normal course is for the convict to be
removed to a public works prison for the rest of his sentence.
There he is employed at such work as he is fit for, and has
latterly an opportunity of learning some useful trade or occupation.
The great mass of convicts are employed at excavating, stone-
quarrying, brickmaking, building, &c., a certain proportion being
utilised as smiths, carpenters, tailors, painters, bakers, and the
like. If the convict is unfit for public works at the end of his
separate confinement, or if, at any time after, he becomes incapaci-
tated, he is drafted to Woking or Parkhurst as an invalid. The
" invaliding " that takes place among couvicts must not be taken
to have the same significance as that which takes place among
soldiers or sailors. In the army or navy, the recruits are all
healthy and up to a certain standard of physical strength and
capacity, but the convict recruit must be accepted whatever may
be his physical condition or the state of his health, and many of
them are invalids on reception. It will be asked—What relation
exists between the influence of imprisonment and the necessity for
invaliding ? And this is a question that may fairly be asked, and
there is no reason why an attempt should not be made to answer it.
Of course, no one is to claim for penal servitude or imprisonment
the merits of an enlivening or fattening process ; and surely no
one will expect that the circumstances attending such punitory
confinement are to exempt criminals from the wear and tear that

attach to all social occupations, and even to the mere process of living.   No do I think anyone can reasonably object if pressure— or more expressively, the "screw"—is put on a little, with the view of exerting a health-giving impression upon minds not the most susceptible.   This being so, if I were asked my opinion of the physical influence of the deterrent hard labour portion of our present system of penal servitude upon the convicts who undergo it, I would state it thus—If its full pressure were exerted and in-discriminately upon the prisoners, that pressure would undoubtedly be hurtful and unjust.   But if, on the other hand, with a fair and reasonable dietary, the pressure is exerted with a due recognition of the principle that the labour-task of each individual must be adapted to his physical capacity for work, then it is not only just but salutary.   If pressure is to be put on, this principle of adaptation (of labour to capacity) must be insisted on as a safety-valve; and happily our Government convict prisons provide the means whereby the operation of the principle may be secured. No doubt, men break down under the influence of the work and discipline of penal servitude, and, no doubt, within the same scope of work and discipline, the health of others improves.   But the deterioration of some few is no more reason for introducing a relaxation of a penal system than is the improvement which takes place in certain others a reason for increasing its severity.   So long as the numbers keep within bounds, the remedy must be applied to the individuals and not to the whole convict body.

The adaptation of labour to physical capacity is attainable at public works prisons, owing to the existence of a variety of forms and degrees of labour.   So that, while, on the one hand, care is taken to prevent imposition, a man may, if he becomes incapacitated for the hard labour at which he has hitherto been employed, be transferred, on medical grounds, to labour of an easier or more suitable description, either permanently or until he recovers his strength.   But in order to the due carrying out of the principle, the medical officers, with whom lies the responsibility,[1] have to carry on their work with close attention and care; repeated in-spections of more than perfunctory character, and reasonable opportunities for personal interviews, being absolutely necessary.

Convicts may be invalided either from bodily or from mental infirmity or disease; for, although a man may be physically fit for the work at any prison, he may on mental grounds be unfit for

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[1] It is at this point, and in connection with the numerous grave and often trying responsibilities of medical officers of convict prisons, that public opinion and the public purse should be found willing to stretch towards an increase of the totally inadequate pay of officials, whose whole time is devoted to the duties of their department under circumstances which are frequently very conflicting.

the discipline.   From tables given by Dr. Guy,[1] the mental and bodily condition of the convicts in confinement on the 31st March, 1873, is expressed as follows:—

	Per 1000.			
	Males.		Females.	
Weak mind, insanity, and epilepsy . . .	30 ⎫		38 ⎫	
Scrofula and chronic disease of lungs and heart .	109 ⎬ 370		90 ⎬ 327	
Deformities or defects, congenital or acquired .	231 ⎭		199 ⎭	
No infirmities or defects	630		673	

That is to say, of the male convicts 370 out of every 1000 (considerably over one-third) were either deformed, defective, or diseased. There is not wanting evidence to show to what an extent this amount of infirmity is *brought into prison* by the criminals, and not acquired there.

In their report for the year 1868 the directors of convict prisons state that, "by returns made in April, 1869, it appears that of 5458 convicts who have passed the period when they were eligible for removal to public works, 1762 were invalids or incapable to such an extent as to be fit only for light labour, and 162 were permanent invalids hardly capable of any labour at all"— *i.e.*, 5 out of every 14 convicts (1 in 2·8) were invalids or fit only for light labour.   And in their report for the following year (1869) the directors tell us that "the number of men of weak or enfeebled constitutions received still continues to bear a large proportion to the total number, as many as 370 males out of 784 received at Millbank being of that character on reception."   And Mr. Gover, the Medical Officer of Millbank (Report for 1868) makes the following statement—"The great majority of the prisoners who were removed to Dartmoor and Woking as invalids were suffering on reception from the diseases or infirmities marked as the cause of removal. Many of them improved very considerably in the interval between their reception and removal; and I may here observe, with reference to the population of the prison as a whole, that the general tendency to improvement, which has previously been the subject of remark, has been as manifest during the year just ended as in former years.   Nevertheless, taking the population of this prison, exclusive of the imbeciles and epileptics, as affording a fair standard of comparison, I have reason to believe that the proportion of convicts afflicted with mental and bodily diseases is greater than would be found in any other section of the community."

In the Report for the year 1870, this important and trustworthy evidence is supplemented by the same authority as follows— "Many of the prisoners arrived in an enfeebled and emaciated

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[1] 'Results of Censuses of the Population of Convict Prisons in England.' 1875.

condition from the county and borough gaols, and suffered from
diseases not only rendering them unfit for discipline, but, in many
cases, necessitating their admission into hospital.   Tubercular dis-
eases, scrofula in various forms, indolent ulcers, and excessive
general debility, have been of frequent occurrence.   The number
of convicts (males) removed to other prisons as invalids was 341,
or 264 per 1000, calculated on the total population.   In but few
cases did the disease or infirmity, which was the ground of in-
validing, arise after transfer to this prison.   It will be seen from a
reference to Table IV, that 74, or 21·7 per cent., were upwards of
50 years of age."

In reference to the opinion that criminals as a class, are
essentially *ill-conditioned*, I may quote the following paragraph
from a former communication[1] to this 'Review'—"It is plain, then,
that a large proportion (probably over one-third) of criminals *bring
with them* into prison impaired constitutions.   This impairment or
defect manifests itself, first, in the form of positive disease or
infirmity; secondly, in a predisposition to certain forms of disease,
mostly of a constitutional or phthisical nature; and thirdly, in a
want of ability to resist the destructive influences of certain other
diseases which are more active and local in their character."

Coming more particularly to the diseases that necessitate in-
validing among convicts, it will be recollected that Dr. Guy showed
that 370 per 1000 male convicts were the subject of some infirmity
or defect (such as, I presume, would interfere to some extent with a
fair labour capacity).   Of these, 30 were mental and epileptic
cases—the relative proportion of mental to bodily cases of in-
firmity being as 1 to 11   Leaving out the *mental* aspect of the
question, I propose to show the nature of the *bodily* diseases
and infirmities of invalid convicts.   For this purpose, I have taken
1000 cases of men invalided to Woking; and as they are taken
consecutively, in the order of their reception, they form, I think, a
fair basis for judging the prevailing causes.

I have arranged them in eight classes, so as to give first of all
an idea of their general distribution.

Causes of Invaliding in 1000 Actual Cases.

1. Constitutional and general diseases and infirmities	363
2. Diseases of the respiratory system	255
3. „ circulatory system	112
4. Crippled and deformed	87
5. Diseases of the digestive system	59
6. „ urinary and generative system	38
7. „ brain and nervous system	18
8. Anomalous	68
	1000

[1] "Statistics of Mortality among Prisoners," 'Brit. and For. Med.-Chir.
Review,' July, 1872.

It is not to be forgotten that not unfrequently more than one disease was present—but the most important is entered in the above list.

The constitutional and general diseases and infirmities are necessarily the most numerous, as they have a wider scope and involve a greater variety of conditions. They include 176 cases of debility, 60 of struma and abscess, 40 of old age, 36 of defective vision, 19 of rheumatism. Syphilis shows only 8 cases, and I may state that this disease does not show itself so prominently as one would expect in such a class of men. No doubt, the presence of the taint is pretty frequently made out as a complication, but, *per se*, it cannot be said to prevail. And I think this may be taken as an evidence of the great value of regular, temperate, and wholesome living in preventing or restraining the progressive development of syphilis into its more active and loathsome phases.

Taking the various bodily "systems," it is found that the respiratory organs invalid a fourth, and the circulatory a ninth, of the number, while the other special organs show a much smaller proportion. The least numerous class is that of the brain and nervous system, which gives only 18 cases out of 1000. Of the 18, 12 were cases of paralysis. But I have found that although least frequent as a cause of invaliding, diseases of the brain and nervous system stand second in point of fatality among prisoners. Their sudden appearance, gravity, and often speedy ending, no doubt limits the possibility of invaliding such cases from one prison to another. Coming to the *individual causes* of invaliding the following are the most prevalent in the 1000 cases :—

Debility	176	300
Phthisis	124	
Bronchitis	90	219
Heart disease	80	
Struma and abscess	49	
Age	40	519
Hernia	39	
Asthma	30	

The five first mentioned in this list—viz., debility, phthisis, bronchitis, heart disease, and struma, contribute fully one-half of the cases. And the first two, debility and phthisis themselves, contribute 300 cases, and they undoubtedly constitute the most important element of consideration in the question of a convict's relationships. The lungs are the convict's weak point, and consumption is his bane. The occurrence of " *debility* " in convicts employed on public works is a signal of distress that should not be overlooked. It tells us that the pressure of his work is too

great for him, and it betokens a liability to break down under some active form of disease—usually inflammation of the lungs. The approach of debility implies a necessity for active interference on the part of the doctor.

Recurring, in conclusion, to the subject started early in this paper—viz. the influence of physical defect or disease in leading on to the commission of criminal offences, I give a list which shows the number of convictions recorded against the 1000 invalid convicts with whose diseases we have just been making ourselves acquainted :—

									Total.
231	were undergoing their		1st	sentence	.	.	.	.	231
152	,,	,,	2nd	,,	.	.	.	.	304
149	,,	,,	3rd	,,	.	.	.	.	447
125	,,	,,	4th	,,	.	.	.	.	500
85	,,	,,	5th	,,	.	.	.	.	425
77	,,	,,	6th	,,	.	.	.	.	462
46	,,	,,	7th	,,	.	.	.	.	322
30	,,	,,	8th	,,	.	.	.	.	240
15	,,	,,	9th	,,	.	.	.	.	135
12	,,	,,	10th	,,	.	.	.	.	120
7	,,	,,	11th	,,	.	.	.	.	77
6	,,	,,	12th	,,	.	.	.	.	72
8	,,	,,	13th	,,	.	.	.	.	104
3	,,	,,	14th	,,	.	.	.	.	42
5	,,	,,	15th	,,	.	.	.	.	75
2	,,	,,	16th	,,	.	.	.	.	32
3	,,	,,	17th	,,	.	.	.	.	51
2	,,	,,	18th	,,	.	.	.	.	36
1	,,	,,	19th	,,	.	.	.	.	19
1	,,	,,	22nd	,,	.	.	.	.	22
1	,,	,,	24th	,,	.	.	.	.	24
1	,,	,,	26th	,,	.	.	.	.	26
1	,,	,,	28th	,,	.	.	.	.	28
1	,,	,,	33rd	,,	.	.	.	.	33
1000 male invalids.					Total convictions	.			3827

3827 convictions among 1000 invalid criminals! Close upon an average of four convictions each. And these unhealthy beings are from a class who live and propagate their moral and physical degeneracy in our midst.

II.—Urea and its Relation to Muscular Force.

By Henry Brietzcke, F.R.C.S. Eng.

The experiments which have recently been conducted by eminent physiologists, with a view of deciding the question whether the urea contained in the urine is a product of the disintegration of muscular tissue during the active exercise of the human body, and may thus be taken as a measure of force expended in mechanical work, or whether this organic compound is mainly produced from, and regulated in amount by, nitrogenous food ingested, have led me to submit for publication a course of experiments recently prosecuted at Millbank Prison, through the kindness of Dr. Gover, the Senior Medical Officer, in hope that some facts which may · assist in the solution of the problem may be placed on record. The circumstances under which the experiments were carried out, were deemed to be unusually favourable for the following reasons : The men were strong and healthy. The amount of work performed could be accurately calculated, as it consisted in turning a crank. The exact quantity of food taken daily was also known. The whole of . the urine was easily collected. The experiments could be performed on a number of men, all under the same conditions, and continued for a considerable time.

The quantity of urea excreted was calculated according to the directions given in the most recent books on the subject, and the results are recorded rather as a collection of reliable data than an attempt to decide the question, or enter into any new theories. It will be seen, by referring to the tables, that six healthy men were taken, weighed at the commencement of each period, the daily amount of urine collected and tested for urea by the nitrate of mercury process described in Neubauer and Vogel's book on the urine ; the amount of nitrogen ingested was recorded ; the work performed calculated in foot tons raised, and the force producible by oxidation of the food consumed worked out according to Professor Frankland's tables. The observations extended over three periods of ten days each. The first during the performance of measured work, the second during rest on the same amount of food, and the third during rest on an increase of food. In most of the experiments performed by physiologists, the total amounts of nitrogen in the urine have been estimated, since it was supposed that without this calculation, the results would not be sufficiently accurate, as nitrogen might pass off in the urine uncombined with

urea; this was not done in the present series of cases. The relative amount of nitrogen, however, escaping as urea under the conditions of muscular work, rest, and rest with extra food, are worthy of record. The late Dr. Parkes, in his experiments, did not estimate the nitrogen escaping by the bowel, on account of its being present in so small a quantity, and probably derived from unassimilated food which had not entered the system. I will call attention to some of the points brought out by the present experiments, and then make a few comments on them as a whole. The average amount of urea excreted by a healthy man on a mixed diet is 500 grains in twenty-four hours; but this, according to Dr. Pavy, is subject to much variation, even when no work is done, and such was the case in my experiments; for instance, when at rest, on the usual diet F. S. excreted on an average for ten days only 349 grains in twenty-four hours, whereas the other five men all passed more than the average, as will be seen by the accompanying tables; a difference in the height or weight cannot account for this, for the second man, S. A., weighed about the same, and was the same height, yet he passed 219 grains more urea daily as an average for ten days; they were both much lighter than the remaining four, weighing only about nine stone each. A point, perhaps, worth mentioning is, that F. S. and S. A. were, at the commencement of the experiment, considerably below their usual weight, for they had been some weeks at crank labour, whereas the remaining four were subjected to experiment soon after their reception at the prison. It is well known that when the food is not sufficient to supply the tissue waste going on in the body, loss of weight takes place on account of the nitrogenous tissues being drawn upon, and urea continues to appear in the urine to the last, even during complete abstinence from food; neither of the six men sustained any appreciable loss of weight during their work, therefore, excessive tissue waste cannot account for the amount of urea excreted above the normal standard in all except F. S. The quantity of nitrogen and carbon contained in the diet is given in the tables : it was only during the first two periods that the daily average nitrogen ingested was 248 grains; during the last ten days on ½ lb. of extra bread per diem, the nitrogen ingested amounted to 292 grains; the force-producing value of the diet, calculated from Professor Frankland's tables, is 3615 foot-tons—that is, 3615 tons lifted one foot high. On comparing the nitrogen ingested with that excreted, it will be seen that F. S. excreted much less nitrogen than he ingested, and he did not lose quite so much weight as the rest; the others excreted more nitrogen than they ingested; it will be noticed also that F.S. and S. A. passed more urea during the second period on no work than they did during the ten days of hard work. I cannot explain this, but Dr. Pavy in his book notices this fact in Dr. Parkes'

second series of experiments. He remarks, page 54, " It is curious, and also, it must be owned, does not appear explicable, that during the periods of both rest and active exercise the daily amount of nitrogen eliminated was in excess of that eliminated during the first two periods of ordinary employment, the figures at the same time for the associated periods respectively agreeing very closely with each other." And now, with regard to the relation of the urea in the urine to the muscular force expended. It will be seen by these experiments, that in all six cases, except S. H., more urea was passed during the ten days of extra diet without work than during the work period. In S. H., I noticed during the last ten days on extra diet that the urine was loaded with crystals of uric acid, and I believe this accounts for the exception to the rule in his case. Uric acid is known to be a product of malassimilation; by some fault in the digestive process, the nitrogen falls short of the production of urea and uric acid is formed. In the last four cases, although more urea was passed during work than when no work at all was done, on the same diet, yet in every case, except the one mentioned, the diet had more influence over the amount of urea than the work, the addition of $\frac{1}{2}$ lb. of bread daily to the food producing a much larger excretion of urea during bodily rest than the heavy mechanical work on a reduced diet.

	Urea Gramme.		Urea Gramme.		Urea Gramme.
Exp. 1. F. S.		Exp. 2. S. A.		Exp. 3. J. B.	
1st. 10 days' average,		1st. 10 days' average,		1st. 10 days' average,	
work . . .	18	work . . .	35	work . . .	35
2nd. Do. do., no work	22	2nd. Do. do., no work	36	2nd. Do. do., no work	34
3rd. Do. do., no work,		3rd. Do. do., no work,		3rd. Do. do., no work,	
extra diet . .	23	extra diet . .	38	extra diet . .	37
Exp. 4. H. A.		Exp. 5. S. H.		Exp. 6. B. A.	
1st. 10 days' average,		1st. 10 days' average,		1st. 10 days' average,	
work . . .	42	work . . .	44	work . . .	35
2nd. Do. do., no work	39	2nd. Do. do., no work	38	2nd. Do. do., no work	34
3rd. Do. do., no work,		3rd. Do. do., no work,		3rd. Do. do., no work,	
extra diet . .	44	extra diet . .	39	extra diet . .	38

It has been stated recently that mechanical work has no influence upon the quantity of urea, but it undoubtedly had in four out of six of my cases; and I should think the small loss of weight which occurred during the work could hardly account for the increase of urea, on the theory that the tissues were drawn upon to supply a deficiency in nitrogenous food. In experiment 4, H. A. was laid up for three days out of the ten days' work period with an inflamed foot; this does not seem to have affected the quantity of urea; perhaps, however, the fact of his having slight febrile

symptoms during the attack, by temporarily augmenting the amount of urea from excessive tissue waste, obviated any diminution which might have otherwise occurred. I could not detect any material difference in the quantity of urea excreted during the Sunday day of rest in any of the six cases. In conclusion, I will remark that, besides the crank work, each man walked daily 1·54 miles, and on Sunday, during one hour's exercise, 3·08 miles ; this is added to the calculation of crank work in each case, in foot-tons raised. The crank work has been calculated by multiplying the weight in pounds required to bring down the handle, by the circumference of the circle described by it in feet, and then by the number of revolutions a minute, giving a product which is equal to the number of pounds raised one foot high per minute. The celebrated experiments of Fick and Wislicenus and others, tend to show that the urea in the urine is not a product of the disintegration of muscular tissue during mechanical work ; the non-nitrogenous alimentary principles supply the force by oxidation in the body, and the muscles are merely the instruments through which this force is converted into motive power ; the nitrogenous alimentary principles, as explained by Dr. Pavy, are " rendered applicable indirectly to force production, but instead of passing into a state of tissue, and thence by oxidation, giving rise to the evolution of force, they undergo (probably by the action of the liver) a splitting up into urea for the one part, which carries off the nitrogen as an unavailable element, and into a slightly oxygenated hydro-carbonaceous residue for the other, which may be looked upon as applicable in the same way as primarily ingested non-nitrogenous matter to force production." Nitrogenous food ingested forms the principle source of the urea in the urine during health. It has been proved that a strict adherence to non-nitrogenous food rapidly diminishes the quantity of urea passing off by the kidneys, and the accompanying tables tend to show that, although muscular work has some slight influence over the excretion of urea, nitrogenous food is by far its most important source. An American physiologist, Dr. Flint, while experimenting on the celebrated pedestrian Weston, found a very large increase of urea during prolonged muscular effort, but these results have been very much disputed, and every one interested in the subject will be anxious to hear the final results of Dr. Pavy's recent experiments, now being published, on the same man during his feats in London.

HENRY BRIETZCKE, F.R.C.S., &c.,
H.M. Convict Prison, Portsmouth.

December, 1876.

Experiment 1.—F. S—, æt. 24. Height 5 ft. 5½ in. Weight 123 lbs at commencement of experiment. Crank, 11,000 revolutions, 14 lbs pressure. Urine, neutral, no albumen. Eats all his food; sleeps well; does not sweat; drinks 1 quart of water daily.

Date. 1875.	Quantity of urine 24 hours. Cubic c.	Specific gravity.	Urea 24 hours. Gram.	Urea 24 hours. Grains.	Nitrogen excreted. Grains.	Body weight. lbs.	Foot tons raised per diem.
Sat., Dec. 11th	2750	1010	28·875	445·599	207·946	123	525
Sun. 12th	2575	1010	18·025	278·161	129·808	...	44
Mon. 13th	2700	1010	21·600	333·331	155·554	...	525
Tues. 14th	2125	1010	19·125	295·137	137·730	...	525
Wed. 15th	2635	1010	17·127	264·303	123·341	...	525
Th. 16th	2410	1010	19·280	297·528	138·846	...	525
Fri. 17th	2050	1009	14·350	221·449	103·342	...	525
Sat. 18th	2250	1010	19·125	295·137	137·730	...	525
Sun. 19th	2000	1010	16·000	246·912	115·225	...	44
Mon. 20th	1700	1010	14·450	222·992	104·062	...	525
1st 10 days' total	23195	...	187·947	2900·549	1353·584	122	...
Tues. 21st	1828	1015	25·592	394·935	184·303	...	22
Wed. 22nd	2700	1014	29·700	458·330	213·887	...	22
Th. 23rd	2600	1010	18·200	280·862	131·068	...	22
Fri. 24th	2470	1009	16·055	247·760	115·621	...	22
Sat. 25th	2060	1009	15·450	238·424	111·264	...	22
Sun. 26th	2290	1011	22·900	353·392	164·916	...	44
Mon. 27th	2450	1011	23·275	359·179	167·616	...	22
Tues. 28th	2480	1011	27·280	420·984	196·459	...	22
Wed. 29th	2640	1010	25·080	387·034	180·616	...	22
Th. 30th	2390	1011	22·705	350·383	163·502	...	22
2nd 10 days' total	23908	...	226·237	3491·283	1629·252	122	...
Fri. 31st	2065	1011	19·617	302·729	141·273	...	22
Sat., Jan. 1st, 1876	2070	1013	25·875	399·303	186·341	...	22
Sun. 2nd	2210	1015	27·625	426·309	198·933	...	44
Mon. 3rd	2000	1012	22·000	339·504	158·424	...	22
Tues. 4th	2400	1011	26·400	407·404	190·122	...	22
Wed. 5th	2400	1013	26·400	407·404	190·122	...	22
Th. 6th	2395	1013	26·345	406·556	189·721	...	22
Fri. 7th	2155	1015	23·705	365·815	170·708	...	22
Sat. 8th	2170	1012	20·615	318·130	148·455	...	22
Sun. 9th	1780	1012	16·910	260·955	121·773	...	44
3rd 10 days' total	21645	...	235·492	3634·109	1695·872	126	...
Average. 1st 10 days	2319		18·795	290·054	135·358	...	...
2nd 10 days	2390		22·623	349·128	162·925	...	...
3rd 10 days	2164		23·549	363·410	169·587	...	...

Action of heart, &c. . 2260 foot tons.   Diet represents . . 3615 foot tons.
11,000 revolutions . 503 ,,   Deduct . . . 2785 ,,
Exercise . . . 22 ,,
                                          Total . 830 ,,
        Total . 2785 ,,

Experiment 2.—S. A—, æt. 20. Height 5 ft. 5¾ in. Weight, 124½ lbs. at commencement of experiment. Crank, 11,000 revolutions, 14 lbs. pressure. Urine, neutral, no albumen. Eats all his food, sleeps badly, does not sweat ; drinks about a quart of water daily.

Date. 1875.	Quantity of urine 24 hours. Cubic c.	Specific gravity.	Urea 24 hours. Gram.	Urea 24 hours. Grains.	Nitrogen excreted. Grains.	Body weight. lbs.	Foot tons raised per diem.
Sat., Dec. 11th	1850	1020	33·300	513·885	239·813	124½	525
Sun. 12th	1870	1021	41·140	634·872	296·273	...	44
Mon. 13th	1300	1021	33·800	521·601	243·408	...	525
Tues. 14th	2130	1018	36·210	558·792	260·769	...	525
Wed. 15th	2140	1017	40·660	627·465	292·806	...	522
Th. 16th	2025	1016	33·412	515·613	240·615	...	525
Fri. 17th	1432	1023	34·368	530·416	247·498	...	525
Sat. 18th	1640	1020	34·440	531·478	248·023	...	525
Sun. 19th	1440	1024	36·720	566·663	264·442	...	44
Mon. 20th	1550	1020	34·875	538·191	251·155	...	525
1st 10 days' total	17377	...	358·925	5538·976	2584·802	124½	...
Tues. 21st	2470	1015	37·050	571·755	266·819	...	22
Wed. 22nd	1650	1020	37·950	585·644	273·300	...	22
Th. 23rd	2500	1013	33·750	520·830	243·054	...	22
Fri. 24th	2330	1016	40·775	629·239	293·640	...	22
Sat. 25th	2430	1016	38·880	599·996	279·998	...	22
Sun. 26th	1566	1021	37·584	579·996	270·661	...	44
Mon. 27th	2000	1017	39·000	601·848	280·862	...	22
Tues. 28th	2350	1017	36·425	562·110	262·313	...	22
Wed. 29th	2255	1016	36·080	556·786	259·828	...	22
Th. 30th	2490	1014	31·125	480·321	224·149	...	22
2nd 10 days' total	22041	...	368·619	5688·525	2654·624	127	...
Fri. 31st	1850	1020	37·925	585·258	273·115	...	22
Sat., Jan. 1st, 1876	2265	1019	38·505	594·209	277·297	...	22
Sun. 2nd	2120	1020	42·400	654·316	305·337	...	44
Mon. 3rd	1465	1021	39·555	610·412	284·859	...	22
Tues. 4th	2210	1019	43·095	665·042	310·352	...	22
Wed. 5th	2125	1020	40·375	623·067	290·754	...	22
Th. 6th	2155	1015	30·170	465·583	217·267	...	22
Fri. 7th	2350	1020	42·300	652·773	304·627	...	22
Sat. 8th	2420	1017	33·880	522·836	243·979	...	22
Sun. 9th	1845	1022	41·512	640·613	298·948	...	44
3rd 10 days' total	20805	...	389·717	6014·109	2806·535	128	...
Average.							
1st 10 days	1737		35·892	553·897	258·480	...	...
2nd 10 days	2204		36·861	568·852	265·462	...	...
3rd 10 days	2080		38·971	601·410	280·653	...	...

Action of heart, &c. . 2260 foot tons. Diet represents . . 3615 foot tons.
11,000 revolutions . 503 ,, Deduct . · . . 2785 ,,
Exercise . . . 22 ,,
                                      Total . 830 ,,
            Total . 2785 ,,

Experiment 3.—J. B—, æt. 18. Height 5 ft. 5 in. Weight 141 lbs. at commencement of experiment. Crank, 10,500 revolutions, 10 lbs. pressure. Urine, neutral, no albumen. Eats all his food except the gruel.

Date. 1876.	Quantity of urine 24 hours. Cubic c.	Specific gravity.	Urea 24 hours. Gram.	Urea 24 hours. Grains.	Nitrogen excreted. Grains.	Body weight. lbs.	Foot tons raised per diem.
Fri., Jan. 21st	1450	1025	42·050	648·915	302·822	141	368
Sat. 22nd	1100	1025	39·600	611·107	285·183	...	368
Sun. 23rd	625	1032	28·125	434·025	202·545	...	50
Mon. 24th	800	1034	34·400	530·860	247·729	...	368
Tues. 25th	820	1034	34·440	531·478	248·023	...	368
Wed. 26th	1100	1031	42·350	653·545	304·982	...	368
Th. 27th	910	1028	30·030	463·422	216·264	...	368
Fri. 28th	1113	1031	37·842	583·977	272·513	...	368
Sat. 29th	873	1034	31·428	484·996	226·325	...	368
Sun. 30th	1015	1031	36·540	563·885	263·146	...	50
1st 10 days' total	9806	...	356·805	5506·210	2569·532	139	...
Mon. 31st	540	1035	22·680	349·997	163·332	...	25
Tues., Feb. 1st	1250	1027	37·500	578·700	270·060	...	25
Wed. 2nd	1500	1021	30·750	474·534	221·449	...	25
Th. 3rd	1255	1020	32·630	503·546	234·983	...	25
Fri. 4th	1200	1025	34·800	537·033	315·430	...	25
Sat. 5th	1505	1023	37·625	580·629	270·955	...	25
Sun. 6th	965	1032	38·600	595·675	277·976	...	50
Mon. 7th	700	1036	30·100	464·503	216·757	...	25
Tues. 8th	1020	1030	37·740	582·403	271·788	...	25
Wed. 9th	1195	1029	38·240	590·119	275·384	...	25
2nd 10 days' total	11130	...	340·665	5257·139	2518·114	136	...
Th. 10th	715	1036	27·170	419·287	195·667	...	25
Fri. 11th	1200	1030	39·600	611·107	285·183	...	25
Sat. 12th	1745	1019	34·900	538·576	251·335	...	25
Sun. 13th	2215	1020	42·085	649·455	303·079	...	50
Mon. 14th	1425	1022	37·050	571·755	266·819	...	25
Tues. 15th	1870	1018	41·140	634·872	296·273	...	25
Wed. 16th	2350	1016	37·600	580·243	270·780	...	25
Th. 17th	1600	1020	35·200	543·206	253·496	...	25
Fri. 18th	2150	1014	34·400	530·860	247·734	...	25
Sat. 12th	2250	1020	42·750	659·718	307·868	...	25
3rd 10 days' total	17520	...	371·895	5739·079	2678·244	140	...
Average. 1st 10 days	980	...	35·680	550·621	256·953	...	...
2nd 10 days	1113	...	34·066	525·713	251·811	...	...
3rd 10 days	1752	...	37·189	573·907	267·824	...	...

Action of heart, &c. . 2260 foot tons.   Diet represents . . 3615 foot tons.
10,500 revolutions . 343 ,,   Deduct . . . 2628 ,,
Exercise . . . 25 ,,

Total . 987 ,,

Total . 2628 ,,

Experiment 4.—H. A—, æt. 18.   Height 5 ft. 9½ in.   Weight 156 lbs. at commencement of experiment.   Crank, 14,500 revolutions, 14 lbs. pressure.   Urine, faintly acid, no albumen. Eats all his food ; sleeps well.   This man was attacked with inflammation of the big toe-joint, from a badly fitting boot, and failed to do work or exercise for the 3 days—22nd, 23rd, 24th—of his first ten days ; he also was excused from daily exercise during the remaining part of the experiment.

Date. 1876.	Quantity of urine 24 hours. Cubic c.	Specific gravity.	Urea 24 hours. Gram.	Urea 24 hours. Grains.	Nitrogen excreted. Grains.	Body weight. lbs.	Foot tons raised per diem.
Fri., Jan. 21st .	1670	1026	51·770	798·914	372·821	156	691
Sat. 22nd .	2300	1017	40·250	621·138	289·859	...	no wk
Sun. 23rd .	1570	1024	48·670	751·075	350·491	...	,,
Mon. 24th .	1325	1027	45·050	695·211	324·426	...	,,
Tues. 25th .	2660	1017	42·560	656·785	303·917	...	691
Wed. 26th .	1800	1022	43·200	666·662	317·899	...	691
Th. 27th .	1550	1025	37·200	574·070	267·899	...	691
Fri. 28th .	1195	1030	43·020	663·884	309·812	...	691
Sat. 29th .	1500	1022	36·000	555·552	259·257	...	691
Sun. 30th .	820	1036	37·720	582·095	271·634	...	no ex.
1st 10 days' total .	16390	...	425·440	6565·386	3068·015	154	...
Mon. 31st .	1000	1033	48·000	740·736	345·676	...	no ex.
Tues., Feb. 1st .	1500	1028	48·000	740·736	345·676	...	,,
Wed. 2nd .	1600	1022	32·000	493·824	230·446	...	,,
Th. 3rd .	1700	1020	37·400	577·156	269·134	...	,,
Fri. 4th .	1255	1030	38·905	600·381	280·167	...	,,
Sat. 5th .	2015	1022	40·300	621·909	290·214	...	,,
Sun. 6th .	2227	1021	46·767	721·708	336·787	...	,,
Mon. 7th .	1100	1022	28·600	441·355	205·955	...	,,
Tues. 8th .	1900	1020	41·800	645·057	301·016	...	,,
Wed. 9th .	1060	1028	32·860	507·095	236·634	...	,,
2nd 10 days' total .	15357	...	394·632	6089·957	2841·705	151	...
Th. 10th .	1840	1022	47·840	738·266	344·524	...	no ex.
Fri. 11th .	1615	1021	41·990	647·989	302·394	...	,,
Sat. 12th .	1750	1023	43·750	675·150	315·070	...	,,
Sun. 13th .	1610	1022	43·470	670·829	313·053	...	,,
Mon. 14th .	2255	1021	46·435	716·584	334·405	...	,,
Tues. 15th .	1610	1025	46·690	720·520	336·246	...	,,
Wed. 16th .	1315	1029	40·765	629·085	293·573	...	,,
Th. 17th .	1670	1026	45·925	708·714	330·733	...	,,
Fri. 18th .	1555	1026	45·095	695·906	324·756	...	,,
Sat. 19th .	1500	1028	46·500	717·588	333·941	...	,,
3rd 10 days' total .	15720	...	448·460	6920·631	3228·695	151	...
° Average.							
1st 10 days .	1639	...	42·544	656·538	306·801	...	...
2nd 10 days .	1535	...	39·463	608·995	284·170	...	...
3rd 10 days .	1572	...	44·846	692·063	322·869	...	...

Action of heart, &c.   .   2260 foot tons.   Diet represents .   .   3615 foot tons.
14,500 revolutions   .   663   ,,   Deduct   .   .   .   2951   ,,
Exercise   .   .   .   28   ,,   ,,
                        Total   .   2951   ,,   Total   .   664   ,,

Experiment 5.—S. H—, æt. 21. Height 5 ft. 7 in. Weight 149 lbs. at commencement of experiment. Crank, 14,500 revolutions, 14 lbs. pressure. Urine, acid, no albumen. Eats all his food; sleeps well. During the last 10 days large quantities of uric acid crystals were passed.

Date. 1876.	Quantity of urine 24 hours. Cubic c.	Specific gravity.	Urea 24 hours. Gram.	Urea 24 hours. Grains.	Nitrogen excreted. Grains.	Body weight. lbs.	Foot tons raised per diem.
Th., May 25th	1875	1025	61·875	954·855	445·599	149	690
Fri. 26th	1659	1022	41·475	640·042	298·686	...	690
Sat. 27th	1700	1022	47·600	734·563	342·796	...	690
Sun. 28th	1114	1027	36·762	567·311	264·745	...	54
Mon. 29th	1651	1020	49·530	764·346	356·694	...	690
Tues. 30th	1420	1022	39·760	613·576	286·335	...	690
Wed. 31st	1024	1024	35·840	553·082	258·104	...	690
Th., June 1st	1077	1027	42·003	648·190	302·488	...	690
Fri. 2nd	1277	1025	45·972	709·439	331·077	...	690
Sat. 3rd	1400	1023	42·000	648·144	302·467	...	690
1st 10 days' total .	14197	...	442·817	6833·548	3188·985	146	...
Sun. 4th	1087	1026	38·045	587·110	273·984	...	54
Mon. 5th	1572	1020	45·588	703·514	328·306	...	27
Tues. 6th	2167	1017	43·340	668·822	312·116	...	27
Wed. 7th	2070	1017	39·330	606·940	283·238	...	27
Th. 8th	1850	1017	33·300	513·885	239·813	...	27
Fri. 9th	1830	1017	32·940	508·330	237·220	...	27
Sat. 10th	2277	1017	33·016	509·502	237·768	...	27
Sun. 11th	2239	1017	38·063	587·388	274·114	...	54
Mon. 12th	1361	1021	40·149	619·579	289·137	...	27
Tues. 13th	1952	1019	38·064	587·403	274·121	147¼	27
2nd 10 days' total .	18405	...	381·835	5892·473	2749·817	...	...
Wed. 14th	2150	1019	38·700	597·218	278·701	...	27
Th. 15th	2122	1017	33·952	523·916	244·494	...	27
Fri. 16th	2400	1017	36·000	555·552	255·590	...	27
Sat. 17th	2200	1019	37·400	577·156	269·339	...	27
Sun. 18th	2431	1014	26·741	412·667	192·577	...	54
Mon. 19th	1300	1029	46·800	722·217	337·034	...	27
Tues. 20th	2062	1019	47·426	731·878	341·543	...	27
Wed. 21st	1961	1020	43·142	665·767	310·691	...	27
Th. 22nd	1468	1020	39·636	611·662	285·442	...	27
Fri. 23rd	1866	1021	42·918	662·310	309·078	...	27
3rd 10 days' total .	19960	...	392·715	6060·343	2824·489	149	...
Average.							
1st 10 days .	1419	...	44·281	683·354	318·898	...	...
2nd 10 days .	1840	...	38·183	589·247	274·981	...	...
3rd 10 days .	1996	...	39·271	606·034	282·448	...	...

Action of heart, &c. . 2260 foot tons.   Diet represents . . 3615 foot tons.
14,500 revolutions . 663 ,,   Deduct . . . 2950 ,,
Exercise . . . 27 ,,
           Total . 2950 ,,
           Total . 665 ,,

Experiment 6.—B. A—, æt. 34. Height 5 ft. 4½ in. Weight 142 lbs. at commencement of experiment. Crank, 14,500 revolutions, 14 lbs. pressure. Urine, acid, no albumen. Eats all his food; sleeps well.

Date. 1876.	Quantity of urine 24 hours. Cubic c.	Specific gravity.	Urea 24 hours. Gram.	Urea 24 hours. Grains.	Nitrogen excreted. Grains.	Body weight. lbs.	Foot tons raised per diem.
Th., May 25th	1090	1031	41·420	639·193	298·290	142	688
Fri. 26th	1050	1029	38·850	599·533	279·782	...	688
Sat. 27th	868	1027	30·380	468·824	218·784	...	688
Sun. 28th	675	1034	35·100	541·663	252·776	...	50
Mon. 29th	860	1030	42·140	650·304	303·475	...	688
Tues. 30th	1000	1028	39·000	601·848	280·862	...	688
Wed. 31st	1052	1026	38·924	600·675	280·315	...	688
Th., June 1st	800	1031	34·400	530·860	247·734	...	688
Fri. 2nd	725	1033	37·700	581·786	271·500	...	688
Sat. 3rd	400	1033	18·000	277·776	129·628	...	688
1st 10 days' total .	8520	...	355·914	5492·462	2563·146	139½	...
Sun. 4th	805	1032	40·250	621·138	289·864	...	50
Mon. 5th	1400	1021	43·400	669·748	312·548	...	25
Tues. 6th	1540	1020	36·190	558·484	260·625	...	25
Wed. 7th	1647	1020	36·234	559·163	260·942	...	25
Th. 8th	1435	1020	30·135	465·043	217·020	...	25
Fri. 9th	1400	1020	32·200	496·910	231·891	...	25
Sat. 10th	1845	1018	32·287	498·252	232·518	...	25
Sun. 11th	1483	1017	27·435	423·376	197·575	...	50
Mon. 12th	1000	1023	31·500	486·108	226·850	...	25
Tues. 13th	1210	1024	37·510	578·854	270·132	...	25
2nd 10 days' total .	13765	...	347·141	5357·076	2499·965	141½	...
Wed. 14th	1445	1022	36·847	568·622	282·024	...	25
Th. 15th	1300	1023	31·200	481·478	224·689	...	25
Fri. 16th	1400	1023	33·600	518·515	241·973	...	25
Sat. 17th	1735	1021	34·700	535·490	249·895	...	25
Sun. 18th	1615	1021	35·530	548·298	255·872	...	50
Mon. 19th	1070	1028	38·520	594·440	277·405	...	25
Tues. 20th	1372	1025	43·904	677·526	316·179	...	25
Wed. 21st	1134	1029	43·092	664·995	310·331	...	25
Th. 22nd	1005	1031	38·190	589·348	275·029	...	25
Fri. 23rd·	1315	1030	46·025	710·257	331·453	...	25
3rd 10 days' total .	13391	...	381·608	5888·969	2764·850	142	...
Average.							
1st 10 days .	852	...	35·591	549·246	256·314	...	...
2nd 10 days .	1376	...	34.714	535·707	249·996	...	...
3rd 10 days .	1339	...	38·160	588·896	276·485	...	...

Action of heart, &c. . 2260 foot tons.   Diet represents .   . 3615 foot tons.
14.500 revolutions   .   663   ,,   Deduct   .   .   . 2948   ,,
Exercise   .   .   .   25   ,,
                                                         Total   667
         Total . 2948   ,,

	Age.	Weight. lbs.	Height. Ft. In.	Revol.	Pressr. lbs.	Foot tons raised.	Urine. Quantity. c. c.	Urea. Gram.	Urea. Grains.	Nitrogen excreted. Grains.	Nitrogen ingested. Grains.	Carbon ingested. Grains.
Exp. 1. F. S. .	24	123	5 5½	11000	14	525						
1st 10 days' average	...	122	...	...	...	...	2319	18·795	290·054	135·358	248	5036
2nd do. do. .	...	122	...	...	...	...	2390	22·623	349·128	162·925	248	5036
3rd do. do. .	...	126	...	...	...	...	2164	23·549	363·410	169·587	292	6023
Exp. 2. S. A. .	20	124½	5 5¾	11000	14	525						
1st 10 days' average	...	124½	...	...	...	...	1737	35·892	553·897	258·480	248	5036
2nd do. do. .	...	127	...	...	...	...	2204	36·861	568·852	265·462	248	5036
3rd do. do. .	...	128	...	...	...	...	2080	38·971	601·410	280·653	292	6023
Exp. 3. J. B. .	18	141	5 5	10500	10	368						
1st 10 days' average	...	139	...	...	...	...	980	35·680	550·621	256·953	248	5036
2nd do. do. .	...	136	...	...	...	...	1113	34·066	525·713	251·811	248	5036
3rd do. do. .	...	140	...	...	...	...	1752	37·189	573·907	267·824	292	6023
Exp. 4. H. A. .	18	156	5 9½	14500	14	691						
1st 10 days' average	...	154	...	...	...	...	1639	42·544	656·538	306·801	248	5036
2nd do. do. .	...	151	...	...	...	...	1535	39·463	608·995	284·170	248	5036
3rd do. do. .	...	151	...	...	...	...	1572	41·846	692·063	322·869	292	6023
Exp. 5. S. H. .	21	149	5 7	14500	14	690						
1st 10 days' average	...	146	...	...	...	...	1419	44·281	683·898	318·898	248	5036
2nd do. do. .	...	147¾	...	...	...	...	1840	38·183	589·247	274·981	248	5036
3rd do. do. .	...	149	...	...	...	...	1996	39·271	606·034	282·448	292	6023
Exp. 6. B. A. .	34	142	5 4½	14500	14	688						
1st 10 days' average	...	139½	...	...	...	...	852	35·591	549·246	256·314	248	5036
2nd do. do. .	...	141½	...	...	...	...	1376	34·714	535·707	249·996	248	5036
3rd do. do. .	...	142	...	...	...	...	1339	38·160	588·896	276·485	292	6023

Chronicle of Medical Science.

REPORT ON SURGERY.
By W. JOHNSON SMITH, F.R.C.S.,
Surgeon to the Seamen's Hospital, Greenwich.

On Surgical Fever after Antiseptic Operations.—Dr. Benno Credé holds that in many of the instances in which, after operations performed under antiseptic conditions, there is high fever which cannot be accounted for by local changes, this febrile state is the result of chilling from prolonged exposure of an extensive surface to the cold spray. In order to determine the degree of chilling produced by the antispeptic spray this surgeon made the following observations on two patients who were similarly affected with large lymphglandular tumours in the inguinal region. The enlarged glands were enucleated and extirpated under antiseptic conditions on the same day, and in the same room the temperature of which was maintained at 52° F. The first patient was uncovered from over the lower margin of the last rib to the middle of the thigh, and on the whole of the surface were directed two carbolic acid sprays, the temperature of which was about 52° F. From a thermometer fixed in the right axilla the temperature was read off every five minutes during the operation. At the commencement of the operation it was 99·2° F.; twenty minutes later, when the dressings were being applied, it was 97·6° F. The pale, prostrate, and cold patient was then placed in a warm bed at noon, and one hour after the operation the temperature was 97° F.; in the evening it was 98·8° F. On the following day the patient complained of headache and was chilly, the morning temperature being 100·4° F., the evening temperature 100.8° F. On the the third day the condition of the patient was normal. In the second case the buttocks and lower limbs of the patient were covered by wadding and flannel bandages, and only the seat of operation was left exposed. The fluid of the spray was warmed. At the commencement of the operation the temperature of the body was 98° F. At the conclusion of the operation, which lasted for twenty-two minutes, it was 98·8° F. On the following day the temperature and general condition of the patient were quite normal. In addition to these, other cases are recorded, in order to show the chilling effect of the antiseptic spray. The author states that he is not able from his small experience to assert positively that in every antiseptic operation in which the patient is not carefully protected, a depression of temperature will take place. He allows

that the extent of surface exposed and the individual capacity of re-sistance may vary in different cases. It is shown, however, that chilling may and often does take place after antiseptic proceedings. The spray acts most injuriously in this way when it is applied to the head, trunk or abdominal cavity. When used in the dressing of large wounds of the extremities it may also cause general chilling, especially if the application be prolonged and a considerable surface be exposed and played upon. So long as this chilling is of slight degree the result to the patient is not serious, but if it consist in a lowering by several degrees it may, especially in a patient debilitated by loss of blood, lead to a fatal termination of the case.—*Centralblatt fur Chirurgie, No.* 12, 1877.

On the Value of Sponges in Surgical Dressing.—Dr. Charles B. Brigham, of San Francisco, recommends the use of clean sponge as an application to open surfaces, and thinks that there are several reasons why this new dressing should be of value. " In the first place," according to the author, " sponges having a remarkable resistance to decomposition allow the dressing to remain untouched for a certain length of time ; secondly, absorbing liquids with great facility and in considerable quantity, they keep the wound dry, and thus favour union by first intention ; thirdly, they are so elastic that they make an equal and continuous pressure about a wound, and not only prevent pus collections from forming, but by pressing gently, surface to surface, they favour immediate union." Peculiar freshness of a wound has often been remarked by Dr. Brigham after the removal of a sponge dressing that had been retained for a week or longer. The discharge from the surface of the wound is trans-mitted by the sponge and deposited in the outside bandage. The wound, it is stated, remains free from all the the excoriations which are so frequent with charpie or cotton-wool dressings. To those who have had any experience of sponge as a dressing two objections to the agent will at once occur. In the first place, there is the close adhesion between the sponge and the surface of the wound to which it has been applied, which causes, at every change of dressing, much pain and free bleeding ; secondly, there is the offensive odour. On this latter point Dr. Brigham remarks, that though it may be objected to the use of sponges in dressings that the odour is offen-sive after the sponge has been left for some time in the wound, it depends a great deal upon the amount of the discharge whether there be any odour or not. In cases of immediate union of the sides of the wound, it is asserted, a sponge will be inoffensive, no matter how long it may remain in place, and in cases where there had been offensive odour it was not found that this had any influence on the appearance of the wound or on the condition of the patient.— *Surgical Cases,* 1876.

On Trephining for Injuries to the Head.—Dr. S. Pozzi, of Paris, in a recent memoir on ' Cerebral Localisations, and on the Relations of the Cranium to the Brain, with regard to Trephining,' has sup-plemented a very clear description of the cerebral convolutions and fissures by some remarks on the bearing of these anatomical data on

the operative treatment of head-injuries. After an allusion to the differences of opinion amongst French surgeons as to the expediency of trephining in cases of head-injury in which there is persistent hemiplegia, with an absence of any external lesion, he puts the question whether the surgeon in the application of the trephine can be better or equally well guided in determining the precise seat of the operation by internal in contradistinction to external signs, and whether, disregarding fractures, depressions, wounds, and local signs of contusion and inflammation, he should rather search for a collection of functional signs sufficiently pathognomonic to supply the absence of a local lesion ; or, if such exist, to be put in the balance with it, and to modify to some extent the determination of the point to which he might apply the trephine. If similar results occurred in a wounded man as in an animal under experimentation, the surgeon would often find a valuable auxiliary in his knowledge of cerebral localisations. According to the appearance of disturbance in this or that group of muscles, he might determine with precision what particular motor centre had been injured. Unfortunately, things are not so simple on the bed of the patient as they are in the laboratory, as the surgeon cannot control the disturbing phenomena. It is especially to cases of recent traumatism, and to such as require primary trephining, that these remarks apply. On reading the reports of such cases one will be struck with the extent and diffusion of the symptoms and their erratic character under the influence of concomitant concussion and contusion. Cases in which there is a record of localised convulsions, or circumscribed paralysis, are very exceptional. If, as very often happens, partial convulsions or hemiplegia appear in a more or less rapid manner, those symptoms cannot be regarded as pathognomonic. Indeed, such disturbances of motility have been observed, rarely, it is true, but still positively, after injuries to the temporal and occipital regions which are not in immediate relation with the motor centres. In a case communicated by M. Marvaud to the Société de Chirurgie, a man who had received a gun-shot wound in the left temporal region and a compound fracture of the squamous portion, presented, first, convulsive movements of different parts of the body, especially the lower limbs, afterwards right-sided hemplegia and aphasia, and finally, after an interval of some days, prolapse of the upper eyelid, external strabismus, and dilatation of the pupil on the left side. The first set of symptoms indicated, according to theory based on recent anatomical and physiological data, trephining over the summit of the fissure of Sylvius; and the second set indicated trephining over the third frontal convolution on the left side. M. Marvaud, on the appearance of the third set of symptoms, trephined at the seat of fracture, that is to say, the temporal bone, and with complete success. Immediately after the operation the patient opened his eyes, the prolapse of the left upper eyelid disappeared, the right hand, previously paralysed, regained its motility, and finally, the patient, in despite of all theory, made a good recovery. This case indicates that the primary symptoms, and

those that occur during the first few days, represent complex lesions due to the propogation to a distance of the effects of traumatism. The circumstances on which are based the indications for secondary trephining are not affected to any great degree by disturbing phenomena. The lesion that gives rise to the symptoms (intracranial suppuration) is evolved slowly and independently, and its effects consequently present a simplicity through which they resemble, up to a certain point, the results of experiments on animals. Indirect experimentation supplied by clinical observation has not, as yet, done much to establish, in the case of man, the certain existence and the exact topography of the motor centres that have been recognised in animals. But one region of the cerebrum can at present be distinguished, viz. the third frontal convolution on the left side,—the so-called convolution of Broca. This is the only region of the surface of the cerebrum, lesion of which can be diagnosed with sufficient certainty to enable the surgeon in his inquiry to be guided solely by rational signs. In a remarkable case reported by M. Broca, the trephine was applied over a purulent collection about the third frontal convolution, symptoms indicating the nature and seat of the lesion having appeared on the twenty-ninth day after an injury to the head. The condition of the patient was much improved for some hours after the operation, but in the course of the same day he became comatose, and died. The autopsy revealed inflammatory softening of the third frontal lobe, and signs of diffuse meningo-encephalitis, which had doubtless started at the seat of the abscess, between the bone and dura mater, near the convolution of Broca. Trephining, as M. Broca states in his report, was resorted to at too late a period, the abscess having already set up irreparable complications. The evacuation of the pus had produced but a temporary amelioration, and the patient succumbed to a diffuse meningo-encephalitis, which could not be removed. According to M. Pozzi, this is the only case that has hitherto presented a real application to surgical therapeutics, of our present knowledge of cerebral localisations, and of cranio-cerebral topography. Recent cases, reported by MM. Proust and Terrillon, and by M. Lucas Championnière, are regarded by M. Pozzi as instances in which the surgical treatment and the seat of trephining were indicated rather by external lesions, such as a wound and fracture, than by any observed relation of symptoms to cerebral localisations. In conclusion, it is argued that, with regard to the treatment of injuries to the head, we cannot, in the present state of our knowledge, expect physiology to throw any light on clinical surgery, but rather the reverse. "A day will come, M. Pozzi believes, when our scientific knowledge will occupy a higher place than it does at present in the art of healing, and then will the surgeon, instead of giving simple relations of his successes or his failures as *contributions to the study of cerebral localisations,* be able to affix to them this legitimately ambitious title—*trephining guided by the localisations of the cerebrum.*"—*Archives Générales de Médecine,* April, 1877.

On Wounds of the Brain from the Auditory Canal.—In a paper

read at the fifth Congress of the German Society of Surgery, held at Berlin in April, 1876, Professor Roser, of Marburg, communicated the results of some investigations that had been made by him in order to determine the topographical relations of the auditory canal to the brain. The object of such investigations was to throw some light on those cases of injury to the head, in which, notwithstanding a discharge of cerebro-spinal fluid from the ear and an extrusion of small portions of brain-substance, no other serious symptoms result, and the patient makes a good recovery. The author reports four kinds of variation in the above-mentioned anatomical relations. The partition between the auditory canal and the cranial cavity may be unusually thin, or it may be much increased in thickness, being there composed of spongy bone. In some instances the tympanic cavity was found to be extended upwards and outwards over the roof of the meatus, and in others this roof contained air-cells in communication with the tympanum. The author's observations show that the roof of the auditory canal is in close proximity with the middle cranial fossa and the inferior temporal convolution of the cerebrum, and that but little force is required to penetrate the skull in this region and to wound the meninges and the brain. It is shown also that the middle cranial fossa may be perforated and the middle cerebral lobe wounded through the ear. The author points out that the prognosis in cases of head injury, with aural discharge of cerebro-spinal fluid and extrusion of brain substance, is not so unfavorable as it is generally supposed to be, since these symptoms do not necessarily indicate extensive fracture of the petrous process and of the base of the cranium. This conclusion had been previously derived from clinical observation. Three cases of head injury had been observed by Professor Roser, in which there was a discharge of cerebro-spinal fluid from the ear, and one case in which there was extrusion through the meatus of cerebral substance, in all which cases recovery took place, contrary to expectation. In one of these cases it was found, on examination by means of the aural speculum, that the tympanic membrane remained intact, and that in the roof of the meatus there was a small penetrating slit. Two other cases of recovery from head injury after discharge of brain substance from the ear, are briefly recorded. The author alludes to the possibility of mistaking in such cases blood-clot for brain-matter, and he asserts at the same time that the objection sometimes made, that the matter extruded cannot be brain-tissue, when there is no discharge of cerebro-spinal fluid, does not hold good, since in some undoubted cases of injury to the brain this co-existence has failed. Professor Roser allows that the surgeon cannot expect to find a perforation of the same part of the skull-wall in every case of injury to the head with discharge of cerebro-spinal fluid and of brain substance. There is, probably, it is admitted, a lesion of typanic membrane in the majority of such cases. Variety in the nature and situation of the lesion is the more to be expected if it be considered that in the comparative study of many crania much difference will be found in the structure and thickness of the

osseous parts usually involved. In most individuals the wall of the skull about the membrana tympani is very thick; in very few of two hundred crania examined by the author was the bone at this situation found to be translucent. It is thought that a discharge of cerebro-spinal fluid may be due, in some instances, to traumatic diastasis of the petro-squamosal suture. There may be, as is well known, an aural discharge when the lesion consists in but an inconsiderable fissure of bone with rupture of the meninges. The author thinks it possible that the discharge may exist independently of any direct lesion of bone, and that the cerebral meninges having been ruptured and the surface of the bone exposed, the cerebro-spinal fluid may transude into the auditory canal. As in osteo-myelitis of a long bone, the medullary fat may be forced into the caniculi, so, thinks Professor Roser, may cerebro-spinal fluid, through intracranial pressure, be driven through the pores of a cranial bone.— *Von Langenbeck's Archiv fur klinische Chirurgie*, Bd. xv, Heft 3.

On Ranula.—In a contribution giving short clinical reports of six cases of ranula observed by himself, Prof. Michel, of Nancy, discusses the nature and situation and the surgical treatment of this form of new growth. In each of these cases excision of the cyst was practised with complete success. From observations made during these six operations, and also from dissection of a ranula in a dead subject, the author has been convinced that, in the majority of instances of this affection, the cyst in its development has no connection with any of the salivary ducts. The view that ranula may be due to dilatation of the ducts of the sublingual or submaxillary glands is not altogether rejected; but it is held that, in the majority of cases, the cyst has some other seat of origin. In all the seven specimens examined by the author there was an absence of any connection between the cyst and the salivary canals, and in each case the tumour had evidently originated in the areolæ of the connective-tissue about the frenum of the tongue. The so-called capsule of Fleischmann, fluid distension of which is supposed by Tillaux and other French surgeons to constitute ranula, consists, according to Prof. Michel, in nothing more than an occasional and abnormal dilatation of one or more of the areolæ of the sublingual connective tissue. On microscopical examination of the contents of the cyst in the above-mentioned seven cases, tesselated epithelium and crystals of cholesterin were found in some, and globular epithelium in others. In no specimen was the author able to obtain a reaction resembling that produced by saliva. Prof. Michel holds that extirpation by the knife ought to be regarded as the general method of treatment for ranula; and he argues that this proceeding, first recommended by Heister, is free from many of the objections that have been raised against it by Sedillot. Far from being an impracticable operation in ordinary cases of ranula, it may, even in cases of severity and long duration, be readily and safely performed. Excision, though more difficult than the usual methods of surgical treatment, such as injection of of iodine, batrachosioplasty, and incision and cauterization combined, is attended with speedy as well as with most permanent results. No relapse had occurred in any of the six cases treated by

the author, five of which have been under his observation from time to time during many years. Two methods of extirpation are mentioned; in one, the ranula is first freely incised and the walls of the emptied cyst then dissected away; in the other, the cyst is removed intact, together with its contents. The choice between one and the other of these methods should be guided by the thickness of the cyst-walls. When this wall is thin, preliminary incision is to be preferred; when it is thick, extirpation without incision should be practised.—*Gazette Hebdomadaire*, No. 16, 1877.

On Rupture of the Œsophagus.—Dr. Reginald H. Fitz has recently recorded at much length and with interesting comment, a case treated by Dr. George Allen, of Boston, which is regarded as one of exceptional importance, from its proving, as is believed, that a previously healthy œsophagus may be suddenly ruptured by muscular action. The patient was a man aged 31 years, debilitated through excessive use of alcoholic stimulants, and subject to frequent attacks of gastritis. He was quite free from stricture or ulceration of the œsophagus. One evening, after having by long-continued and violent efforts expelled a large piece of tough, gristly meat, which had remained in his throat for about three hours, he ejected from the mouth a small quantity of blood unmixed with food or air, and became very prostrate. Emphysema was soon observed in the upper part of the neck, and this swelling spread rapidly downwards. On the following morning the whole neck and the upper part of the chest were swollen, and in the course of the following day the subcutaneous cellular tissues of the whole body had undergone a process of inflation. The patient remained in a state of prostration; he complained of no pain save slight tenderness, on pressure, over the left side of the trachea, and was able to swallow fluids without any difficulty or uneasiness. On the fifth day he had delirium tremens, and on the eighth day from that of the commencement of his disease he died after several severe attacks of tetanic convulsions. At the autopsy, made forty-eight hours after death, there was found a longitudinal rent of the œsophagus, two inches in length, extending through all its coats in front and to the right, opposite to and below the bifurcation of the trachea. The edges were sharply defined, and there was no evidence, microscopic or otherwise, of pre-existing ulceration or disease of the œsophageal wall. There was much emphysema near the surface of the left lung and in old adhesions between this organ and the chest-wall, indicating rupture of the air-passages on the left side. The author, after careful consideration of the clinical reports of supposed rupture of the healthy œsophagus, to be met with in surgical literature, has come to the conclusion that the number of cases of this lesion may be reduced to two. Most of those statements, it is held, are based upon errors of observation, insufficient testimony, and superficial generalisation; and two clinical records only, viz. that of Dr. Allen, given in this contribution, and one published in 1856 by Meyer, are to be regarded as those of indisputable cases from which a knowledge of this class of spontaneous ruptures can be obtained. Dr. Fitz next gives a statement, based on

these two reports, of the conditions and symptoms of the lesion called by him "spontaneous rupture of the œsophagus." This lesion, however, as represented in each of these cases, is really a compound one, rupture of the air-passages playing a far more important part than rupture of the gullet in the production of symptoms and in forming the whole character of the affection. For the occurrence of rupture of the healthy œsophagus, as understood by Dr. Fitz, there are two essential factors: first, impaction of a foreign body in the œsophagus; secondly, the exercise of great muscular force in the attempts to remove this. The rupture takes place between the bifurcation of the trachea and the diaphragm, in the anterior or lateral wall of the œsophagus, and corresponds in direction with the long axis of this tube. Such rents lie wholly within the thoracic cavity, and are from one to two inches in length. "There is no good reason," the author states, "for considering that the act of vomiting can in any way produce this result, nor is it essential that the foreign body should remain in contact with the œsophageal wall long enough to give rise to inflammation from pressure. The fact of muscular action alone being sufficient as the active agent, is of considerable value from a medico-legal point of view, in those cases where the introduction of a probang or a bougie may be asserted as the cause of the rupture." In both the cases on which the author relies there was intense anxiety, associated with impaction of a foreign body in the gullet, and violent straining efforts were made to expel the fixed mass by the action of the respiratory muscles, the chest having been fully inflated. In both cases, also, rupture of the air-passages was indicated by hæmoptysis and emphysema, and laceration of the œsophagus by the regurgitation into the mouth of unmixed blood. Pain is not a very prominent early symptom; when present, it is referred to the region of stomach. What suffering there may be of this nature is due entirely to injury of respiratory organs and subsequent pleurisy. Nausea and vomiting are not constant or permanent symptoms. A small quantity of blood is occasionally ejected by the œsophagus. Fluids can be swallowed without pain. The lesion terminates in death, after gangrene of the posterior mediastinum and gangrenous pleurisy. The tetanic attack in Dr. Allen's case is attributed to inflammation in the posterior mediastinum, involving the spinal nerves.—*American Journal of Medical Sciences*, January, 1877.

On Gastrotomy.—M. Lanelongue, of Bordeaux, has recently communicated to the French Academy of Medicine the following report, with comments on a case of cancer of the œsophagus, in which gastrotomy was performed:

"A man, aged 59 years, without hereditary antecedents of disease, and previously in good health, was suddenly taken with difficulty in deglutition, which continued to increase in degree, so that at the time of his admission into hospital, six months from the commencement of the disease, he was but just able to swallow small quantities of milk. It could be made out that near the middle of the thoracic portion of the œsophagus there was a very resistent and absolutely

impassable obstacle. There was extreme emaciation, but no cachetic tint. All the other organs of the body were healthy. To prevent death from inanition it was found that no treatment short of gastrotomy could be effectual. The operation was performed with strict observance of all the rules laid down by M. Verneuil in a communication to the Academy of Medicine in April, 1876. The immediate results of the operation were satisfactory. The patient remained free from pain and inflammatory phenomena, and was fed regularly through the fistula, which, however, allowed a considerable quantity of fluid to escape. After a time, however, thoracic symptoms supervened, and the patient ultimately died on the twenty-sixth day from that of the operation. At the autopsy it was found that the primary lesion (epithelioma) of the œsophagus had caused a bronchial perforation, and led to fatal phenomena of asphyxia. The stomach was found to be firmly fixed to the abdominal wall, and the gastric fistula well established.

" *Conclusions.*—1. Gastrotomy is a rational operation, based on the history of gastric wounds and fistulæ formed experimentally in animals, and established accidentally in man. 2. It is indicated whenever death from inanition is rendered imminent through aphagia. 3. The operation should be performed in exact conformity with the rules laid down by M. Verneuil, who insists on this among other points: that the stomach should not be opened until it has been well fixed to the abdominal wall by the careful and close application of many sutures. The surgeon may thus prevent effusion, whether primary or secondary, into the peritoneal sac. 4. In incising the integuments the surgeon should not carry his knife below the inferior margin of the eighth costal cartilage on the left side. In consequence of long abstinence, the stomach of a patient on whom gastrotomy is performed is always retracted and elevated towards the diaphragm. 5. The anterior wall of the stomach should be opened near the small curvature, so that the secreted and injected fluids may find a space in which to accumulate, and be thus prevented from running away externally. 6. The operator should avoid applying foreign bodies, such as hæmostatic forceps to the margins of the gastric orifice. These may give rise to laceration and sloughing, and lead to undesirable enlargement of the fistula."—*Bulletin de l'Academie de Médecine*, No. 15, 1877.

On Injuries of the Subcutaneous Structures of the Lower Extremity.

The following conclusions are given at the end of a memoir by Prof. Verneuil, on the severe forms of injury to subcutaneous soft structures of the leg (*coups de fouet*):—

" 1. Under the influence of an extension movement of the foot made suddenly and unexpectedly, and with more or less violence, rupture may be produced of the subcutaneous soft parts of the posterior region of the leg. 2. These ruptures, which vary in extent and situation, within the limits of the region, do not always involve the same tissues. There is no doubt that tendon in some cases, muscular aponeurosis in some, and fleshy parts in others, and even two or all of these parts together, may be involved. In some cases,

vessels and nerves are at the same time ruptured. 3. Notwithstanding the undoubted diversity of the anatomical seat and of the histological lesions, the mode of production and the immediate phenomena present a sufficiently close similarity to justify surgeons in confounding all the varieties, save in the case of complete ruptnre ot the tendo Achillis, which has been described apart, and applying to these the common denomination of sprain (*coup de fouet*). This term, like many vulgar appellations, has its advantages and its inconveniences ; it may be preserved for a time, but sooner or later patholological anatomy and precise diagnosis, based on anatomy, will cause it to be removed from surgical nomenclature. 4. The secondary phenomena, on the other hand, vary in a marked degree, according to a multiplicity of conditions, such as extent, situation, histological nature of the lesion, antecedent conditions of the parts of the injured region, constitution of the patient, and character of the treatment. Hence the necessity for the recognition of forms that are distinct from the first, or that subsequently become distinct through the supervention of complications. 5. Of these conditions one of the most important, though least known, is, without doubt, the existence in the posterior region of the leg of deep-seated varices, and especially of intra-muscular venous dilatations. These conditions fully account for certain unfavorable symptoms which more or less retard recovery, such being large effusions of blood, circumscribed hæmatomata, considerable and persistent œdema, aggravation of persistent and previously disregarded phlebectasis. 6. This condition of varicosity accounts especially for the development of formidable symptoms, having as their starting-point and as the seat of their ulterior development the venous system. The names of these morbid phenomena will at once indicate their gravity —extensive thrombosis, simple or double phlegmasia, alba dolens, embolism, phlebitis and pyæmia. The occurrence of these phenomena, which cannot readily be admitted as consequences of simple rupture of tendon or muscle and of injury of healthy tissues, can at once be explained when we know that a deep-seated varicose net work of the leg has been involved in the primary lesion, and that through phlebectasis of the lower limbs the muscles are riddled by enormously dilated veins, with thin and tender walls. 7. It will probably be demonstrated, ere long, by pathological anatomy, that a certain form of sprain consists exclusively in rupture of an inter-muscular or intra-muscular venous dilatation. 8. The preceding conclusions, in showing the pathology of the morbid phenomena may be taken to explain and justify the excessive but well-founded fears of surgeons of old. The prognosis is assisted, and we learn how great a part is played by antecedent pathological conditions in the course and termination of local lesions apparently of but little consequence. 9. Practical surgery may also profit through these conclusions. The surgeon, when called to treat an injury of the kind described ought carefully to determine whether varices do or do not exist. In case of an affirmative, he should endeavour to prevent and to contend against thrombosis, or to

ought, as a rule, to abstain from any mechanical action that might excite irritation."—*Archives Générales de Médecine,* February, 1877.

On Sympathetic Ophthalmia.—Dr. Adolf Alt, of New York, in a paper 'On the Anatomical Causes and the Nature of Sympathetic Ophthalmia,' presents statistics of 100 enucleated eyes. These statistics, it is stated in the conclusion, show the cause of the sympathetic affections, necessitating enucleation, to be traumatisms in 83½ per cent., leaving 16½ per cent. of the cases attributable to idiopathic inflammation. In 63¾ per cent. of the injured eyes there were injuries in the hard membranes, with incarceration of a part of the uveal tract. Two thirds of the eyes destroyed by idiopathic inflammation presented ulcerations of the cornea, with incarceration of the iris, or the formation of staphyloma. The proportion of eyes which had caused sympathetic troubles without presenting cicatrices in the hard membranes was about 5 per cent. In 44½ per cent. of the cases of injury to the hard membranes no part of the uveal tract was incarcerated in the wound. Cicatrices and foreign bodies in the ciliary body, or incarceration of it, were found in 17⅓ per cent. of the cases, while the affections of the ciliary body altogether amounted to 76⅓ per cent. The iris was found changed in 68 per cent., and the choroid in 73 per cent. Almost the same number of changes is thus shown in each of the parts of the uveal tract, with only a slight percentage in favour of the ciliary body. The retina was affected in 73 per cent. of the cases, and in 58 per cent. of these the membrane was detached.

" What part," the author asks, " transmits the affection from one eye to the other ? If it were the ciliary nerves, diseased in structure or function, V. Graefe's proposition to divide them would have led to a favorable result. If it were the inflamed optic nerve and retina the section of the optic nerve would have proved more successful. From the preceding statistics I conclude that the views of many earlier authors, *i. e.* that the optic nerve plays a great part in the transmission of sympathetic ophthalmia, is strongly sustained, and its action in this respect is attributable as much to inflammatory as to functional changes. Transmission of inflammatory changes appear quite possible, if we bear in mind the semi-decussation of the optic nerve fibres in the chiasm, as demonstrated by the cases of Hirschberg and others, who all, moreover, gave the supposition of functional changes a very plausible explanation ; but it seems to me that he underrates the influence of the optic nerve. The transfer of affections by the ciliary nerves can only be accomplished by reflex action, since their tissue possesses a great power of resistance. It appears to me that the entire nervous apparatus of the eye has the power of transmission ; and even the influence of the sympathetic system must not be left out of view in this consideration. In more than 99 per cent. of the cases we have to deal with changes in the vascular membrane of the eye, consequently with disturbances of the entire circulatory system, it is hardly presumable that these disturbances should not exert an influence upon the sympathetic fibres of the uveal tract."

"The individual varieties of sympathetic affections in the second eye are certainly nothing but difference of degree. There can be no incontrovertible assertion that the locality of the primary affection would determine a certain fixed kind of affection in the other eye. Still it is worth mentioning that in 13 of the cases where eyes were enucleated for sympathetic *irido-choroiditis*, the other had been lost by *panophthalmitis purulenta*, and that of the five enucleated for *neuroretinitis sympathetica*, three showed retinal detachment."

The following practical deductions are drawn from the author's statistics and foregoing remarks:—(1) The entire nervous apparatus of the diseased eye participates in the transmission of the affection to the other. (2) Scars in the hard membranes, whenever combined with alterations in the other parts of the eye, particularly in the uveal tract, retina, or optic nerve, are capable of calling forth sympathetic affections of the other eye at any time. (3) Purulent panophthalmitis makes no exception. (4) Consequently its artificial production, as a prophylactic measure against sympathetic affection, is reprehensible. (5) The time at which sympathetic affections most frequently manifest themselves varies between seven days and eight weeks after the beginning of the disease of the first eye. (6) As soon as the first trace of a sympathetic affection manifests itself (a diagnosis which should be made with the greatest care) therapeutic measures should be resorted to. (7) The only efficient means consists in enucleation of the eye first affected. (8) When it is fairly presumable that the affection has extended to the optic nerve, the removal of a large piece of the nerve, together with the globe, should not be omitted. (9) Should a case come under our treatment in which the iris or capsule of the lens is incarcerated, the incarcerated iris or capsule should be freed before sympathetic irritation has made its appearance. In recent cases an abscision of the prolapse, with or without iridectomy, will obviate, in most cases, the impending danger of sympathetic ophthalmia."—*Archives of Ophthalmology and Otology*, New York, December, 1876.

On Cancer in the Male Breast.—Mr. Wagstaffe, of St. Thomas's Hospital, in some comments on a very rare case of cancer in both breasts in a man, states "that mammary cancer in the male is very commonly infiltrating, and disseminated rapidly through the system, but one of the features of this case was the excellent health and apparent freedom from further disease in the patient. It seems that the occurrence of what appears to be primary cancer at so short an interval in both breasts affords support to the constitutional view of its origin; but why does this not appear oftener, if so? and the fact of its occurring in a male breast gland bears out Sir James Paget's opinion of its origin in unused or effete structures and organs. But it may be fairly asked, Why does not cancer occur frequently in the male breast?'"

The following analysis is given of seventy-one cases of cancer of the male breast, ten of which cases had previously been unpublished. In sixteen of the thirty-one cases where mention is made of which moderate its progress. If the varicose veins be filled by clots, he

breast was affected, the right organ was cancerous, and in twelve
the left organ; in the remaining three cases both breasts were
affected. The age of the patient is given in forty cases. The
youngest is 25 years of age, the oldest 84 years; two were 30
years old, three between 30 and 40, eleven between 40 and 50, nine
between 50 and 60, seven between 60 and 70, six between 70 and
80; so that the largest number of cases occurred between 40 and
50. In eleven out of the thirty cases in which the direction of the
disease is mentioned it was under one year, in eight rather more
than a year; in one it was over three years, in five over five years,
and in two as much as eight years. At the time when the cases
came under observation an open wound had formed in more than
half the number. Of forty cases in which a sufficient description is
given for a judgment on this point twenty-one had an open wound.
The result of surgical operation is reported to have been successful
in twenty out of the twenty-three cases that had been operated on.
Of fifteen not operated on, twelve are stated to have died shortly.
"Probably," Mr. Wagstaffe states, "in these last-mentioned
cases disease had extended beyond the possibility of interference."
One successful case, under the care of Mr. Cæsar Hawkins, is
alluded to, in which the patient died nine years after the operation,
without any evidence of return of the disease. "The most extraor-
dinary case, however," it is pointed out, "is one recorded by Dr.
Warren in his work on tumours, for a scirrhous mass is reported to
have been removed by him from the right breast of a gentleman 30
years of age, and, eleven years after, the patient was still well.—
Transactions of the Pathological Society, vol. xxvii.

On Infra-patellar Hygroma.—According to Professor Trende-
lenburg, of Rostock, dropsical distension of the deeply-seated infra-
patellar bursa cannot be regarded as a very rare affection. Two
cases of this form of hygroma are reported, and a description is
given of its symptoms. The infra-pallellar bursa, the contours of
which are too small, under healthy conditions, to be distinctly seen
or felt, forms, when distended by fluid, a well-marked tumour. The
distension of the bursal sac takes place chiefly in the upward and
lateral directions, an abnormal protrusion being thus formed on
each side of the ligatmentum patellæ. The change thus produced
in the external form of the knee may escape notice if the cor-
responding bursa in the other limb be similarly affected. The
lateral swellings are best marked when the leg is semi-flexed. In
this position of the limb each of the lateral depressions that are
observed under normal conditions is replaced by a prominent
fluctuating tumour. In cases of hygroma of this deeply-seated
bursa, flexion of the leg cannot be caused to its full extent. In
hydrops genu, on the other hand, flexion at the knee is not inter-
fered with, even when there is considerable intra-articular effusion.
There is usually some tenderness in the region of the swollen bursa,
and some pain and a feeling of stiffness in front of the knee after
active movements of the leg. The author made out in each of his
cases a tender spot at the inner side, and just above the level of the

tubercle of the tibia. Pain often comes on spontaneously in the affected region, and the patient complains at times of a sensation of tension below the patella and of weakness in the joint. There is slight lameness, and the affected limb speedily becomes fatigued after exercise. In advanced cases there is a constant feeling of uneasiness in the knee-joint, perfect rest of the whole limb giving no relief. In a female patient having a thick layer of subcutaneous fat the bursal swelling may fail to be distinctly made out, so that the nature of the case may be overlooked, and the subjective symptoms be regarded as those of an articular neurosis. The progress of infra-patellar bursa is usually very slow. The treatment recommended by the author is that of compression of the affected region by means of Esmarch's elastic bandage.

In the concluding portion of his contribution Prof. Trendelenburg states it as his opinion that the pain and tenderness in the infra-patellar region, so often complained of after injury to the knee, may be due to an hæmorrhagic or an acute serous effusion into the deep-seated bursa.—*V. Langenbeck's Archiv fur klinische Chirurgie.* Bc. xxi, Heft 1.

On Acute Cellulitis of the Orbit.—Dr. Sonnenburg, of Strasburg, in an article on acute cellulitis of the orbit, states that this is not an affection of frequent occurrence, although it might be assumed that the orbitar cavity, from the abundance of fat and cellular tissue contained therein, and from its richness in vessels and nerves, would present very favorable conditions for the origin and development of phlegmonous inflammation. The cases that have been recorded in surgical literature show that acute orbitar cellulitis may be due to one or other of very many causes. In some instances the affection occurred in the course of an infectious disease, as typhus, variola, scarlatina; it has not unfrequently been observed as a complication of suppurative meningitis, and has occasionally coexisted with or followed facial erysipelas. Traumatism, surgical as well as accidental, has, in many cases, given rise to this form of inflammation. Instances have been recorded of orbitar cellulitis consequent on operations for squint and on enucleation of the eyeball. The prolonged presence of a foreign body in the orbit, periostitis or necrosis of some portion of the walls of the cavity, and inflammation of the lachrymal gland, have been recorded as occasional antecedents of the orbitar cellulitis. In two cases of rapid and destructive phlegmon of the orbit recorded by the author of this contribution no cause for the affection could be discovered. Acute orbitar cellulitis may occur at any period of life, but has been most frequently met with in young and middle-aged subjects. The most favorable seasons seem to be the spring and the autumn. This affection of the contents of the orbit has, according to the author, well-marked clinical characters, and may in most instances be readily diagnosed. From the facts that the inflamed tissues are enclosed on all sides but one within unyielding osseous walls, and that they are mixed up with many blood-vessels and nerves, the symptoms of acute orbitar cellulitis are, as may be imagined, very severe, and the pain intolerable. The

inflammation is usually preceded by rigors, general uneasiness, and fever. The more prominent symptoms during the attack are, intense pain in the orbit, exophthalmy, swelling and congestion of the eyelids, œdema of the conjunctiva, especially of tarsal folds, which extend to and overlap the margins of the cornea. The movements of the eyeball are usually abolished in consequence of participation of the recti muscles in the inflammatory action. If proper care be taken in establishing the diagnosis, acute cellulitis of the orbit ought to be distinguished without difficulty from any other affection of this region. In general inflammation of the eye exophthalmy is due to distension and enlargement of the globe itself, whilst in cases of genuine inflammation of the adipose tissue of the orbit there is simple dislocation of the eyeball forwards. From acute inflammation of the lachrymal gland, cellulitis of the orbit may be distinguished by the acuity of its process, by the severe pain attending it, and by the position of the eyeball, which is dislocated directly forwards, and is not, as in cases of the former affection, forced downwards and inwards. In periostitis of a portion of the orbital wall the margin of the orbit is generally tender on pressure, the skin and subcutaneous connective tissue of the eyelids and cheek are not so readily and so speedily involved, consecutive inflammation of the orbital fat is generally localised, and the eyeball is dislocated laterally rather than in a forward direction.

In the opinion of Dr. Sonnenburg the phenomena and symptoms attending genuine phlegmon of the orbit indicate that this affection is an infective inflammation. He holds that in those cases where no other cause is to be made out, the orbitar inflammation is connected with inflammatory processes in the deeper parts of the face, as the pharyngeal and nasal cavities. This infective phlegmon is attended with such severe local phenomena, in consequence of the special conditions—pain, pressure symptoms, swelling, and fever—being intensified to the highest degree by the unyielding nature of the walls of the orbit and by the abundance of vessels and nerves coursing its cavity. The occurrence of consecutive meningitis is not so frequent as one might be led to anticipate by a knowledge of the free communication of the lymph spaces of the orbit with those of the cranial cavity. Facial erysipelas is a much more frequent complication. In orbitar phlegmon the eye is generally in much danger. In the two cases reported by Dr. Sonnenburg the functions of this organ were speedily destroyed. There was considerable retinal extravasation of blood in one case, and ulceration with opacity of the cornea an the other. In many cases vision is destroyed through neuritis optici ; sometimes, though less frequently, through detachment of the retina, or through suppurative irido-choroiditis. Orbitar phlegmon terminates most frequently in suppuration, and often in necrosis of the soft tissues of the cavity. The most important details of treatment are deep incisions and the application of moist warmth.—*Deutsche Zeitschrift für Chirurgie*, Bd. vii, Hft. 5, 6, 1877.

On Perineal Lacerations.—Dr. Montrose A. Pallen, of New Yrok, holds that "an operation for laceration of the perinæum

ought to be performed in every case where we can hope for union of the wounded surfaces. The restoration of a conjoined and symmetrical action of the muscles, aponeuroses, vaginal tissue and integuments, which make up the perineal structure, is the object sought to be obtained; and, while we can hardly hope for complete action in an artificial perinæum, yet, under certain circumstances, we can obtain a much stronger one than existed previous to the laceration, one which gives sufficient vaginal support, and insures freedom from the discomforts and ills attending the loss of the base of support to the entire utero-vaginal structures."

The forms of perineal rupture requiring operation are—1. Perineal sundering, when the submucous structures are sundered. 2. Vagino-perineal laceration, when the pre-anal tissues are torn. 3. Perineo-rectal laceration, when the sphincter ani is involved. 4. Recto-coccygeal laceration, when the fœtus is expelled through the bowel and lacerates the sphincter to its coccygeal attachments, the pre-anal perineum being left intact.

Perineal sundering depends on defective muscular action and atrophy and degeneration of muscular tissue consequent on lesion of the superficial branches of the pubic nerve, or may be the result of direct violence during delivery, which severs the union of the transverse perinei muscles from their attachments to the bulbocavernous in front, the sphincter ani behind, and the levator ani above and around. When sundering takes place the vaginal mucous membrane and the skin of the perineal *raphé* remain intact, and the posterior portion of the vaginal outlet forms a thin patulous bag without any sustentative power. This irritation is accompanied with a greater or less amount of cystocele and rectocele, and consequently, often results in retroversion of the uterus. In consequence of the pouching and sinking of the vagina towards the coccyx intractable lencorrhœa often results. The only method of treating cases of this kind consists in rolling the pouch forward from the rectum in order to cleanse the sac and apply lotions. In some of these cases of perineal sundering virification of the posterior vagino-vulvar perineal space and approximation, as in other operations for perineal rupture (except that the integument is not divided but folded upon itself), not only cures the leucorrhœa but relieves the patient of the distressing pain and inconvenience of cysto-rectocele.

Perineal rendering is often met with as a result of the treatment of retroversion of the uterus by large pessaries, which crowd the rectum backwards to the hollow of the sacrum or press the bladder against the pubes. No pessary, the author insists, should ever be used that is longer than the distance from the sub-pubic ligament to the posterior fornix of the vagina, which, in an average-sized woman, rarely exceeds two and a quarter inches. With the use of a pessary of large size, hyperæmia of the vaginal mucous membrane and consequent leucorrhœa, vesical tenesmus, and dysuria are kept up. "A pessary," it is stated, "is nothing more nor less than a splint, and, like all other splints, should not interfere with the nu-

trition of the parts or impede their functions." The author states, in conclusion, "that he is inclined to believe that when the predisposing causes of perineal laceration exist, no skill upon the part of the accoucheur can avoid the accident, unless he relieves the perineal tension by vulvar liberating incisons."—*New York Medical Journal*, No. 5, 1876.

REPORT ON PHYSIOLOGY AND HISTOLOGY.

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SPONTANEOUS GENERATION.

1. Dr. N. ROBERTS and Prof. TYNDALL. *The Influence of Liquor Potasse and an elevated temperature on the origin and growth of Microphytes.* 'Nature,' Feb. 1, 1877.
2. Dr. D. MÜLLER. *Ein Beitrag zur Archebiosis.* In 'Centralblatt fur Med. Wiss.,' No. 18, May 5, 1877.
3. M.M. PASTEUR and JOUBERT. *On the Alteration of Urine in reference to recent communications of Dr. Bastian.* 'Nature,' Feb. 8, 1877, abstract of paper read before Academy des Sciences.
4. BASTIAN, Dr. C. *On the Fermentation of Urine.* 'Nature,' March, 1, 1877, and 'Researches illustrative of the Physico-chemical Theory of Fermentation read before the Roy. Soc.,' June 15, 1876. No. 172.
5. W. H. DALLINGER. *Spontaneous Generation.* Abstract of lecture delivered at Roy. Institution, published in 'Nature,' May 10, 1877.

1. Dr. Roberts' comments on Dr. Bastian's statement that while an acid urine usually remains barren after being boiled for a few minutes, the same urine becomes fertile when similarly treated if previously neutralized or rendered alkaline by liquor potassæ, especially if it be afterwards maintained at a temperature of 115° F. or 122° F. This, Dr. Roberts says, is quite in accordance with the general rule he has himself laid down, viz. that slightly alkaline liquids are always more difficult to sterilise (by heat) than slightly acid liquids, for hay infusions if acid were always rendered barren after a few minutes' boiling, whilst the neutralized infusion invariably became fertile after a similar boiling. He thinks the addition of the liquor potassæ after the boiled liquid had cooled opens a source of fallacy. Both he and Professor Tyndall found no signs of life appeared if the tubes containing the potash were kept for some hours at a temperature a little above that of boiling water. M.

Pasteur instead of using a *solution* of potash, as Dr. Bastian did, employed the solid caustic potash to neutralize the urine. Dr. Bastian replies that liq. potassæ, which has been raised to a temperature of 100° C. (212°F.), must be regarded as a sterile fluid, and this was the material he added to [the urine in his experiments. To settle the point, M. Pasteur has proposed a commission composed of MM. Dumas, Milne-Edwards, and Boussingault.

2. M. Müller's observations have not been favorable to Dr. Bastian's views.

5. Mr. Dallinger and Dr. Drysdale have followed continuously under high powers the life of some of the objects contained in septic infusions, and have satisfied themselves that even in cases where reproduction appeared to be effected by fission, prolonged research showed that spores were produced. They also ascertained that whilst a temperature of 140° F. was sufficient to cause the death of adults, the spores were able to grow after having been heated to 300° F. for ten minutes. Can it be philosophical, Mr. Dallinger asks, with the life-history of Bacteria still unknown, to assume for it a different mode of propagation?

ABSORPTION. LYMPHATICS. BLOOD.

1. A. BUDGE. *Die Lymphwurzeln der Knochen.* 'Archiv f. Mic. Anat.,' B. xiii, p. 87 ; ' Cbl.,' 1876, 958.
2. G. SCHWALBE. *Ueber die Lymphwege der Knochen.* In ' Zeitschrift f. Anatomie und Entwicklungsgeschicht,' Band ii, p. 131 ; ' Cbl.,' 1876, p. 947.
3. R. GERSTER. *Ueber die Lymphgefässe des Hodens.* ' Zeits. f. Anatomie u. Entwicklungsgeschichte,' B. ii, p. 36 ; ' Cbl.,' 941.
4. H. NASSE. *Das Blut der Schwangeren.* 'Archiv f. Gynæcol.' Band x, p. 315.
5. S. v. BASCH. *Die volumetrische Bestimmung des Blutdrucks beim Menschen.* In ' Wien. Med. Jahrbuch,' 1876. No. 4.

1. Budge has found that the blood-vessels contained in the smallest Haversian spaces are surrounded by perivascular lymph-sinuses, which possess a special epithelial investment adhering to the internal wall of the Haversian canals. These perivascular lymph-sinuses are, on the one hand, directly continuous with the lymphatics of the periosteum, and on the other hand, with the stellate processes of the lacunæ which can be injected through them. Budge therefore admits that the roots of the lymphatics of bone are in the lacunæ, and that the lymph of the bones is transmitted from the lacunæ into the perivascular sinuses of the Haversian canals, from whence it passes into the periosteal lymphatics.

2. According to Schwalbe, true lymphatic *vessels* belonging to bone are only found in the outer layers of the periosteum. Lymphatic spaces or sinuses, however, in connection with them occur in the layer of loose connective tissue between the outer and inner

layers of the periosteum, and these again communicate with spaces lying between the periosteum and the surface of the bone. The subperiosteal sinuses are lined by epithelium. Schwalbe finds in the compact substance of bone a system of canals corresponding to the bone corpuscles, and these processes either communicate directly with lymph-sinuses on the inside or outside of the bone or indirectly through the perivascular spaces contained in the Haversian canals. Schwalbe also describes peri-myelar spaces.

3. Gerster states that the lymphatics of the testes form a closed vascular plexus with membrana propria that never communicates directly with the interstitial tissue of the gland.

4. Nasse's experiments extended over a long period, and were made both on man and the dog, blood being taken from the veins of the same individual before, during, and after pregnancy. He found the specific gravity of the blood of healthy women to be 1055·3, the amount of water 802·4 per 1000, and of fibrin 2·36, The specific gravity of blood serum amounted to 1026·5, and its proportion of water 910·44. In opposition to this, the specific gravity of the blood of pregnant women underwent a diminution. It amounted from the commencement of the second to the sixth month to 1052; from the sixth to the end of the eighth months 1049·7; ninth, 1051·3; in parturient women (ten observations) 1053·3. The specific gravity of the blood serum also diminished to some extent, but the amount of fibrin increased to 3·67 per 1000 in the ninth month, and to 3·82 in parturient women. Similar results were obtained in bitches.

5. Basch has investigated, with the aid of Mosso's plethysmograph, the changes of volume which the arm, when supported in a position of complete rest, undergoes when the body generally is also kept at rest. He finds that long waves of unequal weight can be observed which correspond to a rhythmical swelling, and diminution of volume of the arm occur. The subject of the experiment was placed on a bed in a horizontal position, and might even go to sleep without interfering with the occurrence of the waves. V. Basch does not agree with Mosso in attributing the waves to contractions of the vessels, but to changes in the tension of the aorta, since he was unable to observe any synchronous reddening or pallor coincident with the waves. He regards them as analogous rather to the well-known curves of Traube and Hering. Several of the curves, taken just as the subject dropped off to sleep, showed a distinct fall in the curve, which, however, did not last longer than a minute, and which was then replaced by the previously observed waves. V. Basch refers this to a relaxation of the vaso-motor centre; so that the aortic blood chiefly fills the blood-vessels of the abdominal viscera; and as a consequence, the aortic pressure sinks, and the volume of the arm diminishes. In opposition to Mosso, V. Basch finds no variation in the volume of the arm during strong exertion of its muscles. When strong pressure was applied to the abdomen, so that the vessels contained in it could not dilate, the volume of the arm augmented; a similar effect was produced during strong

action of the abdominal muscles. All conditions that lowered the blood pressure in the abdominal vessels caused a diminution of the volume of the arm.

Gland. Secretion.

1. W. Biedermann. *Untersuchungen über das Magen-epithel.* 'Wien. Akad. Sitzungsber.' B. lxxi, Heft 3, p. 377.
2. F. Kretschy. *Beobachtungen und Versuche an einen Magenfistel Kranken.* 'Deutsch. Archiv f. Klin. Med.' Band. xviii, p. 527.
3. W. Kuhne and A. Lea. *Ueber die Absonderung der Pancreas in Heidelberg Natur-hist. Verhandlungen*, B. 1, H. 5.
4. E. Külz. *Zur Kenntniss des menschlichen Leberglycogens.* 'Pflüger's Archiv,' B. xiii, p. 267.
5. E. Külz and E. Frerichs. *Ueber den Einfluss der Unterbindung des Ductus Choledochus auf den Glycogengehalt der Leber.* 'Idem,' p. 460.
6. C. Méhu. *De la Non-Existence du Mucus de l' Urine.* 'Bullet. Gén. de Therapeutique,' 1876. Band xci, p. 161.
7. L. Kleinwächter. *Das Verhalten des Harnes im Verlaufe des normalen Wochenbetts.* 'Archiv. f. Gynæcol,' B. lx, p. 370.
8. Prof. Quincke. *The Influence of Sleep on the Activity of the Kidneys.* In 'Archiv ' f. Exp. Pathol.,' Band. vii, H. 2, and 'Academy,' June 9, 1877.
9. J. Duval. *Sur un Acide nouveau pré-existant dans le lait frais de jument et nommé acide équinique.*

1. Biedermann finds that the gastric epithelium of most Vertebrata consists of conical or cylindrical cells, bounded laterally by a membrane, but open above in every phase of life. The open free extremity is filled with a round or oval body (bung), which proceeds from a peculiar modification of the cell protoplasm, and is generally histologically, but always physically and chemically, differentiated from the remaining cell substance. The bung,which is characterised by its singular power of swelling up by imbibition, and by its relation to a watery solution of aniline blue, for which it has a strong affinity, exhibits when treated with osmic acid (though not in all animals) a peculiar fine longitudinal striation. He considers, therefore, that the view of Heidenhain and Ebstein, that it is merely a portion of cell contents converted into a kind of mucus, is untenable. The mucus cells found by Heidenhain at the entrance of the stomach in Rana esculenta are only morphologically different from the superficial epithelium. The gastric cells subserve the purpose of secreting the gastric mucus, and possibly aid in the resorption of certain products of digestion. The gastric epithelium of a fasting and a digesting animal differ only in the size of the bung closing their orifices.

2. Kretschy's observations were made on a servant-girl, aged 25, who was otherwise healthy, but suffered from a gastric fistula, consequent on the bursting into the stomach of an abscess originating

in disease of the seventh rib of the left side. The opening was three centimeters in diameter (one inch), and from it protruded a red and easily bleeding fold of mucous membrane. A sound could readily be introduced into the alimentary canal, and conversely a portion of any food that was swallowed immediately made its appearance at the external orifice of the fistula. When Kretschy commenced his experiments the opening had already existed five months. Kretschy sought to determine the duration of normal digestion; and as the activity of digestion is dependent on the degree of acidity of the contents of the stomach, he endeavoured to ascertain how soon after food the stomach gave an acid reaction; when the formation of stomach reaches its maximum; how it rises and falls, and how the alimentary canal behaves. He found that the digestion of breakfast lasted five and a half hours, the acid reaction attaining its maximum in the fourth hour, and falling to neutral in the course of the following hour and a half. The digestion of the mid-day meal (dinner) lasted seven hours, the maximum acid reaction occurring about the sixth hour, and the stomach becoming neutral at the end of the seventh hour. Examination of the contents by means of the microscope at the end of the fifth hour demonstrated the presence of numerous muscular fibres and starch granules. The digestion of supper lasted from seven to eight hours. At the catamenial period the stomach never at any time of the day presented a neutral reaction. The addition of 3 ccm. of alcohol prolonged the period of digestion of dinner; coffee prevented the acidity from becoming so great, and prolonged the duration of digestion one hour. Pepsin in 7-grain doses did not shorten the period of digestion. Distilled water did not cause the stomach to become acid. Alcohol becomes converted into aldehyde in the stomach.

3. Kühne and Lea examined the delicate and almost transparent pancreas of the rabbit almost in the normal living state, by drawing the duodenal loop of intestine through a small wound in the abdomen, and placing the mesentery under the microscope. They found that the short tubes and club-shaped extremities of the gland ducts are either smooth externally, or present well-marked projections and sulci which correspond with the number of the subjacent secretory cells. In the former case the limits of the cells are not defined, in the latter each cell is sharply defined by a usually double contour line, the inner border of which bounds the lumen of the lobule. The smooth-walled condition is characteristic of the fasting state, the lobulated of the process of digestion; so that they alternate with one another during life. The lobulated condition could be induced by injecting the ducts with fluid, by irritation of the gland, by means of moderate induction currents, by the injection of jaborandi; the smooth condition by cooling, by strong induction currents, and by poisoning with atropine. They find also that the granules within the cells described by Bernard change their position during secretion, advancing towards the lumen of the tube, and after the process of secretion was continued for some time become

smaller, and ultimately disappear. In the axial canal of the gland small particles of albumen (blood-corpuscles) may be observed undergoing digestion. No digestion, however, of such corpuscles is observed if they were situated between the cells. They describe peculiar cell heaps which are accumulated in certain parts of the pancreas. They do not appear to have been able to follow the nerves *into* the cells of the gland.

4. Külz obtained from the tenth part of the liver of a diabetic patient, in addition to sugar, 0·685 gramme of glycogen, notwithstanding that the post-mortem was made twelve hours after death, and that the last food taken was thirty-four hours before death. The glycogen obtained exhibited the normal reactions, and the sugar obtained from it by boiling with dilute hydrochloric acid rotated light to the right, and was capable of fermentation.

5. Külz and Frerichs found that in three guinea-pigs the amount of glycogen obtained after ligature of the ductus communis choledochus 0·088, 0·1, and 0·112 gramme; in a control experiment it amounted to 0·356 of a gramme. From the livers of rabbits, seventeen to twenty-nine hours after ligature, the quantities obtained were 0·095, 0·053, 0·115, 0·123, and 0·088. There was, therefore, a considerable diminution of the amount of glycogen. The urine of all the animals contained blood, colouring matter, albumen, and biliary colouring matter, but no sugar as Wittich has stated. In a subsequent series of experiments the rabbits were first made to fast for six days before the ductus choledochus was tied, and a solution of sugar was now injected to see whether the formation of glycogen ceased. The amounts of glycogen obtained from the different animals were 0·069, 0·039, 0·079. 0·115, and 0·066 of a gramme. Similarly, even in well-nourished animals, without antecedent fasting, if the duct were tied and sugar injected, the amount of glycogen contained in the liver was small.

6. According to M. Méhu, the cloudy deposit which takes place in urine after standing for a little while, is composed, not of mucus, as is generally believed, but of epithelial cells, derived from the bladder and detritus of the same. In the female, some of the cells are derived from the vagina, or may be pus cells. No mucus is found in this cloudy material, nor is any contained dissolved in the urine. M. Méhu refers to the behaviour or urine containing puscells, or a considerable quantity of the colourless elements of the blood. Such urine, if filtered, becomes cloudy on the addition of acetic acid to it in the cold, a reaction which we consider to be due to the presence of pyin, or some substance analogous to myosin. Acetic acid produces a cloud when added to the urine, even when the bladder is only slightly irritated, but not with normal urine.

7. Kleinwächter's observations on the behaviour of the urine during the first eight days of the puerperal state, were made upon 179 cases, with the following results. The quantity of urine is increased during the first twenty-four hours (being 1325 c.cm. on the average), which he considers to be due to the change of pressure that has

occurred in the vascular system, and perhaps, also, to the psychical excitation of the act of delivery. The quantity falls from the second to the fourth day, in consequence of the commencing secretion of milk and the loss of fluid by perspiration, and by the lochial discharge, but it subsequently augments again. The amount of urea excreted in the course of twenty-four hours is nearly normal (26·5 grammes), though it undergoes some diminution during the first and second days after delivery. The excretion of salt (14·0 grammes) is normal, and is in proportion to the quantity of urine. The amount of phosphoric acid runs parallel with the quantity of urea eliminated, and is somewhat diminished during the first eight days on the whole (the average being 2·2 grammes per diem). On the first day it is augmented (2·5 grammes), on the second and third day diminished (1·7 grammes), on the fourth and fifth days increased (2·3 grammes), and again, on the last three days it falls. The specific gravity of the urine is, on the average, 1015—1016. Its colour, which is at first pale yellow, becomes gradually yellow. As the age of the mother increases, the daily elimination of water, common salt, and phosphoric acid diminishes, its colour becomes darker, its specific gravity higher.

8. Professor Quincke has ascertained that, whereas the urine secreted during sleep is scanty and of high specific gravity, that secreted during the first three hours after waking is more abundant and of lower density than during any similar period of the twenty-four hours. A number of observations were made to establish this point, the subject remaining in bed and taking neither food nor drink for the three hours in question. The fact admits of being interpreted in various ways. We may suppose the absorption of fluid from the intestinal canal to be arrested during sleep, and resumed on waking. This hypothesis is a most unlikely one, for the periodic variation takes place as usual when no liquid has been taken within four hours of retiring for the night. It is probable that the physiological activity of the kidneys may be checked during sleep, owing partly to diminished energy of the secretory nerves, partly to contraction of the renal blood-vessels, partly to a lowering of tension throughout the arterial system, and this is the most probable explanation.

9. Duval states that the milk of the cow contains a new acid, which is obtained in solution by agitating ether extract with water and filtering, by which means the fat is retained ; on evaporation, a syrupy mass is obtained, which, according to Duval, is the pure acid. In the milk it is normally in combination with ammonia.

Respiration.

1. Zweifel. *Die Respiration des Fœtus.* ' Archiv f. Gynæcol.'
     Band. lx, p. 291. ' Cbl.,' 1876, p. 907.
2. Zuntz. *Ueber die Respiration des Sangethier Fœtus.* In
     ' Pflüger's Archiv,' Band. xiv, p. 605.
3. A. Stefani. *Influenza della Respirazione sulla pressione del
     Sangue.* ' Centrablatt.' Abstract, 1876, p. 950.

1. Zweifel showed, by means of the spectroscope, that the blood in
the vessels of the umbilical cord of the fœtus presents the absorption
striæ of oxyhæmoglobin, and therefore contains oxygen, which must
come from the blood of the mother. He opened the pregnant uterus
of an animal lying in a warm salt-water bath, and by inducing
artificial asphyxia, observed whether, and how soon, the blood assumed
a dark colour. He found that after exclusion of air the process of
asphyxia occurred almost as rapidly as in animals that had been
born.

2. Zuntz, whose paper is abstracted in the ' Academy' for May
12, 1877, maintains that fœtal blood contains a smaller proportion
of hæmoglobin than the blood of the adult ; that the embryo can
survive complete occlusion of its umbilical vessels for a much longer
time than closure of its trachea after birth, though the fœtus is
asphyxiated quite as rapidly as the maternal organism, when the
placental circulation remaining intact the mother is deprived of air.
Hence the power of surviving temporary closure of the umbilical
vessels must be ascribed, not to any special independence of oxygen
on the part of the fœtus, but to the slower rate at which its tissues
consume the relatively small store of oxygen contained in the blood.
This is in opposition to Zweifel, who thought the consumption of
oxygen by the fœtus in utero to be relatively equal to that which
takes place after pulmonary respiration has set in.

3. Stefani placed dogs under the influence of curara, and then
made the usual arrangements for obtaining tracings of blood pres-
sure. He found that if the artificially-conducted respiration (neces-
sitated by the employment of curara) were suddenly interrupted
great augmentation of the blood pressure occurred, the rise amount-
ing to from 110 to 194 mm. of mercury. This, however, was not
always immediate, but after the lapse of a longer or shorter period,
the length of which is dependent on the store of oxygen that
happens to be present in the blood at the moment of arrest of the
respiration ; the larger the amount of oxygen the longer the duration
of the interval between the occlusion and the rise of the blood
pressure. An analysis of the blood-pressure curve shows that it is
not formed by a uniformly rising oblique line, but by a series of
undulations. As the pressure rises, the number of pulse-beats falls,
whilst the several pulsations are more protracted, a result that is due
to excitation of the vagus centre, for it no longer occurs when both
vagi are divided. If air be readmitted by recommencement of the
artificial respiration, the pressure of the blood falls, and the pulse

resumes its ordinary frequency and extent.   Stefani explains these
phenomena on Schiff's theory in regard to the respiratory oscilla-
tious of the blood pressure, and repeated his experiments on dogs,
in which the vaso-motor centre was paralysed by section of the
spinal cord.   The result of this section was, as V. Bezold showed in
all instances, a progressive diminution of the blood pressure ; and if,
after the section was made, artificial respiration was suddenly ar-
rested, the blood pressure rose, but never to the same extent as in
the uninjured animal.   If the vagi were divided as well as the cord
in the neck, no increase of blood pressure or change in the heart's
action occurred.   It hence appears that the increase of blood pres-
sure, consequent on arrest of the respiration, is independent of the
action of the vaso-motor centre, though it by no means rises to the
same amount as when that centre is intact; and we must admit
that the results of the chemical changes taking place in the blood
after arrest of respiration act, not only on the vaso-motor centre,
but also on the ganglia in the heart.   By special control-experi-
ments, Stefani shows that the phenomena above described are not
due to the influence of the small vaso-motor centres demonstrated
by Goltz and Nussbaum in the spinal cord, nor to the innervation
centre of the heart, situated in the medulla oblongata, since they
occurred after section of the spinal cord and both vagi.   Stefani
points out the practical importance of these researches by applying
the results to the treatment of apoplexy from effusion of blood,
for, in such cases, the maintenance of artificial respiration may
prolong life by relieving pressure on the respiratory centre, and
also, by diminishing the blood pressure, it may prevent the further
escape of blood.

NERVOUS SYSTEM.

1. W. KÜHNE.   *Vorläufige Mittheilung über Optographische
   Versuche.*   In ' Centralblatt f. d. Med. Wiss.,' No. 3, 4, 11,
   15, 1877.
2. HELFREICH.   *Netzhautpurpur.*   Idem, No. 7.
3. DIETL und PLENK.   *Netzhautpurpur.*   Idem, No 16.
4. H. ADLER.   *Sehpurpur am Kranken und verletzten Menschen-
   augen.*   Idem, No 14.
5. W. KRAUSE.   *Die Nervenendigung in der Retina.*   'Archiv f.
   Mikroskop. Anatomie,' Band xii, p. 742.
6. JAMES DEWAR.   *The Physiological Action of Light.*   ' Nature,'
   March 15, 1877.
7. M. LAVDOWSKY.   *Untersuchungen über den akustischen End
   apparat der Säugethiere.*   'Archiv f. Mic. Anat.,' B. xxxiii,
   p. 497.
8. A. V. MOJSISOVICS.   *Ueber die Nervenendigung in den Epidermis
   der Saüger.*   In ' Wien. Akad. Sitz.,' B. lxxi, p. 242.
9. STEINER.   *The Influence of Temperature on the Nerve and
   Muscle Current.*   'Reichert's Archiv,' and 'Nature,' March 8,
   1877.

10. WEBER-LIEL. *Die Aquæductus des Labyrinths.* In ' Central-
    blatt f. d. Med. Wiss.,' 1876, p. 929.
    U. PRITCHARD. *The Termination of the Nerves in the Vestibule*
    and Semicircular Canals of Mammals. In the 'Quart. Journ.
    of Microscop. Sci.,' vol. xxviii, p. 398.
    H. MUNK. *Ueber Parltialerregung des Nerven.* ' Reichert's
    Archiv,' 1875, page 41; ' Centralblatt,' 1876, p. 948.
11. MORIZ-BENEDIKT. *Der Raubthiertypus am Menschlichen Gehirne.*
    In ' Centrablatt. f. d. Med. Wiss.,' 1876, p. 930.

1. In November of 1876 Boll published a paper in the ' Reports
of the Berlin Academy of Sciences,' stating that the retina of all
animals that had been kept for some time in the dark presented a
purple tint, but that after exposure to a bright light it lost this
hue and became colourless. Kühne, repeating Boll's experiments,
satisfied himself that the colour of the retina reacted to light and was
capable of self-renovation after removal from the body. He speedily
demonstrated that if the image of an external object be thrown
upon the retina as upon a screen, it is practicable to preserve it.
In mammals he found that the purpurogenic function of the retinal
epithelium is lost within a few minutes after death, and hence that
it is important, in attempts to preserve the image, to remove the eye
rapidly after a strong image has been thrown upon it during life.
The head and eye of a rabbit were firmly fixed in immediate proximity
to a window shutter having a square hole cut out of it. The head
was then covered for five minutes with a black cloth, after which the
cloth was withdrawn and the eye exposed to the light thus entering
for three minutes. This head was instantly decapitated, the eye
expeditiously extirpated under the monochromatic light of sodium,
opened, and plunged in a 5 per cent. solution of alum. The other
eye, without being removed from the head, was two minutes after
treated in the same way. On the following morning the milk-white
retinæ were cautiously detached in their whole extent and were then
found to exhibit on a beautiful rose-red ground a sharply defined
square image, which in the second eye was white and in the first
rose-red and somewhat less defined. By degrees on exposure to
diffused daylight both images faded away. There can, then, be no
doubt that physical changes in the retina accompany the perception
of light.

2. Helfreich considers that the general colour of the fundus of
the eye as seen with the ophthalmoscope is due to the normal purple
of the retina, but this is opposed by (3) Dietl and Plank.

4. Adler, from pathological evidence and enquiry, has satisfied
himself that the presence and intensity of the retina-purple stands
n direct relation to the existence and degree of the visual power
of the retina.

5. Krause divides the retina of vertebrata into the following layers:
1. Pigment layer. 2. Epithelial layer, composed of rods (Eichtzellen.
light cells), and cones (Farbenzellen, colour cells). Each rod cell is
composed of rod, rod granule, rod fibre, and rod cone; and each

cone cell of cone, cone granule, cone fibre, and proper cone. Krause's rod and cone granule correspond to the external granules of the older authors and of Max Schultze. 3. The nervous layer, which is divisible into (*a*) membrata fenestrata, (*b*) granule layer, (*c*) molecular layer, (*d*) ganglion cell layer, (*e*) optic cell layer, (*f*) membrana limitans. Neither rods nor cones can be discovered in *Amphioxus lanceolatus* nor in *Myxine glutinosa*. The Petromyzon, however, although so little superior to them in organization, possesses both, and both are present in bony fish. The Proteus amongst Amphibia Krause believes has both; but Boll thinks only one form is present. The rods and cones of the axolotl have very long delicate and conical external segments. The bacillar layer of frogs presents four forms of elements, one of which possesses oil drops; the second no oil drops, but paraboloidal bodies; the third are ordinary cones; and the fourth small rods. Lizards present two kinds of elements—slender structures, one with yellow or yellowish-red or pale blue fat drops and very fine pointed extremities; and the other more ventricose, which in place of the fat drops, present molecular pale yellow ellipsoidal bodies. The blind worm has yellow and greenish-yellow, pale blue and pale greenish-blue oil drops in its cones. In regard to the retina of birds, Krause finds the supposed colourless oil drops to be pale blue. In many rapacious birds (*Aster palumbarius* for one) a red oil drop has several orange-coloured ones in its immediate neighbourhood. He thinks, and in this is opposed to M. Schultze, that night birds *have* cones; but they only become fully developed and numerous in adult life. Cones are also present in many other nocturnal animals, as in bats, mice, hedgehogs, the hyæna iltis. Krause still holds strongly the opinion he expressed ten years ago in his memoir on the fenestrated membrane of the retina, that the true terminations of the optic nerve fibres are to be looked for in the line between the epithelial and nervous layer, that is to say in the vicinity of the membrana fenestrata.

Krause, in a second communication, states that the retinal purple can be dissolved out of the retina by bile, or the purified biliary salts. The solution is of a bright carmine tint, and rapidly bleaches on exposure to light. It allows yellow, orange and red light to pass through it, but absorbs all rays from the yellow green to the violet end of the spectrum. He further finds that the colouring matter is not uniformly distributed through the retina, the posterior surface of the fossa centralis being colourless.

6. Professor Dewar, in an interesting lecture delivered at the Royal Institution, showed by a simple apparatus that the action of light on the retina is attended with the development of an electrical current, the amount of which can easily be estimated by the galvanometer.

7. Lavdowsky distinguishes four differently constructed parts in the terminal acoustic apparatus of mammals—1. The membrana basilaris with the arches of Corti (Corti's organ in the strict sense). 2. The supporting apparatus. 3. The terminal cell apparatus; and

4. The membrane of Corti or membrana tectoria. The supporting apparatus he finds to consist of three parts, viz. of a pars reticularis, a pars fibrosa, and a pars perpendicularis. The membrana basilaris is composed of fibres possessing great elasticity stretched like cords between the points of attachment of the membrane. They are most distinct in the so-called zona pectinata, less marked in the habenula tecta and perforata, where they are more slender and arranged in two rows, between which is a structureless membrane. The membrana basilaris breaks up into segments, each consisting of about ten fibres and corresponding to one of Corti's arches, but there is no continuity of structure between the two. The arches are composed of similar fibres, but the fibres are finer and less adherent to one another. They exhibit clear indications of contractility when stimulated by electricity. The supporting apparatus of the auditory organ is composed of two morphologically different materials, epithelioid tissue and connective tissue. The former constitutes the whole upper part of the supporting apparatus, viz. the lamina reticularis, fixing the terminal cells in position with their two accessory plexuses. The latter is represented by certain connective-tissue fibres found inside and outside of the tunnel of the arches. Lavdowsky includes under the head of the terminal cell apparatus in addition to external and internal hair cells (perceptive elements, L.), the auditory granule layer situated at the foot of the internal hair cells. He agrees with Waldeyer and Gottstein in regarding the external terminal cells as twin cells, and accounts for the discrepancy in the description of them given by different authors by the changes they undergo at different ages. The new-born animals the two cells (Corti's and Deiter's cells) are already independent of one another, though in structure and appearance they are almost identical ; subsequently the Corti's cell grows at the expense of the other. The cells of Corti or rod-cells of Lavdowsky are very regularly cylindrical and consist of a dark granular mass containing a nucleus but destitute of a membrane. Deiter's cells (cone cells of L.) are conical bodies of various form, possessing a membrane but destitute of a nucleus. The auditory hairs are implanted in a horse-shoe-like row on the extremities of the rod cells. They do not themselves become stained with nitrate of silver, but the point of their insertion becomes strongly stained. The cone cells are prolonged into delicate processes, which are inserted into the phalanges of the membrana reticularis. The twin cone and rod cell possesses a single strong basilar process which rises straight up from the membrana basilaris, and these processes are arranged in three very regular rows. Between and parallel with these are wavy fibres belonging to the supporting system, which are in connection with other similar fibres inside and outside of the tunnel. Within Corti's organ he finds two forms of nerve fibres, differing in their anatomical course, mode of termination and function and these he terms respectively radial and spiral nerves. The former pass by a varicose fibre to the inner terminal cells and the cylindrical parts of

the external rod cells, whilst the spiral nerves end exclusively in the cone cells, around which they form a close plexus.

8. Mojsisovics, examining by the gold method the snout of the pig, mole and mouse, finds that the nerves terminate between the cells of the epidermis, in very fine and varicose extremities. He regards the tactile corpuscles as having the special function of enabling the distance between two points to be distinguished, whilst the fine extremities enable sensations of contact to be perceived.

9. M. Steiner has proved that the electro-motor force of the nerve current from 2° upwards is greater the higher the temperature, that reaches a maximum between 14° and 25°, and at a higher temperatures, increases again. The force of the muscle current is likewise from 5° upwards greater the higher the temperature. It has its maximum between 35° and 40°, and at higher temperatures becomes less again, till when rigidity sets in it is almost *nil*. Thus, for the nerve and muscle current, as well as for the other functions of living organic forms, there is a temperature optimum which is as distinctly marked, when by heating we rise to it from lower temperatures as when we descend to it by cooling from higher temperatures.

10. Weber-Liel states that his experiments have satisfied him on the following points : 1. That the aquæductus cochleæ in man forms a connection between the perilymphatic space of the labyrinth and the arachnoideal space, for if a small quantity of a solution of Prussian blue be introduced into the wide external funnel-shaped aperture of the aquæductus cochlea and suction be made with special precaution through the superior semicircular canal, the fluid will enter the perilymphatic space. 2. That the aquæductus vestibuli connects the endolymphatic space of the labyrinth with a sac lying between the lamellæ of the dura mater.

11. Benedikt considers that the great apparent qualitative difference between the brain of man and carnivora constitutes a great hiatus in the descendance theory. Neither embryology nor comparative anatomy has been able to supply a bridge connecting these physiologically closely allied families. The main difference between the frontal portions of the brain of man and of carnivora is that in the latter there are four, in the former only three primary convolutions. This difference is, however, only apparent. Every comparative anatomist knows the small sulci which in man are situated between the first frontal sulcus and the median border. In some brains these sulci become greatly developed into a strongly-marked furrow, which extends to the upper part of the anterior central lobe. By this means the aspect of this part of the central lobe approximates to that of Apes. It hence appears that the first frontal convolution of man originates from the coalescence of the two first primary convolutions of the carnivora and that the first undergoes atrophy. The above-named small sulci should be designated as the first; the frontal sulcus, now called the first, should be called the second; and the second the third. The atrophy of the first frontal convolution in man probably depends upon the feeble

development of the sense of smell in man. In the parieto-temporal region also of man the four-convolution type is easily demonstrated. In the Primates the superior and external surface of the temporo-parietal region is connected with the occipital region by four more or less well-marked convolutions, but they are very indistinct at first sight in man. Closer investigation, however, shows that there are two parietal and two temporal lobes, but in man they are peculiar in running backwards and inwards, instead of downwards. There is still another point which he considers to be important, namely, the coalescence of one or more of the three central longitudinal sulci (Roiland's, the pre-central and the interparietal) with the fissure of the fossa of Sylvius. Lastly, Benedikt maintains that the brains of carnivora and other mammals have well-marked occipital lobes.

MUSCLES.

K. KAUFMANN.—*Ueber Contraction der Muskelfasen.* 'Reichert's Archiv,' 1874, p. 273. 'Cbl.,' 1876, p. 941.

W. R. GOWERS.—*The Automatic Action of the Sphincter Ani.* In the 'Proceedings of the Royal Society,' No. 179, 1877.

J. BERNSTEIN and J. STEINER.—*Ueber die fortpflanzung der Contraction und der negativen Schwankung im Saugethiermuskel.* Reichert's Archiv, 1875, p. 526, and Centralblatt, 1876, p. 949.

Kaufmann refers to the three following views on the morphological changes that take place in muscular fibres during contraction :
1. That of Hensen to the effect that there is shortening in the longitudinal diameter, with proportionate increase in breadth both of the anisotropous and of the isotropous substance. (2) That of Krause, that there is shortening of the longitudinal diameter with diminution of the thickness of the isotropous substance, whilst the thickness of the anisotropous substance remains unaltered, and coincidently an increase in breadth and entrance of muscle compartment fluid between the muscle-rods. (3) That of Engelmann, who admits the entrance of fluid into the longitudinal cylinder. Kaufmann examined the muscles of the insects named *Carabus nemoralis*, *Amara apricaria*, and *Pygœra bucephala* with a power of 800 diameters, and found that the muscular fibre of these animals diminishes in length during contraction whilst it becomes broader ; the isotropous substance, however, only loses in the direction of the length of the muscular fibre, whilst the anisotropous substance does not lose in this direction, or only to so slight an extent as to be immeasurable. This is essentially in accordance with Krause's view.

Dr. Gower's observations had for their object the determination of the form of the reflex or automatic action of the sphincter ani of man when voluntary power over it is lost. This reflex action is believed to depend on an anospinal centre (Masius), situated in the lumbar enlargement of the spinal cord, controlled in health by higher encephalic centres. The larger number of observations were made on a man who by a fall had apparently injured the posterior

roots of all the sacral nerves, and both roots of the lowest sacral nerves. There was loss of sensation in all the parts supplied by the above-named branches, but no muscular paralysis or loss of nutrition, except in the levator ani, the sphincter ani, and sphincter vesicæ. The spinal cord was not affected. In two other cases the patients were suffering with paraplegia from disease of the spinal cord, and had entirely lost voluntary control over the sphincter ani. It was found that in all these cases the condition of the sphincter was essentially the same, and that it was in a high state of reflex activity. A small bag of india rubber was introduced into the bowel and connected by a piece of tubing, with a drum and recording lever, as in Marey's cardiograph. In all the cases, though the incontinence of fæces was complete, the sphincter was habitually in a continuous, slightly varying, contraction. Any slight irritation, such as a jet of air, applied to the mucous membrane of the rectum inhibited this contraction, but if the irritation was by a solid body there was, in the first instance, a slight brief increase in the contraction. There was, in both instances, a latent period, of a little more or less than a second in duration, intervening between the application of irritation and the commencement of a change in the contraction. When the initial rise occurred, it lasted about 1·5 sec., and this was followed by the fall lasting 4 secs. Then came a gradual rise, varying in duration from 10—17 secs. This attained or went somewhat beyond the original pressure, finally returning to the normal. As far as could be ascertained the internal sphincter was alone concerned. The action of the sphincter, Dr. Gower thinks, is only a specialised and concentrated example of the ordinary peristaltic action of the intestine, which is inhibited by the vagus and intensified by the splanchnics. The effect, he goes on to observe, of the presence in the intestine of a mass of fæces would be to cause, first, in the moderately contracted intestinal wall in front of it an increased contraction, the effect of which would be to prevent the diffusion of the contents along the intestine (which would materially interfere with their movement); secondly, complete relaxation of the next portion of the intestinal wall, into which the contents of the intestine could pass; and, thirdly, a strong contraction behind, sustained and moving on the stimulating body as the initial contraction gave place to contraction. The process would doubtless be modified by the contraction of the longitudinal fibres of the bowel, which would prevent undue distension of the relaxed portion, and thus assist the transmission onwards, both of the contents of the bowel and of the resulting stimulation.

REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIENE.

By Benjamin W. Richardson, M.D., F.R.S.

I.—Toxicology.

Poisoning by Cyanide of Potassium.—Dr. Joseph Jones, Professor of Chemistry and Clinical Medicine in the University of Louisiana, to whose most able toxicological labours we have so often been indebted, records a remarkably characteristic case of poisoning by potassium cyanide. The facts of the case, as reported to him by Professor Le Monnier, run as follow :—" On Saturday, the 24th of March, 1877," says Professer Le Monnier, " a messenger, out of breath, entered my office, asking for a physician for the Spanish Consul, who was very ill. In haste we hurried to the Consulship, two squares distant, where I was told the Consul had taken poison. I sent for some ipecac. immediately, and proceeded to examine the patient. I found him in his bed, lying on his back ; respiration deep, difficult, and *slow*, with fluttering of the lips, foam at the mouth during respiration, the tongue once in a while protruding between the lips. The face was pale ; the pupils normal ; the temperature below the normal standard, with a cold clammy skin ; pulse at the wrist slow. The muscles were in a state of complete relaxation. Total absence of consciousness. Whatever substance had been taken had penetrated the nervous system. We tried in vain to rouse him. On his forehead were two bruises, caused by striking against his chair in falling. I was then told that he had taken a dose of " Simmons's Liver Regulator "—a tablespoonful or two—and a few *minutes* afterwards had dropped. I smelt and tasted the contents of the bottle ; the smell and taste, though familiar to me, I could not *then* recall. In again examining the patient, I detected a strong smell of *prussic acid* in his breath. My diagnosis was now *positive*.

Cyanide of potassium in great quantity was in the bottle. In the meantime Dr. Formento had entered the room. I called his attention to this fact, and he verified the diagnosis. The ipecac. arrived (30 grains), which I put in a tumbler of lukewarm water, and by teaspoonfuls administered it to the patient. By pouring the draught into his mouth, and placing the hand over it, he would swallow. The act of deglutition was not under control of the will, as the patient was unconscious and life fast ebbing away. No effect from the ipecac. *Prognosis*—fatal result. I expressed my opinion to this effect to the surrounding friends, and to satisfy them (for there was no hope of saving the patient), sent for my stomach-pump and the antidote for cyanide of potassium. I expressed the opinion that the man would be dead before their arrival. The pulse soon disappeared at the wrist, and the man died, without a struggle or

moan, before the arrival of the stomach-pump.   *Résumé.*—Reached
the bedside of the patient at about 12.10.   At 20 minutes to 1 p.m.
he was dead, from having taken, at about 12, a tablespoonful or two
of a bottle of " Simmons' Liver Regulator," containing an unknown
but large quantity of cyanide of potassium.   When I reached his
bedside, he was already unconscious."

From further observations on this case by Dr. Jones, it appears
that the deceased Consul, at half-past 11 o'clock, after his break-
fast at a restaurant, returned to his residence on Dauphine Street,
and retired to his bedroom with one of his friends, J. A. Bousquet.
After a few moments' conversation, he remarked to Mr. Bousquet
that he had taken poison.   Hardly had he uttered the words when
he fell senseless.   Mr. Bousquet, Signor Rafart, Vice-consul, and
Mr. Samuel Rusch, the Clerk at the Consulate, used every effort in
their power to revive him, but failed.

In the meantime, Drs. Le Monnier and Formento were sum-
moned.   The verdict of the jury was death by suicide.   The post-
mortem appearances are thus described by Dr. Jones:

" The brain was greatly congested, but was healthy through all its
textures.   The brain exhaled prussic acid.   The blood of the brain
upon analysis yielded prussic acid and cyanide of potassium.

" The liver was greatly congested with blood, exhaled prussic acid,
and upon analysis yielded prussic acid.   Both the outer and inner
surfaces of the stomach were congested with blood.   The mucous
membrane presented a deep scarlet, ecchymosed appearance, and was
softened and eroded, apparently by the action (post mortem) of the
gastric juice.

" The stomach contained about ten ounces of partially digested
matters, which exaled a powerful and sickening odour of prussic acid.
Chemical analysis revealed the presence of the cyanide and of
prussic acid in the contents of the stomach.

" The blood from all the organs examined gave out prussic acid
and yielded it upon analysis; and although dark coloured when first
exposed, changed rapidly to the arterial hue.   The coagulating
power of the blood was entirely destroyed.

" The bottle, labelled 'Simmons' Regulator,' from which the
deceased is said to have taken the fatal dose, contained eight and a
half fluid ounces of a dark-red liquid, which upon analysis contained
904·4 grains of cyanide of potassium.   Each fluid drachm contained
13·3 grains of the cyanide of potassium.

" The entire capacity of the bottle was ten fluid ounces; there-
fore one and a half fluid ounces were missing.   It is not probable
that the deceased took the entire amount, as he is said to have been
in the habit of taking one or two tablespoonfuls of the 'Simmons'
Liver Regulator' a short time after each meal.   It is probable that
he took about two tablespoonfuls of the poisonous mixture, which
would yield 106·4 (one hundred and six grains and four tenths) of
the cyanide of potassium—a quantity sufficient to have destroyed at
least 21 (twenty-one) men.   The entire amount of cyanide of
potassium originally introduced into the bottle was about 1110 (one

thousand one hundred and ten) grains, a quantity sufficient to have destroyed 221 men, if the fatal dose be placed at 5 grains.

"I administered 20 minims of the liquid from the bottle, which had caused the death of the Spanish Consul, to a dog, and symptoms of poisoning commenced in 20 seconds ; in 30 seconds, violent spasms with a long piercing cry were emitted by the dog ; coma, preceded by a prolonged spasm of muscles of the back, was established in 60 seconds after the administration of the poison, and death occurred in 100 seconds. The body of the dog was perfectly relaxed and flaccid for 30 seconds before the extinction of the pulsations of the heart and the cessation of the spasmodic respiration.

"The post-mortem examination revealed congestion of the brain and internal organs, and distension of the cavities of the heart by black blood.

"A strong odour of prussic acid was exhaled from the breath of the dog during life, and from the blood and from all the organs and tissues after death."

After narrating a long series of experiments, Dr. Jones draws the following general conclusions as to the nature of the effects of cyanide of potassium and hydrocyanic acid.

1. After the introduction of cyanide of potassium and prussic acid into the subcutaneous tissue, or after their application to the tongue and mucous membrane, a certain period of time elapses before the manifestation of symptoms of poisoning ; and during this period the poison is absorbed, mingles with the blood, and is distributed to the various organs and tissues, and is thus brought into contact with the ganglionic cells of the cerebro-spinal system.

Various statements have been made as to the rapidity of the effects of prussic acid and cyanide of potassium in producing poisonous symptoms and destroying life, which have not been sustained by my experiments.

In the most suddenly fatal cases, the action has been referred by some physiologists to nervous action, transmitted from the points at which the poison touched the extremities of the nerves. The incorrectness of this view has been shown by experiments similar to the following by Blake. The portal vessels of an animal being tied, seven fluid drachms of Scheele's Acid were introduced into the stomach on the *sentient extremities* of the nerves on which the poison is said to act. Ten minutes elapsed without the slightest effect ; the ligature was removed, and one minute afterwards the effects of the poison manifested themselves.

It is evident that those who give this explanation of the sudden effects of prussic acid, leave entirely out of view the fact that hydrocyanic is highly volatile, and that if a drop of the pure acid be approached towards the tongue of a living animal, the acid evaporates and reaches the lungs by inhalation, and is immediately diffused over an immense absorbent surface, before the drop of acid actually reaches the mucous membrane of the mouth. It is well known that prussic acid is most rapidly fatal in the form of vapour. I have been, upon more than one occasion, most seriously affected by the vapours

of the acid during my experiments and post-mortem examinations. The time of the action of this poison should be reckoned from the moment that its vapour reaches the capillaries of the lungs; and the place of action should be considered the extensive absorbent surface of the bronchial tubes and air-cells. Now it is well established that the poison may reach the heart and cerebro-spinal and sympathetic systems, in an almost inconceivable short space of time, from the lungs. That a sufficient interval elapses between the application of the acid, and the moment when its first effects are produced, to allow of its being brought into contact with the central ganglionic masses, will be evident from the consideration of the following facts :

Haller and Sauvages were the first to ascertain, by experiment, with what velocity the blood is carried through the vascular system; their calculations, however, were erroneous, as they were founded on the supposition that the movements of the blood depended exclusively upon the action of the heart. Haller's conclusions respecting the velocity of the circulation in frogs and small fish are more correct, as they were confirmed by autopsies, but his observations were confined to cold-blooded animals, and it is scarcely necessary to mention how hazardous it would be to infer from them the velocity of the blood in warm-blooded animals.

The same remark applies to the experiments of Spallanzani and Dollinger.

In more recent works on the subject, the comparison of the quantity of blood contained in the ventricles of the heart, with the whole mass of the blood, and with the number of pulsations in a certain time, was considered sufficient to determine the relative velocity of the blood; a method the uncertainty of which appears from the circumstances, that the quantity of blood cannot be made out with precision, and that the number of pulsations and the capacity of the ventricles differ very considerably in different individuals. M. Herring, of Stuttgart, found the capacity of the left ventricle in horses differing from 3 to 11 ounces, and that of the right ventricle from 4 to 38 ounces.

M. Herring tried another method, which seems to lead to more accurate results. He mixed a solution of the cyanide of potassium with the blood; he then took, at certain intervals, small quantities of blood from various parts of the body; and from the chemical examination of these different portions of blood, and from the comparison of the time which the substance required to arrive from one vessel into another, endeavoured to ascertain the relative velocity of the blood.

The experiments were performed upon horses, and the following conclusions were established :—

a. The time within which the cyanide of potassium, after having been mixed with the blood, passes from one of the jugular veins into the opposite, is from twenty to thirty seconds; into the saphena magna, twenty seconds; into the arteria mesenterica, fifteen to thirty seconds; into the arteria maxilla externa of the opposite

side, from ten to twenty-five seconds ; and into the arteria metatarsi, from twenty to forty seconds.

b. The cyanide of potassium, within a very short time after its introduction into the blood, is excreted by the serous membranes, but in small quantity.   The time varies from two to eight minutes.

c. In the kidneys the excretion appears to take place with the greatest rapidity ; in all experiments, within one minute after the introduction into the blood, the cyanide of potassium was found in the cortical, sometimes also in the tubular substance, and in a few instances in the pelvis of the kidneys.

d. Only one minute is required to bring the substance from the jugular vein into the thoracic duct.

2. Cyanide of potassium and prussic acid produce no absolutely uniform alterations in the circulation of the cerebro-spinal system reconisable after death ; and the cerebral and reflex symptoms are not due to the engorgement of the vessels.   In some cases, the brain was not at all congested ; in others the veins were distended with blood ; and the blood after 15 hours showed a great tendency to transude through the coats of the vessels, from its disorganization, and thus inducing a much greater appearance of congestion and irritation than actually existed at the moment of death.

The symtoms of cerebral disturbance—delirium, coma, expansion of the pupil—were as strongly marked in the cases in which the brain was not specially congested as in the cases in which it was most congested.   Independent of these facts, it is evident that the mere state of engorgement of the blood-vessels could not produce death in so short a time.   In some cases the spinal cord was not specially congested ; in others the veins were distended with blood ; and in others still, both the veins and arteries were filled with blood ; Violent spasms, opisthotonos, and all the phenomena of aberrated spinal action, were as well marked in one condition of the spinal cord as in the other.   We are compelled from these facts to conclude that cyanide of potassium and hydrocyanic acid produce no absolute uniform alterations in the circulation of the spinal cord recognisable after death ; and that the aberrated muscular actions are not due to the engorgement of the blood-vessels of the spinal cord and its membranes.   Independently of the fact stated above, it is evident the mere state of engorgement of the vessels of the spine could not produce death in so short a time.   Careful microscopical examinations did not reveal any uniform alterations in the nervous elements.

3. The peculiar phenomena manifested by the cerebro-spinal nervous system, in poisoning by cyanide of potassium and hydrocyanic acid, are due to the action of the poison on the nervous elements, conveyed to them by the blood ; to the action of the altered blood on the nervous elements, and sudden arrest of the capillary circulation of the cerebro-spinal nerves, in consequence of the action of the poison on the sympathetic system and muscles and ganglia of the heart ; to the reflex action of the sympathetic system,

and to the complicated actions and reactions of the poison in the individual structures.

4. In poisoning by cyanide of potassium and prussic acid, the disturbances of the sympathetic nervous system are not less marked than those of the cerebro-spinal nervous system. The slow, full respiration, the slow action of the heart in some cases, and its feeble, rapid action in others; the feeble pulse, the diminution of temperature in the extremities, the rise of temperature in the trunk before death during the first stages of the action of the poison; the subsequent fall in the temperature of the trunk before death in some cases; the accumulation of the blood in the large veins of all the organs and tissues, in most cases of poisoning by hydrocyanic acid; the engorgement of the veins of the stomach, small intestines, spleen, liver, and kidneys; the suppression of urine in some cases—all point to aberrated nervous action of the sympathetic system.

The mere congestion of the blood-vessels of the sympathetic nervous system could not account for any of these phenomena; in fact, if the sympathetic nervous system presides more especially over the circulation, the stagnation of the blood in the vessels of the sympathetic, and in fact, in the blood-vessels of the cerebro-spinal nervous system, and of all the organs and tissues, is evidently the effect, rather than the cause, of the aberrated sympathetic nervous phenomena.

From these facts, it appears to be proper to conclude that the aberrated nervous phenomena of the sympathetic system are due to the direct action of the poison, conveyed in the blood, on the ganglionic cells of the sympathetic; to the action of the altered blood on the same elements, to the congestion of the blood-vessels of the sympathetic; and to the reflex action of the cerebro-spinal system.

If the disturbances in the action of the sympathetic nervous system do not precede, they are certainly coeval with those of the cerebro-spinal system; and are in both systems manifested precisely at the moment when the blood containing the poison reaches the nervous elements.

The arrest of the action of the heart, and of the peristaltic motions of the intestines, must be referred to the direct action of the poison on the sympathetic ganglia, and to the action of the poison on the unstriped muscular fibre.

5. Cyanide of potassium and prussic acid act on both the voluntary and involuntary muscles, and decrease or arrest entirely their property of contractility; and after death from these poisons, in many cases, it is impossible to excite contraction of the muscular fibres of the heart by mechanical or electrical stimuli.

6. The blood is altered; its colour is changed, as if prussic acid had entered into combination with the colouring matter; in most cases it coagulates imperfectly, and in some not at all.

7. Cyanide of potassium and hydrocyanic acid produce effects on all the organs and tissues with which they are brought into contact; hence we cannot affirm that their action is confined exclusively to the nervous system; and more especially would it be impossible to

affirm that the action is confined either to the cerebro-spinal or sympathetic nervous system, or that the primary action is on one or the other. Cyanide of potassium and prussic acid induce alterations in the constitution of the blood, and through this medium affect all the organs and tissues. We have established also that these and other poisons act on vegetables, which are destitute of nerves, and hence we may conclude that they are capable of acting on all the individual cells of the living animal."—*Reprint from the New Orleans Medical and Surgical Journal*, May, 1877.

II.—Forensic Medicine.

On the Influence of the Seasons on Suicide.—Mr. Vincent Richards reports that, in 1875, it was brought casually under his notice, by native Doctor Gopaul Chundra Gangooly, that suicides were of much more frequent occurrence in the subdivision of Goalundo during the hot months than at any other time of the year. On making enquiries he found that such was the case in the subdivision of Kooshteah also.

The following table gives the number of suicides, month by month, for five and four years respectively:

Months.			Goalundo.		Kooshteah.		Total.
January	.	.	3	.	1	.	4
February	.	.	6	.	...	.	6
March	.	.	8	.	2	.	10
April	.	.	9	.	3	.	12
May	.	.	12	.	5	.	17
June	.	.	16	.	3	.	19
July	.	.	11	.	2	.	13
August	.	.	7	.	1	.	8
September	.	.	6	.	1	.	7
October	.	.	4	.	4	.	8
November	.	.	3	.	2	.	5
December	.	.	2	.	1	.	3
	Total	.	87	.	25	.	112

Mr. Richards was so impressed by the above fact that he noticed it in his Annual Report in 1875. In an article on "Suicide in France," in the 'Saturday Review' of a month or two since, he read the following passage, which seems to confirm his view, that the hot season has a marked influence on the suicidal impulse: "Inquiring next into the influence of the seasons, we are prepared to find that it is great, for every one is aware how profoundly the weather affects his own health, spirits, and general enjoyment of life. But we naturally expected that it is in winter, when the days are short and the nights long and cold, when rain and snow and frost intensify the sufferings of the poor; when employment is scarcest, and necessities most pressing, and when out-of-door life is not possible to many, that suicide should be most prevalent. The very contrary is, nevertheless, the case. The proportion of the suicides in the first quarter of the year is about 22·1 per cent., in the second quarter 30·8 per cent., in the third 27·1 per cent., and in the last only 20 per cent.

Thus, in the six comparatively warm months, about 58 per cent. of all the suicides occur; May, June, and July, exceed any other three months, June standing at the head of all. The mania increases, in fact, up to midsummer, and then somewhat more rapidly decreases, December having the fewest suicides as June has the most." Through the courtesy of Dr. Mountain, Civil Medical Officer of Bancoorah, Mr. Richards obtained the following details regarding deaths from suicide in that district:

Months.	Number of Suicides.	Months.	Number of Suicides.
January	3	July	7
February	2	August	2
March	3	September	3
April	6	October	5
May	10	November	3
June	10	December	3
		Total	57

If we compare the above data we shall find that the months of March, April, May, June, and July, are those in which the very great majority of suicides occur. Thus, in Goalundo and Kooshteah during these months, 63·39 per cent. of the total number occurred, and in Bancoorah 63·15 per cent. Now, as to the reason, the 'Saturday Review' says: "What the reason of this should be we cannot even conjecture. M. de Foville puts forward a fanciful suggestion that, as the season of flowers is the most delightful to the happy, it aggravates the wretchedness of the miserable." Apart from the "fanciful" nature of the suggestion, M. de Foville evidently presupposes that the majority of suicides are the outcome of deliberation consequent on mental despondency. If such were the case, we should expect to find suicides more common in the cold weather, when, as the 'Saturday Review' says, "The days are short and the nights long and cold; when rains and snow and frost intensify the sufferings of the poor; when employment is scarcest, and necessities are most pressing, &c." But the very great majority of suicides are committed—no matter what the immediate cause may be—during a state of mental excitement; in fact, impulsively, when the nerves are, so to speak, strung to the highest pitch of irritability; even those of the most phlegmatic temperament must have experienced the comparatively irritating effect of the hot weather months. The reason, therefore, that impulsive suicide—as distinct from premediated—is so greatly influenced by the hot season in India, seems pretty obvious. Moreover, when we bear in mind what impulsive, nervous creatures natives are, especially women, who contribute so largely to the crime in this country, we shall at once understand how it is that this influence is so much more pronounced in their case than in the case of Europeans. Mr. Richards has no doubt that an examination of the returns of suicide in the United Kingdom would elicit the fact that this seasonal influence is less marked amongst Englishmen, and, perhaps, still less so amongst Scotchmen. The subject is pregnant with interest, and

would repay investigation by any one who had the necessary materials at his command.   He merely draws attention to it, in the hope that some one with better opportunities will do it justice.

Drs. Sandiford and Davis furnished Mr. Richards with the following information pertaining to the districts of Bogra and Pubna, respectively.   Of the 105 suicides which came to the notice of the medical officer in six years, in the district of Bogra, 15 (14·29 per cent.) occurred in the first quarter of the year, 32 (30·48 per cent.) in the second quarter, 28 (26·66 per cent.) in the third, and 30 (28·57 per cent.) in the fourth.   Of the 211 suicides, which came to the notice of the medical officer in ten years, in the district of Pubna, 48 (22·75 per cent.) occurred in the first quarter of the year, 64 (30·33 per cent.) in the second quarter, 47 (22·28 per cent.) in the third, and 52 (24·64 per cent.) in the last.   In the former district most suicides were reported to have occurred in the months of June, July, and October, and in the latter, during May, June, and December.   It will be observed that, notwithstanding the variation in respect to October and December, the statement that suicides are more frequent in the hot months than in the cold, is again confirmed by the above data, though in a minor degree, apparently, in the district of Pubna.   Tables showing the daily range of temperature and reported suicides in each district, would be very interesting. The temperature here has become suddenly higher during the past two or three days, and two suicides have been reported in that time, whereas there had been only two cases during the previous three months and none for about a month and a half.

Dr. Davis has since sent Mr. Richards the following, which shows the number of suicides for each month during the past four years, as furnished by the Police :—

January	.	.	8	July	.	.	22
February	.	.	10	August	.	.	20
March	.	.	16	September	.	.	7
April	.	.	21	October	.	.	16
May	.	.	22	November	.	.	12
June	.	.	19	December	.	.	9

The influence of the seasons is here strikingly illustrated.—(*Indian Medical Gazette*, April 2, 1877.)

Post-Mortem Appearances in Death by Hanging.—Dr. Robert Harvey supplies an analysis of these signs, as there is still apparent a tendency to rely unduly on particular signs which are not of necessity conclusive, and are thus apt to mislead.   Dr. Harvey's conclusions are derived from the medico-legal returns received from the civil surgeons in the Bengal Presidency during the years 1870, 1871, and 1872.   So many facts for analysis have never before been collected, and they are of such extreme value that we give the analysis at length.

(*a*) *General appearance of the body.*—This varies greatly with the period that has elapsed since death, and there is no point on which

medical jurists have been more divided in opinion. The true state of the case seems to be that, in the majority of instances, immediately after death, the features are placid, the face pale, the eyes not unduly prominent, the mouth closed or half open, the tongue pressed against the teeth, but not protruded, the superficial veins full, but the head, neck, and trunk free from lividity. After a longer or shorter time, however, and apparently after a few hours, in India, all this is changed. Livid patches appear about the chest, back, and shoulders; the face and head become bloated and puffy; the tongue and eyes protrude; and decomposition, setting in early, is most marked in the upper part of the body, which is gorged with stagnant blood. In most of the cases the body had not arrived at the dead-house till these changes had taken place; but the above statement is based on a careful analysis of 63 cases where the body is stated to have been fresh, and the particular points which justify it are as follows:—The face is noted as placid 25 times, livid 5; eyes natural 12, injected 7; mouth closed 23, open 5, half open 4; tongue not protruded 23, protruded or partly protruded 11; veins gorged 12; livid patches absent 5, present 3.

In 297 cases the body was more or less decomposed; in the remainder its condition is not stated.

(b) *Mark of the ligature.*—This was distinct and well-marked in 1162, indistinct or absent in 99 cases, and in the others it is not mentioned. It is noticed as ecchymosed in 165, and not ecchymosed in 37 cases. Its appearance is described as depressed in 280, parchment-like, tough, or leathery in 198, with or without ecchymosed or livid edges. A blistered mark is noted 7 times, and a hard, white, shining translucent band, from compression of the cellular tissue, 30 times. This is the first stage of the vellum or parchment-like appearance, and is chiefly noticed in fresh bodies. The mark was abraded 109 times, and smooth 11 times, the suspending agent being a soft cloth in 8 of these. In shape and dimensions it generally corresponded with the agent used, being narrow and well-defined when a rope, broader and more irregular when a soft ligature, was employed. Exceptions were, however, met with. Casper's observations and experiments led him to the conclusion that "any ligature by which any body may be suspended or strangled, not only within a few hours, *but even days after death*, especially if the body be forcibly pulled downwards, may produce a mark precisely similar to that observed in most of those hanged while alive;" and though few authorities go so far as this, the great weight of Casper's testimony cannot be disregarded. His further conclusion, that "the mark of the cord is a purely cadaveric phenomenon," seems inconsistent with the fact that the characteristic mark is sometimes found in non-fatal cases, though he himself had not observed it. In eight instances in the present returns there was a "distinct" mark; in one at Gauhati, where the patient was discharged from

hospital in a few days, Dr. Curran remarks that " the mark lasted a long time."

(*c*) *Protrusion of the tongue* is noted in 243 cases, while in 69 it is especially stated that the tongue did not protrude. It was most commonly found swollen, pressed against the teeth and indented by them, or partially pushed between them and bitten, its complete extrusion being rare, exept in putrid bodies.

(*d*) *Marks of saliva* running in straight lines down the chin and chest are noticed a few times, but do not seem to be often looked for. The sign (which Dr. Hutchison, of Patna, who first called attention to it, invariably found, and which the writer has seen in the only two hanged bodies he has examined in India) is a valuable one, as indicating, in fresh bodies at all events, suspension during life; and if further experience proves that it is generally present, as seems probable, it will be an important help in doubtful cases.

(*e*) *Discharges* (*a*) *in the male.*—Semen or mucus was observed on the clothes in 46 instances, and is noted as absent in 22; urine was found twice, and blood from the urethra four times. The penis is noted as erected in 5 cases, 3 of them from Nator, but any priapism which may have existed at the moment of death had commonly passed away before examination. In a case at Faridpur, where this phenomenon is described in a highly decomposed body, the erection must have been due to gaseous distension. (*b*) *In the female.*—The genitals were found congested in 24 cases. A mucous discharge from the vagina was observed in 22, and a flow of blood in 9 cases. Fæces were found on the clothes or about the anus 62 times, but it is probable that all these signs would be found more frequently if looked for, although they are of no great value when found. In a case by Mr. E. C. Bensley, in the Rajshahai return for January, 1870, stains and clots of blood about the perineum and anus proved to have come from some internal piles which had burst.

(*f*) *Condition of the brain and its membranes.*—One or both of these are described as congested in 490 cases, and natural in only 24. In the remainder the skull was not opened, or the state of its contents is not mentioned. Serum, plain or bloody, was effused into the lateral ventricles eighty-five times, into the arachnoid twenty-one times, and at the base three times; while in 23 instances, or nearly $4\frac{1}{2}$ per cent. of the cases where the state of the brain is mentioned, blood was effused in or about it—a much larger proportion than has ever been observed in Europe. It is curious, too, that 12 of the cases occur among 151 reported from Dacca, and 9 of these among 47 in the year 1870. The lungs were congested in all but one. In 9 both sides of the heart contained blood, while in 2 only the right side alone was distended. It was empty in 6, but 3 of these bodies were putrid.

Blood had flowed from the ear in 6 instances; but details are wanting.

(*g*) *Injuries of the cervical vertebræ and ligaments* are rare.[1]

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[1] So rare that Taylor—speaking of fracture of the spine in hanging—remarks (in his last Edition, p. 667)·—" So far as I am aware there is no case of *suicide*

Rupture of the transverse ligament of the atlas was noted by Native Doctor Fatteh Ali, of Goalpára, in September, 1870. The subject hung herself with a twisted cloth.

Dislocation of vertebræ.—No. 1, reported by Honorary Surgeon-Major R. F. Thompson, Civil Surgeon of Húghli.

Subject, a Mussulman, aged 39. " Distinct mark of a cord around the neck ; no other marks of violence. Laceration of the larynx and dislocation of odontoid process."

Reported by Mr. W. J. Ellis, Civil Surgeon of Hamirpúr.

Subject, a male Hindu, aged 70. " Mark of a cord round the neck, superficial in front, deep behind. Second cervical vertebra dislocated."

Reported by Surgeon H. G. Hall in the Baital (C.P.) return for May, 1872.

Subject, a Hindu female, aged 22. " The loop was placed between left mastoid process and ear, and was so tightened as almost to cause the head to rest directly on the shoulders ; odontoid process dislocated."

By the same officer in the return for November, 1872.

Subject, a Gcudi, aged 20. " A deeply contused wound caused by a rope which was tightly fastened round the neck ; knot in front of angle of inferior maxilla on right side. Axis dislocated from atlas."

Fractures.—Two cases of fracture of the odontoid process in bodies examined on the same day are given in a return for September, 1871. The *post-mortems* were made by a hospital assistant in the absence of the Civil Surgeon, who has confidently informed the writer that he considers the cases very doubtful and untrustworthy.

Reported by Surgeon E. B. Gardner in the Jhilum return for April, 1872.

Subject, a Khatri, age and sex not given. " Found hanging on a tree; usual signs; odontoid fractured. Rope, in double noose without knot, a common *dooree*, such as is used for drawing water."

(*h*) *Appearances in the larynx and trachea.*—More or less of congestion, varying from a slightly heightened pinkishness to a deep plum-coloured injection, was noted in 303 of 401 cases in which the condition of the windpipe is mentioned. As the windpipe is the *first* organ affected by putrefaction, and is often found of a dull brick or cinnabar-red colour when the rest of the body is almost quite fresh (*Casper*), it is probable that in some of the cases the appearance was due to decomposition. In 47 the mucous membrane was natural or pale. Frothy fluid, often tinged with blood, was noticed 93 times. Prominence of the follicles, with exudation of a gelatinous mucus from them, is noted a few times. The trachea is described as compressed 23 times, and lacerated 11 times ; in two instances the cartilages of the larynx were separated by the cord pressing on the crico-thyroid membrane, and in cases at Dehra Ismail Khan and Maldah the os hyoides was fractured. In five cases fractures were

on record in which such an injury to the neck has been found ;" and Chevers (p. 611) says " fracture or *lateral* dislocation of the vertebral bones appears to afford certain evidence of homicide."

found in the cartilages of the larynx, but there is nothing to show how this accident is conditioned. Extravasation of blood into the cellular tissue of the neck and about the larynx is specified 43 times : the blood was for the most part in soft clots, but in a few instances it was fluid. In one of the Puri cases it had been poured into the thyroid gland.

(*i*) *Appearances in the lungs.*—In 738 of 834 cases the lungs were found congested, in 77 they were natural, and in 19 collapsed. Rupture of the superficial air-cells is noted in a few cases, and the lungs were mottled with spots of sub-pleural ecchymosis in a few more. Surgeon-Major C. T. Paske gives a curious case in the Mirzapur return for August, 1872. The body of a woman, aged 27, was so decomposed that a minute examination was impossible, but a distinct rope mark, with extravasated blood beneath, was found on the neck; the tongue protruded and reddish froth issued from the nose.

" Left lung ruptured posteriorly, with considerable extravasation of blood in left pleura ; no external marks of violence could be discovered ; stomach empty ; womb unimpregnated. Cause of death hanging. The rupture of the lung must have arisen, I think, from intense sudden congestion of the part."

The result of the case is not mentioned.

Condition of Pericardium.	Cases of Hanging.		Cases of Strangling.	
	No.	p. c.	No.	p. c.
Empty	35	26·52	3	9.37
Plain Serum[1]	72	54·54	7	21·88
Reddish ,,	7	5.30	2	6·25
Bloody ,,	18	} 13·64	18	} 62·50
Blood	...		2	
	132	100·00	32	100·00

(*k*) *Appearances in the pericardium and heart.*—In several of the hanging cases where bloody serum was present, there are indications, from the position of the ligature, that death was slow, the supply of air being only partially cut off.

It must be admitted that bloody serum may be found in the pericardium of a fresh body dead of suicidal hanging, although it is no doubt rare.

In strangulation, on the other hand, it is very common. Mr. Stewart found it in every one of "five suspected cases in which evidence afterwards more or less showed that violence was the cause of death."

The conditions of the heart is mentioned 626 times. In 320 the right side was gorged, and the left almost or altogether empty. In 121 cases both sides were pretty equally distended, in 15 the left side alone contained blood, and in 179 both sides were empty. A careful analysis of these cases has been made, but is of little value, as the period between death and examination is very seldom mentioned. The main points seem to be—1st, that in a large proportion of the cases, especially if the body be fresh, the right side of the heart, pulmonary artery, and venæ cavæ are distended with dark

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[1] Includes cases where the pericardial fluid is not described in detail.

fluid blood. In most of these cases the lungs are much congested, and the signs of death by apnœa are well marked; 2nd, that in a considerable number both sides of the heart contained blood. In some of these death has been slow, and the blood in the left heart is found in addition to the ordinary. In others—and they are more numerous than is commonly supposed—the appearances are purely negative; the brain, lungs, trachea, and larynx are perfectly natural, as well as the heart, and the conclusion that death has been caused by hanging rests on the history of the case, the rope mark, and the absence of any other cause of death. In such cases, Casper considers that death is due to neuro-paralysis or nervous apoplexy, such as sometimes occurs after blows on the stomach affecting the solar plexus; 3rd, that when decomposition has advanced all the cavities are very often empty; and 4th, that in a small proportion of cases (about $2\frac{1}{2}$ per cent. in the present returns) the left side only may contain blood, the right being completely emptied by contraction after respiration has ceased (?). In 11 of the 15 cases where blood was confined to the left side the lungs were much congested; this condition is not stated in 4. One was examined thirty-three hours after death. Eighty-one of the 170 subjects in which the heart was empty, are expressly stated to have been decomposed, and Dr. Irving, of Allahabad, remarks, with reference to such a body, that " if the post-mortem had been held sooner fluid blood would most likely have been found in the right side of the heart."

(*l*) *Appearances in the abdominal viscera.*—The stomach was found congested in 111, natural in 127 cases. The bowels were congested 177, natural 142 times, but this includes a number of cases where intestinal disease was the motive to suicide. The liver was congested 155, natural 161 times. Kidneys congested 191, natural 55 times. The only point of much importance is the congestion of the stomach, which might, and in one instance did, lead to suspicion of poison.

The above analyses, imperfect as they are, amply prove, if proof were needed, that there is no one sign which, taken by itself, can justify an opinion that death was caused by hanging, and point to the necessity of a careful examination in every case. Each sign, though of comparatively little weight by itself, is strengthened by, and strengthens every other sign.—*Ibid*, Feb. 1st, 1876.

III.—Hygiene.

Relations of Pain to Weather.—Dr. Weir Mitchell, of Philadelphia, with his usual original spirit of research, addresses his investigations to this singularly interesting subject. He sets forth by stating the long accepted fact that under certain states or changes of weather there is return of pain in cases of old wounds, injuries and diseases of bones, chronic rheumatisms, and the like. In the year 1872, when writing his book on injuries of nerves and their consequences, he had occasion to study the many curious symptoms of pain in the the stumps of amputated limbs, and was struck with the number

of persons who stated that their comfort depended largely on the state of the weather. The difficulties of getting reliable informa- for precise scientific record were, Dr. Mitchell found, very great. At last he was fortunate to find one who was a sufferer, and who, by his intelligent observations, extending over three years, enabled Dr. Mitchell to arrive at some correct and most interesting facts. The gentleman to whom he was thus indebted is Captain Catlin, a captain in the United States Army, aged 35, who in August, 1864, at the Waldon Railroad had his foot crushed by a twelve pound round shot. The wound healed readily, but there was frequent recurrence of pain in it, which has continued ever since. When seen by our author in December, 1874, the pain was said to be in the metacarpo-phalangeal articulation of the great toe, and some- times through all the toes, with more or less the sense of twitching. The attacks are preceded by a tendency to sleep. After describing the symptoms more minutely, Dr. Mitchell proceeds to explain the accurate series of observations which his patient carried out bearing on the variations of weather and the recurrence of pain. The conclusions arrived at are ultimately stated as follows :—

" The relations of pain to the states of atmospheric pressure are in this case apparently definite and nearly constant.

" Thus we find that when the atmospheric pressure lessening, the mercury falls, there is apt to occur during the fall, and before it is complete, an attack of neuralgic pain or of burning in the lost foot, and that this is most likely to take place when the lessening pressure culminates in rain. In some months, as in September and October, 1875, this is most constant, indeed almost invariable, so that, at all events in the present case, a falling barometer followed by rain as a rule insures an onset of pain."

In summing up the author adds :—" The human economy is arranged by nature to have, as it were, a climate of its own, with very permanent states as to temperature, humidity, electric condi- tions, and the like ; but all of these are subject to variations, some of them natural, and, so to speak, rhythmic and chronal ; others more or less irregular. As they are part of the functional activities of the body, so do they, of necessity, enter into every consideration of the causation of pain.

" While, however, we may feel sure that they are thus active, their precise relations to the existence or to the favouring of the birth of pain are too uncertain for us to do more than surmise that they sometimes obscure or interfere with or prevent the positive effects of external climatic states in this direction.

" Any lowering cause, such as dyspepsia, overwork, anæmia, however brought about, is apt to increase this sensitiveness to barometric changes ; and so every enfeebling agency, as it were, tunes a man's nerves up to the capacity of producing pain, when once there exists a permanent cause in the way of neural disease. As an illustration, I know of an officer who is the subject of stump-neuralgia, which is very prone to show when a storm is imminent, but the liability is at the maximum in the spring and fall, when he prone to feel the de-

pressing influence of an old ague-poisoning, and when this is mastered the weather loses its influence.

"A large number of neuralgic attacks seems to be definitely related to those perturbations of atmospheres, which we know as storms.

"The separate factors of storms, such as lessened pressure, rising temperature, greater humidity, winds, appear, as a rule, to be incompetent, when acting singly, to give rise to attacks of pain. Either, then, it is the combination which works the mischief, or else there is, in times of storms, some as yet unknown agency productive of evil. Such an agent may be either electricity or magnetism.

"As concerns the former, we have failed to study its relations to pain, because of difficulties as to instruments and methods of research, difficulties which may, I trust, be overcome. Neither, as I said, have any observations been made as yet as to the influence of magnetism for want of proper instruments; and this is the more to be regretted because of the following facts. In 1867 and 1868, the aurora borealis was frequent and remarkably brilliant, and Captain Catlin then had it most forcibly called to his attention that the neuralgia was apt to prevail when the northern lights were intense. This may be due to magnetic or electric disturbance, but it may also be owing to the fact that an intense aurora is apt to be followed by a storm, indeed, is almost sure to be, if we may trust the recent observations of Lieutenant Weyprecht of the Austrian Polar Expedition; also, it is a common belief among our Northern Indian tribes. The pain, then, which followed the northern light may be merely an ordinary storm pain; but the question deserves a more exact answer.

"There seems, then, to be every reason to believe that the popular view which relates some pain fits to storms has a distinct foundation, and, as we have seen, it has stood the test in this single case of a long and patient scientific study. At the same time we have failed to detect the single element of mischief, and are thus far driven to believe that it is the combination of atmospheric conditions which starts the pain into being.

"A still more valuable and novel conclusion has arisen out of our study. Every storm, as it sweeps across the Continent, consists of a vast rain area, at the centre of which is a moving space, of greatest barometric depression, known as the storm centre, along which the storm moves like a bead on a thread. The rain usually precedes this by 550 to 600 miles, but before and around the rain lies a belt, which may be called the neuralgic margin of the storm, and which precedes the rain about 150 miles. This fact is very deceptive, because the sufferer may be on the far edge of the storm-basin of barometric depression, and seeing nothing of the rain, yet have pain due to the storm.

"It is somewhat interesting to figure to one's self thus—a moving area of rain girdled by a neuralgic belt 150 miles wide, within which, as it sweeps along in advance of the storm, prevail in the hurt and maimed limbs of men, and in tender nerves and rheumatic joints,

renewed torments called into existence by the stir and perturbation of the elements."—*Separate Treatise extracted from the American Journal of the Medical Sciences* for April, 1877.

The Medical Organization of the Factory Acts.—Few measures could tend more to the health of the factory operatives of this kingdom than a perfect system of medical organization. To obtain such perfection is one of the persistent and earnest efforts of Dr. Arlidge, whose latest essay on the subject is now under notice. The author very correctly contends that the primary purpose of the Factory Acts was sanitary, in other words, factory legislation was called into existence to rescue the workers in factories, and especially the youthful workers and women, from physical suffering, degenerescence, disease, and premature death, consequent upon overwork ; unhealthy places of work, and the employment of the too young, the infirm, the diseased, and the deformed. The danger now ahead in respect to medical factory legislation is that the recommendation of the late Royal Commission to dispense with the surgical examination of children after their primary entrance on work, may be carried out. Against this danger Dr. Arlidge protests in the most earnest manner. It is a recommendation, he says, destructive of all good that can flow from medical inspection. It involves in itself the absurd supposition that a child certified as of sufficient strength and free from sickness at two years of age, will continue to be so during the whole course of years it continues at work and is subjected to the conditions of the factory laws. It seems to ignore the possibility of casual disabling, sickness, and accident, the outset of contagious maladies, and the morbid results that specially follow upon factory labour.—*Paper read at the Social Science Congress at Liverpool,* 1876.

[The argument employed by Dr. Arlidge throughout his admirable essay is of the soundest character. It deserves the attention of the sanitarian and legislator, and the gratitude of the industrial class, in whose behalf it so eloquently pleads.—B. W. R.]

IV. SUMMARY.

On the Dead as a possible Source of Infection. By FRANCIS VACHER. Pamphlet, London, 1877.—A pamphlet by the Medical Officer of Health for Liverpool. The author thinks it possible that the dead body may be a source of contagion, and he suggests that better provision should be made against the exposure of infected corpses.

Annual Report for 1876 *of the Committee of Management of the Melbourne Retreat for the Cure of Inebriates, Northcote.* Special Report, 1877. By CHARLES MCCARTHY, M.D., Superintendent.— Dr. McCarthy states that " a death from drink occurs in this colony for every day in the year." He gives a gratifying report of the success of the system at the Retreat over which he presides.

The Law of Life Assurance. A lecture to the Institute of Accountants and Actuaries in Glasgow. By PROFESSOR ROBERT BERRY, M.A. Separate Treatise, Glasgow, 1877. An admirably clear and comprehensive lecture, dealing with the legal aspects of insurance, but containing useful knowledge for medical men.

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Reclamation of Dr. J. W. Moore.

Note to the Review in our last number of Vols. 4 and 5 of Ziemssen's 'Cyclopædia of the Practice of Medicine.'

As bearing upon the discovery by Leyden of microscopic octohedral crystals in the expectoration of some asthmatic patients, we have received a letter from Dr. J. W. Moore, of Dublin, in which he calls attention to the fact that in the 'Irish Hospital Gazette' for July 15, 1873, he has recorded his detection in the sputa of a gouty subject of some uncommon varieties of uric acid crystals. He describes them as presenting three forms: one resembling stearic acid, another as having the appearance of diatoms, whilst some of the largest bore a strong resemblance to spear-headed crystals of uric acid. A second scrutiny proved that they were instantly soluble in liquor potassæ, and slowly so in acetic acid. Dr. Moore is of opinion that the crystals described by Leyden, and those found by himself, are examples of the same pathological element. It is not improbable that this may be the case; but Leyden's remarks being confessedly imperfect, a more positive statement is not yet warranted. We have pleasure, however, in giving further publicity to Dr. Moore's interesting observation.— REVIEWER.

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1877.

Analytical and Critical Reviews.

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I.—The Temperature in Phthisis and Tuberculosis.[1]

In the great development of medical thermometry which has taken place during the last twenty-five years, it was very natural that the chief attention of clinical inquirers should be

[1] 1. JOCHMANN (P. A.) *Beobachtungen über die Körperwärme in chronischen fieberhaften Krankheiten.* Berlin, 1853.

2. RINGER (SYDNEY). *On the Temperature of the Body as a means of Diagnosis in Phthisis and Tuberculosis.* London, 1865. Also, Second Edition, London, 1873.

3. WUNDERLICH (C. A.). *Das Verhalten der Eigenwärme in Krankheiten.* Leipzig, 1868. Translated from the Second Edition by Dr. WOODMAN. *On the Temperature in Diseases.* London, 1871.

4. BOILEAU (Assistant-Surgeon). *The Correlations of Temperature, Pulse, and Respiration in Phthisis.* 'Army Medical Reports,' for 1868. London, 1870.

5. FINLAYSON (J.). *On the Temperature of Children in Phthisis and Tuberculosis.* 'Glasgow Medical Journal,' November, 1869.

6. FOX (ED. LONG). *Clinical Observations on Acute Tubercle.* 'St. George's Hospital Reports,' vol. iv, 1869. Also, *Clinical Observations on the Temperature of Disease.* 'Medical Times and Gazette,' 1870.

7. EUDE. *De la Marche de la Température dans la Fièvre hectique.* 'Gaz. Med. de Strasburg,' November, 8, 1871. (Canstatt's 'Jahresbericht,' 1871, p. 219.)

8. ROGER (H.) *Recherches Cliniques sur les Maladies de l'Enfance.* Tome i, Paris, 1872. (*De la Temperature chez les enfants.* Pp. 203-466).

9. BILHAUT (M.). *Etude sur la Température dans la Phthisie Pulmonaire.* Paris, 1872.

10. BETTELHEIM (K.). *Bemerkungen über einige Eigenthümlichkeiten des Fiebers im Beginne Phthisicher Zustände.* 'Deutsches Archiv für klinische Medicin.' Bd. x. Leipzig, 1872.

11. ALCOCK (N.). *On the Nature and Variations of Destructive Lung Disease.* 'Army Medical Reports' for 1872. London, 1874.

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directed in the first instance to the phenomena of acute disease.
The study of the onset and the decline of pyrexia in its rela-
tion with all the striking symptoms of an acute illness, begun
and ended in a short period of days or weeks, was well calcu-
lated to arouse and sustain a lively interest in the observer.
The protracted course of a chronic disease, on the other hand,
whose beginning or end might be quite unknown, and whose
slow downward progress was apt to be complicated by morbid
processes which were often beyond recognition during life, and
were not unfrequently left in much obscurity even in the event
of a careful dissection, demanded more patience for its study,
and this department of thermometry could scarcely be developed
so early or so completely as the other. When, however, tem-
perature observations became part of the regular routine in all
well-ordered cliniques, the accumulation of such a multitude
of facts raised some hope of success in grappling with the
difficulties presented even by such chronic and such complicated
diseases as phthisis and tuberculosis. And just because of the
difficulties hinted at the subject has been pursued by a succes-
sion of inquirers with the utmost diligence. For if, as we know,
destructive processes can go on in the lungs without affording
proof of their presence by auscultation and percussion, or other
definite evidence, this seemed a strong reason for testing to the
utmost the resources of any newer appliance ; in the quiescent
forms of phthisis, on the other hand, such researches might
correct or supplement the evidence of physical signs in showing
that, although the organs were changed, the morbid processes
leading to such results had themselves come to an end. The
hope of thus aiding our means of diagnosis and prognosis be-
came a powerful stimulus in this inquiry ; questions of treat-
ment, and more especially the testing of the effect of various
remedies, were also directly involved in the same consideration.
But further, the forms of disease clinically grouped together
as phthisis have long been known to present certain very ob-
vious differences when they come to be examined after death,
differences so great, indeed, in the opinion of some, as to demand
the subdivision of phthisis into several varieties, or even to

13. LEBERT (Prof.). *Veränderungen der Körperwärme im Laufe der Tuber-
culose.* 'Deutsches Archiv für klin. Medicin.' Bd. xi. Leipzig, 1873.
14. FOX (WILSON). *On the Temperature, Pulse, and Respiration in Phthisis
and Acute Tuberculization of the Lungs.* 'Medico-Chirurgical Transactions.'
Vol. lvi. London, 1873.
15. WILLIAMS (C. THEODORE). *On the Temperature of Phthisis Pulmonalis.*
'Medico-Chirurgical Transactions.' Vol. lviii. London, 1875.
16. SCHWARZ (ALBERT). *Ueber den Fieberlauf bei Phthisis Pulmonalis.*
'Verhandlungen der Physikal-Medicin. Gesellschaft in Wurzburg.' N. F.
Bd. ix, 1876.

imply a revolution in pulmonary pathology. Here, again, it was hoped that the thermometer might assist in the differentiation of such cases, by the discrimination of the more serious or the more rapid forms of the disease from the more chronic or the more hopeful; while by its means we might, perhaps, trace the onset of complications, whether accidental or otherwise, and so gain an insight into the less obvious pathology of this dreadful disorder.

The most general result of the thermometry of phthisis is to show that, as a rule, the patients present more or less pyrexia during their illness. As this rule, however, is subject to various qualifications and exceptions, we will first consider *the circumstances under which low temperatures— whether normal or subnormal—are met with.* All authorities agree in stating that even in the midst of febrile disease there is occasionally a dipping down of the temperature to the normal or subnormal level, sometimes with and sometimes without other signs of collapse. Such dips occur in phthisis also; the evidence of the reality of these depressions is to be found in the record of such temperatures, even when the measurements are made in the rectum or vagina, and so are more likely to indicate the real heat of the body;[1] and these depressions are specially apt to occur towards the fatal termination of phthisical cases, as will appear when we discuss this subject in a separate section. Again, the *morning* temperatures (those before 10 a.m.) are in many phthisical cases either strictly normal or possibly subnormal; if to these we add the cases in which the elevation of the morning temperature is so slight that we can scarcely pronounce it distinctly unnatural, we have a very large proportion of ordinary cases of phthisis, many of which, or, perhaps, we should say most of which, would present unmistakable evidence of pyrexia if the temperature were taken after midday or in the evening. Dr. Boileau calls attention very pointedly to this fact in the ' Army Medical Reports for 1868,' in connection with the use which invaliding boards might be disposed to make of the thermometer, as these consultations usually take place in the forenoon (p. 292). The importance of noting and considering the hour at which the temperature is taken, before judging of the absence or the degree of fever, is now so universally recognised that we need not insist upon this point.

Of more serious import is this question : whether the temperature, when fairly taken at various periods of the day, and for

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[1] See Bilhaut (op. cit.), pl. i, fig. 3, where the temperature dipped down to 37·8° (100·04°) although usually between 39½° and 40° (103·1° and 104°). Also pl. ii, fig. 10, where it fell to 36° (96·8°) on two occasions, although usually between 38° and 39° (100·4° and 102·2°).

several days in succession, is always elevated in advancing cases of phthisis and tuberculosis. This was the proposition which was generally supposed to be advocated in Dr. Sydney Ringer's book when he published it in 1865, although from the preface to his second edition it seems he has been misunderstood. It appears that he meant that an abnormal elevation at some period of the day was necessarily present only in those conditions leading to fresh depositions of the morbid products, formerly known as grey and yellow tubercle, and that he did not refer to the destructive changes and fatal course which alterations in such deposits might produce. Such a distinction removes the discussion to the realm of theoretical pathology; for however important and interesting such a discussion may be, we must agree with Dr. Pollock in considering that it " has not much practical value."[1] It may, in any case, be confidently asserted that glaring cases of phthisis, characterised, perhaps, by the expectoration of lung-tissue, frequently present a course of temperature which may practically be called normal. Dr. Hillier said that he had " certainly met with some cases of tubercular disease in which the morbid process appeared to be making decided progress, in which there was no distinct elevation of the temperature,"[2] and Dr. Finlayson likewise arrived at a similar conclusion in his inquiry concerning children.[3] No less explicit are the statements regarding adults. Dr Boileau[4] supplies details of a case observed for five months before death, in which 100·2° (37·88°) was the highest temperature noted, while the average of the first three weeks gave 98° (36·66°) for the morning temperature, and 99° (37·22°) for the evening, and during the last week of life the average was 98½° (36·94°). His cases appear to be carefully noted, and they bear out his opening statement, " that a person may progress to the stage of tubercular cavities in his lungs without exhibiting general elevation of temperature."

Dr. Wilson Fox, in his tabulation of cases (Table V) gives eight cases of phthisis as having an average evening temperature not exceeding 99° (37·22°), these eight cases constituting a considerable percentage of the more chronic cases dealt with in his summary. The majority indeed of his cases had an average of the evening temperatures not exceeding 100° (37·77°).

Prof. Lebert found ten cases ($\frac{1}{15}$ of the whole) with an average temperature in the evening of between 37° and 38° C. (98·6° and 100·4° F).

[1] " Clinical Varieties of Consumption." By J. E. Pollock, M.D. ' Med. Times and Gazette,' 1874. Vol. ii, page 98.
[2] ' Diseases of Children.' By Thomas Hillier, M.D. London, 1868, p. 115.
[3] Op. cit., p. 24.
[4] Op. cit., p. 295.

Dr. Schwarz found that 38½ per cent. of his cases could be classified as having an evening temperature within the normal range (38° C. = 100·4° F.).

Dr. Theodore Williams considers the third stage of quiescent phthisis, or that of chronic cavity, to be characterised by normal or subnormal temperatures, so that the results of observations on twenty patients show an average which does not exceed 98·6° (37°) at any period of the day (Table V), and the mean temperature in his " first stage, quiescent," does not exceed 98·7° (37·05°) (Table II).

Dr. Ringer himself, in the second edition of his little book, admits " that in some cases of tuberculization, or of catarrhal pneumonia, there may be no elevation of temperature" (p. 9), and " that a natural temperature must not lead us to conclude that this disease is not slowly spreading" (p. 13).

But the most remarkable opinion as regards low temperatures in phthisis is that of Surgeon Alcock, who seeks rather to diagnose threatened tubercular disease by the detection of an

"Inability on the part of the person in whom the requirements for tubercle exist and are about to be set in motion *to maintain the temperature of the body up to the natural standard,* the deficiency amounting to about one degree Fahrenheit (·55 C.) in the morning, and to ·5 of a degree F. (·27 C.) in the evening reading."

He goes on to say that—

"The value of this sign first forced itself into notice by its having been accepted as a distinct proof of the non-existence of phthisis... men, therefore, having many of the obscure premonitory symptoms, but without any of the physical signs, were in consequence of the low temperature confidently pronounced to owe their delicacy to some other cause than incipient consumption, yet the result unexpectedly proved that the disease had been latent at the time" (loc. cit. p. 404).

Diagrams are appended to illustrate the statement here made, and from these it appears that in six cases in which signs of tubercular disease became obviously developed in the course of a period varying from a month or two up to a year or more, the low temperatures represented in the diagrams had been previously noted. We do not venture to deny the occurrence of a period of abnormally low temperature before the development of recognisable tubercular disease, because opportunities of observing such cases in such stages have seldom presented themselves, but we cannot refrain from scrutinising the evidence adduced with some scepticism. Researches leading to the investigation of non-febrile states always convince the observer of the variability of the human temperature, and of the fre-

quent occurrence of temperatures considerably below the gene-
rally recognised standard.

The diagrams given by Surgeon Alcock represent a range of
temperature between 97° and 98° (36·11° and 36·66°), for the
most part, in this early stage, but occasionally it is recorded to
be nearly as low as 96° (35·55°). Unfortunately the author does
not mention what precautions he took to prevent errors from
imperfections in the application of the thermometer. It is quite
certain that, in special conditions of the surface temperature,
much time and patient care are required to obtain the maximum
temperature of the axilla. It is equally certain that the range
of the normal temperature is very considerable, as we will have
occasion to show further on, and so we must not too readily
pronounce a temperature to be abnormal although it is under
98° or even 97° (36·66° or 36·11°). We must look to army
surgeons for further information on such points, as they have
opportunities possessed by few others of determining the tem-
perature during slight ailments occurring before the distinct
manifestations of phthisis.

More serious still is the allegation that we may have actually
febrile attacks in the premonitory stages of phthisis, as mani-
fested by alterations in the pulse, by sweatings, and by other
general symptoms, without any appreciable elevation of tem-
perature. "Elevation of temperature," "pyrexia," and "fever,"
have come to be used almost indifferently as meaning much the
same thing—a view attacked by Bettelheim, who alleges the
frequent occurrence of disturbances of various kinds in the his-
tory of phthisical patients before their disease is pronounced,
which he does not hesitate to call "febrile," on the strength of
the indication of the pulse, sweatings, and such like; the favor-
able influence of quinine seems also to him to strengthen this
view of the febrile nature of such attacks. This author alleges
that in such attacks, apart from intercurrent inflammations, the
temperature did not, as a rule, exceed 37·3° or 37·6° (99·14° or
99·68°), and scarcely once reached 38° C. (100·4°) Dr. Boileau,
in his paper on the "Correlations of Temperature, Pulse, and
Respiration in Phthisis," likewise referred to the coexistence
of a high pulse rate with a low temperature in phthisis, and
considered "the rapidity of the pulse to be more constant in
phthisis than the elevation of temperature" (p. 292). We are
prepared to admit the theoretical impropriety of using the word
"fever," as simply synonymous with elevation of temperature;
perhaps "pyrexia" is a better word to express this important
element in such cases, and some observations by Dr. Burdon
Sanderson and others seem to imply that certain morbid pro-
cesses may prove fatal without any elevation of temperature in

certain subjects, although in most cases high fever is produced. But we are far from admitting that a diagnosis of fever is clinically justifiable simply from the rapidity of the pulse, or the occurrence of sweatings, even if periodical in their character. The effects of exertion and excitement on the pulse-rate are well known, but their potency can probably be only realised by those who have made careful observations in a regular and systematic manner ; in certain persons and in certain conditions a turn of the body in bed, a word, or a cough, will send it up to a height which is quite misleading. How much we would be misled by an undue regard paid to the quickened pulse-rate and the appearance of sweating may often be seen on examining a rickety child while asleep in bed, although such a position is extremely favorable for securing an undisturbed state of the pulse. Indeed, we reckon temperature observations so important, in cases of phthisis as well as in other diseases, for the due estimation of the value of acceleration of the pulse, the presence of sweatings, and the occurrence of shiverings, that we feel but little inclined to attach much consideration to Bettelheim's remarks, or to any similar observations, unless the investigations are stated in such a way as to carry conviction that the utmost care had been used to avoid fallacies in the taking of the temperature as well as of the pulse.

Before leaving this part of the subject we may add that certain complications of phthisis seem to have a tendency to reduce the pre-existing febrile temperature ; meningitis, hæmoptysis, perforation of the bowel, and pneumothorax may be named amongst these, but this effect is not constant, and is usually quite temporary, for a febrile reaction not unfrequently succeeds the depression, if the patient survives the shock of the last-named accidents.

The character of the pyrexia in phthisis is the next point to be considered. Of this it may be said, in general terms, that it is usually slight or moderate, and only exceptionally very high ; that it is usually of the remittent type, with the morning temperatures nearly normal, or perhaps subnormal, and the exacerbations falling on the afternoon or evening ; but this form of fever is apt to be interrupted from time to time by the inversion of this type, by the occurrence of a double daily paroxysm, and by occasional oscillations of an irregular character, as well as by periods of pretty high continuous pyrexia.

The first effort to formulate the types of fever in phthisis and other chronic febrile diseases was made by Jochmann. His classification was remarkably elaborate, consisting of eight divisions or subdivisions, the total number of cases dealt with

in his essay being only twelve. It must be regarded as a great compliment to his observations that he seems to have recognised most of the varieties subsequently more fully investigated, and to have perceived the irregular way in which one febrile type supplanted another in the same case. His results have been considered so important that short accounts of his investigation, and particularly of his classification, are given by Dr. Finlayson and Dr. Wilson Fox at the beginning of their papers. Subsequent writers have recognised the undue complexity of Jochmann's divisions, but they have not been very successful in substituting anything better. Wunderlich speaks of the pyrexia in acute miliary tuberculosis resembling that found in the beginning of a catarrh, or that observed in the course of enteric fever, or as resembling the course of an intermittent fever; and he says that in acute phthisis, while the type is remittent, presenting considerable remissions, $3°$ C. ($5·4°$ F.), there are interruptions to this course, with periods of moderate or even of normal temperatures; in his remarks on chronic phthisis he refers to the low temperatures apt to be induced in this complaint through the influence of inanition. Various efforts have been made to reduce these forms to three or four types. Thus, Dr. Finlayson suggested three types, stated in a condensed form by Dr. Woodman in his translation of Wunderlich's ' Medical Thermometry' (p. 413), as follows:

"First type. The morning temperatures are normal or rather less than normal, whilst the evening temperatures are more or less high. Second type. The morning and evening temperatures are both high, whilst there are evening exacerbations. Third type. The morning and evening temperatures are both high, but there is a tendency to exacerbations at odd times."

M. Eude, in his remarks on the temperature in hectic fever, tried to reduce the types to four varieties :—First. The morning temperature normal, the evening more or less high. Second. The morning temperature as well as the evening abnormally high. Third. The morning temperature abnormally low and the evening abnormally high. Fourth. The morning temperature abnormally high and the evening normal, or at least less elevated than the morning one.

Schwarz recognises five types; we again make use of Dr. Woodman's abstract (' Medical Record,' 1876, p. 394.)

"A. The type of normal temperatures; both morning and evening temperatures are within normal limits, the evening being a few tenths above the morning (17 cases).

"B. Type with slight evening fever, with normal but seldom subnormal morning temperatures (20 cases).

"C. Type of more intense evening fever, in which we get—

" (1) Morning temperature normal or subnormal, so that the charts resemble those of an intermittent quotidian or tertian fever.

" (2) The morning temperatures are febrile also, but a few tenths lower than the evening ones, thus resembling a continuous remittent fever (50 cases).

" (B. and C. are, however, members of one family.)

" D. The inverted type. In this the morning temperatures are febrile, often extremely so, and the evening normal or only moderately febrile.

" E. The typus inversus intercurrens, which occurs occasionally in all varieties of the fever."

He adds that these types run one into the other, and are seldom constant.

Even this classification, although somewhat complicated, does not include the " duplicated quotidian" type mentioned by Wunderlich, to which we must refer subsequently. All precision and simplicity are marred by the introduction of a type with exacerbations " at odd times," or by such a source of confusion, as the " typus inversus intercurrens."

The reader will not, therefore, be surprised to learn that Prof. Lebert declares for the absence of all specific peculiarities in the tubercular temperature ; he can only speak of it as resembling that of protracted inflammatory diseases, with a moderate or a highly febrile character. Certainly, on reading Lebert's analysis of his cases, one despairs of finding any regularity in the temperature viewed on the whole, or even of finding special groups of cases, or special sets of symptoms, associated with any very definite character of the pyrexia ; this writer attributes the variations in the different forms of the temperature course more to individual peculiarities than to anything else. Dr. Wilson Fox gives full particulars as to the maximum, minimum, and average temperature observed in his different classes of cases, with statements of the amount of the remissions and exacerbations, and the variability of the periods, whether night or morning, at which these exacerbations occur. But although he brings out many interesting points as to the kind and degree of pyrexia commonly found, he cannot venture on any definite classification of fever types ; moreover, his division of cases for statistical purposes is based in part on the high or low character of the temperature itself, and this does not seem well calculated to give value to some of the percentages which he calculates for each class.

Dr. Theodore Williams approaches the question somewhat differently. He classifies his cases according to the evidence supplied by the history and the physical signs, his division being, 1st stage active, 1st stage quiescent; 2nd stage ; 3rd stage active and 3rd stage quiescent, the three stages corre-

sponding to the formation of tubercle, to softening, and to
excavation.   He has also attempted more completely than
had been hitherto accomplished (at least in this country), to
ascertain the course of the temperature of his patients, in these
various stages, at short intervals during the whole period of
the twenty-four hours.   The necessity of frequent observations
during the day and night, for the proper estimation of the
febrile course in these affections, had indeed, been recognised
before.   Jochmann's cases were noted only twice or thrice a
day, but he specially mentions his regret at their deficiency in
this respect.   Wunderlich had evidently been accustomed to
record the temperatures frequently in phthisis, as in other
affections, and in this way, no doubt, detected the "duplicated
quotidian" paroxysms of which he speaks.   We are not aware,
however, of any detailed observations being published by him
regarding the temperature in phthisis.   In Dr. Sydney Ringer's
first edition some of the cases were noted frequently, and Dr.
Finlayson, in one or two of his cases gave the temperatures in
the night and early morning hours as well as during the day,
but their cases, were too few to be of much use in this respect.
M. Eude (whose paper, however, we have not seen in detail)
seems to have had observations made during the night, as he
fixes the hour of the minimum about 4 a.m., and otherwise
remarks on the elevations and depressions during the day.
Lebert insists strongly upon the different view which observa-
tions taken every hour, or every two hours, give us as to the
real course of the temperature, although his published tables
refer exclusively to the morning and evening records.   Dr.
Wilson Fox, likewise, while only tabulating his morning and
evening temperatures, had seen, from even a limited number of
intermediate observations, the necessity of accepting the con-
clusions drawn from such a method with considerable reserve.

Some may be disposed to ask of what use is it to make
observations with such frequency, or at such hours, as to pre-
clude the adoption of the method as a regular means of
inquiry even in hospital work, to say nothing of private prac-
tice ?   We need scarcely, however, point out that the value of
such inquiries may be considerable although but little adopted,
chiefly as rendering intelligible the records obtained by a less
irksome method, or as guiding the choice of the hours for our
observations when they can only be made once or twice a day.
The results of such laborious inquiries come to be available for
the benefit of all practical physicians in the same way as
similar tracings of the diurnal variations of the normal tempe-
rature are useful, although they are not verified in each par-
ticular patient.

Returning to Dr. Theodore Williams's investigation we find that two main peculiarities of the temperature in phthisis are thus stated by him :

"1st. The post-meridian character of the pyrexia when pyrexia exists at all.

"2nd. The remarkable fall at night, and the subnormal temperatures of the early morning."

This last peculiarity, although present occasionally in all forms, is specially marked, according to Dr. Williams, in those cases of phthisis which he classifies as "third stage active." He gives various tables and diagrams to illustrate the course of the temperature, which he thinks may be regarded as pretty typical *in this particular phase of the disease.*

"The general characteristics of 'third stage active' may be summed up :—1st. Afternoon and evening pyrexia.   2nd. Rapid fall during night and early morning.   3rd. Recovery in the later morning hours and consequent normal temperatures."

According to him the febrile rise may be said, as a rule, to begin shortly after 10 a.m., continuing usually till the afternoon, the maximum occurring, or being pretty well sustained for some time, between 5 p.m. and 10 p.m., about which hour the decline begins, so that the minimum is reached or maintained somewhere between 3 and 6, or 7 a.m., when subnormal temperatures are often recorded. The figures obtained from averages range from 98° to 101½° (36·66° to 38·61°), but in individual cases go considerably above and below these numbers.   The author considers this a great departure from the normal course of the temperature, and he appends a diagram constructed from observations made by Dr. Parkes, supplemented by some made by himself, with the view of showing that the normal temperature in the adult does not go through any similar variation, but maintains a pretty even course between 98° and 99° (36·66° and 37·22°) during the night as well as during the day.   This part of Dr. Williams's paper seems the weakest point in it, the range of night temperature in healthy subjects being given from one night's observations on one person.   We do not think it represents the real facts.   Thus, Dr. Parkes (in a paper subsequent to the one quoted by Dr. Williams, 'Proc. Royal Soc.,' Feb. 12, 1874) found, in a soldier, 25 years of age, that the temperature taken in the rectum varied from 97·8° to 100·8° (36·55° to 38·22°), in one day, taken from 6 a.m., to 10 p.m., and the temperature in the axilla underwent similar although slighter variations ; a greater range might, no doubt, have been obtained if the few hours before 6 a.m. had been included.   The frequent occur-

rence of figures but little above 97° (36·11°) in the measurements of the temperature (both axillary and rectal) in this healthy soldier show how careful we must be in admitting the existence of subnormal temperatures as a characteristic of phthisis. We do not understand why Dr. Williams did not avail himself of the elaborate observations of Jürgensen,[1] to which he refers, in seeking to compare the phthisical with the normal course of the temperature.

According to Jürgensen, the temperature of the rectum varied from a maximum of 37·7°, or even 37·9° (99·86° or 100·22°), to a minimum of 36·7°, or even 36·3° (98·06° or 97·34°), while the subjects were in bed: and in the case of one not so confined the variation was from 38·1° to 36·2° (100·58° to 97·16°), a range of 1·9° C. (3·42° F.). Moreover, it comes out in Jürgensen's inquiry that the temperature during the late evening and the midnight hours is much lower than that of the daytime.[2] Now this kind of variation does not seem to differ essentially from the variation regarded by Dr. Williams as so characteristic of phthisis, except that the temperatures in the latter are usually higher, and occasionally somewhat lower, so that the daily increase and remission are more considerable than in health. It seems, indeed, pretty clear that the daily exacerbations and remissions, so common in febrile states, are essentially modifications or exaggerations of the normal variations.

In the first stage of phthisis, when "active," and also in the "second stage," Dr. Williams seems to have found a moderate degree of pyrexia, culminating in the afternoon, associated frequently with the occurrence of subnormal temperatures, especially in the early morning, and traceable in the observations made at 8 a.m. It was but seldom, however, in any of the classes that the temperature went below 97° or 96° (36·11° or 35·55°).

We have referred in a previous section to the range of temperature not exceeding the normal to any serious extent in those cases of phthisis which Dr. Williams classifies as "first stage quiescent" and "third stage quiescent."

It is remarkable that this writer does not seem to have met with cases presenting the inversion of the periodicity of the daily paroxysm, at least with such frequency, or in such a striking form, as to call for his notice. Some slight explanation is afforded of this peculiar type by finding in a certain

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[1] 'Die Körperwärme des gesunden Menschen,' Leipzig, 1873.

[2] Dr. Sydney Ringer's recently published paper "On the Temperature of the Human Body in Health," confirms the existence of a considerable range of temperature, at least in persons under twenty-five years of age. 'Proc. Royal Soc.,' vol. xxvi, p. 186 (1877).

number of such cases the occurrence of a maximum in the forenoon, or at midday, followed by a considerable diminution of the fever (as if the height were too great to be maintained continuously), and then, perhaps, a second elevation in the afternoon or evening, which sometimes falls short of the maximum already attained. In such cases of a double paroxysm, referred to by Wunderlich, Lebert, and Wilson Fox, our morning observation may catch the temperature near its first maximum, and our afternoon and evening observations may happen to coincide with the periods of decline. It is, of course, in the study of such variations that frequent observations are so important, as without them we can never know whether the most extraordinary changes have not intervened between the exacerbations and remissions which appear from mere morning and evening records. This part of the subject has not yet been sufficiently worked out, so that we need not discuss the peculiarities of the daily remissions and exacerbations in their degree and sequence. It seems as if a more intelligent view of the pyrexial course might sometimes be presented by comparing the number, intensity, and duration of the paroxysms and remissions which occur during say 36 or 48 hours, than by calculating the mean of morning and evening temperatures absolutely, as the delay or the acceleration of the pyretic process for an hour or two may give rise to averages for these two periods which are quite misleading. Subsequent observers have confirmed Jochmann's remark as to unusually high temperatures being often followed by unusually low ones, although the converse does not hold. A certain tendency to tertian paroxysms also spoken of by him has been mentioned by other writers as well.

The course of the temperature before death has already been alluded to, but the subject is so interesting that we must refer to it in more detail. Although this part of the course is not always uniform, most of the cases present, for the last day or two days, a certain diminution of the pyrexia, so that we have sometimes normal and sometimes very low temperatures. The reality of this diminution is shown by its occurrence, even when the measurements are made in the rectum or vagina, figures about 94° or 95° (34·45° or 35°) being not uncommon, and even lower temperatures are sometimes noted. Occasionally a general and moderate diminution of the temperature can be noticed for a few days before death, leading up to a period just before the end, when extremely low temperatures are attained; but even in cases characterised by this diminution before death there may be a sudden start up of the temperature again to a considerable height,[1] to be followed by further remarkable

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[1] 'Bilhaut,' pl. iii, fig. 13.

oscillations; usually, however, the oscillation tends ultimately to a lowered temperature. Jochmann and Wunderlich pointedly called attention to the great daily variations in the phthisical temperature as being determined, in part at least, by the process of inanition, which M. Chossat found so potent in causing a diminution in the midnight temperatures of birds and other animals during starvation, although the midday temperature was affected only slightly. In the greater depressions which frequently occur during the last two days of life in phthisical patients, we have a further point of resemblance to M. Chossat's results, as he found that the depression of the temperature, and the consequent oscillations, which had been advancing pretty steadily all through the experiment, became so extreme at the end, that he had to separate the records of the antepenultimate and succeeding days from the other figures, to prevent the indications being swamped by the changes developed in a very extreme manner on the day of death.

But it must not be supposed that the end always occurs in the midst of a lowered temperature. It may coincide with one of those curious exacerbations to which we have referred as arising sometimes in the midst of low temperatures. Occasionally, indeed, as happens frequently in other febrile diseases, the temperature never falls at all, but rather rises towards the hour of death, and this seems to be commoner on the whole in acute tuberculosis than in chronic phthisis; the more frequent course seems to be for the very extreme elevations and depressions of temperature to become moderated by a series of minor oscillations, so that death actually coincides either with a temperature which is nearly normal, or with one which indicates only a slight amount of fever.

We have already hinted that this diminution of the temperature before death might be mistaken sometimes by the inexperienced for an indication of improvement; we must equally add that diminutions of the temperature, remarkably similar to those which are the precursors of death, occur not unfrequently at various stages of the complaint, and are often recovered from, without apparently any very serious change having occurred, so that we must not base an immediately fatal prognosis on such a behaviour of the temperature.

The bearing of thermometry on the diagnosis and prognosis of phthisis must be considered in further detail before we close. As just mentioned, the diminution of the temperature, especially towards the level of collapse, is so common at the fatal termination of this disease that we dare not regard it as of itself a favorable prognostic; we must anxiously consider the general aspect of the patient, and the state of the pulse, before

drawing any favorable inference from the mere abatement of the fever.   Nor does the moderate severity of the pyrexia in a case of phthisis afford much ground for even a relatively favorable prognosis ; many of the most hopeless cases are those with an intermittent febrile course of but moderate severity.   No doubt the cases with intense fever with but slight remissions, and those also characterised by occasional extremely high exacerbations, occurring in an erratic manner, are on the whole the most unfavorable.   On the other hand, periods of high pyrexia occasionally come to a crisis, as it were, being probably dependent on special pneumonic processes or complications, which terminate in this way, although the remaining disorder in the chest is amply sufficient to prolong the fever course.   In the more chronic forms, the absence of fever may be taken as an indication that the disease is more quiescent than if it were associated with distinct pyrexia, although the mere fact of the temperature remaining within the normal limits is no guarantee that the disease is not advancing to the destruction of the lung and even to death.   While, therefore, to one who is familiar with the disease, and the common forms of temperature associated with it, considerable assistance can be obtained from the thermometer as regards the prognosis, no very definite principles can be laid down.   Very high and very low temperatures may in turn indicate approaching peril, or may equally be recovered from for a time ; and a moderate fever, or even a normal course of the temperature, are very common towards the end of this fatal disorder.

As regards diagnosis, most of the writers have a deep conviction of the value of the temperature observations, although when they have attempted to formulate any very definite doctrine, it is apt to break down in their own hands, or, at least, in the hands of others.   The resemblance of the course of the temperature in acute tuberculosis to that of enteric fever is so considerable that Wunderlich takes the latter as indicating one of the types of turberculosis, but notwithstanding this, some assistance is obtainable from the thermometer in the discrimination of these affections from each other.   Sudden elevations, with irregular remissions, are quoted by Dr. Long Fox as points by which we may recognise acute tubercular disease, even when symptoms simulating enteric fever may happen to be present ; he says the temperature " is as irregular in acute tubercle as it is regular in enteric fever."[1]   Professor Lebert, also, who cannot be accused of undue confidence in thermometrical indications, regards the two diseases as presenting considerable

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[1] ' Medical Times and Gazette,' 1870.   Vol. ii, p. 263.

contrasts in this respect. The quick ascent to a considerable height is absent, he says, in acute tubercle; the morning and evening temperatures are, on the whole, less; the remissions are less pronounced, and are besides very unequal; and the inverted type is not unfrequently present. But it frequently happens that cases with symptoms pointing to enteric fever, after preserving for a time the continuous type of temperature, with slight daily remissions not uncommon in both disorders, enter on a period with a lower range of temperature, especially as regards the morning observations, the evening paroxysms being still maintained; such a range, while quite consistent with the notion of the convalescent period of enteric fever, may be prolonged to such an extent that this view ceases to be tenable, and the presence of tubercular disease, or internal abscess, becomes at length only too certain. Protracted suppurations, indeed, present a course of temperature not unlike those found in some cases of phthisis, and possibly the suppuration going on in phthisical cavities is responsible for part of the pyrexia in such cases. Chronic pneumonia, and catarrhal pneumonia, present types of fever which are essentially similar to those found in certain cases of phthisis, and even of acute tuberculosis, and the statement as to the possibility of discriminating 'phthisis with tubercles' from 'phthisis without tubercles,' has not met with confirmation from the most experienced authorities, although from the perturbations of temperature developed in a chronic phthisical case we may sometimes suspect the supervention of miliary tuberculosis. The occurrence of periods of considerable elevation of temperature cannot always, or perhaps even usually, be satisfactorily explained by the detection of fresh pneumonic processes, the appearance of softening, or such like. Indeed, the inexplicable nature of many of the sudden changes in the temperature has suggested the idea of a blood poisoning, from the absorption of morbid products, and it is to be confessed that a certain resemblance to the pyæmic range of temperature may be noticed occasionally in some cases of phthisis and tuberculosis. If we suppose with Lebert, as seems reasonable enough, that these products differ in their toxic influence, we can readily understand the immense diversity of temperature found in cases, which on dissection may seem very similar. Or, if we choose to carry our speculations in a different direction, we may suppose, with Schwarz, that the varying periods of pyrexia correspond to the varying times at which absorption of the toxic products occurs, while the intensity of the fever, or the absence of it, may be attributed to the varying conditions which favour or oppose the absorption of such products.

But while thermometry seems as yet unable to assist materially in the discrimination of the various processes which go to constitute the affections clinically recognised as phthisis, and while it cannot define certain types of tubercular fever and mark them off from other affections, or even discriminate the temperature course in certain cses of phthisis from that found in healthy subjects, there can be no doubt of its immense value in diagnosis.

If by its warning indications we are led to the careful scrutiny of all patients whose temperature continues elevated, without obvious explanation, this of itself is no mean benefit. But we must remember that various diseased states, giving, perhaps, no unequivocal external sign of their presence, may likewise give rise to a continued elevation of temperature. It seems quite certain, for example, that pernicious anæmia is associated at times with a persistent elevation of temperature without any inflammatory complication to account for it, and without any tubercular process being present; and it is likewise very probable that in syphilis, in certain obscure nervous disturbances, and in rare cases of rapid cancer, we may have a considerable elevation of the temperature persisting for some time.

Notwithstanding all the difficulties and sources of confusion and error to which we have alluded, the physician who is familiar with the various phases of phthisis and tuberculosis, as well as with those affections most liable to be mistaken for them, will often be able to gain hints from the temperature, in particular cases, which are simply invaluable, although he may be quite unable to formulate laws which could lay the least claim to a general character.

II.—Ziemssen's Cyclopædia of the Practice of Medicine.[1]

Diseases of the Brain.

THE readers of this volume must admit the fitness of its authors to address them on the subject of diseases of the brain. Their names are all known in this country, and some of them are very favorably known. With one exception they are all professors at German or Swiss universities; they have all devoted much of their time and ability to the investigation of nervous

pathology, and three out of the five have been, or are at the head of establishments for the treatment of lunatics.

To Nothnagel is allotted the task of dealing with the brain in relation to its blood-supply, and in an able article he discusses anæmia, hyperæmia, hæmorrhage, thrombosis, and embolism. The vexed question of the cerebral circulation and the possibility of any variation of pressure taking place in the cerebral arteries, has been set at rest by the discovery of the relations which exist between the cerebro-spinal fluid and the fluid in the perivascular lymph-spaces to the amount of blood in the blood-vessels. The error of Monro in supposing that the quantity of blood within the cranium was always uniform was shown in the first instance by Sir George Burrows, who demonstrated by careful experiment that variations in the relative amount of arterial and venous, and in the aggregate amount of blood, actually do occur. Of more recent experiments, that of Gaetzheus seems the most important, by which it was shown that forcible injection of defibrinated blood into the carotid of a horse was capable of producing a rapid flow of lymph from the lymph-vessels of the neck. As the question stands at present, Nothnagel asserts that variations in the cerebral circulation must be attended by the following results :—

"1. *Increase in the quantity of arterial blood* by displacement of the cerebro-spinal fluid and the contents of the perivascular lymph-spaces; or when the compensation thus made is insufficient, by elevation of the intra-cranial pressure. 2. *Venous stasis* by a retardation of the arterial afflux. 3. *Diminution in the aggregate quantity of blood* by an afflux of cerebro-spinal fluid, dilatation and flooding of the lymph-spaces, or where this is insufficient by diminution of the intra-cranial pressure."

This volume cannot be said to open well, for a more barren article than the one devoted to cerebral anæmia it has seldom been our lot to read. Our Teutonic neighbours have a grand faculty of saying "don't know" in a learned manner, but a plentiful use of Greek words, and the employment of an elaborate classification, will scarcely prevent the merest tyro in medicine from seeing that the present article has been mainly spun from the inner consciousness of the various authorities appealed to. We are told that we must have regard to acute universal cerebral anæmia, universal cerebral anæmia of gradual development, and partial circumscribed cerebral anæmia. The first class is composed of cases of syncope, and death from hæmorrhage. The second comprises the various mental conditions which result from exhausting fevers and chlorosis; and our whole certain knowledge of the third class is derived from cases of ligature of the carotid artery. Thus our acquaintance with

cerebral anæmia pure and simple is very slight indeed, and the author would have done well to have written as little as possible concerning a subject of which we have necessarily very little knowledge, and to have borne in mind the fact that the great aim of scientific authors is to impart knowledge lucidly, and not to concatenate words and sentences.   It is curious, too, that in the clinical description of death from rapid anæmia the occasional occurrence of exaltation both of sight and hearing, and the restlessness and jactitation of the patient should have been omitted.   It is difficult to understand the fascination which the dead languages have for some authors.   What useful purpose is served by dividing fainting fits into three varieties, eclysis, lipothymia, syncope—a releasing, an escape of the soul, and a falling together?   If any glimpse is to be got through these Greek derivatives of the etiology or pathology of the disease, by all means let it stand, but if no such purpose is served, we strongly deprecate this childish clinging to a vain show of mock learning. If, again, Marshall Hall called one class of cerebral anæmias by the name of *Hydrencephaloid*, we feel quite sure that the author would have felt no disposition to retain so misleading a word.   Surely to speak of *Autochthonous thrombi* is a tautology. A thrombus in its conventional medical sense can be nothing but autochthonous—that is, an aboriginal plug of local origin ; for if it be shot into its position from another source, we call it by common consent an embolism.   It need scarcely be said that the thirty-five pages devoted to cerebral hyperæmia are occupied chiefly by speculations on various cerebral pathological possibilities, and that definite statements based on accurate knowledge are few and far between.

Passing on to the next section—that which is devoted to cerebral hæmorrhage—we take leave of shadows, and are more profitably occupied in dealing with substantial facts.   The etiology of cerebral hæmorrhage is a matter which has been the subject of much controversial discussion, but the facts which have been elicited by modern pathologists with much painstaking and industry have been such as to cause a practical concurrence of opinion on all the more important points.   Such notions as that propounded by Rochoux, that softening preceded hæmorrhage, or that (taught by Calmeil) hæmorrhage was due to an inflammatory process, have been absolutely discarded. It has long been recognised that cerebral hæmorrhage is due to disease of the cerebral vessels, to which in some cases no doubt is added an increase of the arterial tension.   The fact which was brought prominently forward by Bouchard and Charcot, that cerebral hæmorrhage was usually the result of the rupture of miliary aneurisms situated on the cerebral arterioles, was of

prime importance as serving to show the complete analogy existing between spontaneous hæmorrhages occurring in the brain with those occurring elsewhere. The correspondence which has been shown to exist between the commonest seats of these aneurisms and the commonest situations of hæmorrhage, is also of as much importance as interest. The order of frequency in a progressive descending scale has been shown to be the optic thalami and corpora striata, the convolutions, the pons, the centrum ovale, the middle cerebellar peduncles, the cerebral peduncles, and the medulla oblongata. The correspondence also between the frequency of occurrence of these aneurisms and of apoplectic attacks (rare before forty, and then progressively more common till advanced age) seems almost to complete the proof of the interdependence of the two conditions. The dispute between Bouchard and Charcot, on the one hand, and Zenker, on the other, as to the local pathological change which produces these aneurisms, is of less clinical importance. Both observers admit the occasional occurrence of thickenings (arterio-sclerosis) in the internal coat, but Charcot and Bouchard differ from Zenker in asserting that the aneurisms are not necessarily dependent on the sclerotic condition. As to the atheromatous change in the large vessels, which has long been recognised, and which was long held to be sure evidence of a similar change in the smaller vessels of the brain, and an indication in fact of the actual cause of the hæmorrhage, opinions have changed. It is recognised that in many cases of cerebral hæmorrhages the large vessels have undergone no atheromatous degeneration, and that such change has only a secondary influence on the occurrence of hæmorrhage—

"By causing an impairment of the normal elasticity of the walls of the large vessels, so that they cease to check the force of the pulse-wave to the normal extent, in consequence of which the blood-pressure in the arterioles (the seat of the miliary aneurisms) of necessity becomes abnormally great."

In the absence of any satisfactory evidence to the contrary it may safely be assumed that alteration in the blood pressure is unable, without change in the arterial coats, to produce hæmorrhage. Eulenburg states that a predisposition to cerebral hæmorrhage only exists with one form of cardiac hypertrophy, viz. that which accompanies the contracted kidney and the arterio-capillary fibrosis of Gull and Sutton, and not with that which results from lesions of the cardiac valves. The other certain facts which we have as to the etiology of cerebral hæmorrhage seem to be but two—that its frequency bears a direct proportion to the age of the patient, and that it occurs more frequently in winter than in summer. The pathological

anatomy of cerebral hæmorrhage presents many points of interest. The explanation of Heubner and Duret of the great frequency with which the corpus striatum and its neighbourhood suffers is probably correct. The branches first given off by the anterior and middle cerebral arteries supply the anterior and middle end of the corpus striatum, together with the lenticular nucleus and the internal and external capsule. The anterior and middle cerebral, being the direct prolongation of the carotid, are exposed to the full force of any increased cardiac action, and thus " direct and indirect causes unite to make the districts which thus supply liable to be the seat of hæmorrhage." The passage of the exuded blood and damaged brain and vessels through gradual inspissation and organization to the formation of a cyst or cicatrix was followed by Cruveilhier, but it remained for Türck to point out the secondary changes which occur in the white motor nerve-fibres. These degenerative changes do not occur with superficial lesions of the cortex, but only with deeper lesions involving white matter as well as grey. Especially have they been observed after lesions involving the corpus striatum and the internal capsule, while they are much less common when the optic thalami are involved. These degenerative changes have been observed to travel through the crus, the pons, and the anterior pyramids to the posterior portion of the opposite lateral column, and to the inner part of the anterior column of their own side.

The symptomatology of cerebral hæmorrhage has been and still is steadily progressing towards greater accuracy. One of the most difficult points is the appreciation of the difference between the symptoms of profound apoplexy and coma from alcohol or opium. Prevost insists that in those cases, in which the general muscular relaxation marks the true nature of the attack, the deviation of both eyes towards the non-paralysed side, together with the occasional rotation of the head in the same direction, will often give a clue towards a correct diagnosis. The significance of convulsions at the outset is doubtful, and the interpretations placed upon them by different observers —that they are due to a large extravasation, to the implication of the pons or medulla, or the rupture of the floor of the fourth ventricle, or the destruction of the walls of the lateral ventricles —may all be said to be in need of confirmation. The explanations offered also of those rare apparent exceptions to the rule, in which paralysis occurs on the same side as the lesion, are scarcely to be accepted without question. There is no doubt that such cases do occur, and the theories invented for their explanation, that the pyramids in such cases do *not* decussate (Morgagni, Brown-Séquard), or that they redecussate (Schiff),

belong merely to the category of ingenious hypotheses. Ambrosi's theory, that the paralysis in these cases is due not to the *primary* but to a *secondary* lesion, is more probable, and one case has been reported which seems to lend some colour to this explanation.

The paralysis of individual nerves as the result of cerebral hæmorrhage is a rare phenomenon ; and it is a noteworthy fact, and one to be borne in mind in reference to modern views of cerebral physiology, that the individual nerves of the limbs have never been known to be paralysed from a purely cerebral lesion.

The movement of paralysed muscles *in association* with others, in obedience to emotional or reflex stimulation, is a matter of very great interest.

" Thus the muscles of one half of the face, usually completely relaxed, may, in connection with those of the unaffected side, perform motions necessary to changes of expression, sometimes contracting even more strongly than do the healthy muscles, as in laughing and the like ; in other cases, on the contrary, the difference between the two sides of the face comes out all the more strongly under changes of expression. Under the influence of emotional excitement, such as anger, the paralysed extremities may be more forcibly moved than the other ; when the patient coughs, gapes, or sneezes, during micturition or defecation, the paralytic member may be jerked strongly into the air, or may fall into a state of convulsive twitching ; if a certain amount of contraction is already present, it becomes still more decidedly marked ; and yet withal the utmost effort of the will may be unable to provoke the least motion."

Nothnagel also draws attention to the well-known fact that when strychnia is administered the paralysed muscles are the first to be the seat of " twitchings."

Those curious cases in which the motor impulse seems to escape from the paths of exact volition, in which the paralysed side imitates feebly the movements of the sound side, or in which during recovery the reverse takes place, or in which, during the voluntary exercise of a paretic limb, the antagonists, as well as the muscles which it is wished to call into play, contract, all afford food for reflection, and remain to be explained.

The post hemiplegic *contractions* of muscles, which are best seen in the flexors of the forearm, have also been the subject of close scrutiny among clinicians. Hitzig has called attention to the fact that they only become absolutely fixed and immovable after a lapse of years.

" In the early period of their existence the contracted muscles may relax so as to allow the paralysed limb to reassume its normal position. This occurs when the patients have remained quiet and at

rest for some time, especially if they have been lying down without moving.   Since these conditions exist pre-eminently during sleep, it sometimes happens that in the morning, on awaking, such patients find the muscles, which were strongly contracted the evening before, relaxed, and in part under the control of the will."

The theory put forward that these contractions are merely due to the irradiation of motor impulses, to certain centres in the lower sections of the encephalon, whence they reach the muscles, seems to us to be so nebulous as scarcely to merit the notice which is accorded to it by Nothnagel.   When the contracture of muscles is clonic, and not tonic, we are confronted with those post hemiplegic clonic conditions which have been long recognised, and which were scarcely in need of Hammond's nonsensical name, *athetocis.*

The fact that paralysis, as a symptom of all nervous lesions, whether central or peripheral, is always more marked than anæsthesia, has long been recognised.   Permanent anæsthesia is one of the rarest phenomena of hemiplegia, and when present is supposed to be due to the implication of the posterior part of the internal capsule, the innermost division of the lenticular nucleus, the external superficial layers of the optic thalamus, and the adjacent parts of the corona radiata.   Localised lesions of the peduncles or pons have also been accompanied by anæsthesia.

The trophic and vaso-motor disturbances which accompany hemiplegia are chiefly remarkable by their absence.   The limbs retain their normal plumpness and colour, and the muscles neither waste nor degenerate.   Neither is there any undue tendency to bed-sores, except in the case of the very feeble or the neglected.   A slight rise of temperature and a slight hyperæmia, together with a spurious kind of slight œdema of the paralysed limbs, causing a swollen appearance of the fingers, is occasionally noticeable.   The occurrence of acute bed-sore is wholly exceptional, and the explanations given of it are, on the whole, flimsy and untrustworthy.   The reference of these phenomena to the influence of "trophic nerves" is, so it seems to us, the giving of names to "airy nothings," whose local habitations we are unable to define.

No more important advances have been made in cerebral pathology than in the localisation of lesions, and Nothnagel is to be commended for the temperate manner in which he has discussed the entire question.   Owing to a sudden illness just as his article was going to press, our author was unable to enter as fully as he had wished into the symptomatology of lesions of the cortex cerebri.   This omission may, we hope, be repaired at some future period, for it is surely greatly to the

benefit of medical science that questions of this kind should be discussed in the judicial spirit which is everywhere apparent in this section, and not by violent partisans of this or that method of experimenting, who, in the zeal for that reputation which naturally accrues to discoverers of new things, are rather apt to attempt to prove somewhat more than the facts warrant. The facts which seem most favorable for the diagnosis of lesions in the pons seem to be *contraction of the pupils* and paralysis of the face on the opposite side to paralysis of the limbs. It must not be forgotten that dilated pupils have been seen with hæmorrhage in the pons, and that the distribution of the paralysis is not constant, *e. g.*, paralysis of all four limbs, paralysis of hemiplegic type without implication of the face, extreme hemiplegia without alternation and with implication of the facial, and hypoglossal on the same side, hemiplegia with paralysis of both facials, and paralysis affecting cranial nerves only, have all been described. The occurrence of alternative paralysis is said to occur with lesions of the lower part of the pons.

The occurrence of anæsthesia, impairment of speech (anarthria), and convulsions are also in favour of a lesion in the pons. Here it becomes necessary to enter a protest against this word anarthria, which is useless and misleading. The word ἄρθρον is used in medicine for *articulation* not of words, but of joints, and although the word ἀρθρόω has a meaning which is applicable to the articulation of words, this is not its conventional meaning. A child who speaks with its mouth full, or one with a cleft palate, or with an ulcer on the tongue, are all equally anarthritic, and there is nothing in the word itself which would exclude ordinary aphasia. A Greek scholar would interpret " anarthria " as a lack of joints, and could not, by the word alone, be led to suppose that it indicated a paralysis of the tongue, a localised *tongue-palsy* (not a bad word, and the same length as anarthria), from implication of the hypoglossal. Leyden has invented this foolish word, and we do not thank him.

Lesions of the cerebral peduncles are often readily diagnosible. The paralyses are of true hemiplegic type, and if the lesion is extensive, there may be considerable anæsthesia as well as paralysis. The distinctive point is the implication of the motor oculi on the side of the lesion. This occurs when the inner half of the pons, which is mainly composed of centrifugal fibres, is implicated.

The symptoms due to lesion of the corpus striatum are well known, and it does not seem possible to distinguish between lesions of the lenticular and caudate nuclei.

Lesions affecting the optic thalamus exclusively are very rare,

and the general opinion seems to be that they do not cause motor paralysis, although if the inner capsule as well as the thalamus be involved, motor paralysis results.

The traditions of modern medicine are well maintained in the present article by the smallness of the space devoted to the discussion of treatment :

"There is," says Dr. Steele, in a recent article on hospital mortality, " no experience more painfully evident in hospital narrative than the absence of guiding principles in therapeutics, and when we come to examine the multifarious remedies which formerly were vaunted as specifics, and which have been replaced by others that in the course of another generation will be classed among the fashionable follies of the day, it is impossible to avoid the conclusion that physic *per se* is powerless as an agent in prolonging life, or in grappling successfully with the ordinary fatal ailments of humanity."

In the eyes of Nothnagel cerebral hæmorrhage is a fatal ailment, which forms no exception to the rule.   In addition to the dietetic and hygienic regimen to be observed, concerning which most practitioners would agree with our author, Nothnagel speaks of the possibility of lessening (in some few cases) the cerebral pressure by having recourse to venesection, and he also lends his countenance to the practice, which is more common in Germany than in this country, of applying galvanic currents to the head and to the affected limbs.   He testifies to the good results occasionally obtained by having recourse to this practice.   Anything which is recommended by so temperate a writer certainly deserves a fair trial.

Nothnagel's articles on thrombosis of the arteries and sinuses of the brain, and on the all-important subject of embolism, are carefully written, and are admirable alike for their method and their matter.   It is only natural that frequent allusion should be made to " aphasia," and it certainly would appear that a full discussion of this important subject should have been made in immediate connection with the causes which produce it.   We are constantly told, however, that aphasia is treated of " elsewhere," but where that " elsewhere " is is not very definitely specified.   It is nowhere in the present volume certainly.

We much regret that it is impossible within the ordinary limits of a single article to do proper justice to a volume of this kind, which contains so many exhaustive monographs, each one of which is worthy of lengthy comment.

The section on tumours of the brain and its membranes, by Oberneier, will be read with great interest, since the introduction of the ophthalmoscope has so greatly increased our power of diagnosis in these cases, and has so largely directed profes-

sional attention to the whole subject. It cannot but be of interest to our readers to set before them Oberneier's opinion on this most important point. Speaking of the implication of the functions of the optic nerves in cases of intra-cranial tumour, he says :

"We have here amblyopia and amaurosis, as the cause of which the ophthalmoscope shows the choked disc or congested papilla, and neuro-retinitis, and attention must be specially directed towards contractions in the field of vision.

"Decrease in the acuteness of vision, even complete blindness, is not only very frequent in cases of cerebral tumours, but also often one of the first symptoms. Upon the basis of a very diligently collected complete complication of cases, Annuske arrives at the conclusion 'that optic neuritis is almost without exception a constant attendant upon cerebral tumours, and consequently occupies the first rank among all the symptoms of intra-cranial neoplasms.'"

"So long as the absence of the choked disc in cerebral tumours is regarded as a rare occurrence, as other ophthalmologists also consider it to be, one cannot value the importance of the symptom in question too highly, nor be insensible to the duty of subjecting 'suspicious disturbances of sight' to a careful ophthalmoscopic investigation. It does not concern us here to enter into a description of the ophthalmoscopic picture, to paint the swollen, opaque, and imperfectly defined optic papilla, with its fine arteries and broad and tortuous veins; it is of more importance for the comprehension of the appearances to remark that we have to deal with a degenerative process in the nervous elements, in which the increase of the intra-cranial pressure plays a chief part. If, namely, in consequence of the development of an intra-cranial tumour the pressure within the cavity of the skull is increased, the fluid between the sheaths of the optic nerve which communicates with that in the subdural space will become dammed up, and the optic nerve at its place of entrance into the globe will become incarcerated, and will undergo an œdematous swelling at this point, which, no doubt, hastens disorganisation in the prolongation of the nerve."

The diagnosis of the situation of tumours is also a subject concerning which the experience of clinicians will be sought for the confirmation or refutation of the teaching which physiologists are offering, as in the matter of functional localisation. The amount of evidence of this kind is, however, not great, and it must be admitted that, for the present, Hitzig, Hiltsch, and Ferrier are ahead of clinical experience. Concerning hemiopia as a sign of cerebral tumour our knowledge is more precise, and the following is what Oberneier has to teach us as to the diagnosis of—

"*Tumours of the Base in the Neighbourhood of the Optic Chiasma.*

"(*a*) *Anterior to the chiasma.*—Disturbances of the sense of smell; injury of the fibres of the optic nerve, which pass to the

inner half of the retina, and consequently hemiopia, in which the defect lies to the outside in the field of vision of each eye.

"(*b*) *Laterally from the chiasma.*—When the tumour advances towards the chiasma, disturbance of function of the optic fibrillæ; those on the side of the tumour supplying the external half, those on the opposite side supplying the inner half of the retina, hence right- or left-sided hemiopia.

"(*c*) *Behind the chiasma.*—Lesion of the optic fibrillæ, which pass to the outer half of the retina; hence hemiopia with the defects inwards on each side."

And here we regret that we are obliged to leave this very important volume, a volume which adds immensely to our stock of organised knowledge. We can have no doubt that, were it possible to purchase this volume apart from its fourteen companions, it would command a very large sale. One can purchase a single volume of the 'Encyclopædia Britannica,' and why not a single volume of this 'Medical Encyclopædia'? We think it would be more public-spirited on the part of the publishers to issue the volumes according to the demand, and this would surely be the most profitable course for them to pursue. Among auctioneers it is a common plan to "lot" the worthless with the attractive articles, but the trade of the auctioneer has its necessities which medical publishers need hardly copy.

Heubner's articles on cerebral syphilis, Huguerin's on inflammatory affections of the brain and its membranes, and Hitzig's on atrophy and hypertrophy of the brain, are all full of information and well written, and it is due to these authors to state that it is only our want of space which compels us to leave their very valuable work unnoticed.

III.—Pathology of Granular Kidney.[1]

THERE is, perhaps, no disease which better shows the methods and success of modern medicine than the form of chronic Bright's disease now generally known as the granular kidney. To those sensitive and faithless persons who have not heart and vigour to see that the time now our own is the greatest time the world has yet ever seen, it might be a sufficient instance to take this one disease, and point out how, within one generation, keen patient observers, ardent in their great calling, have tracked out the causation, the pathology, the clinics, and, let us add, the therapeutics of it, to the great enlargement of our knowledge

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[1] 1. *Lumleian Lectures on the Muscular Arterioles.* By GEORGE JOHNSON, M.D., F.R.S. London, 1877.

2. *Diseases of the Kidney and Urinary Derangements.* By W. HOWSHIP DICKINSON, M.D. London, 1877.

3. ZIEMSSEN'S *Cyclopædia of Medicine,* vol. xv. *Diseases of the Kidney.* London, 1877.

and of our power, to the relief of pain and the lengthening of
life. Even among ourselves there are men—and these not the
least accomplished of physicians—who are heard to say that
the boasted advance of pathology has done but little for us, that
pathology is a pretty study, but one that leads, or has led, to
nothing. They say that the strokes of disease fall as heavily
as of old, that its weapons are as keen and its victims as nume-
rous. Such men are better than their words, for did they realise
what they profess, so curious a search into so painful a subject
as bodily decay would become revolting to them. Their lan-
guage arises from a misapprehension of the kind of help which is
to come. Even yet, dominated by the traditions of the merest
empiricism, such physicians, like their patients, have learnt to
believe in Jordan as little as in the rivers of Damascus, and
seek rather for some trenchant specific than for that power of
gradual counteraction and prevention which can come only of
minute knowledge of cause and consequence. In the malady of
which we have now to speak it is as certain as we have found it
to be in others, that organic disease is most frequently the record of
morbific impressions scored in more and more deeply, line upon
line, until the original characters of life are nearly obliterated.
Little can be done in the later states of such a palimpsest, and
the therapeutics of chronic disease must be rather in the early
detection and interception of such impressions. If we are to
succeed in therapeutics we must learn how, where, and when
we can work that our labour may bring its reward. It would
seem that the labour of many workers—chiefly English, we
gladly observe—has brought up the inquiry into the nature and
phenomena of granular kidney by many converging lines to
degrees which must shortly meet in some central and fruitful
generalisation. It is the difficult duty of a reviewer at such a
time, when other men would be silently working on, to speak,
however imperfectly, in the hope of so laying the matter before
his readers that the many may learn exactly the present direc-
tion of inquiry, and the few be led so to reconsider the present
phase of knowledge as the better to prepare themselves for
the pursuit of further researches.

The earlier differentiations of the diseases of the kidney
discovered by Bright are unknown to few persons, now that
the great divisions of them into tubular nephritis, granu-
lar kidney and lardaceous kidney, have been made familiar
enough by the work of such men as Johnson, Beale, Traube,
Dickinson, Grainger Stewart, Roberts and many others. And,
to go a step farther, it is now well known enough that, what-
ever be the causes of each of these—and on this inquiry
observers differ—all are nevertheless agreed that they arise

independently of each other, and that tubular nephritis never gives rise to lardaceous disease nor lardaceous disease to tubular; and that, again, neither of these, as a rule, gives rise to the granular kidney, however fully we admit that such a sequence may be pathologically intelligible and occasionally seen; indeed, we may take it as generally admitted that when an uneven shrunken kidney does result either from the tubular or the lardaceous disease, this state is not wholly to be confounded with the more definite granular kidney which, in the vast majority of cases, has other antecedents, and runs a different course. It is to this form, and its interesting relations with other changes almost throughout the rest of the system, that we propose now more especially to limit ourselves. How are we then to define the disease under consideration? Perhaps something after the following kind:

Granular kidney is a disease of a sub-inflammatory nature, slow though always fatal at last, in which both kidneys at similar but not identical rates (enlarge wholly or partially and subsequently?) diminish in volume, chiefly at the expense of their cortical portion, their surfaces becoming granular in appearance and their capsules adherent and thickened. The disease is associated with hypertrophy of the heart (especially in its left ventricle), and of certain large groups of the muscular arteries. Its more constant symptoms are high arterial tension, light urine, albuminuria, anæmia, slight pigmentation, emaciation and debility, and at a later stage nausea, vomiting, coma and other common effects of uræmia. Its occasional and intercurrent symptoms are diuresis, spasmodic dyspnœa, hæmorrhages, epilepsy, partial blindness, serous effusions, bronchitis and gout. It ends, for the most part, in apoplexy or uræmic poisoning, or it may be by disease in the serous cavities. Its better known causes—which, however, account only for some of the cases—are (speaking provisionally) heredity, mental anxiety, prolonged venous congestion, as, by way of pregnancy or heart disease, obstruction in the urinary passages or ascent of inflammation, lead-poisoning, alcoholism and gout.

Let us now take up our definition, and discuss each of its points in turn, or such of them as may seem to demand discussion. The disease is a slow one in its course and sub-inflammatory in its nature. The time of death, however, is too uncertain to be surely or approximately predicted in the individual case, the uncertainty chiefly arising from three things— from the liability to acute intercurrent renal inflammation, and to encephalic hæmorrhage, and from the lessened margin of safety in the organism as a whole. Uræmic poisoning rarely occurs till a late stage of the malady unless there arise an intercurrent

attack of acute renal inflammation; and whether it be that gra-
nular kidneys are especially susceptible of inflammation, or that
transient renal congestions which pass over sound kidneys without
harm gather to a dangerous height in the granular, it is hard
to say.   However this may be, intercurrent nephritis in these
cases is common, and may cut the thread of life by acute
uræmia or otherwise.   It is now three years since I was called
suddenly to a man of middle life whom I found in epileptiform
convulsions and coma.   He had been affected with granular
kidney for some indefinite time previously, and after the cere-
bral symptoms and other evidences of acute nephritis were
subdued he drifted back into his former sallow state with big
heart, tense pulse, sallow skin and slight albuminous urine of
low gravity; indeed, under continuous treatment, his condition
has been better during the last two years than for the twelve
months before the sudden anuria, hæmaturia, and increased
albuminuria immediately preceding the acute uræmic out-
break which laid him by for three weeks.   This is a well-
marked instance of a class of cases by no means uncommon.
Again, the patient who escapes intercurrent nephritis may be
stricken down by encephalic hæmorrhage almost in any stage
of the disease.   It is a fact insufficiently commented upon, not
only that patients with granular disease may, as post-mortem
experience proves, be smitten with such apoplexy in the early
stages of the disease, but that this is also true of men under
middle age.   A man, therefore, of the age of forty or forty-five
years with granular disease is but little safer in this respect
than his seniors, whose arteries are otherwise more likely to
give way; so that granular kidney, independently of senility,
causes or coexists with arterial incontinence.   It has been said
of late, indeed, that hæmorrhages both old and recent, too small
to be obvious in themselves or in their symptoms, are found more
or less abundantly in the brains of most of those who die with
granular kidneys.   These details are of great importance, as
we shall see hereafter when we come to consider the relations
between this state of the kidneys and the arterial tree.[1] Thirdly,
patients with granular kidneys are very liable to death before
the time of the direct issue of their malady, because their margin
of safety is lessened.

One of the first questions which the experienced physician
asks himself when called to the bedside of a patient in acute
disease is, what is the factor of safety?   If it be large all
may be well, if narrow the danger is the greater.   In all engineer-
ing works, when the estimated strain has been calculated, the
bridge or crank is made equal to that strain, with the addition

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[1] Cf. pp. 293-4 of the present article.

of a large margin of strength to cover possible excess of strain and certain internal molecular change. This addition, known as the factor of safety, exists in the human frame, and is in young persons very large, but it diminishes more or less rapidly in later years, or in insidious disease. A recovery from pneumonia may entirely depend not on the resistance of the lung, but of the heart or of some other organ, and the condition of no organ is a matter of more anxious questioning at such times of stress than that of the kidneys. Like Archimedes, the physician may say, " Give me good kidneys and I will move the man." When I was first called to the charge of medical wards nothing startled me more than the frequent deaths of patients from fevers and acute diseases, who, to a young observer, seemed likely to recover. A close perusal of the dead body gradually convinced me that such deaths are due not so much to the arrest of the part attacked or to the intensity of the poison received as to some pre-existing diminution of the factor of safety. Moreover, it is in two organs mainly that we are liable to find the factor of safety diminished, and these two are the heart and the renal apparatus. Thus it is, then, that not only may the lives of subjects of granular kidney be cut short before the normal term of that degeneration by intercurrent nephritis, and by apoplexy, but also, and perhaps more frequently and in still earlier stages, by a diminution of the factor of safety, which renders them liable to be carried off by any chance poison or accidental malady. Subject to these inessential changes, however, granular nephritis is a slow disease; that is, it may occupy from five to ten years or more in its course. In the next place it is subinflammatory in its nature, though it must be said that here we are met by some divergence of testimony. Dr. George Johnson, whose name as an early investigator of renal diseases comes second only to that of Bright himself, sees in granular kidney a change primarily intratubular, and seated in the epithelium. Dr. Dickinson, on the contrary, and with him many other original observers, both English and continental, sees in granular kidney a process identical with that which in the liver is called cirrhosis, namely, a morbid outgrowth of connective elements, which after their manner contract as they mature, and thus strangle the kidney mechanically. This process in some exceptional cases may be general and acute, as seen in a plate upon page 365 of Dr. Dickenson's treatise on albuminuria. This observer declares that the changes in the epithelium are entirely of a secondary character, and consist chiefly in those changes of form which mechanical compression would necessarily produce. In the earliest stages of acute general connective hyperplasia of the kidney, and of that which results from pregnancy and heart disease, an enlargement

and condensation of the organ is seen; in the chronic form a stage of enlargement, if it exists, would rarely be seen, save in cases of death from other causes; and it is even probable that the enlargement may not be universal, but so distributed in districts that the bulk of the whole would suffer little or no increase. The contraction, however, is probably as inexorable as that of a burn in the neck, and how little we can arrest that superficial and palpable movement many a poor creature knows but too well. In the kidney it probably bears a definite and necessary relation to the amount of foreign irritation and proliferation, so that the latter being ended the grip of its consequence can in no degree be eluded. If then the usual course of the affection be a *general* hyperplasia, followed by a *general* contraction, interference after the first stage of the malady,—a stage probably of partial or complete latency, would be hopeless so far as structure is concerned. Hence it is that it seems incumbent upon us to learn exactly whether such be the course of the disease, or whether, its early course be a smouldering heat advancing from point to point, and thus susceptible of some arrest or delay. Much of our therapeutical hopes obviously depend upon the question of general or fractional invasion. Once more; it is by no means to be assumed that all interstitial proliferation, even if identical in nature, must in every case lead to contraction as a consequence. Abundant nuclear matter is to be seen in the true skin in many an inflammation which does not lead to scarring, and so, perhaps, few cases of tubular nephritis are to be seen without some measure of interstitial nucleation. Dr. Dickinson, Dr. Klein, Dr. Greenfield and others have shown that in the scarlatinal kidney for instance, interstitial proliferation is almost as copious as intratubular, and yet complete recovery from scarlatinal nephritis is the rule, at any rate in young patients. On the other hand, it is certain that same nuclear proliferation, if it does not vanish in its youth, fibrillates in maturity and shrivels in age, for the end of uncured scarlatinal nephritis is granular kidney. At the Manchester meeting Dr. Dreschfield showed a good section of kidney in which intra- and intertubular proliferation were visible together, and on pp. 259—263 of Dr. Dickenson's work are shown some beautiful woodcuts of the same coexistence. It is probable that these diffuse inflammatory changes are more common in the scarlatinal kidney than in the ordinary smooth white kidney, but the difference is perhaps one of degree rather than of kind. Dr. Grainger Stewart has insisted upon this side of renal pathology for many years. It is of vital interest for us as preservers of life to recognise the forerunners and the initiation of granular kidney, seeing that to recognise

it when fibrillation is widely extended or implied is a compara-
tively useless flash of diagnosis.

Before turning to causation, however, let us complete our re-
marks upon pathology. Our readers know well that the interest
excited by the changes discovered in the kidneys has almost paled
beside the emulous discussions upon the correlative changes in
the heart and arterial tree. These discussions, led by Dr. George
Johnson of the one part, and by Sir W. Gull and Dr. Sutton of
the other part, revolve about two main issues. Dr. George John-
son has it that the whole state of change is rooted in the kidneys
themselves—that the kidneys failing cease to purify the blood,
that the blood thus impure is offensive to the body and its tissues,
that the muscular arterioles therefore defend the capillaries and
the tissues against the entry of the impure blood, that they do
this by contraction of their muscular coat and diminution of
their lumen, that in this way the blood tension on the central side
is heightened, that the pressure upon the inner surface of the left
ventricle is thus heightened, that as a consequence of their ex-
cessive contraction the arterioles become hypertrophied, and
finally, that in consequence of their hypertrophy and increased
resistance the left ventricle of the heart in its turn grows
abnormally. Now, although Dr. Johnson attributes a selective
function to the arterioles, and attributes their overgrowth in
muscle to their excessive efforts in the defence of the tissues
before them, yet this assumption, if true, is needless to his
main argument ; and if other pathologists prefer, as certainly
many of us do prefer, to suppose that the impure blood directly
sets up contraction in the arterioles by irritation and reflexion,
the argument as a whole is but little altered, and not altered at
all in its main bearings. It still stands thus. The kidney
which should purify the blood fails to do so, and the blood so
far is stained with impurity ; the blood thus impure irritates
the arterioles which contract upon it, and in time become,
therefore, hypertrophied ; by their increased average pressure
the thrust upon the ventricle is increased, and finally this latter
is also hypertrophied. I suppose it is not improbable that the
heart, so susceptible to poisonous influences, may also owe
some of its hypertrophy to reflexion of the irritation of the
impure blood from its own lining to its own muscle : to this
point I shall return presently.

Sir William Gull joins issue with Dr. George Johnson both
in respect of fact and inference. Sir William, like many other
clinical observers, appears to be profoundly impressed by the
apparent constitutional character of that malady of which
granular kidney is, in his view, but one of the expressions.
By constitutional character we mean that the whole man seems
to suffer from some widespread or widespreading change, in-

volving all his tissues, or whole and various districts of tissue in a common degeneration. Without quoting Gull and Sutton verbally, one may express their meaning in this way,—that the change is one of retrogression or involution affecting several tissues, especially in and by way of the blood-vessels, and this a sa fibrosis,—that, in a word, the disease is a disease of senility, using the word senility in the sense of organic rather than of solar revolutions. To Dr. Johnson, then, the disease is a kidney disease with its consequences, to his opponents it is a systemic fibroid degeneration, mainly arterial and peri-arterial in distribution, of which the renal changes are but one expression. Upon the facts their divergences are also curiously opposite. Dr. Johnson stated, in 1856, that in granular kidney the muscular coat of the arterioles is hypertrophied, in some degree perhaps everywhere, but especially in those districts which supply the kidneys, skin, and other glandular structures. Dr. Johnson admits, of course, that degrees of degeneration are present, but believes that they are secondary and nonessential. Sir William Gull and Dr. Sutton deny, or almost deny, that any substratum of excessive contractile power is present at all in the arterioles ; that their obvious thickening is but a lower fibroid transformation —a dead or dying surplusage. Here is a difficulty out of which it may be hard to find a way—a direct issue upon facts between eminent observers, which is not easily to be reconciled by smaller men. Indeed, we cannot talk altogether of reconciliation, for no quarter can be given to "facts." If the present writer may speak for himself he must say that the view proposed by Sir W. Gull and Dr. Sutton has something very like truth about it at first sight, as it seems so much in character with the causes, time of appearance, duration and issue of the malady. Moreover, that the kidneys should primarily begin to cirrhose was so far unlikely as this, that the liver, when it thus contracts, does so only in obedience to the poison of alcohol. But granular kidney, if it may be due to alcohol, is not usually so brought about. It is, on the contrary, a disease from within, a disease often undoubtedly of inherited or acquired tendency, and comparable with sclerosis in nervous organs. It is found in persons whose family histories are bad, or who have been subject to unusual wear and tear. So much *in limine*. But for my own part I must confess that other facts seem to me to be as reported by Dr. George Johnson. By the kindness of Dr. Johnson I have been able to examine his preparations with some deliberation and care ; I have had also in my possession for three months Dr. Dickenson's preparations, which may be taken as impartial witnesses herein ; I saw Dr. Sutton's preparations at Manchester ; and, finally, I have examined not a few cases myself in the fresh state. The

result of my opportunities is, that I side undoubtedly with Dr. Johnson, and am led to the conclusion that the arterioles are the seat of genuine muscular hypertrophy, accompanied, as one might well expect, especially in advanced cases, with evidence of secondary degenerations in the adventitia and elsewhere. If this be granted we must reason as follows. Hypertrophy of the muscular coat of the arterioles tells unmistakeably of overwork, and by no means can be regarded as evidence of primary decay. Now, what work have the arterioles to do? I do not agree with Dr. Dickenson that their contractile action can ever be in propulsion of the blood. Their action is not peristaltic, nor does it seem mechanically possible that the contraction of their muscle can have any result save the lessening of their own lumen, and the creation of a hindrance to the passage of their fluid contents. The tense and prolonged blood-wave seen in the sphygmographic tracings of the patients in question certainly proves that the blood-passage is not favoured, but is hindered in the terminal arteries or in the capillaries. Working backward, then, from what we believe to be ascertained fact, we say first that the muscular coat of the arterioles is hypertrophied, that it has therefore been continuously overworked, that its work is one work only, namely, to lessen the calibre of these vessels, that such lessening, not being peristaltic in character, and the contents being fluid, must have resulted in a prolonged interception of the blood flowing towards the tissues. This argument is supported by the fact that in the agony of death from granular kidney the vascular relaxation is often followed by free sudatory excretion, the effused sweat, as it dries, leaving crystals of urea upon the skin and hair. The diminution of blood, on the other hand, being but imperfectly prevented by the hypertrophied heart, seems manifest during life in the early wasting, superficial anæmia, and general languor and nervo-muscular debility.[1] The explanation of this contraction seems to be that the acrid blood acts upon the arterioles as a pin or a drop of acetic acid may be seen to act upon them in the web of the frog. Now, in the absence of intermediate valves the backward pressure due to this terminal arrest must be felt partly in the elastic arteries and partly in the inner surface of the left cardiac ventricle up to the mitral valve. Indeed, as but little of it is converted in the elastic arteries it must practically be nearly all returned to the ventricle. The consequence of this must inevitably in the healthy heart be hypertrophy of the left ventricle; and here we touch facts again,

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[1] That the blood in renal disease is not merely under pressure, but is actually impoverished, is unlikely to be forgotten. Uræmia seems not only to pollute, but actually to destroy the blood.

—the left ventricle as a matter of fact is genuinely hypertrophied, and no more than the arterioles is stuffed with decay.

It has been banteringly said that Dr. Johnson's hypothesis implies a blind trial of strength between heart and arteries, and that it is absurd to suppose that two organs will thus contend at " pull devil, pull baker." I fail to see the unlikelihood or absurdity of such a disturbed or perverted equilibrium, which has many a parallel in disease. We know as a matter of fact that the arteries do give way under these circumstances, and we may surmise or even assume that were it not for the cardiac hypertrophy the tissues would be so completely deprived of their nutrient supply as to fail at an earlier stage of the malady. The cardiac hypertrophy may be partly due to direct reflexion between its own recipient and executive nerves, but no doubt it is mainly compensatory, as will be urged presently. If the patient's life be spared until the heart's hypertrophy recedes, an early termination of the case by uræmia, as I have seen in many cases, is almost certain. Perhaps the commoner event is the occurrence of a rupture large enough to cause death during the fulness of the cardio-arterial tension. That in granular kidneys, however, some " hyaline fibroid" change is seen in the arterioles and about them does not admit of doubt ; and is, I believe, admitted by Dr. Johnson. Indeed, it could not be otherwise, for, as Sir William Jenner observed years ago, taking congestion of the heart as his text, prolonged congestion of a part, leading to continued high pressure and to interstitial effusion, favours fibrotic or fibrinoid degeneration. Now, super-excitation means supercongestion, and continued high pressure in heart and arterioles means slow interstitial and periarterial effusion which in its turn undergoes fibrinoid consolidation or feeds a low fibrillating hyperplasia. In this way the heart ultimately retrogrades not only in advanced granular kidney, but also in cases of hypertrophy more obviously compensatory, as in those of aortic regurgitation. But it may be urged that Dr. Sutton's preparations cannot all be thus explained. Probably not ; but Dr. Sutton does not seem to me to meet his opponents invariably on their own ground. Unfortunately, Dr. Sutton was unable to be present at Manchester, and his preparations were left to speak for themselves. I think, however, the fault is not wholly in myself if I found them unsystematic. Preparations were put side by side which seemed to me to belong to wholly or very different conditions. They were of great and varied interest, no doubt, but we wanted preparations of special interest. Periarteriolar fibrosis is to be found in many diseased states—states which have no more than a general connection with each other. For instance, cerebro-spinal sclerosis may inosculate with nephritic sclerosis, but the two in most

respects are alien to each other. To come a little nearer
again, there is a complaint not yet described, I believe, which
resembles granular kidney, with the part of Hamlet left out.
In it, too, we have high arterial tension and a thickened heart
and rigid arteries; a heart which labours greatly but irregu-
larly, whereas in granular kidney the heart is usually regular
in its contractions. In these cases urine of good weight is
excreted, and the kidneys only show that amount of change
which is common to all the organs, or nearly so, and which,
moreover, would be expected in cardiac disease.[1] I cannot say
what the earlier arteriolar changes are alike in these cases, as I
cannot get specimens to examine; but I suspect some of Dr.
Sutton's slides are taken from these. Now, although these
cases are related to gout, and so in a measure have a cousinship
with granular kidney, yet they are not granular kidney, and
are not to be confounded with it. Nor must it be here objected
that this cardio-arterial disease—this morbus innominatus—is
the same as granular kidney, with the saving that the kidney
is included in the one and excluded in the other—that the
process is the same in both as to its essence, and different only
in its distribution. For the post-mortem table teaches us that
the closest relations of the malady with granular kidney are the
maladies with other kidney diseases. It has been shown, I repeat,
by many observers, and by Grainger Stewart in particular, that
in proportion to the degree in which intertubular hyperplasia is
accompanied by extra-tubular or interstitial, is the tendency of
such cases to end in granulation or in something like it. Now,
in all cases of renal disease, and even in early stages of it, recent
observers tell us that arterial tension may and generally does
arise, and the heart and arterioles tend therewith to hypertrophy;
and I believe Dr. Johnson finds, in respect of these changes, little
difference between, say, the granular conclusion of a scarlatinal
nephritis in a young person and a nephritis in an older patient
granular from the beginning. The common factor, then, in
these cases at any rate is the nephritis, and the nephritis
cannot be regarded, as Sir W. Gull and Dr. Sutton may regard
it, as included or excluded in one and the same process of
cardio-arterial thickening. The conclusion seems forced upon
me that in granular kidney there is mostly or always a true

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[1] Take, for instance, the rough *post-mortem* made for me in one of these cases of
hypertrophy of the heart without renal disease. There was little or no arcus senilis.
Body stout and muscular, though the patient was in advancing years; the *lungs*
were dark and congested, otherwise healthy. *Heart* very large; weight 24 oz.
Left ventricle an immense fleshy mass. All the valves were efficient; the aortic
a little thickened, and the mitral contained little gritty substances. The muscle
showed tendency to fatty degeneration. *Liver* soft, large, fatty. *Kidneys* large
and flabby, but healthy. *Arteries.* On the aorta small subserous deposits of athe-
roma. The smaller arteries rough and calcareous, inelastic, and lessened in calibre.

cardiac and a true arteriolar hypertrophy; such hypertrophy carrying, of course, in its own superexcitation and supercongestion the conditions of decay, that such hypertrophy and the decay of such hypertrophy are to be found in other renal maladies of different causation and different in initiation, that the renal disease is essential to their existence, that they show not an essential but an accidental likeness to fibrotic endo- and periarteritis of other kinds, and that this likeness arises chiefly in their later stages of heterogeny.

I have hinted already that although thus far Dr. Johnson's facts seem unassailable, his conclusions may not be equally so. In speaking of the "stock-cock" theory, I am probably using a term which this keen and able disputant has made so well known as to need no explanation. At the outset one does not see why the renal arterioles should close their doors against blood which must be especially appropriate to the kidneys. Such blood should open and not close the renal vessels. But passing this, his position depends upon the probability that muscular arterioles in their contraction hinder the blood current. Now, is this simply so? Were we to look at the arterioles alone we should have to admit that such is necessarily the case. But they are not alone; they are bound up solidly with the action and pressure in the heart itself. The nervous connection between heart and arterioles was shown by Heidenhain (in a paper I have mislaid, read, I believe, at the Rostock meeting of the German Medical Association), to end in this, that heightened cardio-arterial pressure in health means not a slackened but an accelerated blood-stream in the vessels. This statement, which seems now to be generally accepted, and which is borne out by clinical experience, is explained by the increase of frequency in the heart's action, which more than compensates the lessened blood-stream and the increased friction in the arterioles. Ludwig showed long ago that irritation of the vaso-motor district of the medulla both quickens the heart and contracts the arterial system. If among the phenomena of the frog's foot are seen arteriolar contractions with local anæmia, we must rejoin that a local reflex action within a narrow circle which is compensated by easy diversion of the blood into other channels is not to be compared to a general influence tightening the whole cardio-arterial tree. In this latter case the blood, which cannot change its direction, probably increases its velocity absolutely in spite of narrower canals and more friction. Let us, for instance, take another case of universally heightened blood-tension, namely that produced by digitalis. In asystolic states, as, for instance, in impaired cardiac nutrition, digitalis, which certainly heightens blood-tension in the arteries, does not the more embarrass the heart and starve the tissues; on the contrary,

the patient finds much relief thereby, and indeed often finds his only relief in continuing the use of digitalis for weeks and for months. We must, therefore, explain the improved velocity of his circulation by assuming that the heightened tone of the arteries is more than compensated by the heightened tone of the heart. Dr. Mahomed, who with the kindness and frank generosity of the true lover of knowledge, has placed the whole of his most recent views and facts on blood-tension entirely at my service, shows me that digitalis, in large doses, produces the most marked tracing of high blood pressure that can be had, far surpassing that generated in any stage of granular kidney. And I may add to this that positively in granular kidney the one drug which of all others seems the least appropriate, but which is, on the other hand, the most beneficent both temporarily and permanently, is digitalis. Is it not almost a necessary inference from these observations, if their truth be granted, that the high-blood pressure in granular kidney is a solid result of the cardio-arterial state, and that in it, on the whole, blood velocity contends against narrower canals with some temporary success, and that indeed cardiac hypertrophy is not an evil to be combated but a compensation to be encouraged—that it is not a disease but a remedy? I do not think, therefore, that the stop-cock theory will hold water.

To proceed, what is the cause of this cardio-arterial pressure? Dr. Mahomed thinks that the cause lies in some delay in the capillaries, and on the whole this seems to me to be the most likely hypothesis. It is not an unwholesome feature in modern reasoning that so-called "vital" forces are not called in explanation until the physico-chemical are proved to be inadequate ; still it is not philosophical to forget that undoubtedly there are modifications of force only manifest in organisation, and it is not rash to speculate that disorder in the rhythm of nutrition may well cause delay in the distribution of the nutritive fluid. Perhaps as good a working hypothesis as any is as follows : [1]— We may premise that such is the interlocking and correlation of the parts of the body a function of one part becomes a function of all—that all parts in fact conspire to the performance of its special duty by each. From this truth we may deduce that the presence of uric matters in the blood while they excite the vasal centres of all other parts to resent their presence, so cause a general increase of vascular tension in all parts but one, namely, in the renal arterioles. Here we may suppose that a relaxation takes place, the tension of the renal artery being lowered ; and this is not unsupported

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[1] This argument, and especially the postulation of some modification of urea, which irritates without feeding the renal cells, I owe to my friend Mr. Charles Smith, of Halifax.

by what we know of the phenomena of blushing, of the effect of carbonic acid upon the respiratory centre, and so forth. Thus, in the normal state is renal excretion favoured, and thereby tension continuously equalised. But we may now postulate that the blood, either by liver disorder or otherwise, becomes charged with a substance so far allied to urea as to set up vascular antagonism, but so far removed from it in molecular constitution as not to pass off readily in the renal cells. Given such a morbid variety of urea, we must have gradually increasing arterial tension everywhere, and gradually involving even the more open renal arterioles themselves. Thus the kidneys would be flooded with blood; they could not cleanse. We should, in such a state of things, expect to find what we do find, namely, a large quantity of urinary water of low specific gravity, and the gradual degeneration of a congested organ.

In the absence of any real knowledge I give this ingenious hypothesis for what it is worth. The first argument against it seems to be that the renal arterioles are as much hypertrophied as those elsewhere. It might, perhaps, be replied that while the hypertrophy of the heart and other arterioles is due to reflex irritation by offensive blood, that of the renal arterioles is due to hyperdistension. But I think there is no evidence that what I may call willing hyperdistension leads to such hypertrophy. Perhaps it may be nearer the truth to presume that the modified urea is too unlike urea to appease even the renal arteries.

We must now pass on to consider the more remarkable incidental symptoms of granular kidney. The admirable lectures on pulse tension by Dr. Broadbent, which recently appeared in the 'Lancet,' have been so widely read that we may pass lightly over many of these symptoms interesting as they are. With regard, however, to reduplication of the heart's sounds the presumption of Dr. Johnson that an auricular systole, becoming audible by virtue of hypertrophy and fluid resistance, gives rise to the first systolic sound, is to me at least highly probable, as I had unwillingly accepted the current explanation of an asynchronism in the ventricular discharge as most unsatisfactory. The intimate connection of the two ventricles by their muscular fibres seems almost to necessitate simultaneous action, while, on the other hand, the phenomena of cases of mitral contraction lend countenance to Dr. Johnson's suggestion.

To the public, and even to the practitioner of medicine, no event is more alarming than the sudden occurrence of an epileptic attack in a person not previously known to be in disease. Like the blindness of retinitis, however, this event is not infrequently the first manifestation of granular kidney. A man may think himself dyspeptic, and his friends may think he is

looking older when this terrible seizure bursts in upon him and
albumen is found in his urine. Albumen, however, is often
found in the urine after epileptic seizures when the kidneys are
healthy, so some caution is necessary in pronouncing upon the
nature of an attack, and the urine must be weighed and tested
for some days before a definite diagnosis is given. Dr.
Dickenson remarks that uræmic convulsions are often deter-
mined by mental emotion ; certainly I have noticed that mental
emotion often sets up the peculiar paroxysmal dyspnœa called
uræmic asthma, an affection probably of like generation. It is
important to remember that near the calamus scriptorius, where
a convulsion point is said to exist, there exists also a respiratory
centre and a vaso-motor centre. We may bear in mind also
that rigors and convulsion often replace each other, especially
in children. Now rigors we know are connected with a crisp-
ing up of the superficial blood-vessels of the body, and we have
good grounds for also believing that convulsions depend upon a
like crisping up of blood-vessels within the cranium. In a
paper on uræmic asthma, read to the Yorkshire Branch of the
British Medical Association last October, I ventured to sug-
gest that this affection might also consist in a contractile
spasm of the pulmonary arterioles. In the Lumleian Lectures
of this year, Dr. George Johnson gives the same explana-
tion. Now, if we put together the well-known high tension
of renal disease (and by way of illustration I may add of
pregnancy also), the unquestionable hypertrophy of the
arterioles, and the almost necessary inference that the vaso-
motor centre is also in a high state of susceptibility, we have
conditions which almost compel us to infer that uræmic convul-
sions and uræmic asthma, like rigors, depend upon a spasmodic
closure of the arterioles in certain limited districts. And if
this be so it is easily intelligible that the onset of either seizure
may be favoured by cerebral irritation passing down to the base
of the encephalon. It may be said, however, that as hæmor-
rhages are often found after death from uræmic convulsions
(Mahomed), and as hæmoptysis commonly follows uræmic
asthma, we should rather infer that the vascular state is one of
dilatation. Now, I venture to think that neither hæmorrhage into
brain or into lung is the rule in either case, yet of my knowledge
I admit it occurs in both in a considerable number of instances.
The explanation of this, I believe, is to be found in the arterial
degeneration which marks the later stages of granular kidney.
This degeneration progresses at various rates in various districts,
and as it progresses the vessels so degenerated are less able to
contract. Some of them, indeed, lose all power of contraction
and become brittle, while others have still an excessive contrac-
tile power. If, therefore, many arterioles contract firmly, and

the heart's impulse as suddenly increases, the access of pressure thrown upon the enfeebled and open arterioles must be enormous and their rupture must be a common accident. Dr. George Johnson suggests that the hæmoptysis in uræmic asthma is due to engorgement of the bronchial circulation, which is a very probable supposition. Certainly evidences of sanguineous effusion are more decided in uræmic asthma than fortunately is the case in uræmic epilepsy. Uræmic asthma again is almost always followed by coarse crepitation at the bases of the lungs, and the brain is usually found wet in deaths from uræmic epilepsy. Passing hastily over many points of less importance or needing less discussion, we may now turn to consider the causation of granular kidney, and herein lies all our hope of forestalling a malady which is incurable when fully established. It was long supposed that the chief cause of granular kidney was the abuse of alcohol. The argument seemed to run thus—granular kidney is very like hobnailed liver; now hobnailed liver is due to the abuse of alcohol, and we know that alcohol causes diuresis; therefore, granular kidney is due to alcohol. This argument leaves much to be desired, and it fails to be justified by facts. In my own experience I very rarely find a case in which alcohol seems to have caused the disease. One apparent instance of such causation I certainly have watched for years to its recent conclusion. A young man of very healthy family history fell into tippling habits, and after some years had passed he fell into an epileptic condition, and this was repeated some months later. His aspect and symptoms now began to point clearly to granulation of the kidney, and of this complaint he ultimately died, at the age of 46, as verified by post-mortem examination. Still, I repeat, cases like this in which alcohol and alcohol alone seem to be to blame for the fibroid involution of the kidney are rare. Perhaps from the time of Todd down to the present day no cause has been more confidently credited with this consequence than the gouty crasis. To attribute granular kidney to gout has become a truism, and we are gravely assured by more than one eminent author that the nexus between the antecedent and consequent is the teasing of the kidney by fine crystals of uric acid or urate of soda. Nay, our very eyes are witnesses in the case, for are not the crystals actually shown upon the tubules of the kidney inciting the baser elements into activity? Yet, confidently as this belief is held, it is assuredly false. If granular kidney were the result of the irritation of gouty salts, who of the gouty should escape it? Now, I appeal to any practitioner whose work has extended over some years, and ask him whether he will not support me in saying that the truth is rather the reverse of this? So far from gouty subjects dying of granular

kidneys the fact is that, as a rule, they do not.   Nothing sur-
prised me more in earlier professional life than this common
experience, that the men in whose systems gouty salts had been
settling for decades, who were bound to die of gouty kidney,
and whom I therefore regarded as the barbarians regarded
St. Paul, on the contrary, lived their three score years and ten,
or if they died in earlier years of chronic involution died with
irregular big hearts, chronic endoarteritis, and so forth, but
generally with heavy acid urine and no albumen.   In such
persons, no doubt, we frequently find coarse kidneys, but kid-
neys which, for all their coarseness, many a poor man would
be glad of, as the children's books say.   Then how is it that
the common error, if error it be, has arisen ?   In the old way,
by the confusion of cause and consequence.   Gout is the *conse-
quence* rather than the cause of granular kidney.   Take the
safe instance of lead poisoning.   We know lead poisoning often
produces gout, it often produces granular kidney, and it often
produces the two results together.   It often produces, that is,
granular kidney with no gout at all, whereby we know that gout
is no necessary copula between the lead poisoning and the granu-
lations of the kidney ; on the other hand, it has never happened
to me to find a gouty painter, or other gouty victim of lead, whose
urine was heavy and whose kidneys were presumably adequate.
Again, if we leave lead poisoning and turn to ordinary consti-
tutional changes, what do we see ?   We see a lady or gentleman
of middle or later life who comes to us for gout and we examine
the urine.   We find it not lithatic ; we hear that it has never
been lithatic, of late at any rate, but, on the contrary, pale and
abundant ; we find the specific gravity to be 1010 or under, and
we test the whole day's bulk daily for a week, and find it still
ranging always low ; perhaps we see a cloud of albumen on
pouring nitric acid below it in a beaker, or we find albumen in
more obvious quantity.   We make further inquiries and we find
that our patient has become gouty rather quickly of late years
and has not earned gout by free living, and we decide surely
that the gout is the consequence of less and less perfect renal
work.   The lithatic man, on the contrary, whose urine has been
the despair of the housemaids for twenty years, whose eyelids
and ears are full of concretions, and whose gnarled fingers and
roomy boots tell an old story, limps on his way through a
lengthening tale of years when his brother patient has been dead
of uræmia for years.   Yet, surely, if gout worries the kidneys
into granulation, this man is the one who by all rights should
lie first in the churchyard.   That there is no relation between
gout as an antecedent, and granular kidney as a consequence, I
am not prepared however, to say.

   Dr. Broadbent reminded me, and the suggestion certainly

was in accordance with my own experience, that many persons in whom granular kidney appears are of gouty stock. He mentioned to me a family of five sons, of which two were very gouty, and two others died of granular kidney. Gout was strong in the preceding generation. This I believe is to be accepted as a marked instance of a true proposition, that although gout and granular kidney are not father and child, they may be cousins. It is impossible as yet to find the nexus, but its explanation is, depend upon it, something far more profound than that which we may call the teazle hypothesis. So far as gout and granular kidney coexist in the same person, the latter is the cause of the former, and the common teaching of the contrary must be reversed. How renal inadequacy, long before anything like albuminuria is present, allows the standard of blood purity to be lowered, and before it may cause obvious gout, may turn the scale against the patient in either acute or chronic disease is, however attractive, too long a story to take up in this place. In the meanwhile the reader may be sure that this point is one to which general attention must be turned. Still more, prolonged high arterial tension from gouty blood will not produce granular kidney, so it cannot be said that podagra is a preservative from granular kidney. I have many times seen patients who have suffered off and on for years from exhaustion, melancholy, apprehensiveness, startings, dismal morning awakenings and general pessimism—patients who have taken tonics in vain for years, and who have found life and buoyancy in a brisk saline purgative, whose pulse has been tense for years and have not had granular kidney. So it is with gout and granular kidney. In contemporary journals I have ventured for some years to assert that the most distinct cause of granulation of the kidney is mental anxiety and prolonged distress. What I have said on this subject I have said at too great a length elsewhere to permit of my repeating it in this place. I content myself with adding that Dr. Brookhouse, of Nottingham, and Dr. Dickenson, have both arrived independently at the same conclusion. How it is that under nervous stress the pulse tightens, the blood pales, and the urine grows poorer, no one has yet shown us; but the fact is unquestionable. I have of late been informed that very careful observations of the pulse curves of a young and healthy man have shown increased tension during twelve months of domestic anxiety; but, as aforesaid, the pulse tension and granular kidney can only be regarded as common consequences, and the former does not give rise to the latter. Were it so we should find granular kidney arising in cases of high arterial tension the result of cerebral disease, for some of the very tensest aortas and pulses I have seen have followed apopleptic seizures and other organic encephalic irritations.

Cerebral irritations, then, organic or functional in origin, may give rise to tense pulse, but this tense pulse cannot of itself force the kidney into granulation. More probably the nutrition of the kidney is in some subtle way deranged, as that of the liver is in diabetes [1] from like causes, or some more general influence upon the whole body favours involutions after the fashion indicated by Sir William Gull. Heredity is unquestionably a potent means of propagating granular kidney, and, as before suggested, it may occur with disproportionate frequency in individuals not personally gouty but of gouty stock. Of those, however, to whom this malady has descended as a damnosa hereditas, none are more to be lamented than those young persons who are carried off in the flower of their age as by a galloping consumption. These awful cases, seen not infrequently in early adult life, are very terrible and inexplicable in their course. These cases of premature seizure have in my experience been found always in families of very bad health history. Either they have sprung of actually phthisical families, or of those sallow skinned, cold-fingered, genteel, listless folk who are said to have " no constitution." A young scion of such a house may have amaurosis or epilepsy before his terrified relatives have realised that he is more than a little out of sorts.[2] The still fountains of granular kidney run deep, and we get but obscure hints of its causation and origin ; yet herein lie our only hopes of therapeutics. It is the use of positive methods and of arms of precision which has given us a scientific knowledge of disease, and the sphygmograph and the urinometer are to granular kidney what the thermometer is to fever. By the sphygmograph we measure the tension of the arterial tree, and we learn that tension may arise from many causes, even from direct cerebral irritation. We learn, however, that while pulse tension will not cause granulation of the kidney, yet this latter unquestionably produces the former, so that the one may become a measure of the other. But probably before granulation has actually invaded the kidney, the functions of this organ become perverted, and therewith the arterial tension is raised, and if the malady be recognised by urinary chemistry and by the sphygmograph in this stage the false tendency may be neutralised. By relief from mental anxiety, by alteratives, by the forced inspiration of pure mountain air,

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[1] The mere passage of an excessive run of blood through expanded renal vessels (as in diabetes insipidus ?) probably would not lead to granulation ; or cirrhosis of the liver would be seen in the later stages of melituria.

[2] For example, a young man, æt. 20, was brought to me complaining of little beyond this : that one day when shooting at the butts at Cambridge, he found himself blind of the right eye. He went to Mr. Critchett, who sent him to me, and I found cardiac hypertrophy and albuminuria. His life was spared but a few weeks.

by cherishing the functions of the skin (which seems to become granular *pari passu* with the kidney), by diet and renovation of the blood, that may be achieved which in a later stage has become impossible. It is a growing habit with myself to take sphygmographic tracings, or at any rate digitally to appreciate the pulses of my patients as closely as possible when I am consulted by persons showing some evidence of wear and tear or an increasing sense of fatigue. With this let the urine be minutely interrogated as to the adequacy of the kidney, and let any deviation be closely noted with a sense of the far greater preciousness of such warnings than of the brutal reply of albumen to one's tests, which signifies almost a hopeless state of things in respect of restoration of health. In cardiac hypertrophy we find a compensating process which we may encourage by digitalis; and with digitalis and iron on the one hand, and cutaneous and alveolar stimulations on the other, more may be done than by any other means. Still this very cardiac hypertrophy has its own dangers in straining vessels which, if not primarily diseased, tend to become so secondarily. Nor can any organ change its course, even in a compensating process, without disorder and injury to other parts. And if these dangers be averted, yet the heart, hypertrophied for a while, in time loses its muscular value, and its fibres become degenerate, and even fatty. The pulse then loses its tension, and when this fails, coma does not long delay to seize the victim whom apoplexy in the days of higher tension may have spared. Organic disease cannot be cured, nor its consequences often averted. In this we have no new grounds for despair, but, with the more perfect sanitation of our own bodies, the time may come when the presence of organic disease, like bad farming, will mean that somebody is to blame; let us hope that it may not be the practitioner of medicine.—T. CLIFFORD ALLBUTT.

IV.—Robin on Urine in Typhoid Fever.[1]

THE book before us is a very valuable and interesting contribution to urinology. It is the production of a distinguished student of the Paris hospitals, who is also an excellent chemist.

The study of the urine in disease has for several years been sedulously pursued by some of the ablest physicians and clinical teachers of Paris, amongst whom MM. Gubler, Jaccond, and

Parrot take the lead, and with them M. Albert Robin has been a fellow worker.

In ordinary practice it is generally deemed sufficient if the specific gravity and acidity of the urine be ascertained, and if it be tested for albumen and sugar, or occasionally for the chlorides. Want of time and the complexity of the processes required for an elaborate scrutiny have hitherto, even in hospital work, prevented more complete examination. But as chemistry has now furnished us with simpler modes of procedure for minute investigation, and as most important information may be gathered by putting them in practice, we trust that they will soon be more generally adopted.

As a peculiar interest attaches to the colouring matters of the urine, we shall give an abstract of M. Robin's observations upon them before noticing the special subject of his essay. Unfortunately a needless difficulty has been introduced in their study by the confused nomenclature which exists. Some of them have three or four different denominations, and, still worse, the same name has been given to different pigments.

M. Robin adopts a distinction, introduced by M. Gubler, between colouring matter and chromatogenic matter; the former manifests its presence at once; the latter must undergo further change before it is visible.

Of colouring matters he enumerates the following:

Urochrome. Hémaphéine. Uroérythrine. Hémoglobuline.

And of chromatogenic matters:

Indican and Urohématine.

The latter are the producers of the indigo-blue and red colours.

The origin of all these bodies is generally ascribed to the globulin of the blood, and this appears to be correct, except, probably, as regards indican.

Urochrome is the ordinary colouring matter of the urine, and is derived from hæmoglobuline, directly or indirectly. Jaffé believes that there is an intermediate stage; he thinks that the hæmoglobuline first becomes *bilirubine* in the liver, and that this is transformed into the matter under notice, which in consequence he names *urobiline*. That this may easily and probably often does take place] is undoubted; but both M. Gubler and M. Robin agree that ordinarily the intervention of the liver does not come into play.

As to its clinical value, urochrome is the most complete product of the combustion of hæmoglobuline. It is often in direct relation with the activity of the liver, possibly also with that of the spleen. It is increased in quantity whenever there is rapid destruction of blood-globules. It is diminished in anæmia, in chronic parenchymatous nephritis, and in diabetes.

Uroérythrine and Hémaphéine.—The derivation from the blood of these pigments appears all but certain.

The first is identified with the product well known as *purpurine* or *purpurate of ammonia.* The latter is so closely allied to urochrome that there is reason to believe that it is virtually the same body.

M. Gubler considers both these pigments as indicative of *hepatic insufficiency.* They appear either where there is a destruction of red globules too considerable for the transforming power of the liver, or where, from disease, the hepatic cells are unequal to their normal work. Hémaphéine he ascribes to the first condition, uroérythrine to the second. The pathological states in which they occur are very numerous.

Indican and Urohématine.—The former undoubtedly results from albuminous decomposition. It is characteristic, says M. Gubler, of severe fevers, where, notwithstanding the high temperature of the body, the respiratory combustion seems to be reduced in amount. Hence, as the albuminous waste of the organism is increased, and as for want of oxygen it cannot pass off as urea, it is voided as albumen, albuminose, and indican. The latter thus seems to be albumen slightly changed. But several German chemists maintain that it is analogous to vegetable indigo, and that it takes its origin in *indol,* a product of the digestion of albumen, which is always to be found in the intestines. The indol thus formed is absorbed into the blood, and is discharged by the kidneys as indican. Probably each theory is true to a certain extent. It is met with in these morbid states :—Disorders of the alimentary canal, with ganglionic disturbance ; where albuminous waste is too great for the quantity of oxygen in circulation ; in many affections of the liver, of which carcinoma is the type ; in affections of the spinal cord ; and in Addison's disease.

Urohématine seems to be closely related to indican, but a careful study of its composition and characters is still wanting. It is found in the urine of patients suffering from profound anæmia, acute miliary tuberculosis, intermittent fever, and other diseases where there is a rapid destruction of blood-globules. To these last-named bodies, rendered manifest by ammoniacal fermentation in the urine in cases of cystitis or purulent nephritis, the names of *uroglaucine* and *urrhodine* have been erroneously given.

We now proceed to follow our author in his study of the urine in typhoid fever, and for the benefit of those of our readers who may not have an opportunity of seeing his book we will give a summary of his most important conclusions.

He remarks that M. Gubler has taught for twenty-five years that, from the characters of the urine alone, severe cases of fever

may, from the first, be discriminated from acute inflammatory attacks. He describes the following as the most striking and constant. During the stages of development and maturity in typhoid fever the urine is of a dirty, brownish-yellow colour, like beef tea, and very acid. If to a goblet about three fourths full, nitric acid be gently and slowly added until it occupies the lower two fifths of the glass, there will be seen, in the course of from three to five minutes, at the surface of contact between the two fluids (1) an intense indigo-blue colour; (2) a layer of albumen, more or less thick, but constant; (3) a faint diaphragm of uric acid; and (4) sometimes crystals of nitrate of urea.

During the stage of decline the urine becomes clear, the albumen diminishes or disappears; the uric acid increases, sometimes forming a deposit; the colour produced by nitric acid changes gradually from blue to violet, then to red, and lastly to a brilliant rose, which is an indication of full convalescence; at this period the uric acid is reduced in quantity, and the urine becomes alkaline from an elimination of the fixed alkalies.

The details of this outline, as regards both the physical and chemical characters of the urine, are described and discussed with much minuteness and fulness.

In *colour, aspect*, and *consistency* the urine varies according to the intensity, stage, and complications of the disease; the indications within certain limits being constant and reliable.

As regards *quantity, density*, and *excretion of solid matter*, M. Robin concludes that, in a case of medium severity, during the first and second stages, the amount voided is slightly less than in health, the density and solid constituents somewhat more. During the third week the quantity rises, the specific gravity falls, and the discharge of solid matter is increased to a small extent. When convalescence is taking place the quantity again increases, and the specific gravity falls, whilst the daily excretion of solid matter undergoes considerable augmentation. These proportions, as might be expected, are modified where there is much perspiration or diarrhœa, or where the case is complicated within testinal hæmorrhage or internal inflammation. Peculiarities of *odour* have also been carefully studied. The herbaceous and fetid smell sometimes noticed at the period of decline is usually associated with an increase in the amount of solid excreta, and is ascribed to fermentation in the extractive matters, always voided abundantly during that stage.

As regards the *reaction*, the most interesting fact is the gradual transition from great acidity in the first stage to alkalinity in the third or during convalescence. M. Gubler has pointed out that this character is generally to be noticed during recovery from long-continued, debilitating disease, such as

typhoid fever or pneumonia. It is not usually present during more than five or six days, sometimes only during one. It is due to the fixed alkalies, and is recognisable immediately after the urine is voided. It is noteworthy also that at this stage of the disease fermentation very readily takes place in the urine, and a few hours after being voided it becomes ammoniacal. In direct opposition to the opinion of some writers it must be deemed a favorable indication.

Deposits of one kind or another are common in the urine in typhoid fever. The most frequent are the following :—The ammoniaco-magnesian phosphate, which is generally seen in the later stages and the less severe forms of the disease, and associated with an ammoniacal state of the urine. Urate of ammonia, often abundant in its earlier days, subsides in its mature stage to reappear in its decline. Urate of soda is rather more frequent than the preceding in fatal cases, and rather less so in cases of medium severity ; it is common where there is thoracic inflammation. Uric acid is rare in grave and fatal cases. Indigose is frequently seen in severe forms of the disease. White globules and mucus are not uncommon at all stages of the complaint, but the occurrence of a distinctly purulent sediment must be referred to associated catarrh of the urinary passages. Blood, both globules and dissolved hæmoglobuline, may be present, and is often so in severe cases. Urinary tube-casts and fat-globules are not rare, but are always very grave indications.

There has been much discrepancy in the statements of writers respecting the daily excretion of *urea* in typhoid fever. Becquerel believed that it was reduced below the normal amount, and this was also the conclusion which Anstie drew from his observations. Sigmund and Vogel thought that the proportion was increased during the early stages of the disease, and that it gradually fell to its usual standard during convalescence. Others have been of opinion that there was a relation between its measure and that of the temperature of the body. M. Robin states that nothing is more variable than the elimination of urea in typhoid fever ; his notes of seventeen cases, which do not, however, apply to the incipient stage, gave a slight reduction below the average, and this is more evident in severe than in mild examples of the disease. In three fatal cases he found a large diminution in its amount ; if any inference be warranted upon such data it will be in contradiction to the judgment of Anstie, who thought that he had noticed a direct relation between the mildness of the complaint and the smallness of the quantity of discharged urea. It seems probable that variations in the amount of nutriment taken by patients under observation may account for some of this diversity of result.

Uric acid is increased in quantity in severe forms, and in the earlier stages of the complaint, but is much influenced by the presence or absence of complications.

The extractive matters of the urine comprise all organic principles not included under the preceding heads. Modern chemistry has supplied us with a long list of these bodies, but has not as yet given a simple and rapid mode of estimating their quantity. It appears, however, to be clearly proved that they are always augmented during the mature stage of typhoid fever, and that they observe an inverse ratio with that of urea. In the latter stages they decrease and even fall below the usual standard. The not unfrequent appearance of *albumen* in the urine during the height of the disease has been recognised for a considerable time, but we are indebted to M. Gubler for the establishment of the law of its constant presence. During fifteen years, and in many hundred examinations, he has never found it absent. It occurs in the second week if not before, and its amount is proportional to the severity of the attack and its complications. It should be stated that its quantity is often so small as not to be revealed by the usual mode of testing; it may then be detected by M. Gubler's process already described. This explains, no doubt, the opposing assertions of writers on the subject. In grave cases it appears early, in larger quantity, and continues present longer than in those of average type.

The *inorganic principles* are largely reduced in quantity during the first and second stages of typhoid fever; they undergo a sensible increase during its decline, and sometimes rise above the usual rate, during convalescence. As regards the *chlorides,* it has often been remarked that in all acute maladies where but little food is taken their excretion is necessarily lessened; but of these diseases there are, as M. Robin observes, two classes; in one the chlorine, set free by disintegration of tissue, escapes by the urine; in the other it enters into the composition of morbid products, and little or none passes off by the kidneys. Typhoid fever belongs to the former class; pneumonia to the latter. Variations in the amount of chlorides have a certain diagnostic and prognostic value; the reduction being much less than in pneumonia, and generally also being proportionate to the severity of the attack.

The phosphates undergo less diminution than the chlorides during the first and second stages. The earthy phosphates are reduced to a greater extent than the salts of potash and soda, and undergo a corresponding increase during the third and fourth weeks. An Italian chemist, Signor Primavera, has founded certain indications upon this circumstance, but the results are not sufficiently uniform to be of much value. The

quantity of these salts is much influenced by the presence or absence of diarrhœa. M. Robin states that in cases complicated with much nervous disturbance and delirium, an increase in their amount is constant.

An estimate of the quantity of *sulphates* in the urine is of importance, as throwing light on the degree in which albuminous disintegration is going on in the body. In general, they are but slightly augmented during the first stages of typhoid fever, and decrease during convalescence. But little has yet been made out respecting the *carbonates.* Respecting *pigments,* and *chromatogenic matters,* M. Robin observes that *urohématine* is· found in increased amount in only a small number of cases, and that these are fatal ones. In ordinary attacks it is lessened during the early stages, and becomes normal as the disease declines and during convalescence. He found it augmented by the occurrence of pulmonary inflammation, or of internal intestinal hæmorrhage. *Indican* is more abundant in fatal than in non-fatal cases, and in severe than in mild attacks. It exists in largest quantity at the height of the disease, and diminishes progressively as the fever subsides. It has been copiously found where there has been profuse diarrhœa, or where a high internal temperature coexisted with great sensibility to cold, or where abdominal inflammation was present. When it does not follow the course of the complaint and subside with it, M. Robin believes that a formidable relapse may be anticipated. He founds this opinion on four observations.

Hemaphéine and *uroérythrine* are only exceptionally present in the urine in typhoid fever. When they exist, it is at the height of the disease and generally when it is associated with pulmonary inflammation.

M. Albert Robin is the first to acknowledge that some of these conclusions are, as yet, based upon an insufficient number of facts, and may be consequently hereafter modified. He regards his work as an outline to be filled up. We have not space for a detailed comparison of his results with those of other investigators ; nor indeed, at present, would the process be a profitable one. Before it can be satisfactorily made we must accumulate a considerable body of trustworthy records. Both M. Gubler and M. Robin believe that more aid may be obtained than has hitherto been done, from a careful study of the urine in many doubtful and difficult cases. Several illustrations of this are given by our author, one of which we quote as an example of his mode of dealing with such a problem. One of the diseases most liable to be confounded with typhoid fever is acute miliary tuberculosis. There may exist in common diarrhœa, meteorism, splenic enlargement, headache and deli-

rium, redness and dryness of the tongue, epistaxis, and some-
times, it is said, an eruption of rose-coloured papules.   As
noticed by M. Jaccoud, the thermometric changes are less
regular in the tuberculous than in the typhoid disease, and
the urinary characters are very different.   The urine is usually
clear and of a dark red colour, scanty and of high specific gravity ;
it contains much urea and uric acid, and little, if any, albumen ;
indican in small, and the other colouring matters in considerable
quantity.   The recognition of these distinctions may decide the
diagnosis in favour of tuberculosis.   And so with some other
diseases which under peculiar conditions may be mistaken for
typhoid fever.   The urine in them presents a group of distinc-
tive characters which if not sufficient to establish a diagnosis
may be safely employed to confirm or correct an opinion.

Our prognosis may also be considerably aided by the careful
study advocated.   As the alterations in the urine follow a
regular course with the evolution of the disease, and are even
sometimes the first evidence of bodily changes not yet manifest
elsewhere, so they serve to strengthen or control other indica-
tions.   Thus, when the stage of decline is at hand, and the
temperature begins to fall, additional value is given to this sign,
if it be found that the quantity of urine and the elimination of
extractive matters are gradually increasing.   The same conside-
rations also may govern our estimate of the importance to be
attached to the occurrence of perspiration at this period of the
complaint. If it be casual, both the amount of fluid and excre-
tion of solid matter are notably diminished ; but if it be critical,
they are scarcely, if at all influenced.

The observations and inquiries which we have been consider-
ing tend to elucidate the development, and may help to guide
us in the treatment of the disease.   Although we may doubt
whether it ever be spontaneous, we are all agreed that, however
produced, a poisonous agent is at work in the body.   The death
and disintegration of its tissues proceed at an increased rate.
The waste products, more or less prepared, are thrown out of
the system by various channels, or accumulate in the blood,
and add to the gravity of the attack.   The proof of this pro-
position rests upon the repeated verification of such statements
as these :—the less the elimination of urea, and of solid matter
by the urine, and the more grave the seizure ; in severe cases,
the excretion of solid matter is greatest during the periods of
decline and convalescence ; where there is neither diarrhœa nor
perspiration, unless there be an enhanced discharge by the
kidneys, the danger rises; critical perspiration coincides with
an increase in the solid elements in the urine ; and in cases of
relapse there has been in the early stages of the complaint a

deficient elimination of its organic constituents. M. Robin also alleges, in further support of these conclusions, that in severe forms of the disease, whether owing to the influence of the typhoid person, or to the imperfect action of the lungs, the lowly oxygenised elements of the secretion from the kidneys are most abundant.

It results from these researches that incomplete removal of effete matter constitutes one of the chief difficulties and dangers in typhoid fever. This is mainly due, M. Robin thinks, to its insolubility, a consequence of insufficient oxygenisation, and to the serious implication of the different emunctories of the body, and especially of the lymphatic system, by means of which the chief drainage of the tissues is brought about.

The therapeutic indications follow logically from what has gone before. They are:—(1) To destroy the typhoid poison. (2) To fortify the body against its influence. (3) To aid the expulsion of diseased products.

Our readers have only to compare the conclusions of which we have given an abstract with the statements in our recent treatises on typhoid fever, to appreciate the advance which has been made. More and further observations, however, are wanted for the extension, confirmation, or correction of our present knowledge.

V.—Medico-Legal Aspects of Insanity.[1]

THE work of M. Tardieu claims consideration from the profession in this country, less from its being a systematic inquiry into the legal relations of mental disease, than as an expression of the experience and opinions of French alienists upon this subject. It consists mainly of disquisitions, first, on the circumstances in which the medical man is called upon to determine the mental condition of individuals whose acts demand interference in their own interests or the interests of society; secondly, on the principles and rules by which the medical expert should be guided in forming a judgment as to the mental state, the conduct and the moral and legal responsibility of the insane; thirdly, on the appreciation of the nature, extent, and influence of the different kinds of insanity; while a fourth section is devoted to a collection of typical examples

[1] 1. *Étude Médico-Légale sur la Folie.* Par AMBROISE TARDIEU, Professeur de Médecine Légale à la Faculté de Médecine de Paris, Medecine de l'Hôtel Dieu, Membre de l'Academie de Médecine. Paris, 1872.

2. *Contributions to Mental Pathology.* By I. RAY, M.D., Author of Medical Jurisprudence of Insanity and Mental Hygiene. Boston, 1873.

of medico-legal investigations affecting the life, liberty, and property of those supposed to labour under diseased minds.

In the first division of the subject the author affirms, and rightly affirms, that the seclusion of a lunatic, or of a person suspected to be a lunatic, under whatever circumstances, is most improperly, though often popularly, regarded and represented as imprisonment—as an arbitrary deprivation of personal liberty; whereas the person thus secluded is a patient, not a prisoner, suffering under formidable, it may be fatal disease, involving not merely himself but many members of the community; and in whose case observation and treatment are absolutely required for his own sake as well as that of others. He further affirms that such objects can be obtained exclusively in an hospital where the structure of the house, the domestic and social arrangements, and the services of specially qualified physicians are all brought to bear upon the physical and psychical state of the inmates, upon their accountability and upon the re-establishment of their health and their restoration to their original rights, positions, and privileges. This remark is not a platitude, but refers in an especial manner to that provision under which persons accused of crime, or minor violations of law, but who are supposed, or pretend to be, of unsound mind, may be committed to an asylum previously to trial, in order that their deportment and conversation may be tried and tested and the amount of their responsibility determined by those accustomed to such moral experiments, under favorable circumstances and free from the partisanship and special pleading which sometimes disgrace courts of law, and from the difficulties of diagnosis and the opportunities for deception which mar or modify all examinations of the accused when in prison. When commenting upon the suspicion and discourtesy with which medical witnesses are treated, and the efforts so frequently made to confuse and perplex them rather than to elicit the truth, he illustrates his arguments by narrating an address by M. Troplong, in which alienists are held up to scorn or ridicule as disposed, and prepared to describe every man as a monomaniac, to controvert and counteract the operations of recognised judicial tribunals, to detect in every peculiarity a symptom of derangement and to hold up the fancies of Pascal and the familiar demon of Socrates as illustrations of mental perversion and obscurity. He adds the anecdote of another judge, who, after leading an expert into a dilemma, exclaims: "Ah, doctor, are not all murderers in your eyes madmen?" having previously arrested the attention of the jury as if he were about to say a good thing, by winking to them (un clignement d'yeux, p. xvii). Although spared such vulgar ribaldry on this side of the

Channel, it would be well for the safety of those arraigned, in all
doubtful cases, as, for instance, of the imbecile, the drunkard,
the partially demented, and with the view of avoiding the risk
of such a miscarriage of justice as was nearly committed in the
case of Treadway the epileptic; were such a probationary
course pursued as has been long recognised and is available in
France, and as has recently become statutory in some of the
American States. As an appropriate key-stone to the con-
struction of a treatise on the civil and criminal status of the
insane, M. Tardieu has introduced the law of 1838, and the
Royal Ordonnance of 1839. These legislative enactments have
been unrivalled in unpopularity, have provoked repeated resist-
ance and reclamations on the part of the public officials to whom
they chiefly apply, and have called forth protests so reasonable as
to have elicited promises of reconsideration and amendment from
the government. The precautions prescribed as to the interdic-
tion, incarceration, and management of lunatics may have
proved erroneous and unnecessary, but they so closely approach
in principle, however much they may differ in detail, those
existing in Britain, that it would be supererogatory to particu-
larise them or to advert further to their special characteristics
except where they appear fraught with evil.

In the first place it appears a most perilous adventure that
in terms of Article 3 and 14 the medical officers of public
asylums should be appointed or dismissed by the secretary of
state in conjunction with the local prefect; that transference
from one asylum to another, which cannot take place under a
service of three years, should be effected through the influence
of the same patrons, and this irrespectively altogether of the
experience or opinions of the commissioners intrusted with the
supervision and regulation of the fiscal and general affairs of
the establishments, and who assuredly are the only authorities
possessing opportunities of forming a judgment as to the con-
duct and capabilities of the officials directly in charge, or appa-
rently qualified or entitled to pronounce an opinion on the
suitableness of such officials for another and perhaps wider
sphere of duty.

The apprenticeship of three years here ordained is excellent; but
in a community where political opinion modifies if it does not over-
rule all other considerations, such a mode of promotion must be
regarded with dismay; but a yet more vicious provision is inau-
gurated in Article 6 of the Royal Ordonnance, where not merely
a co-ordinate, but what, confessedly, has proved an antagonistic
authority is conferred upon the physician and upon an officer
called the director, who is empowered to superintend the
internal order, and the police service, within certain limits,

together with the administration of the domestic economy, and of the arrangements for the admission and discharge of the inmates.

In the medical attendance of a public infirmary for bodily diseases it is possible, though difficult, to conceive that parallel powers, the one affecting the existing ailment and the other affecting the comfort and general sanitary and dietetic interests of the patients, may be exercised distinctly yet harmoniously ; but in an asylum for mental diseases, where the furniture, the general movement, the moral influences, in short, every event, however trivial, every accident, however insignificant in the eyes of common observers, affects for good or for evil those whom they reach, and become instruments of cure or alleviation in the hands of a skilled medical man ; it is certain that such independent, if not conflicting authorities as have been described, must clash at every point and stage, to the detriment of those principally concerned and to the neutralisation of many of the most important objects in view.

This machinery has, however, worked advantageously in the production of a large corps of distinguished superintendents and administrators.   In Britain some feeble essays have been made in imitation of this plan, by intrusting factors, and so-called lay directors with privileges as to the employment of patients in occupation, &c., trenching upon the obvious and acknowledged exercise of medical treatment, but these have proved such signal failures or such sources of internecine vexation that the issue of the experiment may be anticipated.

The operation of this law in reference to sequestration may prove cumbrous, but, in the eyes of certain alarmists, affords a guarantee against arbitrary or irregular proceedings in the deprivation of personal liberty.

It provides that no supposed lunatic shall be admitted into any asylum unless certificated by one medical witness, and that, in the case of private establishments, the patient shall within three days be visited, and his condition and all evidence as to the state of his mind examined and reported on by two experts to the administrative authority under which they act. In Paris, where the numbers of private patients are said to be very large, two physicians are specially appointed for the performance of this duty, while elsewhere it is confided to any qualified person. Not satisfied with the security afforded by this machinery, a demand has been made that the primary certificate should contain the opinions of two medical witnesses.   The effect of these steps is to place the individual in circumstances favorable for observation and treatment ; but where interdiction or inter-

ference with the validity of all civil acts and the appointment of a guardian is regarded by relatives as expedient, it is prescribed, in addition, that a personal inquisition or interrogatory, founded upon the medical statements, should be made by a legal Official ; a decision as to the existence of alienation being, of course, practically revoked by the return of reason. This process, closely assimilated to the inquests *de lunatico inquirendo* in Britain, would, if the production of every patient in court or before a judge and jury were imperative previous to confinement, realise the theories of such Utopians as consider existing safeguards insufficient.

Wherever the intervention of the law is necessitated it becomes incumbent to affirm not merely the presence of mental weakness or alienation generally, but to indicate the specific character of the malady upon which any opinion is founded. Three categories are supplied, under which exemption from responsibility or the deprivation of civil rights may be craved. These are habitual imbecility or congenital feebleness and limitation of capacity ; 2ndly, dementia, or the deprivation of mental health and strength consequent upon other forms of disease ; and, 3rdly, fury, comprehending the acute forms of insanity marked by delirium, agitation, and violence. Where the object is, as in a well-constituted community it ought to be, the restoration or preservation of reason rather than any finical solicitude as to the possible infringement of that abstraction called freedom, these terms and the departures from health which they represent would be found amply sufficient to carry out, wisely and humanely, the objects in view. But although broad and inartificial, they have been found practically inadequate, as all, even the most rude and rudimentary nosological distinctions, will be found, to comprehend the proteiform aspects of mental perversion ; and dementia is, accordingly, made to include all such modifications as cannot be classified under feebleness or fury. The difficulties encountered, popularly, scientifically, and judicially, in defining the form and amount of such cerebral affections as appear to justify seclusion or the protection of an unhealthy individual against himself, has been felt to be so great that recourse has been had to description, to a substitution of facts for opinions, to personal as well as collateral experience ; in short the substitution of a picture for a deduction from premisses.

It is obvious that the practice adopted in this country, however faithfully adhered to, has failed not so much in guarding the rights of the citizen ; for the proceedings of a recent parliamentary committee tend to show that these, far from having been invaded, have scarcely been touched ; but in presenting

tangible and universally appreciable grounds for the bold course which they sanction. This failure is in part due to the ignorance, imperfect training, and defective discrimination of those who testify, but, chiefly, to the intractableness of the subject, to the impossibility of conveying in popular language what should be the conclusion of protracted scientific investigation and its results; a dilemma which may ultimately necessitate a return to the time-honoured and expressive but simple formula of a declaration of the existence of "mental unsoundness."

M. Demolombe is quoted in justification of the French legislators, but when he says that "they did not pretend to define with rigorous scientific exactitude the different varieties of mental maladies, and that in adopting the terms which they have used—imbecility, dementia, fury—they sanctioned such as were at once comprehensive and capable of general applicability" (p. 34); he appears to concur nearly in the opinion which we have ventured to advance.

But, in despite of all the precautions resorted to in order to avoid injustice or rashness, French as well as British legal and medical authorities are exposed to suspicions and accusations which must disturb the interests and movements of society. Of these an example may be adduced. One of these public prosecutors or calumniators, as the case may be, asserts that "liberty is violated annually in the persons of upwards of 600 citizens" only culpable of having undergone a more or less marked alteration in the intellectual faculties, by the operation of Clause 489 of the Civil Code," which orders "that all those of mature age who are in an habitual state of imbecility, dementia, or frenzy, ought to be interdicted, even where this state presents lucid intervals." It might have been conceived that protection was afforded by the significant word "habitual," but the addition of the pregnant qualification "lucid intervals" discloses a wide opening for the introduction of doubts, difficulties, and, it should be confessed, of suspicion. What is a lucid interval? Is it a complete suspension or a mere mitigation of the aberration? Is it to be determined or distinguished from remission or intermission by its completeness or its duration? These questions come home to psychologists as forcibly in England as in France. Our law appears at present to rest in an intermediate, perhaps a transition state, and to recognise in the reasonableness of the act performed, whether that be a contract of marriage or the execution of a will, the test of the reality of the re-establishment of sanity; independently altogether of the time occupied in its performance or during which symptoms of alienation could not be detected, and of the nature

of the disease which pre-existed and followed the act, and of the
physical antecedents of the actor.

But, besides the substratum of character and conduct, the
possibility of impairment or instability of will being compatible
and coincident with intellectual clearness, the precariousness
and inchoateness of all mental conditions in those suddenly or
recently awakened from mania or melancholia should enter as
an element in all such deliberations.

M. Tardieu relates an instance of the celebration of marriage
during a lucid interval of two days' duration. We have known,
in a hundred instances, judicious, benevolent, even noble deeds,
continuously and consistently performed by lunatics who were
notably, though not palpably, mad. We have known the dis-
appearance of all moral exaggeration and error for long periods
while the physical signs proclaimed the persistence of general
paralysis. We can recall the history of a lady who appeared to
be quite recovered for eleven months, in whom paroxysms of
excitement depended upon herself; and of many hysterical
maniacs who could simulate sanity and serenity with nearly the
same success as excitement, folly, or fatuity.

The elasticity of the provisions for the discharge of persons who
have laboured under mental disease is strongly contrasted with
the rigidity of the regulations affecting their sequestration. The
liberation of a patient supposed to be cured depends, in a
majority of cases, upon the opinion of the superintendent of the
hospital; and that the standard upon which such conclusions
are formed must differ widely and vary with the mental constitu-
tion of each physician is demonstrable, not merely by the fre-
quency of relapse, but by the fact that suicide and violence have
followed almost immediately the emancipation thus granted;
sufficient time, however, having generally elapsed to permit of
many actions and contracts necessarily falling under the cogni-
sance of the law. It may be fairly inquired whether the space of
time between the fiat of the physician and the murder or the mar-
riage which may have followed should be regarded as a genuine
lucid interval. But still greater perplexity is introduced into
such an inquiry by an innovation now in operation in Britain,
where, by the Act, patients may leave the shelter of an asylum
on what is called probation or trial, and are entrusted to the
care of relatives or guardians, while their restoration is partial,
doubtful, or precarious for the purpose of ensuring a complete
reestablishment of reason or responsibility. The clause by
which this arrangement is sanctioned was dictated, we feel
assured, by sound and benevolent, and, it may be, by economic
motives; but it is obvious that the competency of the indi-
vidual to discharge the duties of a citizen is left undecided, and

that where the currency of the probation is extended over a
long or indefinite period there may be created ample opportuni-
ties for questioning the validity of the whole conduct of the
person in whose favour such a concession has been granted.
In short, is a patient under probation—in other words, recover-
ing from madness—responsible?   There prevails, we under-
stand, in Scotland, a course, likewise sanctioned by statute,
which is calculated to produce still greater confusion.   When
an inebriate feels that his orgies have been carried beyond the
bounds of prudence and safety, when his mind is darkened,
decaying, tottering on the brink of disease, and even when he
has rushed into the gulf beyond, he may, upon petitioning the
Board of Lunacy, obtain admission into an asylum without any
medical certificate or other legal formula, under the designation
of a voluntary inmate ; and retaining the power—we must call
it perilous privilege—of discharging himself after three days'
notice, whenever his fears have declined, his belief in his own
self-control has resumed its sway, or whenever the regulations
and restrictions of his self-imposed retirement have become
irksome or intolerable.   To certain classes who have not sown
the whirlwind in dissolute or dissipated habits, but who may be
sad or forlorn or fickle, who may shrink from the trials and
turmoil and conflict of social life, or who may feel themselves
incompatible with their own surroundings, such a temporary
refuge may prove most salutary, and may save them from
greater evils, and avert that ruin which they dread ; but the
question must arise, and will be found somewhat difficult of
solution,—are such recluses, whether inebriates or abstainers,
irresponsible during their temporary stay in a lunatic asylum ?
Would a testament executed under such circumstances be ad-
mitted ? Would a homicide be exonerated from punishment ; or
would a marriage entered into by a so-called dipsomaniac, even a
few hours after the removal of his self-elected restraint, be
recognisable in law ?

M. Tardieu holds with other psychologists that epilepsy may
coexist with perfect reason ; but his proposition that the periods
between convulsive attacks, even where alienation has super-
vened, should not be identified with lucid intervals, will not,
except as a verbal distinction, be accepted as containing a sound
principle by many of his fellow-labourers.   In conjunction with
this view he invariably advocates the sound doctrine that phy-
sical conditions and morbid bodily complications should be in-
cluded with the mental phenomena in the consideration of moral
accountability.   Indeed, his invariable illustration of the causes
which interfere with the operation of the moral sense is that of
an external force or agent which dominates an impaired or

perverted will. He advances a step further, and expresses a desire that influences altogether external to or apart from the actor, such as provocation, intimidation, and, we would add, temptation, should be placed in the same rank as those irresistible impulses and constraining passions or intellectual errors which originate in the mind itself. It is argued that this disturbing influence, whether esoteric or exoteric, must be present and detectible at the moment of the commission of the crime and of every insane act where no criminality is involved, but that, in judging of the nature or degree of the mental freedom and lucidity during the transaction, the alienist must embrace the whole course of the disease, whatever may have been its duration, and, we would add, the whole previous character, conduct, and the social and family relations of the person implicated.

In discussing the morbid conditions which he designates "external forces," our author recognises the existence of irresistibility in monomania, partial delirium, and brief paroxysms of insanity, but inconsistently protests against the reality of mania transitoria. It is not very clear why the element of time should be mixed up with an analysis of morbid impulses, seeing that pain, a frequent and important factor in the perpetration of crime as well as in the achievement of noble ends, may be as fugitive as ideas or emotions, and that incessant and successive change is the characteristic of states of consciousness, and, lastly, that continuous irresistibility is scarcely conceivable unless a wider range of mental disease or deterioration be admitted.

In connection with the subject of temporary and of impaired but not abolished volition there naturally falls to be considered the theory of graduated responsibility. The prevalent custom in France in the administration of justice, where there is no plea of alienation, has led to the proposal that exemption from the penal consequences of offences attributable to the minor or less demonstrable forms of derangement might be obtained under the plea of extenuating circumstances. This is nothing more than the substitution of a legal term in general use for what is as yet a physiological assumption, that the capacity for perceiving the calls or obligations of duty and the promptings of conscience differ in different individuals at different times, in different circumstances, and are affected, not only by the amount of cultivation, but by the bodily condition, and even by the most trivial incidents which daily occur. It might be difficult to construct a scale in accordance with this view, but from the accomplishment of such a project must ultimately be evolved any possible reconciliation between legal and medical opinion, between the enactments of law and the conclusions of science. One of the

obstacles which will always interfere with success in effecting this object will be that the insane are often as keenly alive to the respective nature of virtue and vice, to the import of their intentions and conduct, to their liabilities; that they premeditate, take precautions against detection, and in the act alone which places them under the judgment of the court and of the world is to be found a proof, or at all events an indication, of mental disease.

Except in the use of the vague term "non compos mentis," our lawyers have interfered but little with the definition of insanity or with the morbid states which may be comprehended under this term. That the employment of a more ample nomenclature, or rather the recourse to a simple nosological arrangement, has not materially assisted our fellow-labourers in France in the elucidation of the subject, or in dealing with the difficulties which are obtruded at every step in estimating moral freedom, may be gathered from the inclusion of numerous morbid conditions, the existence of many of which is still controverted, under the head of imbecility, fatuity, and fury. Of these conditions that which has provoked most discussion in Britain, which would be repudiated as exculpatory by legists, and which, as yet, finds little favour with either metaphysicians or alienists, is "Instinctive Impulses," which are sudden, irresistible, inexplicable, and transient, which are neither preceded by reflexion nor originate in any intellectual operation ordinarily admitted as sane or insane. The passion, propensity, perversion, thus constitute, while they continue, the whole mind or consciousness and the disease. It is perfectly true that, however unpopular and illogical such a ground of exemption may appear to us, even this form of insanity, and perhaps all others of a similar kind, may be covered by the expressions contained in the opinions of the judges: "If from disease of mind he (the criminal) laboured under such a defect of reason as not to know the nature and quality of the act he was doing, or if he did know it, that he did not know he was doing what was wrong," as in a mind agitated and occupied by intense, exaggerated instincts, the ordinary laws of mind are abrogated and suspended, and there is neither reason to determine, conscience to guide, nor any exercise of will except that merged in the impelling feeling. But as the prefix of "disease of mind" to the words "did not know the nature and quality of the act" seems to imply that the inability to perceive the nature of the act was a consequence of an undefined, antecedent mental state, it is to be apprehended that any pleading or reasoning upon the assumed existence of irresistible impulse would be rejected. And even this, when such a symptom is developed in conjunction with others, and when

there is either intellectual obscuration or perverted sentiments or delusions ; but it might be admitted as strengthening an argument in support of the existence of irresponsibility, yet it is very doubtful whether its appearance in epilepsy, in dipsomania, in imbecility, in the degraded, the eccentric, the hypochondriac, the hysteric, during pregnancy, lactation, &c., would be accepted as demonstrating the insanity, and, therefore, the irresponsibility of the individual ; as appears to be the case in France. However desirable a certain expansion of our own code might be, and however readily the group of conditions now enumerated may be admitted as morbid, such admission must proceed on the supposition that they impair but do not extinguish freedom of action ; that they are extenuations of culpability, but not grounds of exemption from all punishment.

Under mental weakness, as affecting accountability, there fall to be included idiocy in its various grades, dementia in its various stages, deaf-mutism and the confusion, the forgetfulness, and feebleness of the moribund, and of those suffering under fatal or severe bodily ailments. Pathology has sometimes been called in to aid in the determination of the nature and extent of alienation. Where suicide has occurred it can be understood that the absence of all structural disease may be received as evidence of the mental health and of the validity of recent doings of the deceased ; but the untrustworthiness of the results as yet accumulated in this department of science must greatly limit the applicability of such a means of diagnosis, and is well exemplified in the work before us. The following case illustrates this observation :—An aged man having died during the preliminary steps necessary for interdiction, his brain presented, according to three medical reporters, engorgement of the veins of the pia mater, false membrane connected with the arachnoid, serous effusions compressing to a considerable extent the surface of the brain, and, lastly, softening of its substance. Upon these appearances was founded the opinion that there had existed in the deceased grave and long-established mental disease. The validity of his will was consequently disputed and a legal inquiry instituted, in the promotion of which the following interrogatories were addressed to the experts, MM. Parchappe, Baillarger, and Leger, retained by the pursuers.

1st. To what cerebral disease are the structural alterations enumerated to be attributed ?

2nd. What condition must have resulted from such structural changes during the latter years of the deceased ?

The conclusions arrived at were : the appearances noted were, without doubt, pathological ; and although not individually connected with any morbid state, in their nature, and especially

in their totality, they must be regarded as connected with dementia. Many of these, particularly thickening of the membranes and effusion, should be regarded as positively incompatible with the normal functions of the brain, and with the integrity of the intellectual faculties; impairing inevitably mental energy and strength, and even motility. Provided the encephalic disorganisation did produce dementia, its morbid characteristics would consist mainly in interference with the exercise of the intellect and the will, but the precise date of its origination or duration cannot be affirmed, although it is highly probable that it had continued for years, and had become more pronounced towards the close of life.

These inferences were combated with great earnestness by MM. Trousseau, Grisolle, Falret, Follin, and Lasègue, who contended that the attempt to determine the mental condition from changes in the brain detected after death, and the conclusion, from the state of the meninges, that the deceased was capable or incapable of choosing his heirs was unqualified nonsense (" non-sens inqualifiable"), and they further speculated that the premises were neither clearly ascertained nor fairly interpreted, inasmuch as the degeneration depended upon may have been the effects of senile decay, of antecedent inflammation, or even of cadaveric changes. But, while admitting that what was observed may have been either the cause of senile dementia or general paralysis, or the consequences of intellectual confusion or weakness, this difference of opinion involving doubt, they hold that the proof of the existence of any of these morbid conditions is too insignificant and insufficient to invalidate the act under consideration; and they emphatically protest against the innovation thus initiated, and against any judgment formed from other evidence as to the existence of sanity or insanity, except that derived from the words and deportment of the individual during life. M. Tardieu pursues a middle course, and would, in the adoption of an opinion, be guided by anatomical as well as physiological evidence. Yet, when estimating the value of the former, he confesses that such a source of knowledge is rarely appealed to, that it is necessarily fallacious, because in certain cases no lesions whatever are discoverable, because such lesions may be encountered in those who have possessed perfect health until the period of dissolution, and, he might have added, that the same lesions may be associated with widely different and irreconcilable mental phenomena. He suggests the expediency of directing microscopic examinations to all parts and organs of the body, as well as to the nervous system; pays a merited tribute to the researches of M. Voisin, although these have been almost exclusively confined to chronic cases where phy-

sical alterations may legitimately be represented as the consequences of morbid action, and concludes with the truthful though now trite remark, that modern appliances may throw incalculable light upon regions which are now dark and unexplored. It should be here observed that these subjects have received but limited consideration from both moralists and psychologists.

In a very extensive category characterised by delirious conceptions may be classed individuals affected with mania, monomania, lypemania, the mania of suspicion and persecution, the mental consequences of paralysis and states of somnambulism; in all of which the civil and criminal relations of the patient are comparatively palpable. In reference to sleep-walking, and the extraordinary, almost incredible achievements performed under its influence, among others sexual intercourse and impregnation, some difficulty may arise; but if it be identical with or allied to epileptoid trance or *petit mal*, this difficulty will be transferred to the investigation of the bearings of epilepsy upon sanity, which, although involved in obscurity, is less impenetrable than that directed to sleep, sopor, coma. But whatever the psychical relation of this state may eventually be determined to be, it must be recognised as morbid, as a delirium, as an acted dream under the guidance of certain of the external senses, as beyond the range of consciousness and partially independent of the empire of the will. Popularly the analogy between dreams and delirium is well known, but these states have a more intimate alliance, or more strictly speaking they approach each other at a number of points, although they may not come positively into contact. Homicidal attacks have been made at the moment of awakening, in that ill-defined, perhaps indefinable confusion which succeeds sleep abruptly broken and that twilight of the mind conceived to connect sleeping and waking when, as has been intelligibly argued by French observers, the moral nature is but imperfectly enlightened by reason, religion, and prudence. In tragedies of this kind a dream may have become a motive, a premeditation; or, what is more probable, a determination previously formed may be carried into effect during a waking dream or the imperfect consciousness which succeeds. We have known an instance where insanity arose in and from a dream and lasted for years; and it is worthy of inquiry whether the vague and clouded impressions which arise during the prevalence of *petit mal* may not precipitate similar catastrophes. This suggestion becomes still more important when such subjective impressions assume the character of permanent false judgments and fixed ideas and become the powerful, it may be the omnipotent regulators and rulers of conscience and conduct.

Mono-ideaism even in the sane is destructive of that equipoise between the reflective and emotive parts of our nature which constitutes the activity and comprehensiveness of healthy mind. Men of one idea, like those of one talent or of one book, generally display great inordinate energy within their limited sphere, but this restriction, as when the eye is fixed upon a single luminous spot, excludes the influence of other faculties and lessens the general force by exalting the special exercise of an individual state of consciousness. It is not, however, in the fixity or isolation of the intellectual condition, but in its nature, that alienation consists. It is, accordingly, an error or a misnomer to place general paralysis in the same class with monomania, for, in the first place, the current of delusions is continuous as well as turbid, and, although the kind or character of these erratic or ambitious thoughts, desires, pictures, be invariably the same, they are innumerable and multiform; and secondly, because while as an entirety this affection is distinguishable from all others, it presents not merely a phantasmagora of imaginations and impulses, but frequent deviations from a fixed or constant course, complexion or complications. In the first stage, where there may be little more than elevation and joyousness of spirits, some trivial interruption to regularity of habits, some hesitation in speech, serious misdemeanours may be committed which it has been found vain or hazardous to explain or excuse on the pretext of incipient alienation; in the second stage, where rank, riches, Herculean or invulnerable power or immortality are arrogated by a feeble tremulous pauper, the mania of pride may be diagnosed from the extravagant delusions and collateral symptoms, but not from the fixity of what is in reality fluctuating in colouring, phrase and extent; while, in the third stage, the physician has before him an example of profound and stationary dementia still tinged with the brilliant hues of hope. This is, perhaps, the only aspect of confirmed derangement which cannot be successfully imitated; whereas many of the less expansive forms, and those complicated with physical diseases, have been impersonated so faithfully as to deceive observers. It may be due to the histrionic aptitudes and tendencies of our neighbours that such impersonations occur so frequently in France.

An entire chapter has been devoted by M. Tardieu to the history of the mode and detection of such dissimulations. He divides the subject into, first, a description of pretended insanity where measures, sometimes cunningly, sometimes clumsily devised, are resorted to in order to transfer suspicion and blame from the really guilty; secondly, where the offender assumes or endeavours to assume the manifestations of a certain species of

alienation. In the search after truths, or rather in the effort
to trace and expose deceit, it is recommended that the previous
state of health, the occurrence of former attacks of mental
disturbance and the seclusion or other kinds of management
adopted under such circumstances should be inquired into; and
that next the present deportment and the act which has called
for investigation be compared with the known disposition, con-
duct, and general antecedents of the accused. All modifications
of aberration have been simulated, from mania to mutism, but, in
general, violence, fury, agitation, are the means of concealment
attempted. The attitudes, gesticulations, costumes, songs,
cries, vociferations, dances, contortions, laughter, weeping,
vehemence, destructive and degraded habits resorted to, under
the impression that these represent real disease, are generally
over-acted, exaggerated, and so grotesquely combined as to pro-
voke astonishment and mirth rather than conviction. The
sudden transitions likewise from grave to gay, from drivelling
insanity and endurance to impassioned oratory and witty sar-
casm, often raise the curtain and reveal the plot and object of
the drama performed. Such transformations are so rapid and
incongruous as to be readily distinguished from the marked
stages; of excitement which merge into depression in folie cir-
culaire. But, however clever or astute these theatrical extra-
vagances may prove, for they are often reproductions from the
stage; and however much they may perplex and even impose
upon the physician, they cannot stand the test of minute and
prolonged vigilance and experiment. It is impossible to dis-
semble as to insomnia, constipation, affections of sensibility
and motility and other physical diseases. In the prosecution of
such an inquiry the most trustworthy means available are pro-
tracted and repeated observation by experts at all times and
under various circumstances, especially within the walls of an
asylum where the restrictions, the supervision and associations
render the doings and designing of the inmates transparent.
In reaching a judgment upon such cases, three guiding rules
may be kept in view in conducting such moral experiments :
1st. The origin and especially the time of the origin of the
symptoms in the supposed lunatic should be accurately ascer-
tained; but as mental perversion may lurk long unobserved, or
may be precisely coincident with crime, such a rule, if rigidly
applied might prove fallacious and lead to fatal error. 2nd. A
comparison between verbal and occasional incoherence and
shrewd or sensible and dexterous behaviour in ordinary situa-
tions and when alone, must be carefully instituted. 3rd. The
wise and humane caution to the effect that pretended diseases
may become real and permanent, that, under long-continued

deceit and affectation, especially when practised under the insti-
gation of fear, or avarice, or pietism, convulsions may pass
beyond the control of the will and of the original motive and
become morbid and involuntary, the mind and even the muscles
wasting and weakening under incessant strain and stimulation.
We have known these, confessedly simulated for a purpose,
pass into genuine epileptiform seizures, when consciousness was
undoubtedly suspended and where these eventuated in fatuity and
death. Upon no very intelligible grounds, chloroform, cautery,
and all tests which are stigmatised as injurious because they
inflict pain, are protested against in these attempts to detect
imposture and, as a consequence, to promote justice, to vindi-
cate social and individual rights, and it may be to save the life
and fair fame of the innocent.

In such examinations we should be impressed with the fact
that we are not engaged in watching the developments of a
dramatic harlequin, that, although it be dogmatically enun-
ciated that there is no type, no gradation, no connection
between passion and frenzy, that in nature there is; and, again,
that, although it is contended that there is no line of demarca-
tion between disease and dissimulation, there actually is; and
that this line is drawn and traced by signs of morbidity, that
such tracing may be the border-land, the boundary, joining
imperfect health with irresponsible unhealth, and that in this
region are met the difficulties and darkness which obstruct the
decision of the jurist consul.

Lemoine is appealed to as trusting to the psychical lesion to
the dethronement of will and moral freedom, to the disorder of
intellect, to errors of judgment, to the anarchy or tyranny of
the feelings, appetites, and instincts, as constituting the founda-
tion and features of alienation, although these cannot be re-
garded as exhausting the constituents of mental disease; while
Falret presses the proposition that the true diagnostic element
must be sought for in pathology, not in psychology; must be
elaborated from the entire breadth of clinical symptoms, and
not from one, and from the physical as well as the moral deve-
lopment of the patient. It need not be argued here that the
latter views are most consonant with the principles and expe-
rience by which experts are influenced in this country; but it is
apprehended that greater attention and importance are still
attached to intellectual perturbations and metaphysical distinc-
tions than to symptoms attributable mainly or entirely to the
bodily condition. Formal precepts would prove rude or prag-
matical, but it is certainly desirable that in the examination of
transactions in which culpability may be involved, all mor-
bid features which can possibly affect freedom of will should

be scrutinised in detail. It is perhaps natural that mental manifestations should primarily attract notice, that multiplicity, incoherence, fixidity or rapidity of ideas, even when the deductions from these are reasonably logical; that the excitation, perversion, or extinction of sentiments, passions, instincts, and hence impairment of will—that natural as well as artificial language as embodied in the facial expression, gait, and peculiarities of manner—that hallucinations, delusions, abnormal visceral sensations, and delusions; should be tried and tested. But, although in these affections, which, according to Calmeil, form the " basis and food " of partial insanity; it is imperative in any generalisation, that there should be considered the form of the head, the presence of asymmetry, of injuries, wounds, in the same region, the integrity and functions of the skin, the condition of the heart and arteries, anæmia, and vitiated digestion, nutrition ; abolished, exalted, perverted, general, and special sensibility ; sleep, paralysis, convulsions, symptoms which, although deserving observation, must be regarded as the causes rather than the proofs of the existence of alienation. Again, any inquiry of this kind would be imperfect were there omitted the family as well as the personal history of the individual, the hereditary tendencies, the training, the surroundings, temperament, habits of thought, tastes, moral tendencies, and style of writing.

This long catalogue—which has been, however, greatly epitomised from the original—will serve to show how extensive, almost exhaustive, the French procedure is when the mental condition is investigated, especially in reference to responsibility. But a more valuable and convincing illustration of this course is furnished in the fourth part of the volume before us, where copious narratives are given of typical cases where the interference of experts was called for in process of law. These reports derive an additional importance as representing the opinions of a very large majority of French alienists. To those familiar with the pages of the 'Annales Medico-Psychologiques' this department of medical literature will be well known, but to a large proportion of our profession, especially those whose practice lies outside the narrow limits of psychology, it will appear both novel and instructive. As the defects of the work under criticism are verbosity and copiousness, if not logomachy, our selection must be very limited. On a demand for interdiction on the ground of alleged dementia or imbecility, it was affirmed that the patient, a lady of rank and affluence, was unfilial and insubordinate towards her mother, whom she appeared to hate, revolted against the discipline of the conventual school where she was placed, and was removed; was passionate

and peculiar in her home, which she left in order to secure access to a domestic to whom she was attached, and whose discharge led to a mortal quarrel with her parent; gave way to violent paroxysms of rage, during which she destroyed vases, her watch, refused to appear at table, continued correspondence with the servant, struck her mother, menaced vengeance and exposure, consummating her threats by escaping to Paris, where, after eight days' concealment, she was discovered in a lodging provided by the servant formerly mentioned. She was visited there by M. Lasègue by the authority of her mother. He certified that she was of feeble intellect and totally deficient in the moral sense. As a consequence of this opinion she was sent to a private asylum; subsequently, on an application from the family council, interdiction was called for, when, during the interrogatory which followed, the replies of the patient indicated perfect coherence, a consciousness of her position, and no mental weakness or waywardness, although some of the questions were crucial and extremely embarrassing. Permitted to leave the asylum in order to meet her friends, she eloped with a person who had sought her in marriage, and with whom she lived several weeks in Belgium. This incident was followed by the resumption of the medical inquiry, which was conducted by MM. Parchappe, Grisolle, and Tardieu, and which consisted, as in all similar cases, of an instruction, in this instance from the first tribunal of the Department of the Seine, to ascertain, under oath, whether the patient is in an habitual state of dementia or imbecility; secondly, of the perusal of all the documents connected with the case, these being the judgments of the Civil Tribunal on a former occasion, the deliberations of the family council, the previous interrogatory, a number of her letters; and, thirdly, of a further examination of the patient, and medical report founded thereon. Their verdict, very much abbreviated, founded to a great extent upon the explanations of the patient, such as that her dislike and desertion of her mother was the result of harshness and tyranny, of blows inflicted by the servants; that there was no improper intimacy or intercourse with the coachman, &c., was substantially that the patient's intellect reached, but did not exceed, the ordinary standard of capacity; that she understood their questions, and displayed correct notions of moral obligations, of modesty and reserve; that her conduct was far from being irreprehensible or justifiable towards her parent; that her accusations and recriminations had not been free from injustice and exaggeration; that her errors of conduct and character had been deeper and darker than what she had avowed; but that she was justified in defending herself against the imputations of mental disease;

and that there could not be discovered in the various steps of the inquiry proofs of the original or acquired imbecility attributed to her. It must be obvious that this woman's deviations from a reasonable and virtuous course were due either to moral insanity or immoral sanity.

As an example of the intervention of experts in elucidating the mental condition of the moribund there may be selected the following :—1st. After an undescribed illness of twelve hours there were found impeded articulation, want of symmetry between the sides of the face, symptoms so grave as to necessitate a consultation. The patient as examined by three medical men was found to be prostrated, with resolution of the muscles, squinting, deviation of the mouth, tympanitis, distension of bladder, slow irregular respiration although the lungs were healthy ; there were no symptoms of intestinal irritation, no dilation or inequality of the pupils, no anæsthesia or paralysis properly so called, but great feebleness both in motility and sensibility ; and he appeared to be in a state of exhaustion of the intellectual powers. Passive while his case and condition were discussed in his presence, he was roused to utter a few incoherent words and to protrude his tongue, but, except by the rude test of raising his arm, the existence of paralysis was not determined. He replied correctly to questions subsequently put and then relapsed into sopor or unconsciousness. One of the witnesses described his articulation as difficult and interrupted, and that it was aided by signs and gestures. No diagnosis was arrived at, but, in the evening, one of the consultants on questioning him as to whether he suffered, obtained the response "there" while the abdomen was touched ; which was supposed to result from the state of the bladder. After a period of twelve hours catheterism was ineffectually resorted to without disturbing the patient, and puncture of the bladder was discussed. It does not appear that on this occasion any attempt was made by questioning to probe the mental condition. At this stage the religious ceremony of marriage was performed. The medical attendant then proposes the application of a sinapism, but the newly made husband seems to have died an hour or two after. In supplementing the very defective, we think discreditable medical testimony, a notary deposes that to a single inquiry as to his health, on the day previous to death, the patient declared that he was perfectly well. To the clergyman who urged marriage in order to atone for the sin of concubinage, he argued against such a course, in consideration of the claims of his own relatives ; then repeated prayers so volubly that he required to be stopped ; but with the exception of the words " yes" and " no," as answers to interrogations by lawyers and

inclinations of the head while the marriage was proceeding—signs which the priest declined to interpret—he did not further manifest consciousness and was either unable or unwilling to acknowledge the caresses of his daughter. The facts above condensed were derived from inquests before the courts of Tonnerre and Paris, and from the personal examination of the different individuals, medical, clerical, and lay, who had been brought into contact with the deceased during the latter hours of his life. The reporters, Lasègue and Tardieu, were of opinion that the cause of death was to be found in the brain, that the symptoms detailed were those of paralysis, depending, probably, upon congestive, apoplectic, or inflammatory conditions, involving the whole encephalon and in different degrees all its functions ; and that, although no information had been supplied as to the state of the circulation or of the renal secretion, such a termination might be naturally expected in one of a gouty constitution. During the somnolency or coma which accompany such affections the will and intelligence are almost or altogether annihilated, although they may be momentarily excited by external stimulants ; and that such revival is confined to mere movement in acquiescence or in compliance by a gesture; that such was the physiological condition of the deceased, incapacitating him for the performance of voluntary acts, and, therefore, that the act of marriage was null and void. By a more circuitous and laborious process and depending upon evidence of a different sort, the medical concurred essentially with the legal arbitrators. Apart from the scientific deductions, the case may be said to have rested upon the spontaneity of a mere syllable.

V. B. executed a will by dictation of his lawyer during his last illness, the validity of which was disputed. The experts, MM. Tardieu, Blanche, and Baillarger, were instructed to consider whether the deceased was or was not able to see on an evening specified, and whether he was at the same time able to read the writing of the testament drawn up by his lawyer. There were submitted in the inquiry the evidence deposed in different inquests, written statements by medical attendants, the deliverance of the Court of Tarbes in nullifying the will, letters by the testator, his will, and a memorandum as to the events of his last days. It was established that, at different periods previous to his decease, V. B. had been subject to articular rheumatism, stupor, attended with feebleness in the right arm, a tendency to somnolency after taking food ; and, prominently, to congestive attacks requiring the application of leeches, but it was not affirmed that his vision was implicated or that its state prevented him from reading without the aid of a glass a few days before his demise,

Another physician described V. B. as subject to gout, as present-
ing many of the concomitants of that malady, as being diabetic,
as having been seized while playing at cards with some cerebral
disturbance attended with temporary embarrassment of speech,
but did not allude to loss or failure of sight. It was distinctly
proved that he could read medical prescriptions, that he could
write legibly and clearly, that he rarely used an eye-glass, and
only in consequence of the degree of presbyopia to be expected
at his age; up to 15th September, 1863. After this date
V. B.'s caligraphy continued precise and correct up to the
month of October, and if any alteration was detectible, then
or subsequently, it should have been attributed to increased
feebleness, and ultimately to prostration, tremor, and muscular
subsultus. It is noteworthy that the glass in his possession was
of low multiplying power which he never sought to increase and
was used, according to himself, merely to correct the dazzling
effect of minute characters, and in no way corroborated the hypo-
theses of long-continued or signal impairment of vision. A sup-
posed error in counting a sum of money was traceable to mental
obscurity, to that sudden stupor under which he had repeatedly
laboured, and not to imperfect vision which was not required in
the operation. Notwithstanding the adynamic state preceding
his dissolution, he replied with accuracy when addressed, could
take hold of objects presented to him, conversed a long time with
his notary; and there was afforded no proof in his voluntary
actions of confused perceptions or a blunting of his usual capa-
city; and that the probability was he could see with the same dis-
tinctness as previously, especially when the writing was familiar
and the transcript of what he himself had dictated; that he
would be roused to exert his powers to the utmost and that he
might have sought the assistance of the glass he was accustomed
to use. The incidents that he called the attention of the writer
of the will to his signature, and devolved upon him the reading
of the document, were held to indicate, not an inability to read,
but the disinclination of the dying man to exert himself. In
opposition to one of the medical witnesses and to the decision
of the Court of Tarbes, it was concluded that on the day libelled
V. B. could have read the disputed document.

M. Tardieu entertains strong convictions on the efficacy of
handwriting as a guide in such difficulties. The appendix con-
tains specimens of letters produced during different forms of
alienation, and he is disposed to connect particular forms and
styles of character with particular mental states. We confess
that, independently of the thoughts and feelings expressed, and
of such pathognomonic signs as are afforded by the omissions and
misspelling of general paralytics, and of the tremulousness and

uncouth characters of dements and paralytic dements ; we have not been successful in confirming this speculation. It is understood that the " echo-sign " or the repetition or reiteration of certain words in epileptics, which has been so much insisted upon in America, reappears in the writings of that class.

Pretended insanity is illustrated by the history of an individual who used the body of his murdered wife as a mattress upon which he lay, that, when the dried and attenuated corpse was discovered, four months after death, the perpetrator inflicted a slight wound on the throat, twice enacted partial hanging in his cell, articulated in a low and altered voice, but neither spoke nor acted irrationally. On examination in the hospital of St. Louis, where he was placed for surgical treatment, he endeavoured to conceal his complicity in the death of his wife by constructing the romance that they had agreed upon committing suicide simultaneously, that she swallowed laudanum and died, that he failed to accomplish his purpose, and that, in order to avert suspicion falling on himself, he secreted the body where it was found. He had been formerly an inmate of Bicêtre, in consequence of having struck a fellow-workman with a pair of scissors, but was almost immediately discharged. But during his sojourn in the hospital, and at the time of the accusation, he appeared in all respects acute and responsible. The partly decomposed and mummified remains of his victim imparted a different aspect to the transaction which he had attempted to explain ; the temporal bone having been found fractured, apparently by violent blows with a hammer, which could not have been self-inflicted, while the other organs of the body were free from disease. The culprit was declared by the expert to be of perfectly sound mind, both at the time of the murder and subsequently.

Our space will not permit of dealing further with this subject, except by reproducing the titles or marked features of a few of the remaining reports, twenty-six in number.

(*a*) Incendiarism in an ineducable youth of limited intelligence, referred to impulsive instincts.

(*b*) Falsification, forgery, referred to a combination of derangement, duplicity, and imposture.

(*c*) Menaces against royalty, vagabondism, absurd claims and pretensions ; referred to moral perversity and intellectual derangement.

(*d*) Eccentricity, perversion, moral and intellectual, hoarding in a ruinous house, nudity, is sent to Charenton, where, on his own petition, he is twice examined by six experts, who find him insane, and recommend, on the score of his personal safety and

of public morals, his being confined in an asylum or confided to the care of a tutor.

(e) Murder under hallucination, saturnine, solitary, vindictive, displayed in assaults followed by imprisonment; heard insults never uttered—shot offender—choosing place for aim—described deed calmly — defence that he was a reasoning homicidal, monomaniac, impelled by hallucinations; but was condemned, according to our author unjustly, to twenty years' penal servitude.

(f) Nullification of will on demand of relatives, whom it disinherited, on the ground that the testator formerly believed himself to be surrounded by enemies, that he secluded himself for two years, during which he constructed a guillotine with which he affected self-destruction.

'The Contributions to Mental Pathology,' by Dr. Ray, Philadelphia, known to our readers as the author of perhaps the best work on Medical Jurisprudence, published in 1836, has been classed with M. Tardieu's 'Medico-Legal Studies on Insanity,' as it is assimilated in scope to the concluding portion of that work. Many of the trials recorded are reprints from various American periodicals, and are but little known in this country; but they are of paramount interest, and in their collected form must take a high place among the standard authorities in the department with which they are connected. They are the production of a veteran student of psychology, in the best and scientific sense of that term; of one practically and for long years acquainted with the relations between medicine and law as affecting the insane; of a calm, deliberative, judicial mind; of a pure, honorable, upright character; of a philosopher as well as a philanthropist, whose beneficent and successful labours in the cause of truth and humanity have gained for him, by the unanimous acclamation of his fellow-workers, the well-earned and noble title of "the Master."

VI.—Sanitary Work in India.[1]

Our July number for 1875 contained a brief sketch of the progress of sanitary work in India in 1872-73, and of the manifestations of the more important forms of disease met with

[1] 1. *Eleventh Annual Report of the Sanitary Commisioner with the Government of India,* 1874. Calcutta, 1875.

2. *Twelfth Annual Report of the Sanitary Commissioner with the Government of India,* 1875. Calcutta, 1877.

3. *Eleventh Annual Report of the Sanitary Commissioner for the Government of Bombay,* 1874. Bombay, 1875.

4. *Report of the Sanitary Commissioner for Madras,* 1874. Madras, 1875.

during that period. We now propose to place before our
readers an outline of the prominent facts of the two following
years.

The first point which naturally presents itself, and that on
which the weight of all our statistical investigations of disease
depends, is the degree of trustworthiness of the registration of
deaths which has now been in force, to a varying extent in
different localities, for several years. The results at the com-
mencement were, as was to be expected, very unequal and
imperfect; but strenuous efforts have been made to improve the
mode of collecting the facts, and the Sanitary Commissioners
and their provincial subordinates give, in the reports under
consideration, several illustrations of how far these now repre-
sent the actual death rate among the population at large.
Thus, in Bengal proper, where the registration was commenced
several years after the other provinces, with a population of
60 millions, the deaths enumerated in 1875 amounted to 10·01
per 1000 only, while in certain selected areas in that province,
with a population of $2\frac{2}{3}$ millions, in which more care is given
to the subject, the mean ratio was 24·8, but with rates varying
from a minimum of 14 to a maximum of 45 at different points.
In the North-west Provinces, with $30\frac{1}{2}$ millions of population,
the mean ratio was 21·82 per 1000, varying from 13 to 37 in
different districts; in the Punjab, with $17\frac{1}{2}$ millions, the mean
was 25·57, varying between 9 and 42, though, had certain
frontier districts which are less in hand been excluded, the
minimum would have been 18 instead of 9. The Madras
Presidency, with $30\frac{1}{4}$ millions of inhabitants under registration,
had a mean mortality of 21·1, varying from 12 to 43 in different
districts; and that of Bombay, with $16\frac{1}{4}$ millions, had a mean
of 23·15, the extremes being 6 and 32. Great difference of
mortality must be looked for in different parts of a country so
extensive as that from which these details are obtained, not
only from year to year, but even in the same year, under the
varying incidence of the severe epidemics to which they are
liable; but it is obvious, from what we know of death rates
elsewhere, that the lower ratios in the respective provinces just
given must be very much under the truth, and that the regis-
tration in these at least is still far from perfect. In the larger
towns of the Central Provinces, and in the municipalities of the
Punjab, where special attention has now for some time been paid
to the subject, the death rate is very high, being 31·6 per 1000 in
the former, and 43·7 in the latter. With regard to the Punjab,
Dr. De Renzy states, " The returns of this (1875), as well as
of those of previous years, show that a death rate varying from
about 40 to 60 per mille is the normal standard of mortality

in Punjab towns, and in appreciating the importance of these results it is to be remembered that the returns still err on the side of defect, for there can be little doubt that many deaths occur that do not appear on the register." Though a great and valuable commencement has thus been made, it is apparent that it will still require unremitting attention, and possibly much time, before the registration can be rendered tolerably complete; in the meanwhile, as regards certain forms of disease at least, it supplies us from month to month, and from year to year, with approximations to the mortality they cause, which, imperfect as they are, are of far greater value than the loose estimates we had to fall back on before they became available.

As cholera occupied the chief place in the remarks of the various Commissioners in previous reports, so in those forming the basis of this notice it continues to receive the greatest share of their attention, and the varying incidence of the disease during the last two years presents features of the highest interest. By referring to our last notice it will be seen that, in 1872, there was a severe epidemic of cholera throughout Hindustan, but in 1873, while in Bengal proper there was an increase over the previous year, and in British Burma a considerable epidemic, the Punjab, the North-western Provinces, and Oudh, had a large decrease of mortality from cholera from the previous year, and the remainder of Hindustan was unusually free from the disease. Its fluctuations in 1874-75 may be best appreciated from the following table of the deaths caused by it in these years, in which the different provinces are arranged from north to south, as nearly as can be, except British Burma, which, being to the east of the Bay of Bengal, cannot be placed among the others. The numbers for 1873 have been added for comparison.

	1873.	1874.	1875.
Bengal	148	78	6,246
North-western Provinces	15,268	6,396	41,106
Oudh	3,961	68	23,321
Bengal Proper	60,498	56,876	112,276
Central Provinces	344	14	14,643
Berar	...	2	22,465
Bombay Presidency	283	37	47,573
Madras Presidency	840	313	94,547
Ceylon	14	...	1,817
British Burmah	8,109	960	761

Here it is seen at once that, while the mortality in Bengal proper underwent a sensible reduction in 1874, in every other part of Hindustan the disease diminished very much, and in British Burma was scarcely one eighth of what it was in 1873. In 1875, again, there was a very severe and extensively diffused

epidemic, reaching from Ceylon to the Punjab, while in Burma the mortality was smaller even than in 1874. To complete the view of the relations of these epidemics it is necessary to add the following remark by Mr. Cornish in the Madras Report for 1873 :

"While the Peninsula of India has on this occasion escaped invasion, it must not be forgotten that cholera has been noticed in great force in the territories to the east of the Bay of Bengal. In Burma, Tennasserim, Cochin China, the Straits Settlements, and the islands of the Indian Archipelago, the destroyer has reaped a goodly harvest of victims" (par. 384, p. 91).

Thus it appears that in 1873, while the epidemic, which was experienced extensively over Hindostan in 1872, was undergoing rapid diminution in most places, a fresh one became developed over the countries to the east of the Bay of Bengal, extending from the Indian Archipelago and Singapore in the south, to Bengal in the north ; in 1874 this epidemic, so far as our information goes, became much reduced in force over these countries, while in Hindostan, except in Bengal proper, and the adjacent portion of the north-west provinces, cholera as an epidemic may be said to have disappeared. In 1875, again, while to the east of the Bay of Bengal the disease does not seem to have been frequent, it commenced in Ceylon in January, and soon after became active at various points on the mainland, culminating in a very severe epidemic embracing the whole peninsula, with certain exceptions, as far as Lahore. These facts have a most important bearing on our speculations as to the causes of this disease ; they show that, in 1873, factors sufficient to determine its epidemic prevalence were in operation from Singapore to Bengal, while in most of Hindostan and Ceylon they were in abeyance, and, in 1875, these became equally active in the latter countries, while in the former (with the exception of Bengal) their influence had almost ceased. What hope can be entertained of our being able to extend our knowledge of the causes which determine the appearance and diffusion of epidemics if we continue to neglect, as we have done hitherto, the unavoidable inferences from such facts.

The steps by which cholera extended over India in 1875, too, are full of instruction. Hitherto, there have been two prominent theories on this subject, one advocated by Dr. Bryden and the other by Mr. Cornish. The former, following up the idea announced many years ago by Jameson, believes a material miasm is produced in what he has designated the "endemic area," embracing Lower Bengal and some neighbouring districts, which not only produces cholera among those exposed to it there, but portions being carried by the winds

from the endemic area not only cause cholera in other localities, but, if circumstances be favorable at the time, may then also serve as the seed for a fresh growth, part of the new crop exciting the disease in the locality, and part by a similar transfer originating it to other places in advance. In this way mainly, Dr. Bryden believes, the active cause of cholera is transmitted from the endemic area to the north-west by the south-east winds from the Bay of Bengal during the south-west monsoon, and from the valley of the Ganges, across the peninsula to the west coast and to the southward, by the northerly and easterly winds, which prevail during the north-east monsoon. Mr. Cornish, on the other hand, while of opinion that the cholera epidemics of Southern India originate in the endemic area, believes the disease to be communicated from person to person, and attributes their extension to the movements of persons from one part of the country to another, in the usual course of their avocations; and further, he describes the epidemics of Ceylon as being derived from those in the neighbouring districts in the Madras Presidency. The remarkable immunity of Ceylon, and of all Hindustan beyond the endemic area, from cholera in 1874, permitted of the steps which led up to the severe epidemic of 1875 being traced with a clearness unattainable on previous occasions, in consequence of the lingering remnants of preceding outbreaks masking the first indications of those which followed, and the evidence they afford militates against both these theories in several important points, which it is well to particularise.

As already mentioned, cholera was frequent in Bengal proper in 1874; the mortality was greatest in the districts to the east of the Hooghly in the early months of the year; in these it declined in July, and remained low from August to October, but in November and December it became very active again, and remained so to May, 1875. To the west of the Hooghly, and south of the main stream of the Ganges, with little of the disease in the first four months of 1874, the deaths from it were pretty numerous from May to September, but fell in November, and, with the exception of Balasore and Cuttack, did not show any disposition to increase until March, 1875. In the Balasore and Cuttack districts, on the coast, though the deaths were few from November to January, in the former there was a marked increase in February, and in March in the latter, which led up to an epidemic lasting into July. In the Ganjam district of the Madras Presidency, adjoining Pooree and Cuttack, there were, in 1874, 243 deaths from cholera up to September, after which no other was registered until February, 1875, when, and in March, there was one each month. Practically, during 1874

and up to February, 1875, cholera as an epidemic was not met with anywhere on the continent of India to the south or west of a line extending from Ganjam on the coast to Benares, and from thence north to the foot of the Himalayas.

But, although in 1874, cholera as an epidemic was confined to the limits just specified, it appeared sporadically at several places beyond them. Sixty-eight deaths from it are recorded in Oudh, in a population of eleven millions ; and in the portion of the North-west Provinces, extending from Oudh northwards, in a population of fourteen-and-a-half millions, there were 462 deaths, nearly equally distributed over the different months, and spread over fourteen of the eighteen districts embraced in it. In the Punjab, with seventeen-and-a-half millions of people, there were but 78 deaths ascribed to cholera. To the south and east of these provinces the traces of cholera were even much fewer ; in Rajpootana, the Central Provinces, and Berar, occupying the whole country between them and the Bombay Presidency, there were but eight deaths registered as from this disease in 1874. In the Bombay Presidency there were thirty-seven deaths from cholera, of which nineteen were in the city of Bombay from January to October, and fourteen of the remainder were in the districts of Surat and Ahmedabad, to the northward on the Gulf of Cambay. In the Madras Presidency there were 313 deaths registered from cholera in 1874, of which 243 were in Ganjam, as already stated ; of the remainder thirty-three were in the district of Tanjore, nineteen in that of south Arcot in its vicinity ; these were distributed over almost the whole year, February in the former, and January and June in the latter, being the only months in which no death was registered, and twelve deaths occurred in Malabar, on the west coast, from May to August, and another in November. As to the nature of these cases in Southern India, Mr. Cornish remarked in his Report for 1873—

" There is, in South India, an endemic form of cholera which seems to differ from the epidemic variety only in not spreading, and it must be further remembered that the deaths now and then registered as cholera in non-epidemic seasons may really be due to other causes," p. 95.

" In what particular respects the endemic cholera of Tanjore and other localities of the south, differs from the epidemic variety of the disease, is a subject for investigation and report when a sufficient number of facts have been accumulated," p. 95.

And in the Report for 1874 is the following statement :

" Correspondence with the Zillah surgeon Tanjore, on the subject of the cases of cholera which occurred in that district, showed that the disease was sporadic, with no tendency to become epidemic ;

although the symptoms were, in several cases, undistinguishable from those of true epidemic cholera, the majority of them were directly traceable to improper or excessive food ; and in no case was the disease communicated," p. 69.

The above details will enable our readers to realise the amount and distribution of cholera in the Indian Peninsula in 1874, and its manifestations in the endemic area in the early months of 1875 ; with a clear perception of these they will be able to appreciate the full significance of the various stages pursued by the disease in spreading over the country in 1875.

The Madras Report usually contains a detail of the deaths from cholera in Ceylon, but as there is no notice of the disease there in that for 1874, we conclude there could have been very little, if any, in the island that year. In the Report of the Sanitary Commissioner with the Government of India for 1875, there is a return of the deaths from cholera each month in Ceylon that year, from which it appears there were 12 at Colombo in January, increasing to 57 in February, and 194 in March. The disease seems to have been confined to Colombo in January, but in February there were deaths at Negombo to the north and Galle to the south, and at the latter during the next three months these amounted to 111. It was only in June that the Manar district near the northern part of the west coast, which is in common communication with the opposite shore, became affected, and in the same month the disease extended along the central districts of the island as far as Kandy. In July and August there seems to have been but little extension, but in September the disease appeared in a considerable number of fresh localities, and this month only became prevalent in Kandy, though to a moderate degree ; indeed, at no time during the year did the epidemic acquire much force at any point.

In the Madras Presidency it has been shown that sporadic cases of cholera were met with in the Tanjore and South Arcot districts up to December, 1874 ; in the former there were 13 deaths from cholera during January, February, and March, 1875, and in the latter 9 in January and February ; 2 deaths in Ganjam, 5 in Malabar, and one in Trichinopoly adjoining Tanjore, were the only others recorded during these months in the whole Presidency. In April the deaths from cholera in the Tanjore district rose to 65, in May they were 1589, in June 4972, and in July 7184. In the South Arcot and Trichonopoly districts the disease became epidemic in June, in that of Tinnivelly in the extreme south of the peninsula, where there had been an increasing number of sporadic cases since April, the disease became epidemic in July, and the same month Madura, lying between Tanjore and Tinnivelly, where previously there

had been a single death from cholera in May, came under it. In July also the districts of Salem and Coimbatore west of Tanjore, Cuddapah and Nellore considerably to the north, were under the epidemic, while the districts of North Arcot, Chingleput, and Madras town, lying between these and South Arcot, had a few sporadic cases, and only showed epidemic prevalence the following month. In August, too, the Bellary and Kurnool districts north of Cuddapah, the table-land of the Mysore, and the Malabar district on the west coast, were invaded.

From these facts it is clear that cholera became epidemic in Ceylon in 1875, at a time when there were but a few sporadic cases at distant points in Southern India, and consequently the outbreak in the former could not be referred to extension of an epidemic from the latter, as Mr. Cornish supposes, but must have arisen independently. In the Tanjore district, where cholera first became epidemic in the southern part of the peninsula in 1875, there had been, as we have seen, a series of sporadic cases through 1874, the symptoms in several of which were " undistinguishable from those of true epidemic cholera;" these continued in the early months of 1875, and in April became more frequent, assuming epidemic proportions in May, at which time all the rest of the Madras Presidency had no trace of an epidemic. Subsequently the disease spread over the surrounding country, embracing the Bellary and Kurnool districts in August, where it met the epidemic, which by this time had reached the river Kistna from the north. It is obvious, then, that this epidemic arose in the Tanjore district without connection with one coming from the north through the intermediate country, and, after so originating, the disease actually extended to the north. It is possible, therefore, for an epidemic to spring up in the south without continuous connection with the endemic, are in the north; and hereafter, the mere fact of such connection cannot be received as satisfactorily explaining the extension of the disease as a series of factors similar to those which led to the development of the epidemic in the Tanjore district, may have come into operation at each intermediate point successively, and so have caused its appearance there; and, unless this possibility can be excluded, the fact of continuity of manifestation can establish nothing further than that these factors were active over the area under the epidemic. We have long thought that the opinion so common in India, that all the cholera epidemics of Bombay and Madras proceeded from the endemic area, was not satisfactorily established, and it is but due to Mr. Cornish to state that he noticed this some years ago; in Appendix I of his Report for 1871, referring to the epidemic of that year, he says:

" Simultaneously with the disappearance of cholera in the south, there was a distinct movement of the epidemic to the north and west. In this new movement we find the districts of Cuddapah, Nellore, Kistna, and the Nizam's country reinvaded, not by a new emanation travelling from north to south, but apparently by a retrogression of the cholera wave of the south," p. 7.

The progress of cholera in the endemic area at the end of 1874 and in the early part of 1875, has already been mentioned. In the North-west Provinces and Oudh, as far as Allahabad and Banda, cholera became active in March and April; further north, though present, it did not present much activity before June, and in the Punjab a month later. To the south and west of the endemic area the first indication of its extending westward was met with in the Bilaspur district, where in February there were 7 deaths, and in March 77; Raipur, still further west, had 16 deaths this month, and at both it became epidemic in May. In March, 10 deaths occurred from cholera in the Nasick district of the Bombay Presidency, which lies just east of the western Ghauts, in lat. 20° N., and on the line of railway from Bombay, to Allahabad at the junction of the Jumna and the Ganges. In April, the deaths at Nasick rose to 481, and the disease appeared in the Poona and Ahmednagar districts and City of Bombay, to the south of Nasick, and in those of Khandesh to the north, and of Kaira and Panch Mahals, at the head of the Gulf of Cambay. The districts of Sholapur and Satara, south of Poona, became affected in June; those of Kaladgi and Belgaum, still farther south, in July; and Dharwar, which adjoins Bellary in the Madras Presidency, in September. Up to March, when the disease appeared in the Nasick district, there had been only 2 deaths from cholera recorded in the Bombay Presidency in 1875—at Bombay itself, and through the country to the east none was known to exist nearer than Raipur, and the Rewah territory south of Allahabad; none was met with along the line of railway, and, even up to the end of the epidemic, a considerable portion of this on either side of the Jubbulpore ran through country that was scarcely touched by cholera, if it did not wholly escape. In Nasick then, as in Tanjore, cholera sprang up, and soon became epidemic over a large extent of country, a long way from, and without continuous connection with, places where it was already prevailing, thus presenting another instance of the possibility of the disease becoming active and extending widely, without being preceded by its approach more or less measured through the districts intervening between its supposed starting-point and ultimate destination.

The points where the epidemic first appeared, both on the

east and west of Central India, have been mentioned already ; from both it extended towards the centre of the peninsula, and from July to September was active from Najpur to Hyderabad, south of which, as stated above, it was met in the Kurnool and Bellary districts by the extension of the epidemic from the south.   To the north of the Nerbudda the epidemic which had appeared in the Kaira and Panch Mahal districts extended into the neighbouring native states for some distance, but, as in 1872, there was a devious tract of country with little or no cholera, extending from Rajpootana south-east to beyond Jubbulpore, and from that place continued some distance between the rivers Nerbudda and Taptee, interposed between the cholera fields along the Jumna and Ganges to the north-east, and the central provinces, native states, and Bombay Presidency to the south-west. Two other tracts which escaped the epidemic were the Kistna, Godavery, and Vizagapatam districts, extending along the east coast from the Kistna river to Ganjam, and the south and north Kanara districts on the west coast from 12° to 15° N. Though there were 42 deaths from cholera at Karachi in November and December, Sindh generally escaped the epidemic.

There are several subordinate points of interest in connection with cholera on which the experience of 1874-75 is detailed in the reports, but to which our space does not permit us to do more than allude.   We have given the most interesting occurrences during the two years at considerable length, and the facts leave no doubt that epidemics of cholera may arise at various points in Hindustan besides the endemic area, and the distribution of the disease from 1873 renders it important that the causes of the epidemic in India, in years past, should be re-examined in connection with those in neighbouring countries to the greatest extent that trustworthy evidence concerning its progress in these will permit.   Inasmuch, too, as the factors which are necessary to develop a cholera epidemic elsewhere cannot present any material difference from those which lead to the same result in India itself, the obvious conclusions from the facts observed there in 1875 require the most careful consideration of epidemiologists.

In 1873 smallpox had caused a high rate of mortality in the Punjab, North-west Provinces, Oudh, Berar, and the Madras Presidency.   In 1874 the ratio in the North-west Provinces remained high, 3·03 per 1000, while that in Madras was somewhat, and those in the Punjab, Oudh, and Berar, much more reduced, but in the central provinces the ratio rose from 1·37 in 1873 to 2·38 in 1874.   In 1875, with the exception of the Central Provinces, in which the ratio rose to 2·73, and of the Punjab, in which it increased from ·69 in 1874 to ·78, the mor-

tality from the disease underwent a diminution everywhere, reaching very moderate proportions in a country as yet so little protected by vaccination.   Dr. Cunningham states that small-pox was generally more fatal in the towns and districts where innoculation is still practised, and he especially notices the marked exemption of Kumaun and Garhwal in 1875, where vaccination has been carried on with so much success; in Kumaun the deaths from the disease were only 1 in 100,000 persons, and in Garhwal 5 in the same number, whereas in Lalitpur, a district to the south-west of Allahabad, where small-pox was prevailing, the deaths among the same number of the general population were 1612, and in Narsinghpur, in the same vicinity, 1696; taking the children in the latter district by themselves, the deaths from smallpox were in the ratio of 4179 in 100,000. These results, it is true, are for a single year only, and in Kumaun and Garhwal may be modified materially in a subsequent one by the advent of an epidemic, but so far they are in accordance with experience elsewhere, and indicate what may be done by well directed efforts, even with the apathetic and prejudiced population of India.

The vaccination of the general population is under the super-intendence of different authorities in the different provinces; thus in the Madras and Bombay Presidences, in the Central Provinces, Berar, Oudh, and British Burma, it is included among the duties of the Sanitary Department, while in Bengal and Assam it is under the Surgeon-General of the Indian Medical Department, and in the North-west Provinces, and Punjab, there are separate Superintendents-General of Vaccination, who are directly responsible to the local governments. There are returns of vaccination for one year (though not all for the same one), in the above-named provinces of the Bengal Presidency, and also in the Madras Report for 1874 for that presidency; from these we find that in the former the successful vaccinations were on the mean 14·5 per 1000 of population, varying from 2·0 per 1000 in Oudh to 22·3 in Berar, and 27·7 in the Central Provinces; in the latter, as a whole, the successful cases were 11·2 per 1000.   Much has yet to be done before the population can be regarded as fairly protected, but these results show that a decided impression has been made, and as the natives come to see the advantages of the operation, their prejudices against it will gradually give way.

Fever, to which, with its complications and sequelæ, about six tenths of the mortality among the civil population in India is at-tributed in the returns, evidently embraces many other forms of disease which elsewhere, or with persons sufficiently acquainted with their characters, would be separated and classed under other

designations. The numbers given the returns, therefore cannot be viewed as more than approximative; yet a fair estimate of the fluctuations of fever in particular districts may be formed from these when they are borne out by the more precise evidence afforded by the troops or dispensaries, or the observations of the local officials. From these various sources it appears that while in 1873 fever had reached a minimum in most of the provinces of Bengal, and in the other two Presidencies (each taken as a whole), in 1874 there was a sensible reduction in the Punjab, the ratios in the North-west Provinces and Oudh, and in the Bombay and Madras Presidencies, remained much the same; but in the Central Provinces and Berar there was a marked increase; in 1875 the increased rates were maintained in the last two, and the Bombay Presidency, Oudh, and the Punjab, all had very considerable additions to their mortality from this cause. Thus fever, which in the great majority of instances in that country is malarial, undergoes fluctuations which embrace an extensive area at the same time, and which must be due to the influence of some factor or factors of equally extended operation acting in combination with those local factors to which alone the disease is now more commonly attributed.

Enteric fever among the European troops continues to receive attention. In 1874 there were 166 cases returned in the three presidencies, with 84 deaths, and in 1875 the numbers were 243 cases and 101 deaths; in the former year there were cases at 53 out of 100 stations occupied during the period, and in the latter at 43 out of 94. In both years the majority of the attacks were in men under two years in the country. The difficulty of distinguishing this form of fever from remittent, in its early stages, will always leave a doubt as to its nature, unless in fatal cases, in which the diagnosis can be confirmed by finding the characteristic intestinal lesion. The general impression seems to be that the disease is of local origin, and has not been introduced from this country, as was suggested some years ago, but the local conditions under which it arises have not been well defined.

A good deal has been done, if not as yet to ameliorate, at all events to initiate amelioration in the sanitary condition of the towns and villages throughout the various provinces, among which may be classed legislative sanction for improvements in drainage, for better conservancy in towns and villages, and for supplying these with water; also for the prohibition of innoculation of smallpox; while rules for the conservancy of villages, in the vernacular, have been circulated in Bengal, and considerable benefit is reported to have resulted from bringing home to

the people in this way a knowledge of the conditions which promote health and obviate disease. There is no doubt that the present rate of mortality among the native population may be very much reduced by good sanitary arrangements, and the course most likely to affect this desirable result is to bring home to those concerned the necessity for improvement, while suitable enactments enable them to take united action when that may be required.

In the appendix to the Report of the Sanitary Commissioners with the government of India for 1874, there are two reports by Drs. Lewis and Cunningham, one on the so-called " Mycetoma," or fungus disease of India, the other, " On the Soil in its relation to Disease ;" and in the Report for 1875 there are other two by the same gentlemen, " On the Oriental Sore," as observed in India, and " On Leprosy." These are all papers of considerable length, bearing the impress of the care and discrimination which characterises the observations of these authors. It is impossible for us to do more here than mention the results they announce.

As to the Mycetoma, Drs. Lewis and Cunningham were unable to detect, or by cultivation to obtain, the fungus to which it is said this disease is owing, and they attribute the peculiar products found in it to a local degeneration independent of the growth of fungi. The points to which attention was directed in connection with the soil were the moisture, temperature, and varying quantity of carbonic acid in the soil air, at different depths. There are records of the distance of the water from the surface, from February, 1872, to August, 1874, and of the temperature and quantity of carbonic acid from July, 1873, to October, 1874, and for some time similar observations as to the carbonic acid were made at a point about fifty yards from the other, which was found to give results differing very materially as to quantity from those observed at the same time at the first. As to the connection of these conditions with disease, it is admitted the period was too short to allow of any decided opinion being formed, and the necessity for observations of a similar nature being prosecuted elsewhere, to afford a basis for satisfactory conclusions pointed out.

Drs. Lewis and Cunningham's observations on the " Oriental Sore," as met with by them at Delhi, where it is common, has led them to the conclusion that it is due to a growth of granular lymphoid cells, from $\frac{1}{4000}$ to $\frac{1}{3000}$ of an inch in diameter, in a matrix, from which they may be brushed as in ordinary adenoid tissue. This growth is found in intimate relation with the adventitia of the vascular tissues, notably those of the lym-

phatics, of the corium, and the more superficial portions being pushed forward by the continued generation of fresh cells beneath that find their way to the surface through rents in the papillæ, and ultimately form the prominent ulcers characterising the disease. They consider this affection as analogous to lupus, and to be unconnected with scrofula, or syphilis, or the action of parasites; they attribute it to the employment of water largely impregnated with mineral salts, and containing much unoxidised organic matter.

The report on "Leprosy" gives the general distribution of that disease in India, as ascertained during the census operations in 1872, from which it appears there are 99,000 lepers in the British territory, the distribution varying from 1 (or fewer) in 100,000 of the population, in some districts to 21 in the same number in Kumaun. The minutes and records of the Leper Asylum at Almora were carefully examined, and as the outcome of the whole evidence the authors conclude that it contains nothing in favour of contagion being the cause of the disease, while it indicates that hereditary influence is a most important factor in determining its occurrence.

VII.—Picard on Diseases of the Prostate.[1]

THIS is the first of three volumes on diseases of the urinary organs, in preparation by the author, and treats of the affection of the prostate gland; the second volume will be concerning diseases of the urethra; and the third upon those of the bladder.

The work exhibits a good knowledge of the literature of the subject, and though in some parts rather diffuse is carefully and clearly written.

It commences with a clear description of the surgical anatomy of the gland, and of its relations to surrounding parts. The arrangement of the fasciæ, and of the venous plexuses of the part is given with especial care, and is shown to have much surgical importance. Several measurements of the size of the prostate are given, and its structure defined according to the researches of Professor Robin. The second chapter is devoted to physiological and pathological considerations, in the course of which the influence of prostatic diseases upon the bladder, kidneys, and even upon the heart is pointed out, as well as its importance both to the generative and urinary systems. The characters of the prostatic secretion are also given.

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[1] *Traité des Maladies de la Prostate.* Par le Dr. HENRI PICARD, Professeur libre de Pathologie des voies urinaires à l'école pratique. Paris, 1877.

Absence of the prostate occurs in three conditions : 1st. With complete absence of the genital organs; 2nd, in many cases of extroversion of the bladder; 3rd, in a few rare cases the prostate only is wanting.

There may also be a want of union between the two lobes of the gland, in which case the wall of the urethra is only separated from the rectum by a little thin tissue.

Injuries of the prostate form the subject of the fourth chapter, and are divided into contusions, wounds, and false passages. Contusions are not common, and give rise to the ordinary symptom of inflammation. Wounds are chiefly those made by the surgeon, and if extensive or bruised, are sometimes followed by phlebitis, cellulitis, or obliteration of the ejaculatory ducts; but if simple, clean and small, usually heal rapidly.

The next chapter deals with the subject of inflammation and abscess of the prostate, and has a subdivision upon prostatorrhœa.

Inflammation of the prostate, whether acute or chronic, is hardly ever primary, but most often secondary to urethral mischief, and may have its origin in gonorrhœa, the use of irritating injections or of the catheter, the presence of a calculus, the operation of lithotrity, or any other source of urethral irritation.

Exposure to cold, excessive drinking, and chronic constipation are alluded to as aggravating influences. The gland may also be the seat of secondary abscesses, the result of purulent infection. The author points out that inflammation of the prostate is a disease of adults ; it does not occur in children, and when seen in old age is never attended with suppuration.

In acute inflammation the most striking pathological change is the swelling of the gland, which may rapidly go on to abscess. In the chronic form there is less swelling, but the excretory canals are filled and enlarged with altered secretion, or even with pus. When abscess occurs it bursts most often into the urethra, sometimes by several openings, or it may open (and this is the next in order of frequency) into the rectum. More rarely the pus makes its way into the perineum, the ischio-rectal fossa, or even into the bladder. In abscess connected with tubercular disease the prostate becomes excavated into numerous cavities, and the matter is apt slowly to burrow into surrounding tissues.

The symptoms of acute prostatitis are described as frequent and painful micturition, pain in defæcation, tenesmus, a sense of fulness in the perineum, swelling in the rectum, and, perhaps, retention of urine. There is also constitutional disturbance, and if suppuration occur there are rigors. The pain attending the disease increases till the bursting or opening of the abscess.

In the chronic form of the malady the pain is less acute, and occurs most at the end of micturition ; it is said by the author that

the pain is worse in the morning, on account of the congestion of
the pelvic veins, produced by lying in bed. This, however, is by
no means always the case, for the suffering is often greatest at night,
after the movements and exercise of the day, so that in this as in other
respects the symptoms frequently much resemble those of calculus,
as Thompson has pointed out. Picard alludes to the sensation,
which is often present, of there being something in the rectum
which requires to be expelled, so that the patient is tempted
to force the action of the bowels. There is a discharge of pus or
of ropy fluid from the urethra, and seminal emission also may some-
times be provoked by the irritation of the prostate.

The diagnosis of chronic inflammation of the prostate is very
carefully considered; and the author points out that in doubtful
cases much aid may be obtained by the method, practised by M.
Richet, of pressing out the secretion of the prostate by the finger
in the rectum, and by an examination of the fluid thus obtained.

A section is devoted to the so-called "prostatorrhœa," wherein
the author shows that there is no such thing as a true prostatorrhœa;
that is to say, the discharge in inflammation of the prostate is
never composed solely of the secretion of that gland, but is a
mixture of the secretion of the various glands of the part along with
urethral mucus. A description of these secretions is given, and the
diagnosis between this condition and true spermatorrhœa is clearly
described; this latter is believed by the author, as by M. Robin, to
be nearly always consecutive to disease of the spinal cord, the
brain, or the bladder.

The treatment of inflammation of the prostate is elaborately
described, and we think that this is the best part of the work.
Antiphlogistic measures are advocated for the acute stage, and these
may be aided by suppositories and enemata. The author advises
that the administration of opium should be avoided, and we have
ourselves seen an opium suppository greatly aggravate the symptoms.
Belladonna, in the form of suppositories, or as an ointment, applied
to the perineum is especially useful. The occurrence of suppura-
tion may terminate in the opening of an abscess into the rectum, the
urethra, the ischio-rectal fossa, or the perineum. It is the chronic
form of the disease, however, which receives the greatest attention
from the author, and the troublesome nature of the affection quite
justifies the space he has devoted to its treatment. He insists upon
the necessity for keeping the rectum empty, and particularly recom-
mends cold enemata and various local applications. The methods
of using these remedial agents are described, as well as the internal
remedies that have been found most useful.

Periprostatic abscess is next treated of, and an early opening is
advised, to prevent as much as possible the extension of the suppu-
ration.

The seventh chapter is upon ulcers of the prostate; these are divided into inflammatory and diathetic; several varieties of each are described which must often be very difficult to distinguish during life. We think that the author is inclined to take too limited a view of these affections, some of which, the scrofulous for instance, we believe, are rarely if ever confined to the prostatic urethra. He gives, however, a proper value to constitutional treatment, which is often of more avail than any local measures.

Cancer of the prostate is next treated of. The disease is rare, but when it does occur is most often of the encephaloid variety. The curious fact is mentioned that not only are the pelvic glands invaded, but in some cases even lumbar, mesenteric, and inguinal lymphatics. This glandular infection is the most valuable diagnostic sign.

Tubercle in all stages may be found in the prostate; grey granulations, caseous nodules, or similar masses softening and breaking down, with resulting excavations and fistulæ. There is an interesting chapter on prostatic calculi, in which the varieties of these concretions are well described.

The prostate of nearly every adult contains a varying number of small brown concretions, having a nitrogenous nucleus; and so constant is their presence that it is a question whether they are not a natural product. Of more importance are the prostatic calculi, often found in considerable numbers and occupying cysts in the gland.

Senile hypertrophy of the prostate is very fully described by the author, who quotes M. Sappey's researches, showing that an increase in the number and volume of the normal concretions plays an important part in causing this affection. Moreover, the increase in the stroma and the dilatation of the glandular canals of the gland, together with the presence of concretions, gives to many of the cases of enlargement of the prostate more of the character of a senile degeneration than of a true hypertrophy. The author agrees with the opinion originally stated by Sir E. Home, and more recently by M. Mercier and others, that these changes depend chiefly upon a retardation of the circulation through the part—an opinion confirmed by the observations of the last-named writer, which show that prostatic enlargement is most common among those who suffer from varices, hæmorrhoids, and feeble venous circulation.

The section devoted to the treatment of enlarged prostate is less satisfactory than the rest of the work, and no mention is made of supra-pubic puncture of the bladder. We think also that the importance of thoroughly emptying the bladder in cases of enlarged prostate might have been more prominently alluded to. The work closes with a short chapter on atrophy of the prostate.

It has a table of contents, but no general index; the latter would have been a convenient addition to a work of nearly 400 pages.

VIII—Ireland on Idiocy.[1]

THIS is a large book on a subject which is forcing itself gradually and pertinaciously, not only upon the notice of the profession, but also upon that of the legislature. The sympathies of mankind are strongly manifested towards the helpless-ever-childlike, for asylum after asylum is raised and fostered as this great social skeleton of idiocy unshrouds itself; and thus practical benevolence, leading to scientific treatment, is yearly developing in their behalf. And this truly earnest desire to aid the idiot has brought its reward, not only in the increased comforts, better education, and happiness of the class, but also in stimulating research into the causes and treatment of this great incubus on humanity. Most people who may take up one of the few books on idiocy, or who see the word at the head of a magazine or review article, pass on to something more congenial; or if they dip into the matter, soon become uncomfortable at the recital of the defects, and incredulous of the numbers of the most unfortunate of the human family. But a few true philanthropists, and still fewer medical men, have laboured year after year, most unselfishly, in the cause of the idiot, and now the public can know, if it likes, the inevitable results of certain social crimes and follies, and that the number of the idiotic may be diminished by a scientific and philosophic morality and wisdom of life. It is horrible to think of the thousands of idiots amongst us, especially when, as Dr. Ireland remarks, the indisposition of the public to admit the idiocy of children has diminished the census return at least 25 per cent., and still the numbers in 1871 in England and Wales were 29,452. Where are they? If there is one imbecile in every 771 persons, what is being done for them? The answer is most unsatisfactory, for there are not 2000 of these unfortunates under reasonable care and education in the United Kingdom. The rest are in lunatic asylums, workhouses, unions, cottages, and at home, hidden up out of sight, out of mind, a scandal to our legislature, with its imperfect conception of the distinction between the insane and the idiotic, and to our sound sense as a nation. The origin of the idiot asylum was a matter of purely Christian charity; the objects for it were the subjects of a most disinterested philanthropy, and, therefore, a good wholesome sentimentality carried along with it a few of the medical profession. Science was in this instance stimulated by something else than stern logic, and the scientists who have taken up the unsavoury subject have not been without singular and characteristic mental qualifications. One can

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[1] *On Idiocy and Imbecility.* By WILLIAM W. IRELAND, M.D. Edin. Pp. 413. London, 1877.

fancy that a man without a good deal of discursive talent and without literary accomplishments would soon sink down to the level of a drudge or simple money-maker, were he to devote his energies to the treatment of the idiotic; and we may believe that the peculiar mental endowments of the author of the book before us have been of no small assistance to him. Sympathy and a love for a varied literature are, however, not good foundations for exact science, and hence idiocy is still without its great book, its pathology is still obscure, and its treatment a matter of question. Dr. Ireland's work consists of original essays, which have already appeared elsewhere, of compilations, and of some new matter. In his preface he does not lay claim to have made much progress in the study of the treatment, causation, and prophylaxis of idiocy, but considers that the principal merit of the work is, that it brings together the widely-scattered studies of able observers on the subject. He hopes that the book, although mainly intended for medical men, will be more or less useful to those who are interested in idiots and imbeciles. This is a very fair and modest self-criticism, and it is evident that the profession will benefit by the perusal of the book, and that outsiders will find it full of interesting anecdotes, curious information, and not a little useful wisdom. Dr. Ireland considers that " idiocy is mental deficiency, or extreme stupidity, depending upon mal-nutrition or disease of the nervous centres, occurring either before birth or before the evolution of the mental faculties in childhood." " Idiocy bears much resemblance to the ordinary condition of infancy. In idiots the mental state may be said to be fixed in the infantile state, or very slowly to move towards the efficiency and maturity of the motor and reasoning powers which characterise the normal adult." Seguin, in his very wonderful ' Traitement Moral, &c., des Idiots,' 1846—the first and still un-surpassed book—has an amusing chapter on the definitions of idiocy, in which he laughs at the futile endeavours of his predecessors to define this very wide condition, and he wisely declines to commence his work with a hard-and-fast and positive definition, and proceeds to state what idiocy is not. Probably he would have found fault with that just given. The first part of it says nothing about the extraordinary bodily defects which are invariable in some form or other in idiocy, and would reduce the condition to a psychological infirmity. In the second part, the statement of the analogy of idiocy, with a kind of persistent normal early childhood, is certainly not correct, and requires both modification and explanation. Is there a correct resemblance mentally and physically between a small-headed profound idiot of five years' growth and an ordinary baby of one year? Certainly not. Compare the same idiot at one year with a baby of one month's growth, and the intelligence and slight powers of movement and expression of the last are superior to those of the

first. It is some physical defect, some loss of motor or sensual power, some deficiency or increase of sensibility, that, with a lack of the ordinary baby intelligence, first make the mother tremble for the future of her offspring. The value of this kind of comparison has been thus stated by Duncan and Millard : " It is a very striking method of showing the *mental* deficiency of a member of any one of these classes (congenital idiots) to compare its mental gifts with those of children of perfect mind at younger ages." Thus a boy of the fourth (highest) class, aged eighteen years, may not have greater mental power than a perfect child of four years ; he is to all intents and purposes four years old and dull for his age. An idiot of the second class, of eight years of age, may not be more than eighteen months old when compared with perfect children, whilst the gaiety and spontaneousness are wanting." With regard to the first (profound idiots) class these authors state : " Grant to the highest of them all the gifts ever seen in any of its class, and compare them with the powers of a child of the same age, and how striking is the difference (Duncan and Millard's ' Manual of Idiocy,' 1866). It would appear that this comparative guide is all the more fallacious as the idiocy is deeper, so that at last the profound idiot and the normal babe are not comparable.

A good distinction between the demented and the idiotic, almost unnecessary, however, follows on this definition of idiocy ; but here again exception may be taken with some assertions. "The expression of the idiot is generally soft, good-natured, and confiding ; that of the dement is heavy and sullen : past grief and pain have left their deep cross furrows on his brow, and traced broad wrinkles below the eyes ; while the forehead of the idiot remains smooth till a late age." This is not our experience ; and there are cross-looking wrinkle-browed idiots as well as good-natured, soft, simple-looking, glad demented ones.

After separating the idiotic from the demented, and omitting to distinguish between the highest idiots and the lowest of the normal-minded, and really not having satisfied the general reader what an idiot really looks like, Dr. Ireland plunges into the statistics of idiocy. In this important part of his work he has been greatly assisted by the labours of the Charity Organisation Society, and an examination of the figures is well worthy of the consideration of the statesman. There is a curious point with regard to the ratio of idiots or imbeciles to the population in the several divisions of England. Thus, whilst in the northern districts there is 1 idiot in 1028 of normal population, in Yorkshire 1 in 901, North-Western 1 in 833, there is an increase in the Midlands, Eastern Counties, and to the south. Dr. Ireland notices that the Earlswood Asylum and other institutions containing people of this class, being in the south-east divison, raises the ratio there, but he does not explain

what appears at first to be very remarkable. In London the ratio is only 1 in 1708, but really there are four or five thousand imbeciles at Caterham and elsewhere which belong to the metropolis, so that the ratio is of no value. The want of reliable statistics in Scotland is noticed ; and Dr. Ireland believes that there are twice as many idiots and imbeciles amongst that reticent and cautious nationality than appear on the statistics. After considering the interesting French, German, and Swiss statistics, our author analyses those of Norway, and makes the important inquiry, why there should have been an increase in the number of the insane and a decrease in the number of idiots in Norway in the ten years from 1855 to 1865. There is not a chapter upon the influence of locality on idiocy, and it is therefore to be presumed that a very curious Danish work in which the geological nature of the districts and the ratio of the idiot to the rest of the population are treated of, escaped notice.

After noticing the want of value of the American statistics, Dr. Ireland passes to the causes of idiocy, and he considers them under the heads of heredity, consanguine marriages, scrofula, drunkenness, gynagogues, fright to the mother, and he puts in some remarks on idiots born into healthy families, and on the causes of deafness.

There is nothing very new in this part, except the testimony of Alfred Huth and George Darwin upon the unreliable nature of the evidence that is believed by the public, to show the marriage of first cousins to be pernicious. One point relating to this popular belief in the inadvisability of the marriage of first cousins has escaped notice. In the East, consanguineous marriages are and have been matters of course, for hundreds of generations, yet idiocy has not deteriorated those pure races. Moreover, there are cousins and cousins, and the fool of a family is very likely to fall in love with the first girl who is not distant with him, and who does not require the usual polite approaches due to a stranger. The cousin, especially if there be property to keep in the family, is apt to consent too readily, and the marriage certainly is often not productive, or else the children are often characterised by deficient innervation. On the other hand, the results of the marriage of cousins and of that of the offspring of cousins are notoriously not invariably to add to the idiot stock. On the influence of drunkenness Dr. Ireland very properly writes: "Drunkenness generally brings other debasing influences along with it, such as poverty, disgrace, and disappointment, and thus a drunken father may lower the whole tone of health of his family. That alcoholic intoxication has a lowering effect upon the constitution, which may lay the foundation of idiocy, or neurotic diseases bringing idiocy in their train, is probably what none will deny." The increase of idiocy in the State of New York has been referred to certain immoral habits which are classed under the abominable Greek derivative gynagogue. Dr. Ireland leaves

much to the imagination as to what Seguin and gynagogues mean ;
but it may be supposed that 'Fruits of Philosophy,' philtres, Mor-
monism and Free Love are in the back-ground.   One can readily
admit them as proximate and ultimate causes of idiocy.   Fright to
the mother during pregnancy is undoubtedly a cause, although it
has been denied over and over again, but it is certainly very extra-
ordinary that, long after the development of the cranial bones and
general form of the skull and the body, a fright should produce
deformity.   Nevertheless, there is no doubt about the correctness,
and Dr. Ireland's statement that he sees no reason for denying that
such influences (fright and extreme distress) may in some cases
produce idiocy in the child of healthy parents who would otherwise
have been born free from it.

   A presumed cause of idiocy is not noticed in this chapter which
is certainly its proper place, and it will have be sought in the part
of the work which treats of traumatic idiocy.   There the question
of artificial and natural pressure to the skull in early life is ably and
interestingly treated, and Dr. Downs' statistics and views on the
obstetrical aspects of idiocy are fairly considered, and appear to
coincide in the main with Dr. Ireland's experience.   Suspended
animation, prolonged pressure in the maternal passages, especially
in primiparæ, and the inevitable pressure of the forceps, are to a
certain extent factors, and the second especially.   But as hosts of
perfect children and able men have developed out of babes whose
heads have been compressed in the passages and subsequently not a
little squeezed out of shape by the forceps, and whose lungs have
had to be assisted by artificial respiration, one must, even admitting
the natural and unnatural violence to be causes of idiocy, look with
great reason on their having been only adjuvants or assistants ;
or, as Dr. Ireland states, the injuries become the proximate causes
of idiocy.

   With regard to the effects of artificial production of the flat head
by pressure during early life, it may be very safely stated that it has
but a doubtful effect in developing idiots ; and the experience of
the profession is not in favour of the opinion that direct injury to the
skull by accidents often produces idiocy.   The exceptions are more
numerous than the rule, and the bearing of this is ably put by the
author, who writes : " In our study of the physiology of the brain,
the desire to formulate our knowledge is continually checked by
exceptions which occasionally seem to clear the way even of the
vaguest generalization."

   Dr. Ireland remarks that " in some families, indeed, there is much
disease, especially of a scrofulous or tubercular kind, and all or most
of the members are feeble of mind and body, and the frequent con-
nection of idiocy and hereditary neurosis is indisputable." " On the
other hand, it often happens that idiots are born into a family where

the father and mother and the sisters and brothers are apparently quite healthy, and live in obedience to the laws of health." He notices that an idiot more frequently appears in a large healthy family than when the number of children is small; but here, again, there are thousands of large healthy families whose members have no taint of mental decadence or deficiency. It is a matter of experience that the last child of a large family is more frequently the idiot than any of the others, and in one remarkable instance, its appearance was contemporaneous with the evident mental aberration of the father, whose private insanity had been a source of anxiety to the wife for a year before. This question of the occurrence of idiots in really healthy and sober and sedate families, although a most unsatisfactory one and beyond our comprehension, may turn out to be that which will be at the bottom of the solution of all that relates to the *modus operandi* of the causation in idiocy. An unsatisfactory question for the moralist and religionist is this production of sour grapes and thistles from a good vine and well-cultured field, and one can excuse a gentle lady writing in the weariness of her spirit to ask the physician, " Will my idiot child rise from the dead in all its horrible ugliness of body and want of spirit, to live for ever thus ? " Well may the pessimist urge that suffering humanity is a mistake, and difficult is the task to the optimist to reconcile the terrible fact with his philosophy.

The classification of idiots has already been attempted by Dr. Ireland, and his method has not met with much favour. But the subject is comparatively new, and very few careful observers have dealt with it at all; so, if this classification is useful and practical, its truly scientific merit may be, in the present aspect of cerebral pathology and its relations to psychology, considered of secondary importance. It is true, as remarked by the author, that no classification of diseases was ever attempted which was not liable to numerous objections, and that it is the duty of opponents to find a better one, and not to find fault only. This is true and to the point, and therefore it is necessary to look back at what has been done, especially as Dr. Ireland knows of no "tolerable" classification of idiots existing before his own. But before examining one or two of the systems of classification which have appeared, let us consider what a classification of idiocy should relate to. Pathology is the philosophical basis of nosology, but when the pathology is imperfect the classification by symptoms is alone possible, and it becomes scientific because it deals within the limit of exact knowledge. Now, the pathology of the nervous centres implicated in the idiot is in its very infancy, and there are not a score of men in the United Kingdom who are competent to dissect brains and slice, and prepare by recondite methods for long-continued and exhaustive microscopic comparison. The skill of a Lockhart Clarke is required, and such an

amount of time and means that one brain would take months for its critical examination. It will not do to use the unprepared brain, for the enormous amount of cell matter, fatty granules, and connective tissue prevents the true structure of the superficies of the convolutions and roots of the commissures being properly seen.

The same kind of symptoms indicating idiocy may be shown by children who have never been free from them, by others who were healthy and who have suffered from cerebral disease, or who have met with accidents in which disease of the great nervous centre has supervened. Therefore presumably there is the same pathology in all these cases. But is the microscopic structure of certain parts of the brain identical in them all? Do not comparative hardening and too pulpy a condition of brain produce the same symptoms of absence of mental power, spontaneity, conduction, and special limb ailments? The answers to these questions are definite enough to any medical observer who has studied idiocy, and their effect is to antagonise the practicability and value of the philosophic method. Seguin, in 1846, showed that all sorts of diseases more or less mimetic of idiocy were classified by Esquirol as belonging really to the state, and was led to disentangle a true idiocy from others. His true idiot was so from before birth; the affection was congenital and the condition was simulated by non-congenital cases—the product of causes acting after birth. He distinguished backward children, the demented, &c., from those cases where there was a mental and physical defect ingrained in the constitution before birth. Idiocy as a disease occurring during uterine development was clearly separated by him from consecutive or induced diseases, with many of the same physiological symptoms. His classification included profound and superficial idiocy, the one state merging into the other, and both characterised by an original vice of construction or nutrition of the brain. In 1866 Duncan and Millard classified idiots in two great groups; one the congenital, which included Seguin's two groups, and the other, the non-congenital or imbeciles, which contained supervening or induced idiocy, the result of post-partum disease of the brain, epilepsy, hydrocephalus, and injury; and they added a class resulting in adults from a certain vice. They clearly saw that Seguin was right in separating congenital idiocy from conditions where the physiological symptoms were more or less identical, but where the same pathological condition could hardly be. Denying, however, the existence of hard-and-fast lines between their classes composing their groups, they made the following classification: Group congenital idiots—Class 1. True or profound idiots; 2. With slight intelligence, able to stand and walk a little, and capable of some instruction; 3. Able to walk, run, use their fingers, to be made to attend slightly to dreary mechanical work, to feed themselves, memory and perception very weak and variable in power;

4. Feeble-minded. Group non-congenital, or imbeciles—Class 1. Subsequent to birth—diseases of the brain (traumatic included) which have been arrested have left their results ; 2. With permanent diseases of the brain—epilepsy and paralysis; 3. Cases born with hydrocephalus which has stopped ; 4. Cases degenerate from vice. The diagnosis of these classes is given in full with illustrative cases, and it is evident that the first class of the congenital idiots is synonymous with Seguin's, and that the second, third, and fourth classes are identical with his superficial idiocy. These classifications are not "tolerable" according to Dr. Ireland, or rather he does not mention them, and states he has not found a tolerable classification. He quotes Seguin, so must have studied his work, and in one place he pooh poohs the other authors, so it may be assumed that he glanced at their work. Under any circumstances, according to the recognised rules of scientific ethics, prior views have a right to be brought forth, and it must be confessed that if they had in this instance, Dr. Ireland's classification would have been shown to be open to the application of the old saying—What is new is not true, and what is true is not new. Dr. Ireland states that, " coming to the state of idiocy, after having gained some experience in medicine, I have from the beginning viewed it from the standpoint of pathology ; and my idea of idiocy is compounded of the following classes, which are generalised from individual existing idiots who resemble one another by having the same or similar diseases, as they resemble the type of idiocy by having mental deficiency along with a corporeal disease." This is rather difficult of comprehension, but the classification explains itself. The divisions are as follows : Genetous, microcephalic, eclampsic, epileptic, hydrocephalic, paralytic idiocy, cretinism, traumatic idiocy, inflammatory idiocy, idiocy by deprivation—making in all ten forms. It is impossible to separate the first two divisions, and the word genetous is a recondite barbarism. The next two and the fourth may well be combined, and both inflammatory and the accessory group of hypertrophic idiocy may well find a place in some clearly-defined superinduced class of idiocy. The descriptions of the cases, the carefully collected pathological details, some of which are very worthless, the selection of quotations, and the ideas of treatment, are of course excellent, and render a disagreeable subject most readable ; but the impression remains that the classification is very intolerable, and not as good as those already mentioned. Dr. Ireland has a splendid field before him, and the profession will hail a work on the pathology of idiocy from him, and when it is written his candour will probably cause him to alter his old classifications.

Before concluding this part of the review of this interesting book the remark must be made that in many of the idiot asylums of this and other countries the classification has been so lax that the non-

idiotic have been admitted. Some of these intruders have been made the "show pupils," and such an one was the shipmaker at Earlswood; but the desire for science being greater than the philanthropic impulse, it is not probable that this state of things can persist. It may also be remarked beneficially that the general racial characters, both as regards temperament, ability, and constitution, should have some influence on classification, and should influence our estimate of some foreign observations. Thus Dr. Kind's observations about the growth of the Langenhagen idiots is true for them, but not for those in England, unless the exception nearly equals the rule. And when Dr. Ireland states that he never saw an imbecile who appeared naturally malicious, save one, a boy who was also born deaf, and who when at home used to beat his little brothers and sisters unmercifully, and to put skewers into the fire with the intention of running them into his very respectable mother; those of us who have seen a good many, must acknowledge that racial characters are not without their influence on the characters of idiots.

The chapter on insanity in children and insane idiots and imbeciles is interesting, and especially Dr. Ireland's own contributions to the subject. He gives some cases which show how difficult it is to draw hard-and-fast lines between some forms of quasi idiocy and insanity. "We had a boy who was always trying to tear the other's stocking, turning on the gas to let it escape, or attempting to set things on fire; when punished he would howl, but never shed a tear, and the effect soon passed away. I never saw him angry, nor showing any ill will to those who had punished him. He was detected putting stones on the railway line, and had to be removed at the age of fourteen. Though of short stature he was physically strong and unusually energetic for an idiot. He might have been taught to work for his bread, could his tendency to mischief been kept within bounds." This case is one of a not uncommon type, and the grade of idiocy into which it could be admitted is a high one. Such cases are the great troubles of many families, especially when unusual wealth has civilised the unfortunate to a certain extent. The so-called eccentricities and immoral scandals of many who are hedged round by social position and wealth, are frequently the result of this unsatisfactory union of simpletonism, slight idiocy, and an amount of want of self-control, which is insane.

Mania, melancholia, and delusions have been observed by Dr. Ireland in imbeciles, and his illustrative cases are a proof of this assertion. One case of melancholia to which a portrait is attached is true to the life, and pitiful indeed both to the beholders and to the compassionate.

In treating of the sensuous and mental deficiencies of idiots, Dr. Ireland asserts that "the essential deficiency of idiocy consists in

want or hebetude of the intellect, not in imperfection of the senses ; "
and he adds, at the close of his opening sentence, "In idiots the
senses are often more or less defective." To the first statement
there will probably be much objection, and the second is doubtless
correct, unless it be insisted that the word "often" should be omitted,
a suggestion contradictory to the above rather dogmatic assertion,
but certainly consistent with experience. There are idiots that
hear but will not listen, see and do not perceive, touch and do not
feel, and use their organs of taste and smell with results contrary to
those accompanying ordinary and normal enjoyment of those par-
ticular senses. Again, there are idiots mute, incapable of distin-
guishing certain sounds, unable to fix their eyes so as to distinguish,
and either having excessively tender or senseless spots on the surface.
Finally, in the highest of the imbeciles there is a sensuous want
somewhere or other, or some abnormality of sensation. So constant
is the sensuous abnormality that the definition of idiocy is incomplete
without it, and as the intellect owes much to the senses, it is all the
more clouded as they are deficient or perverted in idiots. The
physical defect is not only in the nervous centre, but in the peri-
pheral nerve structure, not only in the dynamic grey substance,
but also in the commissures. Just as perfect elegance and elaborate
combination of normal muscular effort is impossible to the idiot, so
the correct employment of the senses is never seen. Both defects
depend on primary nervous abnormalities, and are as intrinsic in
idiocy as is hebetude and bad memory.

After noticing the authorities relating to the deficiency of the
sense of touch and of general cutaneous insensibility, Dr. Ireland
proceeds to consider visual defects, but he does not enlarge on
that very characteristic local excess of sensibility which did not
escape the acute Seguin and Duncan and Millard. The visual
apparatus of idiots is generally good, in Dr. Ireland's opinion, and
we may presume from his remarks that the function is usually so
also. It is a well-known fact that there is a very remarkable
staring vision in profound idiots, accompanied by insensibility to
strong light, and by incapacity to direct the eyeball on any stimulus
or apparently by volition. In such cases the conjunctiva is remark-
ably insensitive, and, indeed, it is hard to believe that the function-
less eye has a normal structure. Equally unsatisfactory are the
observations on hearing, but he is correct and in accordance with
experience in the description of the nature of taste and smell.

The mental symptoms of idiocy are considered in this book in a
very wide sense, and certainly the thirty pages which relate to them
are most readable, interesting, and amusing. Vast is the learning
displayed, and the following authors are quoted therein :—Aber-
crombie, Esquirol, Aristotle, Averroes, Homer, Wilde, Adam Smith,
Darwin, Itard, Parrish, Tyler, Laplace, Carpenter, Du Chaillu,

Duncan, Millard and Cheadle, Æschylus, Wilbur, Madame de Gas-
parin, Trélat, Dickens, Cardan, Nièpce, Gottfried Mind, Forbes
Winslow, Griesinger, H. G. Atkinson, Dr. Home, Guggenbühl,
and Foderé. The author, as may be imagined, does not contribute
much except by way of criticism to this subject, and hence it is
treated more as a literary or review article. It is a pity that Dr.
Ireland should not have been aware of Seguin's terse pages, or the
resumé of the mental defects given by Duncan and Millard. The
first paragraph or two of Dr. Ireland's notice of the *mental
symptoms* may be taken as typical of the confused manner in
which he treats the subject :—

" The study of the mental symptoms in idiocy is of the first im-
portance. No amount of skill in the diagnosis of pathological
conditions can dispense with the careful analysis of the existing
mental powers. On the other hand, by ascertaining the amount of
intelligence, we can form some notion as to the amount of the
nervous or cerebral lesion. An experienced observer can generally
on a single examination, assisted by parents and friends, gain a pretty
correct estimate of the amount of intelligence possessed by an idiot.
It is more difficult to find out whether he is educable (*sic*) or not."

Great exception will be taken to these statements, for they intro-
duce the preposterous idea that in the present condition of the
knowledge of the state of the brain and spinal cord in idiocy, their
abnormalities can be predicted from the mental symptoms.

Commencing his subject, Dr. Ireland rushes off at once to the
distinction between idiocy and dementia, and he states : " The best
psychical classification of idiocy is that of Esquirol, in which he
takes speech as the criterion." Then Dr. Ireland's classification of
idiots from these mental manifestations is given. The value of the
standard of comparison between idiots and ordinary children at a
given age, and this which has been carefully considered by one or
two authors whom Dr. Ireland persistently neglects to quote, leads
to the first generalisation which relates to the subject on hand.
" Idiocy in its mental manifestations at least may be viewed as a
fixed infantile condition. Idiots remain all their lives children in
intellect ; often so in their feelings and desires." This statement
is wisely qualified as follows :—

" Of course there is always this difference between an idiot and
another child, that though at a given time the potential intellect
of the one is no greater than that of the other, the idiot has the
benefit of a larger experience. Nevertheless, we must have some
scale of comparison, and if we neglect this one we are not likely
to have any other."

The value of this standard has already been noticed, and the
more experience one has with idiots and normal young children the
greater objections arise to its employment. The early development

of movement, hearing, and of taste, with a decided muscular action in normal children, is then considered, and we are told that "there is no sense so variable in its development as speech." Then the deficiencies of the idiot are further considered :—

"In idiots this evolution of the senses is sometimes much slower; some can scarcely be brought to suck at all, though others do so without difficulty. The child does not notice things, smile, or stretch out its hands to grasp them like other infants. Idiots of the lowest class seem to have nothing more than the passive intellect (have they even this?); the optical apparatus of the eye are perfect; but if the creature apprehends the sensation of light, it does not use it as a perception, or perhaps it only notices a sudden flash of sunshine, or the difference between day and night, or perhaps objects swim before his eyes like the waves of the sea, so confused and unnoticed that he can scarcely be said to see at all." "As regards the perception of sensation, the idiot may be somewhat in the condition of a man half asleep, or heavy with extreme fatigue, or on the verge of fainting, or deeply intoxicated. The sluggishness of idiocy may be occasionally owing to all efforts of attention beeing painful, so that impressions are allowed to wander through the mind without any attempt being made to fix or exercise them."

This notion can hardly be accepted, for it assumes a normal amount of receptive power of the brain. Dr. Parrish is then quoted to show the want of volition, although this defect has been noticed by the earlier writers, and Dr. Ireland gives an instance of astonishing strength of the will. The author then considers the slow and tentative manner in which the use of the "muscular apparatus" is gained, and very properly, although one does not see what it has to do with the mental defects, states that, "in general, idiots or imbecile children are awkward in their motions, and slow in learning to walk." "Idiots in general have a bad balance." "In the lower grades of idiocy the grasp is often very deficient." Why? Nothing can be more satisfactory than the answer given by Seguin, and enlarged upon by Duncan and Millard. It is not a mental defect, but it has to do with that want of co-ordinating muscular power which is as peculiar to idiocy as is mental deficiency, and which has doubtless a physical origin in an abnormality of the commissural tracts.

Dr. Ireland does not appear to grasp this lack of co-ordination as a symptom of idiocy, and does not explain or enlarge on the automatic or mechanical movements so evident in idiots.

The difficulty of receiving the idea of numbers, by idiots is very properly considered in relation to abstract matters of form and colour; and Dr. Ireland remarks, with regard to memory, that sometimes the teacher in an idiot school finds that what he has taught his pupil is totally forgotten, fading gradually or suddenly away out of its mind. "This is most common in epileptic idiocy

after renewed fits; but occasionally it is noticed in other forms of
idiocy, without an accompanying symptom." It may thus happen
that the same lesson has to be taught three or four times over."
The credulity, unsettled purposes, weak judgment, and feeble power
of comparison of the idiots are very striking, and when some educa-
tion has been given the result is not always, or indeed ever satisfac-
tory. Dr. Ireland states : "Often after having accumulated a
considerable store of acquired knowledge the original childishness
remains, sometimes brought into ludicrous relief by the increased
power of display given by education." The opponents of the
"payment by results" system, and of the existing mania for exa-
minations, will be delighted by the following statement of our author:
"I have seen individuals who had sufficient mental power to pass
college examinations, take degrees, and even gain prizes, who were
so manifestly unfit to conduct themselves in the ordinary affairs of
life that they were a laughing-stock to the most ignorant people
around them." Clothe one of these feeble-minded in black, get
him to hold his tongue, invest him with wealth and a family name, and
there is no telling where he may not go to in the social scale ; but
place such an one in a class where he has to compete with his
fellows, to hold his own in life, and to suffer from the results of
incompetency, and he will soon find his level, and say, Blessed are
the rich, for they inherit the earth. The odd wit of idiots is
amusingly illustrated by Dr. Ireland : " B— D— spoke in a very
stolid manner, never trying to be humorous, but almost everything
he said had something droll in it. Once when he was in hospital
the doctor said to the nurse, " Give him a pill and a half." He said,
"Oh, just mak it twae and a half, doctor." Another time he called
to the nurse, "Oh ! Mary, give me more water to wash my mouth,
and I will give you it back when I am done with it." He used to
say, "Alexander is my right name, Sandy is my left." An imbecile
boy from the north had still something of the shrewdness and
caution of canny Aberdeen. I once said to him, " Jamie, why do
you not offer me a bit of your cake?" " Because you would tak
(take) it," was the laconic answer. The temper, disposition, and
the influence of puberty are slightly noticed, and there are some
interesting notices of idiots possessing wonderful gifts. The chapter
on the best methods of educating idiots and imbeciles commences
with the history of the subject. St. Vincent de Paul heads the list
of truly benevolent men who have sacrificed much wealth and
comfort for the idiots. Dr. Ireland notices that the first training-
school in England was founded at Bath, in 1846, by Miss White.
She had four idiot children under her. The institution at Highgate
was founded in 1847, and this was destined to lead to the present
development of idiot asylums. Dr. Ireland should know that
Mr. W. Millard, whose description of the first attempt at Highgate

is so well worth knowing, did the hard and practical work on which
resulted Essex Hall and Earlswood. He carefully says nothing
about Essex Hall, and indeed little about Earlswood, yet we are
under the impression that some scientific besides philanthropic work
has been done in both places. Essex Hall was founded in 1850,
and Earlswood arose out of it. There is not much new valuable
information on the methods of education, and indeed the author
writes: "In writing on the training of idiots we cannot be expected
to enter into the whole subject of education, which the instructor
must learn from another source." He does not mention the source.
He criticises and suggests considerably, and therefore fails to give
that definite instruction which is required at the hands of one who
has such abundant opportunities for original work.

The results of the treatment and training of idiots and imbeciles
are principally treated by quotation from what Dr. Ireland calls un-
impeachable sources. No statistics are given, nor are any of his
own conclusions, but there is a sentimental speech from a late
commissioner in lunacy, forming not a bad but well-deserved
advertisement for the institution at Larbert, and there is also an
extract from a paper by Dr. Bucknill, F.R.S., which deals in the
vaguest generalities, and ends with the assertion, which we contradict
most emphatically, that "the helpless imbecile may sometimes be
educated up to the point which renders it possible to introduce him
into the social life of our age as an independent and efficient man."
There is also an extract from Dr. Home, and also from one of the
reports for the New York Asylum for Idiots, but neither gives any
statistics; and finally, Dr. Ireland mentions the resolutions of the
Special Committee on Idiots, London, 1867, which is, of course, to
the point, but still is only a general statement. Statistics are of
the greatest importance in this and on all other subjects connected
with the insane, for there is a shrewd suspicion afloat that the
satisfactory results are not invariably confined to the patients. The
latter part of this book, indeed, no less than sixty pages of the
whole, is devoted to the laws for idiots, and a treatise on wolf-boys.
The absurdity of making the same law for the lunatic and idiot is
now admitted, and Dr. Ireland notices the grievous hardships many
idiots had to undergo by imprisonment in lunatic asylums. He is
not quite correct in his statement about the different attempts
which were made to get a change in the law by the friends of the
imbecile. He thinks that two bills were actually prepared, one by
the late Mr. Lutwich, and another by the Earl of Devon. But
no bill has ever come to light from Mr. Lutwich's pigeon-holes,
and it was a mere draft, and is not in existence; and Mr. William
Millard drew up certain suggestions for Earl Devon, and these never
came into any bill. It is not necessary to consider the wolf-boy
stories, and they probably will be omitted in the next edition. In

reading this really very interesting and instructive work everybody will pay a tribute to Dr. Ireland's literary powers; but the student of the low types of humanity will long for that light which the author can give if he likes. We believe, with Seguin, that there is a speciality of idiocy, a condition imitated by the results of brain disease, but in the abstract a real affection of the nervous centres and peripheric prolongations. What is required are careful dissections and microscopic analyses after preparation by Lockhart Clarke's plan, in relation to well-recorded cases of true idiocy. The amount of time and trouble required in doing this is great, but the position held by the medical superintendents of large asylums should enable them to devote some hours a day to science. The skill in preparation is not difficult of attainment, but reading nature requires much experience, a quality very necessary to avoid mistaking normal structures for abnormal. Until such researches come from the great asylums of the country, idiocy will still remain a blot on medical science.

IX.—Tait on Diseases of Women.[1]

THE chief object of the author, he tells us, has been to offer the results of his own experience in as condensed a form as possible. More than one third of the book is occupied by the author's Hastings prize essay on diseases of the ovary, which was published some four years ago, and now appears in a somewhat amplified form. This has been already fully noticed in our columns, and we do not propose to offer any further observations upon it now. It was an essay which unquestionably added much to the author's reputation, and embodied much conscientious microscopical and practical experience.

The arrangement of the work is hardly that which will commend itself to the reader; it is too artificial. The female generative organs are so intimately associated with their function—the one with the other—that it is difficult to treat separately of diseases of the mons veneris, vulva, vagina, uterus, broad ligaments, fallopian tubes, ovaries, and pelvic bones. In fact, the classification is not altogether satisfactory, according to the author's own admission. Thus, on page 171, under the heading "Broad Ligaments," he states, "Most of the diseases of these structures either have been discussed in connection with the uterus and ovaries, or will be when I speak of diseases of the fallopian tubes." Again, on p. 114, perimetritis is described under the head of ovaritis; and little more need here be said about it, &c.

[1] *Diseases of Women.* By LAWSON TAIT, F.R.C.S., Surgeon to the Birmingham Hospital for Women. London, 1877.

On the other hand, there are some advantages in thus dealing seriatim with the various disorders. We scarcely recognise the necessity of separating the mons veneris from the vulva, the diseases of the one often extending to the other, the division appearing somewhat arbitrary. The various eruptions and parasites, inflammations and ulcerations, abscesses, tumours, and malformations, are severally discussed, and many important pathological distinctions given. The directions for treatment are not as complete as could have been desired. Thus, in speaking of warts, the author merely states, "there is no treatment of them so rapid, safe, and satisfactory, as removal by scissors." Nothing is said of the advantages of keeping the opposed surfaces separate and dry by means of desiccating powders, tannic acid, alum, oxide of zinc, calomel, &c. In speaking of mucous tubercles, the author believes they are not primary sores, but is quite certain they may be the source of primary infection in the other sex; in fact, he is satisfied that the great majority of men who suffer from syphilis are infected by these soft sores; whilst men, on the contrary, convey the disease from hard sores.

Œdema of the vulva he regards as pathognomonic of vaginitis from infection, the condition not being present in simple catarrh.

Congenital cheilosyncleisis is the most appropriate distinctive title the author can manufacture for cellular union between the nymphæ.

Some valuable original remarks are given of a peculiar degenerative and atrophic change occurring in the nymphæ at or after the climacteric period; very often, but by no means always, associated with vascular caruncle of the urethra, causing much misery and a great deal of the climacteric drunkenness too common among women. Spots, varying in colour from a palish brick-red to a bright purple, transitory and spreading, extend serpiginously on the inner surface of the labiæ. During its progress the vestibule of the vagina slowly contracts until frequently it is almost obliterated.

Great relief is obtained, though only temporary, by the application of strong carbolic acid to the red spots.

In speaking of vascular growth of the urethra the author tells us "the only remedy is removal by scissors." This statement few will accede to who have had much to do with these troublesome growths. The galvano-caustic is a most valuable and effective method of destroying these growths without incurring the risk of hæmorrhage. The application of chromic, nitric, or carbolic acid is often sufficient to arrest their growth and prevent the necessity of resorting to any more serious operations.

We commend to the notice of practitioners generally the advice given that "the rectum should be emptied by a small enema every

morning for at least three weeks after the operation for ruptured perinæum," and that "the stitches should not be removed until after the twelfth or fourteenth day."

How frequently is the success of the operation marred by re-moving the stitches too soon, and confining the bowels for the first week or ten days by means of opium, and then trusting to some ignorant nurse to administer an enema to relieve the bowels.

Any one who has tried the two methods will soon find the immense gain to the patient's comfort in having the bowels regu-larly relieved; and if only proper care be taken the practitioner will not have to regret any non-closing of the rent, or irritation of the passage of the fæces. On the contrary, the part will be far healthier and much more likely to unite firmly than if irritated and unduly distended by the unnatural accumulation that is still frequently allowed to take place.

We doubt whether the author is justified in making the remark that in chronic granular inflammation of the inner mucous surface of the os—the so-called ulceration of the womb—" a simple astrin-gent lotion of sulphate of zinc or of alum will, in the majority of cases, speedily cure it." Patients very frequently present them-selves with manifest symptoms of this chronic cervico-endome-tritis, and state that they have been under treatment for it for months past, it may be even years, employing astringent lotions and taking internal remedies. On inquiry they tell you they have never been examined; it was not considered requisite. If now an examination be made, the actual condition of the cervix detected, and some escharotic such as the nitric acid, nitrate of silver, chromic or carbolic acid be properly applied at ap-propriate intervals, more progress will be made in a few weeks than has taken place in months before, and a complete cure will ordinarily be effected within three months at the most, lotions being meanwhile persevered with. Under ordinary circumstances the os uteri is more or less closed, and lotions as usually in-jected do not gain access to the cervical canal, which in these cases is the seat of the disease, the granular condition of the os itself being but an external indication of the state of the cervical canal as well.

The clinical part of the book is not as satisfactorily dealt with as the pathological. The treatment given is as a rule vague and sketchy. Thus in uterine cancer we are told how to arrest the hæmorrhage, but there is no word telling how best to assuage the pain which is generally the symptom most urgently demanding relief.

In speaking of subinvolution, the influence of bromide of potas-sium and ergot in expediting the process of involution are justly insisted on, and the evil influence of iron referred to.

The remarks on endometritis membranacea, which term the

author employs in place of membranous dysmenorrhœa, do not altogether agree with the views generally entertained at the present time. He believes that it is an adventitious structure, the result of an inflammatory action, and not, as usually considered, an exfoliation of the entire mucous membrane of the body of the uterus—a true menstrual decidua thrown off at each catamenial period. Microscopic research seems to confirm this latter view, the cast being found to consist of the living membrane of the uterus, hypertrophied in all its elements, almost exactly as it is in pregnancy.

Mr. Tait tells us he has never seen the membrane passed by a virgin, though he does not assert that every case of membranous endometritis, where the mucous surface of the uterus is shed, must necessarily involve unchastity in an unmarried patient. He regards it as a menstrual miscarriage, to which he has given the name of "fruitless pregnancy," believing it to be caused by an absence of complete fertility in one or other parent, most probably on the part of the male; and that the so-called hydatidiform mole is merely a variety of it.

Perimetritis and parametritis he regards as useful terms in expressing the difference between inflammatory action in the peritoneal investment of the uterus and inflammatory action in the cellular tissue in the neighbourhood of the uterus, and therefore retains them, though the latter term is not strictly classical.

Pelvic hæmatocele affords the author an opportunity for some practical remarks. The relative frequency of intra- and extra-peritoneal hæmatocele is yet a matter of discussion; and, if we take only post-mortem records into account, we can understand those who hold with Bernutz that the intra-peritoneal is the more common, and that extra-peritoneal hæmatocele is very rare. Nevertheless, from his own clinical experience he believes that the extra-peritoneal is probably ten or twelve times more common than the other, and also that it is very seldom fatal.

He thinks that all cases of intra-peritoneal hæmatocele should be left alone, save under the most exceptional circumstances, and should be treated on general principles only; for suppose that the peritoneum be tapped, or opened from the vagina, is it likely either that the clots can be removed or the hæmorrhage arrested? And there is the immense risk of setting up peritonitis where it did not exist, or of aggravating it if already threatened.

Thus far we quite agree with the author; but "if the case be one of tubal pregnancy the doubt of a favorable issue to the operation for the arrest of hæmorrhage is, in my opinion, so great as to place it out of the question," is an axiom we are unwilling to admit. If the history of the case be clear, and the symptoms so marked that an error of diagnosis is hardly possible, we should consider

ourselves perfectly justified in opening the abdomen and attempting to secure the bleeding vessels and to obviate the otherwise inevitable tendency to death from continuous internal hæmorrhage.

As to uterine flexions and versions, Mr. Tait cannot find that any more complete and satisfactory account of them has been given since the appearance of Simpson's clinical lectures. The reader will seek in vain for any "satisfactory account" of these conditions here. "Simpson's original ring pessary will be found far more generally applicable than any other," is about all Mr. Tait has to tell us. We quite agree that, "in all cases where it is possible, intra-uterine stems should be avoided, for they are always sources of anxiety, and sometimes of danger." Still it would have been well to have entered somewhat more in detail into the various forms of instruments least objectionable and most useful in cases of flexion attended with dysmenorrhœa, sterility, &c., that often prove of great value in the hands of experienced gynæcologists.

The terms fibroid, fibrous, and fibroma, as applied to uterine tumours, are, the author believes, so completely erroneous that they should be banished from pathological nomenclature. He has examined a large number of solid uterine tumours, and has never met with one in which simple fibrous tissue played any but a very subservient part, unless in extremely exceptional cases. The chief constituent of the tumour is fusiform muscular fibre. The most common natural ending to the growth of uterine myomata is the arrest at the menopause, a process Mr. Tait has attempted to imitate by the removal of the ovaries, but hitherto without success. He considers it, however, an operation which, in suitable cases, will prove a more successful plan than removal of the uterus. The suitable cases are those of women still distant from the menopause, and in whom the hæmorrhage is menorrhagic.

The other natural terminations of myomata are cited, and indications for treatment deduced therefrom. Here the author can speak with authority, for he has directed much attention to the subject, and his remarks are suggestive and very valuable to any intending operator, though the young practitioner would doubtless like to have seen the circumstantial details more fully given.

The question of extra-uterine pregnancy is very briefly considered. He maintains that every case is tubal in its origin, and that it may become intra-peritoneal or extra-peritoneal, just as the tube happens to burst, and that none of the cases of ovarian pregnancy will stand the test of criticism. In not a single instance which he has seen, nor in any of which he has found on record, has the pregnancy been anywhere but in the tube. Yet the late Dr. Parry, in his recent valuable contribution to the literature of this subject, after much careful research, states : "The weight of authority is in favour of the possibility of ovarian pregnancy."

We are at a loss to comprehend what place to assign to Mr. Tait's volume. It is certainly not a student's book ; it contains but one illustration; it makes no attempt to enter upon the symptomatology of the subject ; the indications for treatment are the vaguest possible in many instances, and altogether insufficient. The young practitioner, if he attempt to take it for his guide, will be sadly disappointed with the meagre details of many important subjects. And yet there is something in it for every one to learn—many useful pathological researches, records of individual experience, hints for further investigation, and suggestions as to treatment. Though we can hardly call it a complete or exhaustive treatise, it is more a contribution of personal experience upon subjects connected with gynæcology, for which the author deserves our thanks.

X.—Magnan on Alcoholism.[1]

THE profession and the public must be well-nigh weary of the discussion as to the use of alcohol as a food and a medicine, and the searcher after truth may be ready to despair of ever attaining to it in the presence of conflicting statements and of opposite but equally positive inferences drawn from observation. The work before us happily does not take up the debateable ground, but is occupied solely with the one aspect of alcohol as a morbific agent, and seeks to make clear what are the changes in structure and function caused by its introduction into the system, and how to relieve or cure them.

This being its object, we concur with the translator, Dr. Greenfield, that the treatise fills up a gap in medical literature. It is, moreover, the production of a physician having ample experience of the subject he writes about; for its author, Dr. Magnan, is one of the two physicians attached to the Bureau d'Admission of the department of the Seine, in Paris, to which are brought all cases of delirium and mental disturbance caused by drink or otherwise which fall into the hands of the police. It is, indeed, a place only for temporary lodgment, but, in fact, the whole treatment called for by a large number of cases of delirium tremens and simple alcoholic delirium from the lower and middle classes is there carried out and completed. This institution has, besides, an out-door department, where those who are suffering from the chronic results of alcoholism, in the

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[1] *On Alcoholism : the various forms of Alcoholic Delirium and their Treatment.* By Dr. V. MAGNAN, translated by W. S. GREENFIELD, M.D. London, 1876.

shape of nervous disorders, and especially of epilepsy, come under treatment.

The volume opens up with a physiological disquisition on the effects of alcohol exhibited experimentally in animals, and of the comparative effects of simple alcoholic drinks and of absinthe in man and some of the lower animals ; and it well brings into view the distinctive and specially destructive properties of absinthe, which has unhappily become so popular as a liqueur in France.

It will be enough to point out that the action of absinthe is especially upon the medullary and cervical regions of the cord, whilst that of alcohol is on the lower portions of the cord. Hence we find the former productive of giddiness with confusion of thought, disturbances of intellect, and convulsions ; whilst the latter, although productive of mental disorder in the form of illusions or hallucinations, becomes so at a later period and after longer abuse than in the case of absinthe, and, instead of convulsions and epileptic symptoms, is rather productive of tremor and of paraplegia. M. Magnan rightly objects to the loose manner in which the term delirium tremens is applied, and attributes to this circumstance the cause of the discrepancy of statistics of the curability and of the mortality of the disorder. On his part he deals with the delirium produced by alcohol under two heads, viz. alcoholic delirium, and febrile delirium tremens.

The delirium of alcohol is marked by hallucinations, seldom otherwise than painful or annoying, but at the same time very changeable, and apt to be associated in their character with the previous employments and incidents in life of the sufferers. Magnan distinguishes maniacal, melancholic, and " stupid " varieties of alcoholic madness, of which the first is the most frequent in occurrence. Further, according to the type is the character of the actions aroused by the mental excitement. At the same time, cases of a mixed kind are common, and transitions occur altering the psychological manifestations.

The intellectual aberration usually commences at night, and presently extends to the day, and when recovery supervenes recedes in the reverse order. The eyes and ears are much oftener the seat of illusions and hallucinations than are the other senses.

The author classes the victims of alcoholic delirium in three groups, according to the clinical data afforded by the antecedents of the patient and the course and termination of the disease. These groups are as follows :—*a*. Patients with easy, complete and rapid convalescence. *b*. Patients with slow convalescence and tendency to relapse. *c*. Predisposed patients

affected with alcoholic delirium with frequent relapses. In other words, patients may have slight attacks and promptly throw them off entirely; or may have more lingering seizures, in which the intellect does not so readily clear up; or lastly, may inherit, or may produce by dissipation, a weak brain, incapable of furnishing them with moral power or control, easily acted upon by alcohol and slow to recover from its disordering influence, even when possibly the dose taken is comparatively small and its physical manifestations of slight intensity.

This classification is of little value practically; it is but an after-construction, when the cases have run their course and betrayed their historical antecedents; and though it may at times, as in the instance of patients belonging to the third group, influence prognosis, it affords no guide to treatment.

Magnan admits the truth of the general impression that drunkenness is one of the principal causes of suicide, but the statistics collected by himself respecting this point do not bear out those put forward by some other writers with regard to the frequency of this cause. He says—

" Our figures are less than those given by other authorities for two reasons : we have cut off the list of homicides and suicides all accidents (and they are numerous) arising in a casual manner. For instance, a person affected in this way by alcohol imagines he is pursued and jumps through a window, thinking it is the door ; this is an accident, and not an attempt at suicide. Another sees in front of him a phantom, an armed man ; he seizes a chair, strikes and injures his child which is lying near him ; this, again, is an accident, and not an attempt at homicide. Furthermore, we have only mentioned cases in which we possess positive information. This statistical result should, therefore, be considered a minimum " (p. 62).

The next section is occupied with a clinical history of the action of absinthe on man. One of the first noticeable peculiarities of this liqueur is the rapidity with which it developes hallucinations, accompanied by attacks of fright and anger, of outcries and agitation. They appear before the combined alcohol has had time to excite motor disturbances, and with them are also associated, in the earlier and slighter stages, muscular shocks and vertigo, and in the later and more complete stages of intoxication epileptic seizures.

Respecting epileptic attacks in alcoholic delirium the author remarks :—

" The attack of epilepsy is not, as many physicians think, the highest expression of the disturbance of motor functions in alcoholics, it is not the most extreme degree of that general tremulousness which is seen in *delirium tremens*, but it is a symptom of a different order, which is superadded to the other motor phenomena.

To be convinced of this it is only necessary to note the circumstances under which the attacks are produced. Sometimes they surprise the patients at the onset, sometimes at the decline, of the alcoholic delirium, when the trembling is but slightly marked; at other times, on the contrary, they show themselves at the middle of the attack, when the trembling is at its maximum; but whatever be the time of appearance of the attack, the trembling remains the same after as it was before. It subsequently diminishes if the attack of alcoholic delirium is on the decline, whilst, on the contrary, it increases if the fit has come on at its commencement....It is not then a question of trembling, but rather of attacks preceding the trembling, and which, moreover, themselves disappear. There is then no correlation between the attack and the degree of trembling" (p. 75).

Magnan farther on observes—

" When fits of epilepsy come on in the course of violent attacks of *delirium tremens*, one finds in the antecedents of the case the abuse of absinthe, so that we may say in a general way, 'no fits, no absinthe,' with the exception, however, of some rare cases in which the patients have abused white wine, bitters, or vermouth."

In fact, this poison possesses the peculiar property of stimulating, in a fixed, definite manner, the excito-motor functions of the cord and medulla, without the intervention of determination of blood to the brain, supposed by some to play the principal part in the production of convulsive seizures. Moreover,the cerebral hemispheres take no part in the production of the convulsive attacks, which, under the influence of absinthe, occur with the same characters in animals previously deprived of the lobes of the cerebrum.

In his introductory remarks on febrile delirium tremens the author guards his readers against supposing that the differential diagnosis between it and simple alcoholic delirium is to be found in the intensity of the delirium; for if this be commonly more pronounced in febrile delirium it is not necessarily so, and may be equalled in simple delirium. The first and most important distinctive sign is the presence of fever, not accounted for by the existence of concurrent disease:

" If the temperature taken in the rectum, after having oscillated two or three days about 102°, rises to 104° or 105°, the prognosis will be grave, and its gravity will augment with the increase or even the persistence of this elevation of temperature. When, on the other hand, after a somewhat rapid elevation to 102° or 103°, one sees the temperature go down at the end of twenty-four or forty-eight hours, the prognosis ceases to be unfavorable, and soon all the symptoms taken together enable us to foresee a favorable result. In the case of acute alcoholism of mild form the mercury

may rise to 101° or 101·5,° in consequence of the agitation, extreme at times, of the alcoholic, but this limit is rarely exceeded, and in all cases, as soon as the patient has a moment's respite, the column rapidly descends, and oscillates about 100·4°" (p. 105).

The pulse does not follow the course of the temperature, and furnishes no precise information. But, besides the fever, another important sign are the disorders of movement, with respect to which we have to distinguish intensity from activity, and to note their duration, persistence, and degree of generalisation, for on these particulars must greatly depend our prognosis.

Wide-extended but transient trembling need not alarm, but if the tremors, or rather quiverings and shocks, affect all the muscles of the body, even though not intense, and if they persist during sleep, nervous exhaustion may be looked for about the second or third day. We must not trust to what we observe in the face, or to simple inspection of the surface, which, indeed, is often moist, and does not to the hand betray the febrile heat, but must carefully feel the muscles to appreciate the irregular contractions, and to estimate the extent of the irritative progress going on in the spinal cord. A third, though less constant prognostic sign, is to be found in the enfeebling or actual paralysis of the extremities, mostly of greater extent in the lower limbs.

From what has been already noted of the pathology of simple alcoholic delirium and febrile delirium tremens a differential diagnosis may be formed. Magnan goes a step further, and describes the differential diagnosis of febrile delirium tremens and delirium tremens associated with intercurrent diseases and with injuries, but the description is too long for quotation. It remains for us to notice the third principal division of his subject, viz. chronic alcoholism. The account he gives of this diseased condition is very complete and clear.

In the chronic alcoholic we have a greater or less transformation of the individual in respect of his mental powers and of his bodily organs. Permanent changes, by long-continued indulgence, have been wrought in the nutrition of his organs, involving fatty degeneration and local active morbid changes and irritations, witnessed by sclerotic alterations. "And according to the predominance of steatosis or of sclerosis in the nervous centres do we see the chronic alcoholic progressing towards dementia (steatosis and atheroma) or towards general paralysis (diffuse interstitial sclerosis)." Likewise in the liver and glands generally, the change towards cirrhosis or fatty degeneration is set up.

From the tendency or mode of termination of chronic alco-

holism Magnan distinguishes three varieties, viz. cases tending to dementia, cases terminating in general paralysis, and cases accompanied by hemiplegia and hemi-anæsthesia of common sensation and of the senses.

The psychical phenomena of progressive dementia need no description; but besides the cerebral we have likewise distinct spinal symptoms; nevertheless the two sets of phenomena in relation to each other follow no regular order of development nor of intensity. The spinal disturbances may dominate over and precede the intellectnal, or, on the contrary, be less pronounced. The two will not advance *pari passu,* and their relative preponderance may from time to time vary in the same individual. The alterations noticed in the cord at one time indicate diffuse myelitis, at another fasciculated sclerosis and usually more or less diffuse sclerosis, at another degenerative changes due to fatty deposit or to atheroma. In like manner in the encephalon similar morbid processes pursue their course and affect more or less of its substance and membranes, giving rise to characteristic symptoms. Thus it happens that when alcoholism passes, as is the rule, gradually into general paralysis, there is superadded to the fatty and atheromatous degeneration a diffuse interstitial sclerosis of the brain matter. Withal, the sclerotic change is not confined to the nervous tissue, but invades that of other organs, particularly of the liver and kidneys.

"The most frequent mode (writes Magnan) of termination of chronic alcoholism is dementia, which is often associated with partial paralysis; this condition answers to fatty degeneration accompanied by atrophy of the cortical layer, together with more or less numerous foci of hæmorrhage or softening, generally scattered in the form of lacunar spaces in the optico-striate centres and in the white matter, or at the periphery of the encephalon, all of these being lesions analogous to those of senile dementia, and having their starting-point in the changes in the vascular system, that is to say, the atheromatous patches and sometimes miliary aneurisms of the arteries. These anatomical lesions, varying in degree and distributed in an irregular manner throughout the vessels of the encephalon, may lead to the most diverse localised affections, some of them of sudden occurrence and rapid course, others, on the contrary, taking place in a slow and progressive manner, or by successive steps. Hence arise numerous symptomatic manifestations, variously combined, and often confused with each other, both as regards the intellectual phenomena and those of a physical nature, whether motor or sensory."

Of such complex results the hemi-anæsthetic termination of chronic alcoholism affords an example—marked at the same time by paralysis of one side of the body and diminution or

total loss of the general sensibility and of the special senses. The anæsthesia is not confined to the surface, but extends to the deeper parts of the paralysed region, and is likewise accompanied by loss of the muscular sense. Even the sensibility of the mucous surfaces, such as the conjunctiva, the nostrils, and mouth, does not escape, and the special senses fall a prey to morbid change, becoming weakened in every degree up to entire destruction of function.

It would be instructive and interesting to follow the author in his analysis of the symptoms of the hemi-anæsthetic state in question, to note the attendant psychical phenomena, referable to dementia, and to examine the relation of the features generally of the malady with the pathological changes discoverable after death. The examination he has instituted, although illustrating the great advance of late years in nerve-pathology, and the value of pathological research in unfolding physiological truth, yet shows how wide still is the field of research open to observers. But we must here content ourselves, in conclusion, by briefly referring to the final chapter of this valuable work, on "the combination of alcoholism with various mental conditions, and its association with intercurrent diseases."

Under this heading Magnan adverts to the action of alcohol in cases where some disease is pre-existent, pointing out the modifications it produces in the symptomatology and course of the primary affection. He first devotes two or three pages to dipsomania, regarding it as a distinct condition from alcoholism, as being "a peculiar form of instinctive monomania, having its source most frequently in heredity. Alcoholism, on the contrary, is a simple poisoning which appears in the same manner in all persons, and, we may add, in animals as in man." The article, however, on this subject adds nothing to our knowledge, and is only of value on account of the two cases quoted in illustration. The same may be said of the short essay on alcoholism and general paralysis, and, in fine, the whole chapter under consideration must be held to be superficial and incomplete. It is but fair, indeed, to say that it is introduced with an apology, as only an outline of a wide subject which did not precisely fall within the compass of the work the author had taken in hand. Yet it is to be regretted that so competent a writer was unable to deal with the matter in the way it deserved. At the same time, we may well be grateful to him for the amount of information he has conveyed in his valuable work, of which we have endeavoured to give a brief outline—one, at least, of its pathological teachings. For the therapeutics of alcoholism in its different forms, and for the illustrations appended to the description of each form in the shape of cases narrated, we must refer

our readers to the work itself, which will well repay their attentive study.

In conclusion, we must thank Dr. Greenfield for introducing this able treatise by translation to English readers.   He has well executed his part as translator, with which and the writing a preface he has been content.

XI.—Granville on Care and Cure of the Insane.

THIS work is the most voluminous of all books on the subject it handles published in this country.   It possesses peculiar merits and peculiar defects.   The latter originate in the circumstances under which it was called into existence.   As the title-page informs us, it is a reprint of the " Reports of the ' Lancet' Commission " on the lunatic asylums for Middlesex, the City of London, and Surrey.   As a report it presents a sketch of the general structure, the administration, the arrangements, and the treatment pursued in each asylum, visited in turn.   The several notices consequently are detached and fragmentary, and require collation to enable the reader, who would get general views regarding the care and treatment of the insane in English asylums, to arrive at the desired result.

As a matter of course, a reporter on the asylums inspected had not only to recognise excellencies, but also to spy out defects ; in other words, he was perforce critical.   And it must be admitted that he was a most competent and intelligent critic.   Moreover, from all that appears he viewed matters as an outsider, as one officially unconnected with asylums and not committed to the stereotyped notions and usages which cling so tenaciously to asylum officers.   At the same time he proves himself to be sufficiently instructed in matters touching the care and cure of the insane to apprehend the end and aim of the asylum system ; to grasp and sufficiently to appreciate the means employed, whether administrative, moral, or medical ; to recognise their weakness and deficiencies, and generally to point out a more excellent way.   But this critical department, again, has the same inherent weakness as the descriptive, and from the same cause.   The criticisms, the suggestive notes, and recommendations recur from time to time, and useless repetitions of sentiments and opinions as a consequence encumber the pages of the treatise.

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[1] *The Care and Cure of the Insane ; being the Reports of the ' Lancet' Commission on Lunatic Asylums, &c.*   By J. MORTIMER GRANVILLE, M.D.   In two volumes.   London, 1877.

These defects in the arrangement and construction, therefore, being, as before intimated, due to the mode in which the tale he has to unfold had to be placed before the public, small blame attaches to the author for their existence. But in his desire to make his original notices more complete he has very copiously added to them severally historical and critical matter, with the necessary result of heaping up fragmentary observations and multiplying repetitions, when, otherwise, he might have left those notices in the shape they appeared in the 'Lancet,' and have grouped together, under appropriate headings and in systematic order, the conclusions he had arrived at and the opinions and principles he had to enunciate, expanding them as he deemed needful in a subsequent portion of his work. In fact, he has done the one thing and not left the other undone; an action somewhat of the supererogatory sort. For, after the copious additions to his original notes and reflexions, he has produced in the second volume a retrospect and a large chapter of "notes," bearing on all the topics which pertain to asylum administration and the care of the insane, and which, in the detached notices of the several asylums visited, he has more or less already amply dealt with. What we urge as preferable would have been a volume of notes on asylums, with current remarks, and a second volume, setting forth the writer's general impressions and conclusions regarding the existing aspect of the care and cure of the insane, together with a systematic discussion of the principles he advocates for the better provision and treatment of lunatics, embracing all the points now debated in different disconnected portions of the present treatise.

With respect to the merely descriptive notes which appeared in the pages of the 'Lancet,' their immediate value is but ephemeral. They exhibit the institutions as the reporter found them, and they will doubtless be interesting to students of a future time diligent in learning the state of asylums in the latter portion of the nineteenth century. On the other hand, the writer's own impressions and opinions of what he saw should be read with interest by the present generation of asylum officers and rulers, particularly as they come, as already intimated, from an outside and presumably unprejudiced observer.

We have enlarged sufficiently on the defects of these volumes; it is a much more agreeable task to refer to their excellencies, and it is one we can, in this case, perform with much confidence. The good sense and intelligence of Dr. Granville make him a good reviewer of the conditions and circumstances submitted to his scrutiny. He is decided in his views, and not sparing of his criticism; at the same time he is no reckless fault-finder, but seeks to make sure of the basis for his remarks,

The appendix of communications from superintendents of asylums indicates how seldom he has misrepresented, or rather misinterpreted, what he saw or heard.

His convictions and his principles concerning the care and cure of the insane are most fully set forth in the supplementary observations appended to the notices originally published in the ' Lancet,' in the " General Remarks on Public Asylums Visited," at the close of the first volume, and in the concluding retrospect and chapter of " Notes " in the second volume.

At the very outset of his work, on the seventh page, in introducing the account of the new Surrey County Asylum at Brookwood, the great problem of the day touching the public provision for the insane forces itself upon him. The story of Brookwood points the moral of the mistake, repeated with sad uniformity throughout the country, of trying to overtake the demands for accommodation for the insane by erecting additional huge new asylums, and filling them forthwith by a population of incurables, drafted from all the workhouses around and from private houses receiving insane paupers ; a proceeding whereby recent and curable cases become excluded altogether from the possible benefits of the asylums, or else more or less lost sight of as far as curative treatment is concerned, amidst the crowd of hopeless incurables among whom their lot is forthwith cast.

Dr. Granville thus very truly and plainly puts the matter :

" How best to secure four objects essential to the result and interdependent :

" 1. The existence of a highly curative establishment, replete with every convenience, furnished with all necessary or desirable appliances, and so organized as to afford the greatest facility for the prompt, rapid, and effectual treatment of insanity in its various forms and under divers conflicting conditions.

" 2. That every case of mental disease falling under the control of the public authority, and chargeable to the rates, shall be placed at the earliest moment, without being delayed or intercepted by any other machinery, in the institution already described.

" 3. That no case proving incurable, or passing into a condition which renders active treatment hopeless or unnecessary, shall be allowed to occupy space or waste power in the curative establishment.

" 4. That the asylum or hospital in which ' cures ' are to be effected shall be able to accommodate the whole of the recent, or curable, cases in the district it is designed to protect."

These principles or objects to be kept in view and acted upon are repeated in the author's comments, with more or less expansion, in connection with the history of several asylums visited, and further recur in the latter sections of the second

volume, where he discusses the "asylum question." They are principles, we must add, which recommend themselves to common sense, and have been laid down by not a few writers on asylums and the insane. In one or more papers in this Review they have been earnestly advocated; and we may indulge in the hope that ere long they will take hold of the public mind, and lead to a reversal of the present mischievous policy, upheld, as it mainly is, by the inertia of routine.

The author's conception of the needs of the insane and the purposes of treatment is most fully unfolded in the additional remarks attached to the account of the Brookwood Asylum. He lays it down as an axiom, that the insane are not only irresponsible but also incapable of self-control. This definition is open to the objection that it is too wide. The whole of the so-called moral treatment proceeds on the assumption that the insane are, in varying degrees, capable of self-control, under the stimulation of external influences and conditions, or of circumstances overmastering or displacing the dominant and enslaving powers or motives which have possessed them. We will not dispute the statement that "every true lunatic is a slave to some infatuation;" but we cannot endorse the author's conclusion that it is "vain to hope to achieve his freedom by moral violence;" for otherwise, "moral" treatment could find no place for recognition, no scope for usefulness. It is an act of "moral violence" to shut up a lunatic in an asylum, to cut him off from his relatives and surroundings, and to subject him to the control of strangers and a host of conditions foreign to those he has been accustomed to; and we know that such an act, almost it may be *per se*, will often be attended by the happy rescue of the slave from his infatuations.

At the same time we quite coincide with Dr. Granville's views generally as to the principle of placing lunatics on the footing of children, with a view to their *re-education;* placing them under "conditions and surroundings as nearly as may be like those of the life they are afterwards to live," "with the circumstances of sane life, and then with carefulness and watchfulness, by personal example, counsel, and such measures of enforcement as are adopted in the training of a judiciously managed family of children, instructing them in the avoidance of danger, the self-restraint of wrong propensities, and the intelligent choice of good and evil ways and courses of conduct in preference to those which are bad and must end in moral and physical disaster."

This teaching is right enough as a protest against making a "madhouse like a prison in its internal arrangements, and against proceeding on the assumption that lunatics are to be

jealously kept out of reach of any possible instrument and ap-
paratus which may prove a source of danger, instead of being
taught and in a gentle way compelled to apply these imple-
ments of domestic life to their proper and ordinary purposes."
Yet we contend, notwithstanding, that relegation to an
asylum is something more than a change of residence, and it is
that something which makes the act one of " moral violence,"
and a successful factor in loosening the insane individual from
the fetters of delusions which have fastened themselves upon
him. Indeed, the histories of recoveries from insanity abound
in examples of " moral violence," of strong mental impressions,
and even of the accessions of bodily sickness, operating as
curative agents.

It appears to us that, in propounding the principles of treat-
ment quoted as of universal application, the author has erred
by reason of keeping too prominently in his mind the more
chronic examples of lunacy, and losing sight of the recent
forms. Just as with acute bodily maladies at large, so with
uncomplicated acute insanity, it has a natural tendency to re-
covery; and "physiological rest" is as important an element
of treatment in mental as in corporeal lesion. An acute
attack of madness, although very materially influenced by the
surroundings, offers limited scope for the re-education por-
trayed by Dr. Granville; it is too much of an explosive con-
dition to be dealt with by counsel and the sort of training
requisite to the judicious management of a family; it calls for
control, for discipline, for conditions of constraint, if not of
restraint, and for calmatives—for " physiological rest." It
demands something more than the surroundings of every-
day life, and in every well-constructed and well-administered
asylum it finds that something.

We are happy to endorse Dr. Granville's view as to the in-
expediency and folly of endeavouring to keep every instrument
or implement, or constructive detail, capable of mischievous or
hurtful employment, out of the way of patients. Fully to
achieve the result is an impossibility; to aim at it as far as
possible, a mistake. Our forefathers failed signally in the
management of the insane by opposing morbid activity by
mechanical appliances, and by making their patients close
prisoners, with the view to keep them from harming them-
selves or others. If our modern superintendents have advanced
on the road towards freeing madmen from mechanical restraint
and sore bondage, they have halted on their onward march, as
it were, to make sure their position, and have expended their
energies in small devices contrived to outdo the morbid inge-
nuity and the proclivity of their patients to disorder and

mischief; and further, to secure quiet and order, they have devised a system of rigid routine, pervading the entire administration of asylums, and sooner or later bringing all unruly members within its paralysing grasp. Indeed, it happens unfortunately that both administrators and those ministered to alike become victims to its injurious influence. It usurps the place of treatment, and leaves the duly " classified " patient a mere member of a group subordinated to the same general scheme of management.

With Dr. Granville we can distinctly aver that " uniformity is the too dominant idea ; " the individuality of patients is sacrificed to it, and therewith also the individual treatment essential—pre-eminently so in curable cases. And we concur with this physician's opinion that it is probable the mental treatment of insanity will make no " real progress, or even assert its intrinsic claims to confidence, until psychologists begin to recognise the full scope of its intention. The aim should be to deal directly and specifically with the individual mind organism, its idiosyncrasies and sources of weakness, those defects which left the citadel of the intellect an easy prey to the enemy. . . . Moral treatment must be individual. It is no more possible to propagate principles of self-control in a multitude of minds dealt with *en bloc* than to make sincere believers by prohibitory or compulsory religious ordinances."

Real progress in the curative treatment of the insane, history and experience tell us has not been effected, notwithstanding the immense advancement made in some directions, the enormous expenditure incurred on behalf of the insane during the last fifty years, and the vigorous efforts made to improve their condition. Our best attempts to do good have been paralysed by some imperfection in the mode in which our reforms have been carried on. That this is so Dr. Granville's statistical inquiries respecting the rate of cure in former and latter years clearly demonstrate. This demonstration, moreover, is only confirmatory of a general conviction among medical men, gathered by observation and by research in the past history of insanity.

The author remarks—" The circumstance I should like to see discussed by writers on insanity, and, if possible, explained by medical practitioners among the insane, is, that there has not only been no substantial improvement in the results obtained since the abolition of restraints, but what amounts to a positive diminution of the proportion of cures."

Towards an explanation of these discouraging results Dr. Granville arrives, as the general result of his inquiry, at two inferences—" First, that the existing method of dealing with lunatics is chiefly notable for its negative advantages. It is

free from the objections which public opinion urges, on grounds
of humanity, against the restraint system, but it has few posi-
tive excellences of its own, and of those few scarcely one is
remedial.   The second inference is, that the method of provid-
ing for the insane in asylums at present pursued is faulty.   It
can never overtake the need for accommodation . . . and it
deprives recent cases of the full advantages of treatment.   It
is, therefore, opposed to the dictates of true economy, and
injurious to the welfare of the insane."

By collating the opinions and remarks of the author, scattered
up and down in the two volumes of his work, we arrive at a
pretty clear notion of his apprehension of the defects of the
prevalent plans obtaining in asylums, of the impediments to
curative treatment, and of the reforms demanded to place the
care and cure of the insane on a proper footing.

In the first place, he holds the colossal asylums of the present
day to be a mistake.   The error is increased a hundredfold by
bringing together all varieties of insanity, acute and chronic,
within the same building and under the same medical and
general management.   For such a mixed community, for the
purposes of management, an attempted classification of its
members must be made; but the sort of classification commonly
seen is not a classification for cure, but for convenience.   It is,
as the author calls it, " a system of shelving, by which work
that cannot be done in detail, because the task is too vast, is, in
a fashion, done in the mass.   It would be more candid to confess
that it is not done at all.   .   .   .   Classification such as this,
suggested by convenience, is the bane of asylum practice, and
one of the greatest obstacles to progress."

In accord with this kind of classification is the whole system
of routine and uniformity so elaborately organized as an essen-
tial element in the government of large masses of people, though
at the same time undoubtedly inimical to curative treatment.
We have this routine in the uniform dress of patients, and
often also of attendants ; in the prescribed and rigid dietary ;
in the fixing of hours for all the requirements of life,—for
knocking patients up in the morning and sending them to bed
at night; for turning them out for exercise, &c.; in limiting their
exercise to confined courts and leaving them therein to disport
themselves as they please, satisfied only that the courts are
safe against escapes, and that their accessories do not bristle
with dangers.   The patients by all this lose their individuality,
and are dealt with as units of a group possessing some features
in common which render their grouping a convenience.

To quote again Dr. Granville's very pertinent remarks,
" There is a radical fault at the base of the asylum system,

and everything is made to conform to it.　Uniformity is the too dominant idea.　It is impossible to deal with minds in the mass.　A body of men may be trained to act together like machinery, but the training and results will be simply physical. In an institution designed for the cure of mental disease, where minds are to be recovered from derangement, and from which it is hoped they may be sent back to the world, the individuality of patients should, on no account, be sacrificed to uniformity.　Individual treatment, personal obligations to self-control, variety in surrounding objects, diversity of clothing, of occupation, as far as possible even of food, are essential, and ought to form component parts of the governing idea."

As an accessary to routine in dealing with masses of persons, not a few of whom are liable to injure themselves or others, we meet with the principle of minimizing danger, as carried out in a multitude of constructional and mechanical details, and upon which far too much pains have been expended by asylum officers.　It would be, indeed, folly to lose sight of the principle, but harm arises from its exaggeration, and whenever and wherever intelligent oversight and control, watching and tending, can effect the purpose it more rudely seeks to serve.

To quote Dr. Granville's own words, " Frankly, I do not believe in minimizing the pains and trouble required of those in personal charge of the insane.　By multiplying the mechanical appliances and regulations devised to lessen the dependence on personal care, the sense of personal responsibility is diminished. . . . . . Further, it is not cheap to piece out meagre personal service."　Continuing with the author's catalogue of defects, we come next to a most important one, viz. the filling of asylums with chronic and incurable cases, among which the recent and curable ones are so mingled that they lose the benefit of appropriate care and treatment, and very largely drift into a chronic state of insanity, adding to the already existing accumulation.

Routine having usurped the place of rational and intelligent treatment, it is no wonder that the medical superintendent of a large asylum is made small account of, that his services are looked upon principally as incidental, and only required against special medical emergencies ; and that the lay members of the committee, assisted by the host of lay assistants they themselves appoint, and under their direct control, deem themselves competent to carry on the very simple rules dictated by a rigid uniformity.　This lay government obtains at the Surrey Asylum, Wandsworth, and at the Middlesex Asylum, Hanwell, but presents its maximum development at Colney Hatch.　Were not the consequences too sad, it would be amusing to recount

the instances of over-weening self-importance and self-suffi-
ciency on the part of the Committee of Visiting Justices.   They
record in their own reports the labours they perform in the
general management, in the suggestion of treatment, in the
supervision of the measures pursued by their medical officers in
treating the patients, in the hiring and discharging of attend-
ants and servants.   "They exercise supreme authority; and the
medical officers, with more or less docility, carry out their in-
structions," humbly submitting themselves to adopt or to lay
aside any particular practice they may have pursued in the
treatment of the mental lesion, and to have the daily surround-
ings, the discipline, and the moral régime of the patients, ordered
and administered by the committee and their officers.   The
medical superintendent is but a humble servant of his com-
mittee; and the asylum, in lieu of being a place of treatment, is
simply one of detention.

We have not exhausted the list of defects, of errors in con-
struction and management, of our large mixed asylums.   Nor
have we unfolded to our readers any actually novel views and
statements, for the like have long been insisted upon by various
writers, and notably by Dr. Arlidge, in his essay ' On the State
of Lunacy ' (1861); but we are persuaded that demonstration
enough has been afforded of grave defects, clearly detrimental
to the welfare of the insane and the progress of enlightened
and successful treatment.

Let us now turn to our author's views as to the nature and
scope of the reforms demanded for the care and cure of the
insane.   In respect to these, we again find that Dr. Granville
has been largely anticipated by the author just referred to;
and, indeed, follows equally in the wake of several writers, par-
ticularly among those of recent years.

Admitting that insanity is a curable disease in a large pro-
portion of its varieties, and that it calls for early individual
treatment, the first suggestion is, that there should be special
asylums, or rather hospitals, for the insane when first attacked,
to be furnished with all necessary appliances for the physical
and moral treatment of insanity.   Attached to this kind of
hospital Dr. Granville would have, "so far as the control of
its general arrangements is concerned," one, or if necessary
more than one, institution to which patients, not either requir-
ing or likely to be benefited by active personal treatment, but
still needing medical care, might be removed, when, in the
course of each individual case, it becomes apparent that this
transfer will not prejudice the final chances of recovery.   In
connection with the workhouse, or in some appropriate institu-
tion—for example, a " workhouse asylum "—provide suitable

wards for the senile and infirm, whose recovery is impossible, and who tarry only for death. This threefold system being efficiently organized, the necessary circulation of cases may be readily maintained, without either indefinitely multiplying costly buildings or allowing the curative establishment to become blocked against recent and curably, by the stagnation of chronic, cases."

The curative establishment must be kept of small magnitude, and the number of its inmates not exceed that which it is possible the physician in charge can individually treat. Together with proper structural arrangements must be associated an organization fitted to make the establishment an instrument of treatment, all its details leading up to one purpose. "The medical superintendent of a curative establishment for the insane should be its chief and immediate director. Every subordinate officer, from the matron to the meanest official, ought to look to him as master. An appeal to the committee is fatal. Justices should hold their physician responsible for everything that concerns the institution under his control," and not weaken his influence by personal interposition, but allow to him an undivided authority.

Besides the threefold mode of distributing lunatics, Dr. Granville recognises as advantageous to a class of pauper lunatics the plan of boarding-out "with friends;" but he would restrict the expedient within very narrow limits, and rather advocate the construction of detached buildings, in connection with asylums, for the accommodation of trustworthy and convalescent patients, who should have almost perfect liberty and contribute by their work towards the cost of their maintenance.

We have now passed under review the principle opinions, conclusions, and recommendations of Dr. Granville in the two interesting and instructive volumes before us. There are others referring to minor points touching the care and cure of the insane well worth quoting, but which our waning space warns us to leave unnoticed. To thoroughly appreciate the value of his opinions it is necessary to read them as more fully recorded in his own language, together with the arguments he has employed to enforce his recommendations.

It is a melancholy tale that the reports of the Lunacy Commissioners, and of the many asylums—ever growing in number and size—have to tell us year by year, namely, that the demands for accommodation of the insane exceed the supply, although this is actively kept up, and that every asylum is crowded by chronic lunatics and imbeciles, and contains few inmates whose recovery can be anticipated. With such a history before it, it might reasonably be presumed that the public would show a

lively interest in any work undertaking to discuss and solve the pregnant question—what are we to do with our lunatics? But any such interest has hitherto been wanting, and a glaringly faulty system of dealing with our insane has been allowed to continue and to expand, as though the British public had handed itself over to the doctrines of fatalism, accepting the present state of things as inevitable. Nevertheless, we must yet hope that the public mind will be aroused to contemplate the serious question we advert to; and are consequently pleased by the appearance of this work, as a means of supplying the information needed to arrive at a satisfactory conclusion.

XII.—Forensic Medicine and Hygiene.[1]

THERE is nothing in the history of our country more remarkable than the present development of thought and action on the subjects of health and life. For much longer than the quarter of a century we have, in the pages of this Review, been active in season and out of season in teaching the truths of sanitary science. Sometimes we have stood alone, then we were for a brief period joined by other journals specially devoted to subjects of public health. Again we have stood alone,

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[1] 1. *A Handy Book of Forensic Medicine and Toxicology.* By W. BATHURST WOODMAN, M.D., F.R.C.P., and CHARLES MEYMOTT TIDY, M.B., F.C.S. London, 1877.

2. *Sanitas Sanitatum et Omnia Sanitas.* By RICHARD METCALFE, F.S.S. Vol. i. London, 1877.

3. *House Drainage.* By ROGERS FIELD, C.E. London, 1877.

4. *Société Française d'Hygiène, sa raison d'être, son but, son avenir.* Par M. le Docteur de PIETRA SANTA. Paris, 1877.

5. *Functions of a General Police Force for the extinction of fires and administrative improvements especially needed in the metropolis.* By EDWIN CHADWICK, Esq., C.B. London, 1877.

6. *Report on the Air of Glasgow.* By E. M. DIXON, B.Sc. Sanitary Department. Glasgow, 1877.

7. *Our Medical Charities.* An Address delivered by SAMPSON GAMGEE, F.R.S.E. London, 1877.

8. *Contagious Diseases and the Insufficiency of the measures adopted for their prevention.* Pamphlet. London, 1877.

9. *Statistical Sanitary Report of Margate.* By W. KNIGHT TREVERS, F.R.C.S., Medical Officer of Health. Margate, 1877.

10. *" Dust to Dust."* Sanitary Modes of Burial. By SAMUEL PHILLIPS DAY. London, 1877.

11. *The Quarterly Journal of Inebriety.* Published under the auspices of the American Association for the Cure of Inebriates. Vol. i. Nos. 1, 2, 3. Hartford, Connecticut, 1876-7.

12. *Restriction and Prevention of Scarlet Fever.* Document issued by the State Board of Health of Michigan. 1877.

and now we may say that, instead of standing alone, we are
practically swamped in the sea of literature on health topics
which has gathered around us. We are not jealous on this
account. On the contrary, we are proud that it has been our
mission, quarter by quarter, to hold steadily the light of science
when often there were no other hands to hold it. We look,
indeed, on the present results as a proof of the usefulness of our
work, and, as we give a last gaze back on it, are content to leave
it to the judgment of those who in the future will be sure to
judge it justly.

As we now write our table is loaded with books inviting our
attention. They deal with a multitude of subjects, all appa-
rently different, all mostly bearing relation to the one subject—
the health of the people and the mode in which it is best
maintained and promoted.

Our hand rests on the first volume named on our list, the
work called ' A Handy Book,' by Drs. Woodman and Tidy.
We touch it with a feeling of great respect, because one of the
authors of it has not lived to see the result of his labours. A
mind too anxious, a heart too anxious, cannot always live the
natural span, and Bathurst Woodman, long our friend and
worthy colleague, though he fell under his work, unhappily, is
one not soon to be forgotten by those who knew how earnestly,
how honestly, how lovingly, he laboured for his race in the short
space alloted to him for labour.

As to the book called " a handy book," a handy book it is
not. It is a very large book and very laboured. It has all the
faults, the invariable faults of a book written by two heads,
which, whatever they may be in other affairs, are never in
book writing wiser than one. For though we detect easily
enough in this work that one head was the more active, we
do not fail also to detect the absence of unity of design and
method which follows the dual effort. We dare not say the
book is a great book in the way of literary effort. It may be
the nucleus of such a work, but as it stands it does not appear
to us as a work specially called for, and therewith naturally
supplied. It is deficient in all the charm and originality of
Beck, and is equally deficient in the experience and personal
knowledge of Taylor. At the same time it is not guilty of the
great defects of Taylor's really great work. It is conceived in a
better spirit, is free of all attempts to submerge opposition by
an avalanche of attack, and in so far it may be considered a safer
if not a sharper forensic guide.

In detail the book is accurate, and the strain that has been
made to finish detail without the introduction of new science

is probably at one and the same time its chiefest virtue and its gravest failure.

The book 'Sanitas Sanitatum' is of a very inferior order. Its writer is an advocate of the Turkish bath, and is, we believe, the well-known proprietor of the baths in Paddington. His book is naturally in favour of the bath, the whole bath, and nothing but the bath, and, in short, it is only written for the bath, although it deals with an infinitude of other subjects, relevant and otherwise. The most interesting, as it is also the most amusing, part of the work is included in a series of testimonials on the bath, extending from the 225th to the 323rd page of the comely volume. They are called "letters from statesmen, noblemen, medical men, literary men of eminence, and others," and surely in another day will be read with much humour and insight into character. How they were got is itself worthy of note.

The commissioners of baths for the parish of Paddington were, it seems, seized with an irresistible and laudable impulse to improve the health of the people of that ilk. They therefore undertook to find baths and washhouses for the working classes —a very wise procedure. But before setting to work the authorities must needs ask a few questions. Some proposed that Turkish baths should be added, "Mr. Metcalfe and other commissioners supporting this proposal." The commissioners, therefore, asked the opinions of the statesmen, noblemen, medical men, and others whose names are hereinafter mentioned as having furnished replies. The said replies follow, and a curious congregation they are. Mr. Spencer Wells tells the commissioners that he has published his opinions long ago. He objects to giving anything like a testimonial on any subject. "Oh, wise and far-seeing man, with eyes so wide open, what shall be thy reward?"

George Cruikshank washes in cold water all the year round; he has actually never used a Turkish bath;—he has actually never seen one. Unable, therefore, to give any opinion on the subject, he refers the ardent inquirers after truth "to the Medical Inspector of Paddington." Prudent George Cruikshank!

Mr. Spurgeon believes no one has ever been clean who has not taken a Turkish bath, but he fears the time required for its use will prevent the working classes using it extensively. Common sense Mr. Spurgeon!

Sir William Gull succeeds in giving an opinion without signing it. Somebody, the letter says, is directed to write for him, to present his compliments, and to say, "that the advantages to the working classes of properly arranged baths and washhouses cannot be over-estimated, but that Turkish baths are

more adapted to the indolent and luxurious than to those who toil for their livelihood." How luxurious this opinion is in itself our readers will see. The mystery, and there is a mystery about it, is, who it was that was directed to write it. Was it the great Mogul? We regret our deponent sayeth not in this edition of his work; in the next he will, perhaps, enlighten us, for surely he holds the secret.

To these specimens " we could and we would" add some dozens more. We forbear. Our author is not, indeed, altogether satisfied himself about some of them, and so we will not pain him by repeating disaster. He says that with regard to the letters from medical men generally, they may be considered highly favorable; " but Drs. Watson, Parkes, Sir W. W. Gull, T. King Chambers, Andrew Clarke, Burrows, Sieveking, C. B. Williams, Ballard, Bristowe, and Nicholas, all agree upon the medical efficiency of the Turkish baths, but add some remarks *which are scarcely worth noticing.*" Morsels of similar delicacy are to be found in other parts of the work; but we must stop. We stop with the expression of the deep regret that any such opportunities should even have been supplied for exhibition of character and caricature.

The book we rest on next is once more of an entirely different kind. The work on administrative organization, by Edwin Chadwick, is a book pretending to little more than pamphlet form, but full of the most useful, the most thoughtful advice and instruction. It may seem at first sight that an essay on the functions of a general police force for the extinction of fires has a purpose removed far away from the subject of sanitary reform. In fact, however, it is the heart of such reform. In many pages the author insists that the police service and the fire service should be one and the same, and that, as matters now stand, the police are deprived of half their beneficent duties as a great organization. He comes at last to the consideration of " sanitary evils and waste from disjointed sanitary works and want of administrative organization and skill." In this part Mr. Chadwick adverts to the connection which the administrative arrangements in question for the prevention and spread of fires have with the larger subject of the prevention of epidemic diseases and excessive mortality. In his report of 1842 on the sanitary condition of the labouring population of Great Britain, he had reason to observe upon the inefficiency and waste from the disjointed action of local administration, from the all-pervading want of principle in the arrangements for the public service. He then said—and many of the old sanitary reformers will remember well the statement— that division of labour in the arts derives its efficiency from

combination, adaptation, and subordination of action to one end; but that which appears to be a division of labour in local administration is, in fact, an insubordinate separation, weakening the means of procuring adequate skill and power, occasioning obstruction and defective execution and enhancing expense. "Were pins and machines made as local works are constructed, shafts of pins would be made without fitting heads; in machines screws would be made without sockets, and it might be confidently stated that there would not be a safe or well-working machine in the whole country." He illustrates the truth of this observation by reference to the water supplies of different towns. In his reports as a sanitary commissioner, he and his fellow-commissioners provided, amongst other combinations, those of hydrants to be applied by the police. These arrangements were allowed to fall through in the metropolis, but were partially adopted in the provincial cities with the great results of increased security and reduced expense. But even, says the author, in those cities the arrangements proposed for carrying away the waste or fouled water were not comprehended, and have been neglected, with the effect of the aggravation of the evil of undrained sites and the frustration of the results due to the improvement of the public health by the introduction of improved supplies of water.

In Liverpool this error has at last been detected, and Mr. Chadwick very naturally is triumphant on the results which have been recently published by the chairman of the water committee. The facts are sufficiently important to deserve a short notice in this place. The chairman of the water supply of Liverpool gives the results obtained in fourteen test districts where the distribution of water had been at about the main rate of distribution in the metropolis, viz. 33 gallons per head of the population. In one district, where the consumption had been 54 gallons per head per diem, it has been reduced to 17 gallons; in another, where it had been 41, it has been reduced to 10; in yet another district, and that one in which water-closets are used, it had been reduced to 6 gallons per head per diem. The mean of the reduction has been at the rate of 13·32 gallons per head. This has been done by a system of outside observation, by meterage; and it has been shown that the saving of water effected has been equal to 1,180,789,428 gallons per annum. The financial result is not less important. It stands as follows :—

Increase of revenue	£27,526	0	0
Annual value at cost price of the 8,501,167 gallons per week of absolute decrease in the quantity delivered, which water is now stored at Rivington . .	13,954	0	0
	£41,480	0	0

This amount capitalised represents a sum of £1,037,000. Turning next to the sanitary results, it is shown that the death rate in Liverpool has been reduced from 35 in the 1000 in 1871 to 27 in the 1000 in 1876. It is, of course, very difficult to distinguish all the causes which have led to so great a decrease in the death of a great centre like Liverpool in so short a time, and it may be fair to say that the more systematic supply of water is only one of the causes of such reduction. Of this the chairman of the water committee is himself candidly aware, and he does not claim more than the just share of credit for the labours of his committee. When it is remembered what his committee has done we can, indeed, reasonably and willingly admire the quiet modesty with which every claim is made. They have blocked up the drains of 1896 cottages, and so prevented the distribution of sewer air through them ; they have avoided the necessity, in most instances, of emptying the mains for plumbers' repairs ; they have removed defective lead service pipes in the drains and alleys ; they have prevented storage of water in tubs and other receptacles in the poorest parts of the town ; and they have given a constant supply of pure water by night and by day, which even in summer is cool and refreshing. The chairman adds, that when it is remembered that a reduction of the death rate by 1 per 1000 represents 559 lives saved annually in the borough alone, and that those 550 lives after all bear a small proportion to those suffering from sickness, the question at once strikes us as one of the most important that can occupy attention.

To these observations of the chairman of the water committee at Liverpool Mr. Chadwick alludes with justifiable satisfaction. He quotes the following from the same official authority :—

"In addition to the pecuniary results, we have converted a nine-hours' service into a constant service ; we have saved water sufficient to meet the increasing wants of the town and neighbourhood for eight years longer than would otherwise have been the case ; we have given additional facilities for the extinguishing of fires ; we have relieved the cottage-owners of the maintenance of 9700 taps in the courts and alleys ; and we have, with the assistance of the Health Committee, undoubtedly contributed to the lowering of the death rate."

And then he proceeds to add, in comment—

"As to the sanitary results obtained from the amendment of the ill-regulated distribution of water and bad house service, I can corroborate them from earlier experience obtained in a number of of towns, where the principles of sanitation for the reduction of the damp of habitations were then set forth in the instructions of our Board, were fairly attended to.

" In 1865 an inquiry was made, at the instance of the medical department of the Privy Council, into the results of works constructed more or less in accordance with the principles we promulgated. I might take exception to the completeness of that inquiry as it was conducted ; but the general ascertained result was a considerable reduction of the total death rates, and especially of the death rates from the foul air diseases. It was, however, particularly noted by Mr. John Simon that the inquiry involved facts for which he was 'not in any degree prepared,' that the drying of the soil in the improved towns had let to the diminution, more or less considerable, of phthisis. 'The facts which are yet in evidence seem most strongly to support this conclusion. Should it be substantiated, it will constitute a very valuable discovery, evolved by Dr. Buchanan, from the inquiries here reported on.' 'It will be seen that the reduction of phthisis where certain works have been executed, is far too large and too general to be regarded as an accidental coincidence.' 'The reduction in the first fifteen towns in Dr. Buchanan's table are as follows :—Salisbury, 49 per cent. of its previous rate; Ely, 47 per cent. ; Rugby, 43 ; New Banbury, 41 ; Worthing, 36 ; Macclesfield, 31 ; Leicester, 32 ; Newport, 32 ; Cheltenham, 26 ; Bristol, 22 ; Dover, 20 ; Warwick, 19 ; Croydon, 17 ; Cardiff, 17 ; Merthyr, 11.' On this, which I must treat as a revival of sanitary doctrine in 1865, I have to state that, in my Report of 1842 on the sanitary condition of the labouring population, will be found a chapter setting forth the investigations then held on the results of land drainage, or of the reduction of damp, in the health of men as well as of animals ; the facts on which I was led to specify as preventible next after—epidemic, endemic, and contagious diseases, fevers, typhus, smallpox, measles—the great category of the 'diseases of the respiratory organs, consumption, pneumonia, and others ;' and later on, in 1848 and 1850, the causes, the dampness of sites, induced by a discharge of pipe water in urban districts, equivalent to the saturation of the subsoil with a double rainfall. Mr. Simon, speaking of the recorded results then obtained by inquirers, which he promised should be continued, observed that ' they may serve to fulfil very important provisional uses, not only to confute persons who have despaired, or affected to despair, of any great preventibility of disease, but still more to justify in the public eye, and to encourage in some of the noblest of human labours those who for long weary years have been spending their powers in this endeavour, and to whom surely it will be the best of rewards to see the demonstration of the good they have wrought.' But the official examinations which we had contemplated as a fixed and responsible duty of the first health department were not systematically proceeded with, and if they had been it must have been to record such continued and fatal dereliction of principle as have been hereinbefore set forth. A further illustration of the lack of competent inspection and warning may be presented in respect to the common conditions of the water supplies.

" In a number of towns, Liverpool itself included, fresh supplies of

water have been introduced, and, lo ! the death rates have not been
reduced as expected and promised, but have even been increased.
Engineers, whose only practice has been for trading companies to
carry water to the doors of houses, and who have nothing to do with
it afterwards, or any concern as to how it is to be carried into
houses, or in what condition it is left there or gets away, and even
some medical men, have pointed to the fact that pure soft water
supplies have had no effect in reducing the prevalent heavy death
rates. It has been observed, moreover, that the introduction of the
new and pure supplies of water has not had the effects promised—
of reducing the drinking habits of the population !   How should it,
when the cold of the wretched absorbent brick habitations is made
worse by increased damp, and those miserable depressing conditions
are aggravated which impel even persons of higher position to have
recourse to alcoholic stimulants to withstand them ?   Again, the
water, by the conditions of internal distribution in which it is kept
stagnant, absorbing the mephitic gases, is rendered mawkish, impure,
repulsive to the taste, and itself a provocative of intemperate habits.
The use of such excessive quantities of water is excused on the score
of the necessity of cleansing the drains and sewers, which it does
not do, inasmuch as it merely runs over indurated deposit, the result
of forewarned and unamended bad constructions.   But it is to be
observed that the sanitary conditions and increasing waste have
been commonly treated as constant by engineers, and as a pressing
case of necessity for some great engineering works for augmented
supplies—leading to augmented waste.   In Liverpool itself, very
recently, the introduction of a lake supply has been urged as of an
immediate necessity to meet the demands of the increasing popula-
tion, which the waste-prevention achieved in the important Report
cited, will serve, as it states, to postpone for ' eight years' to come.
For the metropolis two projects have been urged for nearly doubling
the quantity of the existing supplies as insufficient, and that they
cannot be too ample in the face of demonstration that they are as
distributed in pernicious excess.   Amongst the evils of this excess,
and one requiring distinct notice, is the injury done to the manurial
value of manure by the excessive quantity of water with which it is
diluted, as well as by storm water, by which, at times, it is impro-
perly encumbered.

" It admits of renewed demonstrations.

" That the like sanitary evils arising from errors in the internal
distribution of water into houses, described as having been preva-
lent at Liverpool, prevail to grievous extent throughout the metro-
polis, especially in the houses of the poorer classes in the lower
districts.

" That the like reduction of waste, with attendant sanitary im-
provement, may be effected to as great an extent in the metropolis,
that has been achieved in Liverpool.

" That such reduction of waste may be effected in the metropolis
with even greater proportionate economy than has hitherto been
in Liverpool.

" But that, for the attainment of these results for the benefit of the population, the entire works, internal as well as external, must be placed under unity of management, by competent public authority, responsible for the execution of the work as a special trust.

" When it is proposed to vest the works for the supply of water under a public trust, it is to be considered that they now are, virtually, under a public trust constituted by statute. The companies are traders, contracting for the sale of supply of water at prices, and of qualities and under conditions, set forth and regulated under Government officers or water a examiner as to quality, and of an auditor as to prices, by a Government department. What is needed is, then, really an enlargement of the powers and of the conditions of that public trust, including the abrogation of the contract with those traders, on compensation to them, as may be given on settled principles. Viewing the Government as trustees for the public under the exceptional conditions of the metropolis, what should be required of them under those conditions may be considered as if they were trustees for a private estate or property ? What would be their responsibilities for allowing it to continue in its present condition of disorder and waste of life and of property ? Would they be sanctioned in allowing their trust to devolve upon such a body as the Metropolitan Board of Works, as an incident to other and desperate duties, and to a divided and distracted and irresponsible attention—upon a body who had proposed a double and even a fourfold expenditure, and who had ignored proved practical large economies ? Would it be sanctioned in intrusting the work of fire prevention in charge of a numerically inferior force, and dispensing with the service of the numerically superior force ? of continuing the work of water supply in hands apart from those charged with its effective application ? The example of the success of the provincial municipal authority may be superficially regarded as countenancing the notion of administering the trust in question by a metropolitan municipality. But the success of each of the provincial municipalities was only attained after years of favour to recognise and act upon the principles propounded ; and even now, when examined, there are yet serious shortcomings and defective action, especially in respect to our outer ring, which it were unnecessary to discuss. In magnitude, the metropolis is equal to eight Liverpools. In action and in conception to meet its requirements the course of the so-called metropolitan representative body will be found, on an impartial examination of the evidence, to be flagrant and reprehensible and conclusive as to its dangerous untrustworthiness. The population of the seat of government of the empire may surely ask for a befitting special executive authority responsible directly to their representatives in Parliament, at least provisionally, for the purpose of organisation, by an extended trust. This I can declare as an expression of the concurrent opinion of provincial officers, as well as of engineers of great practical experience and observation."

We have extracted at length the statements of Mr. Chadwick

for the simple reason that it is impossible in any way to condense them. They are, moreover, so instructive, they deserve all the attention that can be paid them. We rejoice as we read them to feel that the hand which so ably raised the standard of sanitation in 1838 is now, in its fiftieth year of sanitary work, still so efficient and still so wisely and powerfully pursuing its craft for the good of the State and the vital interests of the people.

The remaining books that lie before us are all of interest, and some of true value. The essay of Mr. Rogers Field is a good exposition of sanitary details in reference to house drainage. Dr. Pietra Santa's doctrine explains the origin of the French Society of Hygiene, a society recently founded in France after the plan of the Sanitary Institute of Great Britain. Dr. Dixon's report on the air of Glasgow is drawn up with much scientific care, and is a work which gives promise that we have in its author a new hand who will be heard more of in due time. Mr. Gamgee's address on ' Our Medical Charities ' is the work of a master who never takes up the pen without having some original thought for its work, who is never happier or more useful than when he is laying bare some great evil, and who never laid bare more skilfully a greater evil than that which he now brings to the fore.

' Dust to Dust,' by Mr. Samuel Phillips Day, is the work of a scholar who is well conversant with the sanitary modes of burial. Mr. Day inclines clearly to cremation as the best mode of disposing of the dead ; but, pending the advanced time when this method will be consonant with the sentiment of the nation, he advocates the earth-to-earth system, which Mr. Wyman Herden has so earnestly and ably supported. The 'Report on the Statistical Sanitary Report of Margate,' by Mr. Trevers, indicates how excellent a medical officer of health he is, and how much the oldest and still the most popular of watering-places near to London is improving in sanitary condition.

The new journal, to which we call attention at the commencement of this review, and which is named ' The Quarterly Journal of Inebriety,' is a singular indication of the progress of thought on what is popularly known as the drink question. The journal is published in Hartford, Connecticut, and is the official journal of the American Association for the Cure of Inebriates. The papers in the first three numbers are of unequal merit, but sound and practical. They show how greatly in advance the American nation is in its management of the drunken insane.

The last book to which we refer is the document issued by the State Board of Health of Michigan, on the restriction and prevention of scarlet fever. It is drawn up by Dr. Baker, with

whose sanitary labours English readers are so familiar, and we need not say it is most ably done. We do not see that it contains any original suggestions, but it is a simple working model of a report which all who will may read, and we name it specially as a model which we in England may take pattern from, as we progress in the sanitary education of the masses of the people.

This education is the grand medical work in the future of medical science.

XIII.—On Contagion: What do we know regarding it.[1]

THE two problems at present absorbing the attention of scientists (medical and lay) are, the Evolution of Life, and the Germ-nature of Contagium. In the following pages we desire to side neither with the panspermists nor with the heterogenists. We shall trace the origin of contagium no further than published researches enable us, shall avoid all hypotheses respecting its relations to the evolution of life, and shall keep ourselves uninfluenced by any theory of fermentation and putrefaction. There exist a sufficient number and variety of observations to allow us to state certain potent reasons for applying the term *germ* to the ultimate entity of contagium, in so far, at least, as found in some contagious diseases. With the primary origin of such an entity or entities we do not concern ourselves, as this is as yet an unfathomed ocean; on the other hand, recent observations throw light, to a certain extent, on the behaviour of contagium under certain conditions; but of the further development of any one contagium, or its transmutation in form or substance, we know nothing positive. So, also, the relationship of contagia to one another is as yet a strange inquiry.

Dr. Maclagan's excellent *résumé* of the whole subject, the 'Germ Theory of Disease,' as also Dr. Ross's very thoughtful and suggestive monograph, the 'Graft Theory of Disease' (which has been already reviewed in this Journal), and Drs. Braidwood and Vacher's Reports to the Science Grants Committee of the British Medical Association, on the 'Life History of Contagium,' are good examples of work done by industrious and thoughtful

[1] 1. T. MACLAGAN. *The Germ Theory applied to the Explanation of the Phenomena of Disease.* London, 1876.

2. *Reports of the Medical Officer of the Privy Council.* New Series. No. vi.

3. BRAIDWOOD and VACHER. *First and Second Contributions to the Life-History of Contagium.* 'Brit. Med. Journal,' 1875, 1876, and 1877.

4. W. ROBERTS. *On Spontaneous Generation and the Doctrine of Contagium vivum.* 'Brit. Med. Journal,' Aug. 11th, 1877.

practitioners " during odd hours snatched from the busy routine of practice." On the other hand, we dare not overlook the researches of Drs. Burdon Sanderson, Klein, and others embodied in the Privy Council Reports, and others published in 'Ziemssen's Cyclopædia of Medicine,' even though we regard the results of these observers as " not proven."

What reasons are there for believing that " many diseases are due to the presence and propagation in the system of minute organisms, having no part or share in its normal economy ? " On what observations is the Germ Theory of disease based? What proof have we of the " *competence of germs to produce the phenomena of disease* ? " In the next place, are all germs capable of inducing disease of similar natures—are they of like form, of uniform habits, or in what respects do they differ ? Since the middle of the seventeenth century, when Hauptmann first suggested that epidemic diseases might be caused by the presence in the air of invisible germs, there have been published, from time to time, many similar guesses at truth, many more or less plausible efforts to interpret the phenomena of contagion by theories leaning on probabilities or analogies, sometimes only on possibilities. There is no evidence that all contagious diseases are communicable by infecting liquids, nor are all varieties of contagia, liquid or dissolved, or suspended in liquid. It must, however, be admitted that the contagium, or the several contagia of diseases known to be *inoculable,* must be contained in the liquids with which we can perform inoculation ; of this class the contagium most readily obtainable and most easily handled is that which forms the infective principle of the various group of diseases, and which is found in the vesicles of variola and vaccinia. Accordingly, most investigations into the nature of contagium have been made with vaccine virus. To discover this contagium the microscope was first used, and Dr. Lionel Beale, in December, 1863, announced the discovery of transparent particles of extreme minuteness in vaccine lymph, and expressed his conviction that the contagious or active properties of the lymph lay in these particles. This observation is now admitted beyond doubt, but the relation of these particles to infection, and their physical properties, were debateable points, till a few years later M. Chauveau submitted vaccine lymph to physical tests. Having proven, through separation by subsidence and through filtration, that the leucocytes and other constituents of vaccine lymph did not retain its infective power, but on the contrary, that its serum (which by microscopic examination revealed these minute sparkling particles) was still virulent; M. Chauveau proceeded to separate these "elementary granules " of the serum, now often termed micrococci, by

means of diffusion. From such experiments he concluded "that the vaccinal serosity is not virulent, and that the activity of vaccine resides in the solid granules, either in all or only in one part of these little elementary organisms." Mr. Chauveau's researches, contained in two papers in the 'Comptes Rendus des Séances de l'Académie des Sciences' for 1868, led Dr. Burdon Sanderson to test these observations, improving the method of procedure in various particulars. Still later, Dr. Braidwood and Mr. Vacher employed diffusion for the purpose of separating the contagium of vaccine, and by attention to certain details overlooked by their predecessors, they may be said to have avoided all possible sources of error. All these observers have arrived at the same conclusion, viz. that " we possess the strongest proof (we can have no direct proof till we have learned how to wash off all trace of plasma from the bodies it suspends) that the contagium of the virus with which we are most familiar consists of transparent particles, not exceeding (according to Sanderson) the 20,000th of an inch in diameter, neither soluble in water nor in watery liquid, and not capable, without losing its properties, of assuming the form of vapour. Further, inasmuch as these particles do tend to subside, though (according to Chauveau) they never dispose themselves completely in the lower layers, we are assured that they are of a specific gravity only slightly greater than that of the plasma surrounding them, while, from their not sensibly diminishing the transparency of the liquid containing them, they refract light in the same degree as this, and by examining fresh vaccine microscopically any one can observe these minute, highly refractive, colourless bodies to be moving, and to form groups of threes and fours. Now, these minute, distinctive particles (Sanderson's micrococci) are to be seen, not only in vaccine lymph, but also in the lymph of human and ovine variola. Hence it may be concluded that such are the physical characters of the contagium of the virus of all the diseases composing the variolous groups.

We come next to consider whether such an end has been reached in the study of any of the other contagious diseases which attack human beings and animals. At this point we are met by the great, almost insuperable, obstacle that no other class of contagious affections supply us with a secretion suitable for inoculation and for microscopical and physical examination.

No contagion, except that of the variolous group, has hitherto been at all capable of isolation. It is true, that some continental investigators, consider that they have satisfactorily proven by experiment, the contagion of erysipelas and diphtheria to consist of, or to be intimately connected with, the microphytes

or bacteria, associated with the septic transformation of nitro-
genous matter (*vide* Dr. Sanderson's Reports for 1874) ; while
others regard such contagious diseases, as typhoid and relapsing
fever, and the splenic fever of veterinary pathology, to be due
to "a vegetation of recognisable peculiarities of form and de-
velopment, differing from those met with either after death in
the normal tissues or liquids of the body, or during life in the
products of primary or secondary inflammation ; " of the former
class of investigations it may be stated that they require con-
firmation, while respecting the vegetable origin of typhoid fever
in as far as such has been delineated in the ' Reports of the
Medical Officer of the Privy Council,' new series, No. VI.
Dr. Creighton's investigations, published in the ' Proceedings of
the Royal Society,' vol. xxv., No. 172, show that bodies closely
resembling this vegetable organism are to be met with "when
fresh tissues are put in solutions of chromic acid and alcohol,
causing coagulation of mucus and other albuminoid fluids."
The peculiar rod or staff-shaped bodies first described by
Pollender as the characteristic contagium to be found in the
blood and tissues of animals which have succumbed to or suffer
from milzbrand or splenic fever have been observed in this
affection by Brunell, Bollinger, and others. The pathology of
splenic fever has more recently acquired a wider interest from
this disease being identified by Buhl with that affecting the
human subject which he termed mycosis intestinalis. The
rarity of the disease, at least in this country, may be regarded
as the cause of its not having been studied here. But the
elaborate experimental investigation of this affection by Koch,
leaves no reason to doubt that the rod-shaped bodies, about as
long as the breadth of a blood corpuscle, found in the blood,
spleen, lymphatic glands, and in some other tissues of animals
suffering from spleen fever, are intimately connected with the
contagious nature of this disease. Koch found that mice were
peculiarly susceptible to the virus of splenic fever. The
minutest particle of the fresh blood or spleen of an infected
animal invariably produced the disease when brought into con-
tact with the living tissue of a mouse. He found further that
he could cultivate the organisms artificially outside the body.
After being placed in the incubator the rods were observed in
a couple of hours to begin to lengthen, growing to twenty or
a hundred times the length of the original rods, and they
began by-and-bye to assume a dotted appearance. "The dots
gradually increased in size and distinctness until, after the lapse
of fifteen or twenty hours from the beginning of the experi-
ment, they acquired the appearance of strongly refractive oval
bodies, which were placed at regular intervals along the threads.

Finally, the threads broke down, and the oval bodies, which could be nothing else than spores, were set free and sank to the more depending parts of the drop. If the supply of nutriment were then exhausted, the process ended here, and the spores remained permanently unchanged ; but, if additional nourishment were provided, the new spores were seen presently to elongate into rods, exactly resembling those originally existing in the blood or spleen. If the conditions were favorable, the new rods, after a period of rapid multiplication, in their turn entered on the formation of a new generation of threads and a new generation of spores." (Dr. W. Roberts' address, ' Brit. Med. Journ.,' August 11th, 1877.)

With the view of testing the pathogenic activity of the rods and spores cultivated in this manner, Koch introduced minute quantities of the rods, or of the spores alone, subcutaneously by incision into a mouse, causing speedy death from splenic fever in every instance.

"Koch found, without exception, that if the tested materials produced threads and spores in the incubator, it also produced splenic fever when inoculated into the mouse ; and, on the contrary, if no such growth and development took place in the incubator, the tested material produced no effect when inoculated into the mouse."

The variable duration of the activity of the contagium of splenic fever was now explained.

"Koch found that the rods had only a comparatively fugitive vitality ; they lost their infective power generally in a few days—at the most in about five weeks. But the spores retained their infective activity for an indefinite period, in spite of all kinds of maltreatment. They could be reduced to dust, wetted and dried repeatedly, kept in putrifying liquids for weeks, and yet at the end of four years they still displayed an undiminished virulence."

Cohn further found that the organism above described as characteristic of splenic fever was identical in form and development with the *Bacillus subtilis*—a form of bacteria.

Dr. Obermeier's (of Berlin) discovery in 1872 of minute spiral organisms (spirilla) in the blood of patients suffering from relapsing fever, has been fully confirmed by subsequent observations. These organisms are found during the paroxysms ; they disappear at the crisis, and are absent during the apyrexial periods. They consist of spiral fibrils of the most extreme tenuity, varying in length from two to six times the breadth of a blood-corpuscle, and in the fresh state they move about actively in the blood. They have not been detected in any of the fluids or secretions of the body except the blood, nor in any other disease except relapsing fever. " In form and botanical characters they are almost identical with the *Spirochæte pli-*

catilis of Ehrenberg, a species of bacteria," and have been termed by Cohn *Spirochæte Obermeiri.* The most recently published monogram on this subject is by Dr. Heydenreich, of St. Petersburg, whose conclusions are based on a most careful study of forty-six cases of relapsing fever.

" He found that every rise of temperature, whether that of the paroxysm, or that following a pseudo-crisis, or those occuring during the intermissions, was invariably preceded by the appearance of spirilla in the blood. They disappeared entirely shortly before the crisis, and remained absent during defervescence and the subsequent apyrexial periods. During the whole of the main paroxysms spirilla were usually to be found in the blood, but their number varied in the most puzzling manner from day to day. One day they were abundant, the next day they were scanty, and the day after again abundant; they even varied at different hours of the same day; sometimes they vanished altogether for a time, and then reappeared in vast numbers a few hours later. Throughout these variations the temperature remained steadily high, or with only slight or moderate oscillations " (*vide* Roberts' Address).

Previous investigators had observed these discrepancies, but with Heydenreich rests the credit of explaining them.

" He found that, when a little blood containing spirilla was abstracted from the patient and kept at the ordinary temperature of the room, the organisms lived in it for several days; but if the blood was placed in an incubator, and maintained at the normal temperature of the body, they died in from twelve to twenty hours, and if the temperature was kept up to fever heat (104° F.) their life was still shorter—they only survived from four to twelve hours. This led him to the conjecture that, during the main paroxysm, not one, but several successive generations of spirilla were born and died before their final disappearance at the crisis. He surmised that in the usual course the broods would overlap each other more or less, the new brood making its appearance before the last survivors of the old brood had passed away. This explained the variable number of spirilla found on different days and different hours of the same day. Sometimes the old brood would have altogether perished before the new brood reached maturity; this explained the occasional absence of spirilla from the blood; it also explained the remissions or pseudo-crises sometimes observed in the course of the paroxysms " (Dr. W. Roberts' Address).

From Heydenreich's careful observations only one inference is eligible, that the spirilla are, or form the actual virus of relapsing fever. This seems to be confirmed also by experimental investigation, for experiments made in Russia on individuals who voluntarily submitted themselves to inoculation with the blood of patients suffering from relapsing fever, show that such blood is infective only during the paroxysms, but not

at the crisis, nor during the apyrexial periods. Moreover, none of the fluids or secretions of the body, except the blood, were found infected when tested thus.

From these observations it may be concluded beyond dispute that certain infectious diseases (those we have specified) are induced in the system by a "morbific agent, which is propagated in and given off from the bodies of the sick, and is capable, when received into a susceptible healthy body, of producing in that body a disease similar to the one during whose course it was formed " (Maclagan, p. 5). Further, the particulate nature of the contagium, or morbific agent of infectious diseases, is not only sufficiently defined by the observations above described, but it offers a ready explanation of a well-known, and on any other view, an inexplicable fact in the history of infectious diseases, viz. that of two persons situated in exactly the same circumstances, and exposed in exactly the same degree, to a given infectious disease, the one may be seized after a single exposure, while the other may be equally exposed for months without being attacked, and may even escape altogether.   The contagium particles in a patient's breath resemble an enemy's bullets.   The breath would be harmless without the particles, just as an enemy's powder would be without his bullets.

But what evidence is there in favour of regarding the contagium particles as organic germs or products of animal or vegetable development.   As Dr. Baxter states with emphasis in his " Report on an Experimental Study of certain Disinfectants " (' Reports of the Medical Officer of the Privy Council,' new series, No. vi), the " characteristic quality of a contagium or virus of a communicable disease is its capability of undergoing almost unlimited multiplication when introduced into an appropriate medium ; " hence also its effects are to a great extent independent of the quantity introduced into the system. " This power of development, and this faculty of breeding true, were the first, and have ever been regarded as the chief arguments in favour of the germ theory of disease," observes Dr. Maclagan, " and it is difficult to see how these properties of contagium can be accounted for on any other view of its nature."   It would be superfluous here to follow Dr. Maclagan's description of the chemical analogy of contagium to the process of crystallisation advocated by some.   Another argument in favour of the germ nature of contagium is that, like all organised matter, it is prone to undergo change when separated from the conditions essential to its vitality ; and this very proneness becomes an argument, not against, but in favour of the view which regards it as consisting of minute organisms.   " If all the ova of

every parasite came to maturity, few animals would be free
from these pests.   If every smallpox germ which came into
existence developed and reproduced its kind in the same way as
the parent from which it sprung, the civilised world would have
been all but depopulated before the days of Jenner."   While
chemistry and physic, moreover, throw no light on the mode of
growth or reproduction of contagium particles, the microscope
has assisted though it has not unravelled this mystery.   Dr.
Beale, for example, has figured the reproduction of these par-
ticles from the contagium particles found in the secretion in
various diseases.   He summarises the results of his observations
thus in his brochure 'On Disease Germs,' p. 244.

   " Without, therefore, pretending to identify the actual particles of
the living bioplasm of every contagious disease, or to be able to
distinguish it positively from other forms of bioplasm, healthy and
morbid, present in the fluids, on the different free surfaces, and in
the tissues in such vast numbers, I think the facts and arguments
I have advanced prove—first, that the contagious virus is living
and growing matter ; secondly, that the particles are not directly
descended from any form of germinal matter or bioplasm of the
organism of the infected animal, but that they have resulted from
the multiplication of particles introduced from without ; thirdly,
that it is capable of growing and multiplying in the blood ; fourthly,
that the particles are so minute that they readily pass through the
walls of the capillaries, and multiply freely in the interstices between
the tissue elements or epithelial cells ; and lastly, that these par-
ticles are capable of living under many different conditions—that
they live and grow at the expense of various tissue-elements, and
retain their vitality, although the germinal matter of the normal
textures, after growing and multiplying to a great extent, has ceased
to exist."

   Still, more distinct evidence is furnished by Drs. Braidwood
and Vacher's observations.   They state in their 'First Contribu-
tion to the Life History of Contagium,' that the corpuscles,
whose multiplication forms one of the characteristic appear-
ances produced by the vaccine virus, " are to be seen in the
crypts or hair-follicles budding or throwing off minute,
round, highly refractive bodies."   Similar corpuscles, seen in
sections of skin affected by variola, are stated by these ob-
servers to be noticed germinating or throwing off spores on the
fourth day of the eruption.   At this point we exhaust our
positive knowledge of the origin and reproduction of contagium.
As stated by Dr. Sandersen many years ago, " all microzymes
are not contagia, but all contagia may be microzymes."   All
forms and varieties of bacteria seen in contagious fluids and
in diseased tissues are not contagium particles ; and this is the
point which specially concerns us.

"From the presence of bacteria in a given fluid we can postulate nothing as to its disease-producing properties" (remarks Dr. Maclagan. "The fluid may be full of bacteria, and yet be incapable of causing disturbance; it may contain few, or none of them, and yet a single drop introduced into the circulation may give rise to a most virulent disease."

The next stage in this inquiry refers to the circumstances favorable to the propagation of contagium particles, and to those inimical to their reproduction. For the development of organisms certain external conditions are necessary; they will not grow in very low temperatures, and are destroyed in very high; they cannot thrive without water, and largely appropriate nitrogen in their growth. But contagium particles require something (as yet unascertainable) additional to these.

"We know," observes Dr. Maclagan, "that the contagium of typhus, besides requiring for its development the conditions requisite to the propagation of organisms, has need of something more, which it finds in the human body; that the contagium of measles also requires its own peculiar element, which is also found in the human body, and is quite different from the element appropriated by the typhus contagium; and so on, with all the other contagia."

The subject is only imperfectly understood until chemistry or some of the other sciences discloses this something or potency, which is a necessary constituent of the contagious state. We cannot know and cannot understand the natural history of contagia till we can tell the exact circumstances under which each prospers, and can indicate the special elements which each appropriates to itself. Here we would point out a false step which has been frequently taken by those studying contagium, an error pregnant of much mischief. The action of contagium on the animal economy has often been compared to that of medicinal agents or poisons. This has led to further error. As inorganic poisons and certain organisms can be made to exhibit their special properties external to the natural conditions in which they are found, it has been attempted to submit contagia to such an ordeal, and to unravel the mysteries of contagium by propagating it outside of the body.

"We do not think," observes Dr. Maclagan, "that any fair or legitimate analogy can be drawn between the effects of either an inorganic or organic poison acting on this or that organ, and those of an organised substance, which is reproduced to an enormous extent within the system, which requires for its reproduction the elements which are requisite to the well-being of its victim, and whose morbific action is intimately connected with its organic reproduction."

This point is ably discussed in Dr. Maclagan's monograph

(p. 35), and he puts familiar truths in plain, striking language, with logical terseness, and with a complete knowledge of the subject handled. After the introduction into the animal economy of a poison or of a contagium a definite sequence of phenomena is noticed. Each has a definite and specific action; a certain time elapses after the reception of the substance into the system before morbid symptoms are developed; the severity of the symptoms varies with the dose of the substance introduced; the effects are modified by temperament or constitutional peculiarity on the part of the recipient; and after a time the substance is eliminated from the system. But, the existence of so many definite and distinct contagious diseases proves that the poisons which give rise to them are specifically distinct. " For the explanation of the phenomena which are common to the eruptive fevers," remarks Dr. Maclagan, " we look only to the organism; for the interpretation of those which constitute their specific differences we look beyond the organism to the disease germ."

Now, what are some of the conditions affecting the life of a contagium? The first to suggest itself on reflection is the influence of dilution, gaseous or liquid, on the vitality or infective power of contagium. The amount of air or water with which a contagium should be diluted to render it inert is a point not yet decided. M. Chauveau found that vaccine mixed with fifty parts of water ceased to act with any degree of certainty; sheep-pox virus, after being diluted to one part in a hundred of water was successful, and even when diluted with ten thousand times its weight of water it was not inert. In the next place, with regard to the influence of drying on contagium, the depriving it of one of the essentials for its growth, Dr. Burdon-Sanderson found that mycrozymes and the germinal particles which they produced were rendered inactive by thorough drying, without the application of heat (Appendix No. 5 to the ' Thirteenth Rep. of the Med. Off. of the Privy Council,' p. 61). This observation is confirmed by Dr. Bastian (' The Beginnings of Life,' vol. ii, p. 5). These very meagre results show that here is offered wide scope for experimental research. Not only as an interesting and important point in exact science, but of great value in preventing the spread of contagious diseases, it would be well worth labour and time spent to determine the amount of dilution with air and with pure water sufficient to render a contagious atmosphere or a contagious liquid innocuous. Further, the analytical method of depriving a contagium of one source of nutriment after another, would no doubt assist in solving some of the problems suggested by a study of the life-history of contagium.

The influence of certain physical and chemical conditions on the vitality and activity of some contagia has been pretty fully demonstrated by the experimental researches of various observers. The earliest recorded study of the effect of increased temperatures on the vitality of contagium is Dr. Henry's (Manchester) original article in vol. x (new series) of the ' Philosophical Magazine,' pp. 363-9, dated October 14th, 1831, and his further observations recorded in vol. xi, 1832, of the same journal. He considered it established by his experiments " that vaccine matter is not destroyed by a temperature of 120° Fahr; and it is even probable that it would sustain, without losing its efficiency, a heat several degrees higher;" but it is " rendered totally inert by exposure to a temperature of 140° Fahr." He stated further that the virus of scarlet fever was rendered inactive by a temperature of 204° Fahr. These observations were conducted by Dr. Henry with the object of ascertaining whether or not the heating of raw cotton prevented its actiug as a medium for the introduction of the plague into this country. Hence, they did not proceed further than was necessary to determine the amount of heat which such material could stand without receiving injury, and an amount sufficient also to act as a disinfectant. Many imperfections and errors are noticeable in the conduct of Dr. Henry's observations. This line of enquiry has been pursued much more carefully by Drs. Braidwood and Vacher in regard to the influence of increased temperatures on vaccine lymph.

Their observations were conducted with strict attention to every detail, and the heat was conveyed through water to the contagium particles in their natural moist state. The results obtained were these: Lymph could not be safely exposed without impairment to a temperature above 139°; did not lose all its specific properties at 146°, but its activity was totally destroyed at 149·5° Fahr. Further, exposure for a few minutes was as effective as exposure for two hours.

Our knowledge of the influence of low temperatures on contagium is not even so far advanced as our knowledge of the action of increased temperatures. Fluids containing fungi and bacteroid bodies have been exposed to low temperatures by means of frigorific mixtures; and because certain organisms maintain their vitality for a time surrounded by solid ice, it is inferred that contagia may survive being frozen; but the amount of cold necessary to destroy any of the specific known contagia remains to be discovered. We are equally ignorant as to the effect of sudden falls of temperature on contagia, though such changes are doubtless not less inimical to them than to higher organisms. The only carefully conducted observations on the influence of

very low temperatures on contagium are those of Drs. Braid-
wood and Vacher, published in their second contribution. They
found that the exposure of vaccine in the fluid state for an hour
and a quarter to a temperature of 166° Fahr. below the freezing
point of water did not in the slightest impair the activity of the
lymph.   It would not be logical, probably also not correct, to
conclude from these observations that other contagia possess a
like immunity from the influence of cold.   The marked
differences in the severity and duration of epidemics which have
been noted during almost the entire history of medicine indicate
that atmospheric conditions, as alterations of temperature and
amount of moisture, to affect the vitality and activity of conta-
gium.   It is to be desired and to be hoped that ere long this
problem will also be solved.

   The behaviour of various contagia in the presence of chemical
substances, termed disinfectants or germicides, has been studied
by many observers, and results of considerable importance have
been reached. The knowledge which has been obtained by noting
the effects produced by various chemicals employed in attempts to
arrest the spread of contagious diseases, though of much prac-
tical value, is necessarily unscientific.   Information of this kind
is very plentiful, and is well furnished by such works as
Chevalier's ' Traité des Désinfectants.'   Whatever little direct
evidence we have upon this subject is mainly derived from
experiments with vaccine lymph ; and of the numerous obser-
vations conducted both on the continent and in this country
with the object of elucidating thus some portion of the mystery
surrounding contagium only the most recent need be noticed.
Dr. John Dougall (' Glasgow Medical Journal'), conducted an
extensive series of experiments to ascertain the relative power
of various substances in preventing the germination of animal-
culæ, and the action of certain vapours on vaccine.   He exposed
separate portions of vaccine lymph to various volatile media
under bell-jars for twenty-four hours, after which each portion
was liquefied with glycerine, and its reaction ascertained.
" Vaccinations with lymph exposed to the vapour of carbolic
acid, chloroform, camphor, sulphuric ether, or iodine (the re-
action of the mixture being in each case neutral or alkaline),
were successful, while vaccinations with lymph exposed to the
vapour of sulphurous, of nitrous, of hydrochloric, or glacial
acetic acid, or to chlorine (the reaction of the mixture being in
each case acid), were unsuccessful. Dr. Dougall's further expe-
riments tended to show that carbolic acid, though it suspends for
a time, leaves unimpaired the infective property of vaccine." [1]
   The next series of observations to be noted are those of Dr.

   [1] *Vide* Braidwood and Vacher's ' First Contribution,' &c.

Baxter (" Report on an Experimental Study of certain Disin-
fectants," in the ' Reports of the Medical Officer of the Privy
Council,' new series, No. vi).   The method employed by Dr.
Baxter for the purpose of testing the disinfectant efficacy of
any substance consisted in allowing the reputed disinfectant
to act on matter which is known to have the power of communi-
cating specific disease, and inoculating subsequently therewith.
" The positive or negative result of inoculation," he says, " must
needs furnish a reliable test of the ability of the disinfectant to
fulfil the duty laid upon it."   The only legitimate test of the
effectual disinfection of any virulent substance is its inability,
when introduced into a suitable host or medium, to bring about
the specific train of phenomena which coincide with its multi-
plication.   " The disinfectants," that is, " agents capable of so
modifying the contagium of a communicable disease, during its
transit from a sick to a healthy individual, as to deprive it of
its specific power of infecting the latter "—tested by Dr. Baxter,
were potassic permanganate, sulphur dioxide, chlorine, and car-
bolic acid.   The contagia selected for disinfection were vaccine,
the virus of infective inflammation in guinea pigs, and the virus
of glanders.   The results Dr. Baxter obtained with the first
of these three contagia we shall alone notice ; for the discre-
pancies connected with the others, and the dubitable ground
entered upon complicate rather than simplify the question.
Dr. Baxter examined the action of disinfectants on vaccine
using this contagium both in its normal fluid and in the dried
state, and found that the proportion of potassic permanganate
required to destroy the infective energy of fluid vaccine is
relatively large, " but that, in such excess, it is capable of
doing the work ; " that unless chlorine be added in sufficient
quantity to render fluid vaccine acid, it " has no appreciable
influence in restraining or abolishing its infective power ;" that
one per cent. or less of carbolic acid exerts no influence on the
activity of fluid vaccine, while a proportion of one to two per
cent. renders its action irregular, and two per cent. " seems
enough to destroy its infective power with certainty."   Of his
observations on the action of disinfectants on dried vaccine
Dr. Baxter remarks, " One striking result is manifest, the very
marked superiority of sulphur dioxide to carbolic vapour and
chlorine gas, and this under conditions which seem, above all
others, to render the virulent particles less susceptible to
destructive influences."   " It is sufficiently obvious," he adds,
" that the quantity of chlorine given off into a room from a
basin filled with chloride of lime, or the vapour of carbolic
acid generated in a vaporiser, would be utterly inadequate to
destroy vaccine virus, and by inference, the contagium of small-
pox, when imbedded in a matrix of dried albuminous matter ;

and it is not unlikely that the virulent matters for whose destruction aërial disinfection is employed are commonly protected in some such way."

Very extensive series of experiments to test the influenc of disinfectants or germicides on our typical contagium, vaccine, are to be found in Drs. Braidwood and Vacher's reports already referred to. These observers added the germicide solution to fluid vaccine, and inoculated with this mixture human subjects and heifers. The disinfectants employed were carbolic acid, sulphurous acid (in solution or in fumes), quinine, ozone, permanganate of potash solution (B. P.), chloralum, chlorine (in solution and in fumes), salicylic acid, boracic acid, cupralum, terebene, and feralum. The results obtained were as follows : a solution of carbolic acid required to be some time in contact with fluid vaccine to render it inert ; sulphurous acid, in the fluid or in the gaseous state destroyed the energy of vaccine at once; ozone required some time to produce this effect; chloralum was inefficacious; chlorine seemed to be active, like sulphurous acid, whether applied in the liquid or in the gaseous form, and destroyed the activity of vaccine at once; an aqueous solution of quinine muriate acted efficiently after having been some time in contact with the vaccine; salicylic and boracic acids seem to be innocuous ; but cupralum, terebene, and feralum, rendered fluid vaccine sterile ; and lastly, Liq. Potas. Permangan. (B.P.) appeared to destroy the activity of the lymph in freshly prepared mixtures, but, when the mixture was kept, the vaccine regained its power.

Thus far in our inquiry we have learned somewhat regarding the physical nature and the behaviour of the contagious particles or entities of certain infectious diseases. We shall next endeavour to interpret certain of the phenomena common to all infectious eruptive fevers, by studying another line of research, which has been pursued more especially by continental investigators. "Though each of the eruptive fevers," remarks Dr. Maclagan, "has its own definite history and phenomena, the whole group have certain features in common. Each has a tolerably definite period of incubation. Each has for its most prominent symptoms the existence of that aggregate of phenomena to which we apply the term fever. Each possesses a characteristic local lesion. Each has a pretty definite period of duration. Each, occurs, as a rule, but once in a lifetime." It would occupy too much space to examine these several phenomena separately, as Dr. Maclagan does in his excellent monograph. Suffice it for us to look into and endeavour to explain that aggregation of phenomena included by the term "the febrile state." Have we any definite knowledge, then, of the causation of the high temperature, the quick pulse, the

thirst, the wasting of the tissues, and the increased formation of urea, which characterise that state of the system termed fever.

The view most generally accepted at the present time to explain febrile increase of temperature is that of Professor Virchow, which attributes this phenomenon to increased tissue change. According to this theory, " fever consists essentially in elevation of temperature, which must arise from an increased consumption of tissue, and appears to have its immediate cause in alterations of the nervous system ; " but, as Dr. Maclagan remarks, this theory " serves to indicate rather than to explain" this febrile condition. " Why is there increased consumption of tissue ? and why should the nervous system exercise other than its usual and normal action ?" The reply to these queries would carry us a step further back than the point from which Virchow starts, but even then a constant and essential feature of the febrile state, viz. increased consumption of water would remain unexplained. The late Dr. Parkes, who confirmed and ably elaborated Virchow's theory, endeavoured to account for this last febrile phenomenon by supposing the existence in the blood of some substance which has an unusual attraction for water. He conjectured that this substance "may be some gelatinous compound which is formed in the rapid metamorphosis of the albuminous tissues, and which is ultimately converted into urea and uric acid ;" but, as argued in the monograph under review, " why is this gelatinous compound formed in such enormous quantity as it must be to cause so great a consumption of water ? and what becomes of the urea and uric acid into which it is ultimately resolved ?"

To give a satisfactory reply to such queries as these, which crop up the more closely we study the subject, it is necessary to abandon Virchow's view and seek for an explanation elsewhere. The late Professor Traube, in 1863, ascribed febrile heat, not to increased production, but to increased retention of heat, consequent on contraction of the minute arteries, an hypothesis the inaccuracy of which has been demonstrated since then by Liebermeister and Leyden. Later (1873), Senator has stated " that there are periodic diminutions of loss of heat, together with a constant though not great increase of heat production." The former he attributes to occasional contraction of the minute vessels resulting from the action of the fever-cause, the latter he attributes to increased oxidation. Thus we find one further problem to embarrass the inquiry. As Dr. B. Sanderson remarks (A, Appendix, in the ' Reports of the Medical Officer of the Privy Council,' No. vi, 1875), to explain the nature of fever and its relation to the febrile process two possi-

bilities are open to us, " One is, that fever originates in disorder of the nervous centres, that by means of the influence of the systemic functions the liberation of heat at the surface of the body is controlled or restrained, so that by retention the temperature rises, and finally, that the increased temperature so produced acts on the living substance of the body so as to disorder its nutrition. The other alternative is, that fever originates in the living tissues, that it is from first to last a disorder of protoplasm, and that all the systemic disturbances are secondary. By both hypotheses it is tacitly assumed that fever is the product of a material fever-producing cause contained in the blood or tissue-juice, the morbific action of which on the organism is antecedent to all functional disturbances whatever." This leads us to the same conclusion as that arrived at by Dr. Maclagan, that fever " is a collection of different and various phenomena, all of which are abnormal, and all of which are developed subsequently to the reception of the contagium, and the co-existence of which in the body is conveniently characterized by the term fever."

The contagium being, then, the cause of the fever, becomes the cause of the individual phenomena which constitute the febrile state. To understand our position, it must be borne in mind that each of the minute organisms—contagium particles—reproduces myriads of similar organisms, and in so doing appropriates for their growth elements requisite for the nutrition of the body in which they grow. Here we have a starting-point, from which, by careful logical reasoning, the various phenomena of the febrile condition are gradually evolved by Dr. Maclagan. He shows clearly that the nitrogen essential for the vitality of the contagium particles is derived from the constructive store, the albumen of the tissues, that " this consumption of nitrogen by the contagium particles is the primary cause of the rapid wasting of the nitrogenous tissues which takes place during fever," that the agency which thus eats up the tissues acts also in diminishing their nutritive supply. " Blood continues," he remarks, " to be supplied to and to circulate through the tissues, but it is blood charged with an organism which utilises for its own ends the materials which ought to go to nourish and build up the body." Moreover, not only do the contagium particles use up an excessive amount of nitrogen in their growth and deteriorate the blood, but they also consume water in large quantity, thus deranging nutrition and diminishing the bulk of the tissues.

Further and very valuable evidence in favour of this explanation of the most important phenomenon characterising fever Dr. Maclagan derives from Salkowski's observations on the elimina-

tion of potass and soda during fever. Salkowski found " that the quantity of potass eliminated on a febrile day was three or four times, sometimes as much as seven times, greater than on a non-febrile day ;" and that " the soda discharge was reduced to a minimum during the febrile state, and rose again when the fever had ceased." Now, potass exists in largest quantity in the muscles and blood-corpuscles, while the chief seat of soda is the liquor sanguinis. The contagium particles, then, according to the experimental evidence quoted, consume the nitrogen, water, and potassium salts of the constructive store albumen found in muscle and blood ; and this affords a full explanation of the wasting and irregular elimination of urea which characterise the febrile state. According to Dr. Burdon-Sanderson (ibid. cit.) clinical observations show—

" That in the early stage of fever a patient excretes about three times as much urea as he would do on the same diet if he were in health ; the difference between the fevered and the healthy body consisting chiefly in this, that whereas the former discharges a quantity of nitrogen equal to that taken in, the latter wastes the store of nitrogen contained in its own tissues."

It is not necessary to apologise for having so fully discussed the preceding very important known facts regarding contagium, but it would be unprofitable to pursue the subject further.[1] The extracts quoted show the exhaustive and trustworthy method pursued by Dr. Maclagan in his monograph ; the observations we have cited enable the reader to estimate the very important advances which have been made during recent years in our knowledge of contagium, its nature, and its behaviour. We have now only to desire that ardent, truth seeking investigators will not hereafter be wanting to work patiently, industriously, perseveringly ; their noblest reward will be the diminution, through their efforts, of that greatest scourge of human life—the class of diseases induced by contagium.

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[1] The hypothesis we have found to account for the wasting of the tissues applies also to the other phenomena of fever. The propagation of contagium particles in the tissues induces an excitement of the normal process of nutrition in these tissues ; this implies a greatly increased demand for blood, which in its turn is met by a general hastening of the blood-stream in the minute vessels, and necessarily increased frequency of the heart's action. Now, such a disturbance of equilibrium of necessity affects the power of tension in the machinery implicated, and clinical observation has long ago demonstrated that one of the principal causes of death in febrile attacks is weakness of the cardiac muscle. Further, the heart has not only a great strain thrown on it, but it participates with other viscera (especially, as we have seen, the muscular viscera) in the deprivation of material necessary for its nutrition. " We accordingly find that, with a large propagation of the contagium, or, in other words, in a severe attack of fever, the heart's action is not only rapid, but, after a time, feeble," and that malnutrition often gives rise to alteration in the cardiac structure.

Bibliographical Record.

United States Medical Library.[1]—These works deserve notice in this Review owing to their size and importance; they give at the same time an opportunity of recording what progress has been made in the attempt to form a complete collection of medical literature in the United States. Dr. J. S. Billings, to whom the medical profession will ever owe a deep debt of gratitude for his wonderful labours, justly says—

" Comparatively few persons have any idea of the amount of medical literature in existence, or of its proper use and true value, and the result is that the same ground is traversed over and over again. . . . . The record of the researches, experiences, and speculations relating to medical science during the last four hundred years is contained in between two and three hundred thousand volumes and pamphlets; and while the immense majority of these have little or nothing of what we call ' practical value,' yet there is no one of them which would not be called for by some inquirer if he knew of its existence. Hence it is desirable, in this branch of literature, as in others, that in each country there shall be at least one collection embracing everything that is too costly, too ephemeral, or of too little interest to be obtained and preserved in private libraries."[2]

The National Medical Library, which was formerly called the Library of the Surgeon-General's Office, though formed within the last twelve years, now numbers about 40,000 volumes and 40,000 pamphlets. It is comparatively full in American, English, French, and German medical literature of the present century, and in works relating to surgery, pathological anatomy, and hygiene. It possesses a few valuable manuscripts, the oldest of which is a fine copy of the ' Lilium Medicinæ' of Bernard de Gordon, dated 1349.

A very considerable approach has thus been made towards the

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[1] 1. *Photographs illustrating rare books in the National Medical Library (International Exhibition of 1876, Medical Department U. S. Army).* Two vols., fol. Philadelphia, 1876.

2. *Catalogue of the Library of the Surgeon-General's Office, United States Army.* Three vols., 4to. Washington, 1873-74.

[2] Public Libraries in the United States, chap. vi, p. 171. The reader will

ideal in this short period, and a medical library has been formed which may justly be compared with any in Europe in extent. It is, perhaps, superior in one respect—I mean, in its collection of periodicals, for of the 10,736 volumes extant, 8214 have been already procured.[1] It is, also, very rich in inaugural dissertations, a class of literature important to the librarian, and the value of which is usually underestimated. To obtain complete series of these is even more difficult than to get journals, for the reason that they are more ephemeral, and because it is scarcely possible to ascertain what have been published or when the series may be considered complete. For a few schools, lists have been published of the theses presented by their graduates, such as Paris and Edinburgh, but even for Edinburgh, the only catalogue of the theses which the writer has been able to obtain does not show when the regular printing of all theses ceased. Callisen has been led into error in this way in his otherwise very complete Bibliographical Lexicon, in which he gives the titles of many theses which were never printed, notably of the universities of Pennsylvania and Transylvania. The value of these theses is fourfold. As material for the history of medicine they may be taken to represent the theories and teaching of the school; they often contain reports of cases, or accounts of investigations made by the student under the direction of a professor, which are of much value, and they are necessary to medical biography, the more so as in most of the German universities a sketch of the life of the candidate is appended to the thesis. In addition to this, prior to the era of medical journalism, it was the custom for the president or one of the professors to add an introduction of ten or twelve pages to the dissertation, treating on some subject usually having no direct relation to the thesis, and forming the sort of paper which would now be sent to a medical journal.[2]

In so large a collection there must be many books that are uncommon and some that are decidedly rare. Specimen pages of some of these have been reproduced by photography, and such

find in this chapter a very interesting account of medical libraries. Further reference may be made to the chapter contributed by Dr. Billings to "A Century of American Medicine," Phil., 1876, in which there is much statistical and other information, not only as to medical libraries, but as to books printed in the United States, medical journals, societies, and schools. His remarks on medical museums might be applied to many collections in this country with good effect. "The practical value of large special museums in connection with good libraries devoted to the same specialities is great, but they are useful rather to the educated physician than to the student; and the numerous small collections which are scattered over the country in hospitals and private cabinets, are simply so much wasted and unused material, in a scientific point of view, and, though gratifying to the owner as trophies or mementoes, are of little more real use than the strings of teeth which the barbers of old hung out as signs of their skill."

[1] Loc. cit., p. 177.
[2] Loc. cit., p. 178.

copies form the two volumes quoted at the head of this article. The photographs in all number 104, and may be considered as consisting of three series. The first is taken from early printed books and from other works which are rare at least in the United States. The second relates to surgery and especially military surgery; and the third series is from rare books and pamphlets connected with American medical history. We may mention the subjects of a few of these as examples. No. 1 is a specimen page from the manuscript of the ' Lilium Medicinæ ' already mentioned. No. 8 is the title-page of the 'Regiment der jungen Kinder,' printed at Augsburg in 1473. No. 15 is the first page of the 'Artzneibuch ' of Ortolff von Beyrland, printed at Augsburg in 1479. No. 16 is the first page of the first edition of the surgery of Petrus de la Cerlata, printed at Venice in 1480; the page in the original is adorned by illuminated letters. No. 28 is the title-page to the celebrated tractate on syphilis by Grunpeck de Burckhausen. No. 29 is the first page of the treatise on the same disease by John Widman, 1497. Nos. 49 and 50 contain a copy of an autograph poem by Philip Melancthon, ' De consideratione humani corporis,' written on the fly-leaf of a copy of the first edition of Vesalius, ' De humani corporis fabrica.' No. 71 is the title-page of the first edition of Harvey's ' Exercitatio anatomica de motu cordis et sanguinis in animalibus,' published at Frankfort in 1628.

The catalogue of the library is in three volumes quarto, and was published at Washington in 1873-74. The first two volumes contain the works of known authors arranged alphabetically according to the author's name. The titles are very fully copied; the number of pages and often of the plates is mentioned. The paper and printing are excellent, and though I have found here and there an error in a title, I can testify to the very great care taken in correcting the press. I hope that supplementary volumes of this catalogue will be published from time to time, and would suggest that from any such continuation non-medical works should be omitted, such works for example as H. L. Abbot on siege artillery; simple reprints of papers from medical journals should likewise be rigidly excluded. Works published in a collection should be entered under the title of the collection, with only a cross-reference under the author's name (*e. g.* the three works in the catalogue under "Actuarius" should be under "Medicæ artis principes," with merely a reference at "Actuarius"). Lastly, journals or collections should never be entered under the editor's name.

The third volume contains in four divisions the anonymous works, transactions, reports, and periodicals. The anonymous works are arranged under the name of the subject to which they refer; the transactions are distributed according to the countries and towns; the reports are classified according to the subjects; the

periodicals according to countries alone. In my opinion this volume should be cancelled and replaced by one containing all these works in a single series, the titles being arranged alphabetically according to the rules in use for anonymous books (or, better still, be included in the supplementary volumes just suggested). On the other hand, I consider the mention made of journals not in the library by titles printed in small type most useful.

About a year ago a specimen fasciculus of a fresh catalogue of this library was issued, "in order to show the character and scope of the collection, to obtain criticisms and suggestions as to the form of catalogue which will be most acceptable and useful, and to furnish data for the decision as to whether it is desirable that such a work should be printed and distributed."

The specimen submitted is of a combined catalogue of subjects and authors, arranged alphabetically in a single series. It contains also references to articles in journals, transactions, and collections, arranged according to the subjects, and printed in small type. I must preface the following remarks by saying that a careful examination of the specimen soon after its publication[1] caused me equal pleasure and surprise. The execution of the work was admirable. I was charmed with the accuracy and surprised by the industry shown in every page. Many doubts, however, arose in my mind as soon as the first feeling of delight had passed, and the result of some further consideration and research was a conviction that the plan proposed would not be the best in every respect.

The first and most important question is the size of the proposed work. It is stated in the preface that " the complete catalogue on the present plan would make five volumes of about one thousand pages each." Is this correct? The seventy-two pages of the specimen correspond to about ten pages of the former alphabetical catalogue, and there are in the two volumes of authors' names 2147 pages, so that, assuming the same proportion to hold good, about 15,000 pages would be needed, or three times the estimate. This would probably be under, rather than over, the amount of space required, owing to the many additions still to be made to the references in the specimen. The same conclusion appears certain from an examination of the number of entries. There are of them about 55,500 in the three volumes of the former catalogue, and 300 fresh entries correspond to 200 old ones in the specimen (more exactly 327 to 218), so that with cross-references (say 5000) there would be at least 142,500 entries under authors' names in the complete work.

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[1] The substance of these remarks was given in a MS. review which I sent to Dr. Billings at that time. I insert them here because the profession throughout the world is interested in the success of his undertaking, and because in the many notices of this specimen in the journals no attempt has been made to examine it critically and no suggestion has been offered for its improvement.

Each entry must be repeated once or more under the respective subject. We should have thus far a total of 285,000 entries. A page in the specimen contains about forty entries, so that 7125 pages would be occupied. There are in addition thirty-two out of the seventy-two pages in the specimen filled with references to journals, and for these 5700 additional pages would be required (= 32 of references to 40 of authors). This would make a total of 12,825 pages. We have still to add many pages for additions and especially additional references; indeed, if the latter are to be moderately complete, they must be very much extended. I, for my part, after comparing the references given with others, think it would not be safe to estimate the additional pages so required at less than 6000. The conclusion is that the complete catalogue, printed as the specimen, would occupy more than 15,000 pages, or say fifteen to twenty very thick volumes. It is scarcely necessary to point to the cost and other objections in order to prove the desirability of diminishing the size if possible.

Now, the utility of the three portions, which form by their union this specimen, is very different. A catalogue of books arranged according to authors' names is absolutely necessary for the officials of a large library for the ready finding of the books demanded by readers, and, indeed, in all cases to determine at once whether a given book is in the library. For this purpose it must be complete, and such completeness is usually and most readily effected by successive manuscript additions; in a rapidly increasing library a printed catalogue a year old is already out of date. Such a catalogue is also, indeed, of occasional assistance to the few who are interested in medical bibliography. All this has, however, been provided for to a large extent in the two volumes described; to reprint the titles contained in them would add to the expense and size, but in no considerable degree increase the utility of the new catalogue. I must repeat that I shall be glad to see what has been so well done in respect to an alphabetical catalogue supplemented from time to time by similar volumes.

The list of books arranged according to their subjects would be of service to a much larger number, indeed in some degree to all who can refer to a large medical library, but chiefly to those who have ready access to Washington. Finally, the references to periodical literature would probably, if not in too cumbrous a form, be used by almost all readers, in fact by all who can refer in a private or public library to one or more sets of journals. I will not attempt to decide whether it would be better to combine the two latter parts, but will suggest as worth consideration whether the index to periodical literature would not be more serviceable if divided into parts according to the language. The great majority of medical men read but one language;

even those, who can read more, rarely have access to the books themselves. Be that as it may, I should urge that if such an index is to be reliable and permanently of use, it must be exhaustive for the period chosen; the references should include all original papers and cases, and a list of the journals, transactions, or collections, indexed, should be appended. Any attempt at a selection of papers would be fatal.[1]

In conclusion I warmly congratulate the profession of the United States on their good fortune in possessing so able a bibliographer as Dr. Billings, who has the intellect to appreciate the importance of a great public medical library and the enthusiasm and diligence sufficient to overcome the endless difficulties met with in the formation and management of such a collection. Surely his efforts and his great success will have a reflex action on European, especially on English, librarians. Nowhere are there such numerous opportunities for buying rare and curious books and manuscripts, as in the London auction-rooms, yet no use seems to be made of them by any of the London medical libraries at the present time.

<div style="text-align: right">Thomas Windsor.</div>

Works of Paracelsus.[2]—Every one knows that the knowledge of history in general has been steadily advancing in the present century by the publication and careful investigation of the statements found in original documents, state-papers, accounts of eye-witnesses, and the like. A similar study of the history of medicine would inevitably clear up many disputed questions and correct many erroneous assertions. In many cases, however, a preliminary examination of the writings themselves is required to show how far they can be admitted to be genuine. This is what Dr. Mook, who intends to publish hereafter a biography of Paracelsus, attempts in the 136 quarto pages of his volumes. He commences with an introduction, in which he quotes many authors to show how various and contradictory have been the characters given of Paracelsus at different times and by different authors; he was a pious man and a blasphemous heretic, a cabalist and a charlatan, a reformer of medicine, a homœopath and magnetiser, the introducer of the German language into scientific medicine, a giant who destroyed authorities and doctrines universally received, and, again, a half-witted fanatic. Little is known with certainty about his name, his life, or his works. It would almost seem as if, in regard to this man, who inaugurated the transition from the medicine of the middle ages, we had no history; histories indeed!—history not.

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[1] I may mention that, many years ago, I offered the New Sydenham Society to prepare a complete index to all British medical journals up to the year 1860 (see also this Review for Jan., 1863, p. 88).

[2] *Theophrastus Paracelsus.* Eine kritische Studie von Fr. Mook. Würzburg, 1876.

The reason of this is to be found in the present condition of his works, in the wilderness of writings published under his name, and which yet require critical examination.   Dr. Mook counts six editions of the collected works, three German and three Latin, and 247 separate works in all, most of which appeared between 1561 and 1578, many years after the death of the supposed author.

Marx was the first who endeavoured to point out certain marks by which the genuine books could be recognised, but unfortunately the criteria proposed by him are, according to Dr. Mook, quite valueless.   Dr. Mook lays down the following propositions :

Genuine are—
1. The original manuscripts.
2. The works published by Paracelsus himself during his life.
3. These, being compared with Huser's edition, must decide whether Huser's statements are worthy of belief.
4. If this point is settled in favour of Huser, we must recognise as genuine all writings stated by him to have been printed from a copy in the handwriting of Paracelsus.
5. The writings which, compared with those so far recognised to be genuine, bear clear signs of his authorship both in form and contents.

Dr. Mook has been unable to find any of the original MSS., though he expects that some of them at least will be recovered.   He has, however, found eleven works published during the life of Paracelsus, of which three appeared in a second edition during the same time ; six of the eleven contain predictions, the remainder are medical. Now, after comparing these with Huser's text and his statements about the sources he drew from, the author concludes that there is no reason whatever to question Huser's truthfulness in any respect, so that all must be attributed to Paracelsus, of which Huser states, that he takes it *ex manuscripto Theophrasti*.   The fifth and last point is left to the decision of future critics.

A large portion of this essay is occupied by a detailed account of all the works and editions with which the author has met.   In addition to his account we may mention that some seven of the works in question are also in the library of the Manchester Medical Society, and among them a manuscript copy of the 'Tractatus de peste cum commentariis Jobi Kornthaueri,' which is dated 12 Maii, 1613, and is probably an original. There is also in the Chetham Library, at the same place, a manuscript translation from the German of Huser into Dutch, which has, we believe, never been printed.   An edition of another work is in our hands, which differs somewhat from those mentioned by Dr. Mook; its title is 'Prognosticatio ad vigesimum quartum usqȝ annum duratura, per eximium dñm ac Doctorem Paracelsum, ad illustrissimum ac potentissimū principem Ferdinandum, Roman Regem semper Augustum, &c., Archi-

ducem Austriæ, &c., conscripta. Anno xxxvi.' Then follows the
" Marcus Tatius ad lectorem," as in the edition No. 10 (p. 27), and
the " Cum gratia," &c., except that " ne quis imprimat " takes the
place of " ut quis imprimat." Twenty-four quarto leaves without
pagination. No date, place, or name of printer.

We have read this work with much pleasure, and hope that the
author will soon favour us with his promised biography.—T. W.

Catalogue of the Radford Library.[1]—The library contains about
3400 volumes, among which there are many consisting of tracts
bound together. Formed by Dr. Radford, it was given by him in
1853 to the St. Mary's Hospital, and he has since continued to
enrich it by many additions.   He has placed an endowment fund in
the hands of trustees, which will become available after his death.
Works on obstetrics and the diseases of women and children predo-
minate, and we think it would be wise to confine further purchases
to these subjects.   Glancing over the catalogue we miss many books
which ought to be found in such a collection; such are Siebold's
History of Midwifery, the old but still useful bibliographical work
by Osiander, the very curious collection by Schurigius, the sperma-
tologia, the gynæcologia, the syllepsilogia, the parthenologia, and
the muliebria. We should like to see, too, tolerably complete
series of the editions of some of the early books, such as Raynalde's
woman's book, of which there appear to have been ten or eleven
editions, extending for more than a hundred years after 1540, the
date of the first edition.   The woman's book was itself a translation
from the Latin edition of (Rhodion) Rösslin's ' Der swangern
frawen und hebammen rosegarten ;' of the latter a copy of an early
edition, perhaps the first, is in the library of the Manchester Medical
Society.   Raynalde's book is usually supposed to have been the
first treatise on this subject published in English ; we have, how-
ever, the impression that some twenty years earlier another work
had appeared in London. Though writing from memory, and
without any opportunity at the present moment of confirming our
impression, we mention this all the more because Dr. Aveling, in
his very careful and interesting book on English midwives, quotes
no " book in the vulgar tongue" before this translation.

We may notice that many reprints of papers from such publica-
tions as the ' Obstetrical Journal ' are entered in the catalogue ; in
our opinion they should be almost invariably excluded, as serving no
useful purpose.

A few rare books are here to be found ; the works of Louise
Bourgeois ; the translation of H. van Roonhuyse, ' Some observa-

tions and practices relating to some extraordinary cases of women in travel;' Chaupin's theses, which are said to have been suppressed, ' De partium externarum generationi inservientium in muliebribus naturali, vitiosa, et morbosa dispositione;' a Latin edition of Rousset, ' De partu cæsareo', his ' Dialogus apologeticus pro cæsareo partu,' and a manuscript copy of his ' Responsio ad Jacobi Marchant declamationem;' there is of Marchant's work the De-clamatio 1a et 3a (did the second ever appear?); a French translation of Rhodion, published at Paris in 1536 ; S. Roulliard, ' Capitulaire auquel est traicte qu'un homme nay sans testicules apparens, et qui ha neantmoins toutes les autres marques de virilité, est capable des œuvres de mariage;' Tagereau, ' Discours sur l'impuissance;' and Wolveridge's ' Speculum matricis.'

The books have been arranged, and the catalogue has been pre-pared, according to the methods in use at the library of the Man-chester Medical Society, by Mr. Cullingworth, who deserves much praise for the accuracy and care everywhere displayed.   The printer, Mr. Alcock, has also done his part well.—T. W.

Spender on Bath Waters.[1]—A book of this kind deserves the especial patronage of the profession, because it differs from most medical works in dealing largely with the experience of the past, which in modern days we are apt to forget.   It offers many in-teresting glimpses of the medicine of former days, and furnishes amusing accounts of the habits and the ways in successive genera-tions of society at the Bath, as it was called *par excellence*.

The cures wrought in old days were as real as any that are wrought now.   Of the *rationale* of the cures we do, perhaps, know a little more than formerly ; but after all it is only a little more— much still remains to be explained.

There are various oversights and omissions in the work to which we could easily point ; for instance, Dr. Spender tells us that Tissot spoke of a bath in the Valois where people spent most of their time in the bath.   Immediately after, Dr. Spender says that this is the case even now at Leuk, not seeming to know that it was the place alluded to by Tissot.

But we shall not enter into the ungracious task of minute cri-ticism, and can say with every justice that a great deal will be found in this work which will be useful to practitioners who think of sending patients to use the waters of Bath, or even to reside in that city, as also a great deal that is interesting to all persons of an antiquarian turn of mind.

We cannot but hope that this book will help to draw attention to waters which, notwithstanding admirable bath arrangements and

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[1] *The Bath Thermal Waters, Historical, Social, and Medical.* By JOHN KENT SPENDER, M.D.   8vo, pp. 300.   London, 1877.

the comforts of the city in which they are situated, are by no means appreciated as they once were or as they deserve to be.

The appendix to the work gives an ample account of the characteristics of the climate of Bath, which was undoubtedly a desideratum.

Myrtle on Harrogate Waters.[1]—It is satisfactory to learn that many of our mineral waters still keep up the number of their visitors in spite of foreign competition. Harrogate and Buxton are crowded every season, and the physicians at these places occasionally favour us with the results of their practice. This is not the first time that Dr. Mrytle has given us some account of his large experience. His present work contains a good deal of shrewd, practical observation, and, as he hopes, may serve as a sort of guide to medical men in selecting patients for Harrogate. But if the book be intended for medical men chiefly, we do not see why it should have been written in so familiar, we might almost say so slang a style. Indigestion, affections of the liver and jaundice, gout and rheumatism, and some nervous derangements, but above all skin diseases, are the affections most usually considered fit subjects for Harrogate, and are the ones which are chiefly and very lucidly treated of by Dr. Myrtle. Perhaps something might have been added on habitual constipation and hæmorrhoids, a class of patients that ought to profit by Harrogate. It is a little surprising to find that Dr. Myrtle, though essentially a practical man, is full of theories. He tells us that sulphur waters " burn up useless fat," and finding that there is no complete explanation of the operation of Harrogate waters (as of many of the ordinary articles of the Materia Medica for that matter), he tries to explain an *ignotum per ignotius,* and calls in certain possible electric action of rubidium and cæsium which have not as yet been discovered in them, and which possibly never may be found. We also do not see the object of his classing together waters of such different qualities as Carlsbad, Strathpfeffer, and Harrogate, Püllna, and Friedrichshall, even if he did not convey the idea that he supposed Püllna and Friedrichshall to be places to which patients resort or even have resorted. Notwithstanding such drawbacks, the book may be consulted by practitioners with advantage.

Philadelphia Exhibition.—Medical Objects.[2]—Since the date of the Crimean war great care has been bestowed in all the armies of

[1] *Chronic Diseases best fitted for Treatment by the Harrogate Mineral Springs.* By A. S. MYRTLE, M.D. London, 1876. 8vo, pp. 92.

[2] 1. *International Exhibition of* 1876. Philadelphia, 1876. 2. *Description of the Models of Hospital Cars.* 3. *Description of the Models of Hospitals.* 4. *Description of the Models of Hospital Steam Vessels.* 5. *Description of Perrot & Co.'s Improved U. S. Army Medicine Wagon.* 6. *Description of the U. S. Army Medical Transport Cart Mode of* 1876. By D. L. HUNTINGTON and G. A. OTIS, Assistant-Surgeons, U. S. Army.

Europe on hospital arrangements in the field, and on the systematic supply of medical stores and comforts. A further impulse was given by the German and still more by the Franco-German war, and by the various charitable societies whose efforts were called forth by them, and were devoted to providing medical men and nurses and hospitals to supplement the regular services of armies.

In England the subject has not been neglected. A committee was appointed, which sent in a valuable report in 1868, and, ever since, the medical authorities have had under their careful consideration how to supply the best ambulances and modes of transit for sick and for medical stores. Medical officers, who formerly could learn such duties only by experience in the field, are now systematically instructed in them at Netley, and we believe at Aldershott, and thus can enter on a campaign with some distinct ideas as to their duties in the field.

The great question has always been how to provide ambulances and waggons that are at once substantial and not too heavy.

In endeavouring to solve this question our American brethren appear to us to have applied their native ingenuity to good purpose, but our notice of what they have done must, unfortunately, be very brief, and therefore imperfect.

One of the reports treats of the best methods of adapting the ordinary rolling stock of American railways to form conveyances for the sick. We observe that the cars are made to rest on semi-elliptical plate springs; while in the ambulances, after an examination of the different kinds of springs in use, india-rubber springs were adopted as having great advantages over steel.

In the American war the medicine waggon, though strong and most convenient, was too heavy, and required four horses or six mules to drag it. A lighter one has now been invented, though it has not been used in the field. It weighs, including fixtures and all supplies, 2630 lbs. It seems to be a very complete article. Besides the medical stores inside, which are so arranged that compounding can be done under shelter, and that almost all the whole back half of the waggon is left empty for the use of the surgeon, the waggon has strapped on the inside two camp stools. On hooks attached to one of the waggon bows are three lanterns. On the outside are four hand-litters, and under the waggon the amputating table is attached.

The lighter or medical transport cart has not yet been tried in the field, but appears to us to be singularly convenient. It contains three boxes, a medical, a surgical, and a mess or medical-comfort chest. The weight of the cart is only 420 lbs., and when fully equipped, counting the driver, the weight does not exceed 1220 lbs.

But perhaps the portion of these reports that is most interesting to us as a maritime people is the account illustrating the plans

actually employed during the war of 1861–5 for adapting the ordinary steamboats of the rivers and the merchant steamers of the coast to the transport of sick and wounded soldiers.   The following extract conveys some valuable hints for adding to the comforts of our steam-vessels in the tropics :

" A large quantity of ice was carried in the hold of the river steam-vessel ; this was taken advantage of by an arrangement of pipes to convey ice water to the different parts of the hospital.   A tank was placed on the upper deck forward, connected with the steam-pump, and kept constantly filled with water ; pipes from this ran down into the hold of the vessel to a coil embedded in the ice, whence the cold water made its way through pipes to the several parts of the boat where it was required.   This worked admirably, giving all the hands plenty of ice water, and with great economy in the use of the ice.

A fan ran through the whole length of the main ward, worked by the machinery below.   It made about twenty-nine revolutions a minute, and as the transom-windows opened just above it at the sides it created a pleasant current of air, and had besides another effect, that was not anticipated at the time when the fan was ordered, viz. that it drove out all flies and mosquitoes."

There are many hints to be gathered by us from the models exhibited at the Philadelphia Exhibition.

Wythe's Microscopist.[1]—This work, the third edition of which we have for review, claims in its second title to be ' A Compendium of Micro-mineralogy, Biology, Histology, Micro-chemistry, and Pathological Histology.' This somewhat extensive range of subjects is discussed in about 250 not at all closely printed pages, space being still left for explanations of the bearing of microscopical science on the progress of the arts, commerce, and theology.   As a manual of histology, which might serve to teach a student either structural facts or methods of manipulation, this book is by the mere fact of its size foredoomed to failure, and we prefer, therefore, to look on it as an essay on the scope and importance of the microscope in all departments of science and art.   After an introductory chapter, in which the praises of the microscope are sung in no measured terms, the optical principles on which the simple and compound instruments are constructed are fully explained.   Those troublesome luxuries, the several microscopic accessories, are next shortly enumerated, and the preliminary part of the book ends with a chapter on the use of the microscope and the avoidance of some of the common errors of observation.   The ground having been thus far cleared, the next chapters are devoted to the methods of exa-

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[1] *The Microscopist, a Manual of Microscopy and Compendium of the Microscopic Sciences.* By T. H. WYTHE, A.M., M.D.   Third edition.

mination and preservation of objects. Here we have found nothing that may not be read far more carefully described in books published several years ago, and notably in Beale's ' How to Work with the Microscope,' to which Dr. Wythe seems largely indebted.

Nothing can be more bald than the manner in which these "modern methods of examination" are explained, and a single example will be sufficient to show the uselessness of such loose descriptions.

"Nitrate of Silver Fluid.—Fresh membranous tissues exposed to 0·5 to 0·2 p.c. solution of nitrate of silver, washed and exposed to the light, often show a mosaic of epithelium, &c.

"Chloride of gold.—The solution should be similar to that of nitrate of silver. Exposure to light stains the nerves, &c., a violet or red colour."

The " et cetera," here so lightly used, seems to us to cover a multitude of sins.

The microscope in geology and in chemistry forms the text for a couple of short sections containing a very fair account of the application of the microscope to these sciences. In " The Microscope in Biology" Beale is again closely followed, even to his illustrations ; while the chapter on vegetable histology is a very pale reflection of Carpenter, with the addition of a classified list of Diatoms taken from the ' Micrographic Dictionary.'

It is hardly worth while to consider in detail the remainder of this book, which is occupied with zoology and animal histology, for the complete absence of detailed information makes it valueless to any one wishing to gain the veriest smattering of the subjects. We cannot, however, conclude without noticing the illustrations, or, as they are here called, "plates." In these we have not been able to find a single original drawing ; most have been taken from the first edition of ' Carpenter's Microscope,' published a quarter of a century ago, and it excites in us almost a feeling of indignation to find these time-honoured old woodcuts coming out again in all the glory of hot-pressed paper and coloured inks.

Tibbits on Medical Electricity.[1]—This second edition of Dr. Tibbits' well-known work has been amplified somewhat and improved by the incorporation of experience gained by the author since the appearance of the first edition. Dr. Tibbits has attached his name to certain batteries, and we trust this is not a symptom that practitioners who employ electricity are about to imitate the tactics of obstetricians who employ pessaries. "Tibbits'" batteries are only modifications of others which are well-known. Thus the " Tibbits' " voltaic battery is composed of Leclanché elements with collector and commutator not materially differing from those which have been

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[1] *A Handbook of Medicine and Surgical Electricity.* By HERBERT TIBBITS, M.D. Second edition, revised and enlarged. London, 1877.

long in use. In fact, the commutator might be altered with advantage, as it is inferior to the barrel commutator in not having a neutral point. The only real innovation in the "Tibbits'" consists in a piece of wood which prevents the lid of the battery from being fastened while the poles are in "contact." This is, though useful, a small matter and hardly of sufficient importance to warrant the impression of the inventor's name inside the lid of each box.

International Otological Society.[1]—A list of members of the society and the minutes of the first meeting are followed by a report on the progress of otology in 1875-1876, divided into two parts, one by Dr. C. H. Burnett on anatomy and physiology, the other by Dr. C. J. Blake on pathology and therapeutics. There are also the following papers and cases :—Aspergillus glaucus in the tympanum, primary acute periostitis of both mastoid processes, a case of exostosis of the external auditory meatus drilled out by the dental engine, test-sentences for determining the hearing power, two cases of acute inflammation of Schrapnell's membrane, a comparative sketch of the early development of the ear and eye in the pig, together with a new account of the development of the meatus externus, drum, and Eustachian tube, a case of perforation of the membrana tympani with scarcely a symptom of disease, one of remarkable vertigo following an injury of the middle ear, on gaseous interchange in the tympanic cavity, paper dressings in the treatment of perforations of the membrana tympani, a case of abscess over the mastoid region extending to the squamous portion of the temporal bone and involving the brain without harm to the auditory apparatus, syphilis of the cochlea, hyperostosis of the mastoid. Many of the cases are very instructive, but none has interested us so much as that in which an exostosis nearly occluding the meatus was drilled out by Dr. A. Mathewson so successfully that at the last report the meatus was almost restored except at one point, where a thin remnant of the exostosis projected from the anterior upper wall : the posterior lower part of the membrana tympani could be seen. There was no irritation or unpleasant symptom of any kind, and the hearing had risen to nearly the normal standard. The author remarks that—

"In comparison with any of the methods employed in the cases collated, that with the dental engine seems to me the best, as being less tedious, less dangerous, and more effective. That it is less tedious, a few tests of this and any hand-drills in perforating dense bone will demonstrate. It is less dangerous for the reason that, with the rapidly revolving drills, perforations can be quickly made with so slight a pressure that there is little risk of injuring the

[1] *Report of the First Congress of the International Otological Society, New York, September,* 1876. Published by a Committee composed of C. J. KIPP, A. MATHEWSON, J. S. PROUT, and J. D. RUSHMORE. New York, 1877.

deeper parts of the ear, or the walls of the meatus, by the slipping so likely to occur in operations with hand-drills or with hammer and chisel. The instrument is also held between the thumb and fingers in such a way as to interfere to a less degree with the illumination and inspection of the meatus during the operation. The drills and burrs can also be used, not only to perforate with their points, but also to enlarge openings already made, to any desired extent or direction, by lateral pressure with their sides. Other uses for the dental engine in surgery readily suggest themselves. There is a great variety of burrs, drills, and saws—more than three hundred in all—figured in the manufacturer's catalogue, which can be attached to the engine, and would be of great assistance in operations on bony growths occurring elsewhere, like those of the orbit, in some cases of resection, and in any case where there is small space to employ the ordinary instruments effectually without endangering the neighbouring parts."

The form of engine known as Elliott's suspension engine was used in this case.

We may safely recommend, though any commendation can scarcely be required after the list of papers which we have quoted, this publication to all aural surgeons. The society will hold its next meeting at the same place as the international ophthalmological congress (Heidelberg, in 1880), and the provisional committee consists of Prof. R. Voltolini, chairman, Prof. A. Politzer, Prof. S. Moos, secretary, and Dr. N. Löwenberg.

System of Ophthalmic Medicine.[1]—This great work, in seven closely printed volumes, is near its conclusion, a part of one volume alone being yet wanting. The contents of the volumes are as follows:

I. Macroscopical anatomy, by F. Merkel; microscopical anatomy, by W. Waldeyer, A. Iwanoff, J. Arnold, and G. Schwalbe.

II. Development and malformations, by Manz; comparative anatomy, by R. Leuckart; circulation and nutrition of the eye, by T. Leber; physiological optics, by Aubert.

III. Functional examination of the eye, by Snellen and Landolt; operations, by Arlt.

IV. Diseases of the conjunctiva, cornea, and sclera, by T. Saemisch; diseases of the eyelid, by Michel.

V. Glaucoma, by H. Schmidt; affections of the lens, by O. Becker; diseases of the retina and optic nerve, by Leher.

VI. Abnormal states of mobility, by A. Graefe; abnormal states of accommodation and refraction, by Nagel; diseases of orbit, by Berlin.

VII. Diseases of lacrymal organs, by Schirmer; relations of ophthalmic to general diseases, by Förster; history, by Hirsch.

Like all works written by a number of authors, the articles are

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Handbuch der Gesammten Augenheilkunde. Redigirt von Prof. A. GRAEFE und Prof. T. SAEMISCH. Leipzig.

unequal ; all are good, but some better than others. The portion
on comparative anatomy might well have been omitted, and that on
the examination of the eye considerably extended. It would be
more convenient to have the bibliographical lists together in the
same volume. A chapter on the dissection of the eye, on its micro-
scopical examination, and on the preservation of specimens, would
be of value. The reader may remember that the portion by Prof.
Arlt on operations has been already noticed in this Review. For the
rest we need only say that, owing to its fulness and the large
amount of original matter, the work will for many years be most
useful to every ophthalmic surgeon for study or occasional reference.

Magnus on Cataract.[1]—The author, who is already well known as
an ophthalmic surgeon, and who has for years paid much attention
to the history of ophthalmic medicine, gives in this work a most
careful account of the successive opinions and discoveries in respect
to the pathology and treatment of cataract. He justly remarks
that, whilst ophthalmology has been rendered in recent years one
of the most exact branches of medicine by the labours of men like
Graefe, Donders, and Helmholtz, the critical study of its history
has been much neglected. This is to be regretted, because a
thorough insight into its present state is scarcely possible without
a knowledge of its former fortunes. In fourteen chapters the
gradual evolution of our knowledge is traced from the Egyptians
to the present time, the apprehension of the earlier opinions being
rendered easier by a picture of a section of the eye, copied from
Ryff's 'Kleine Chirurgie,' printed at Strasburg in 1542, and drawn
according to the ideas of the ancients. We heartily thank the
author for his work, in which, by a constant reference to the
original sources, he has thrown much light on many difficult ques-
tions.—T. W.

Recent Neurological Essays.[2]—The American Neurological Asso-
ciation, as judged of by its professed object, "the cultivation of neuro-
logical science in its normal and pathological relations," taken together
with the contents of the first volume of its transactions, places itself
on the level of the British Medico-Psychological Association. If it
be less fully occupied with the consideration of mental diseases, it is
that it has not originated, as in our own case, with those engaged,

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[1] *Geschichte des grauen Staares.* Von Dr. HUGO MAGNUS. Leipzig, 1876.
8vo, pp. xii and 315.
History of Cataract. By Dr. H. MAGNUS.
[2] 1. *Myelitis of the Anterior Horns.* By E. C. SEGUIN, M.D. New York,
1877.
2. *Transactions of the American Neurological Association.* Vol. i. New
York, 1875.
3. *Considérations pratiques sur le traitement des Nevralgies.* Par le Dr.
FROISSAC. Paris, 1877.

more or less exclusively, with asylum duties or the treatment of the insane. This volume of transactions, however, is not wanting in papers that will be of special interest to the alienist.

We notice among the names of those who have initiated this association those of many distinguished cultivators of neurological pathology — names familiar to all students of this department of scientific medicine. It may suffice to enumerate those by whom the association was called together, viz. Drs. Hammond, Bartholomew, Clymer, Jewell, Seguin, Putnam, and Cross.

We could have desired to place before our readers an analysis of this first volume of transactions, but the limitations of space prevent. We must rest content with brief references. Dr. Putnam relates a case of "analgesia," and appends thereto a lengthy dissertation upon the physiology of sensibility. With reference to the pathology of the case related he arrives at this rather indefinite conclusion:

"Whether this impairment of function concerns the entire nerve (or all of the terminal filaments) or only certain portions which have become endowed with special properties, and exactly what molecular changes are involved, can only be settled by further physiological study; but it is highly probable that both of the conditions referred to at times prevail."

Dr. Schmidt, of New Orleans, has contributed a valuable essay on the structure of nervous tissues. Dr. Hammond has a short paper on pigmentary deposits in the brain as a result of malarial poisoning.

The editor, Dr. Kinnicut, publishes two cases of "Insanity in Children induced by Masturbation." The ages of these two patients at the time of the development of the melancholia were severally twelve and thirteen, masturbation having been previously practised in both instances. In one of these complete recovery took place under careful treatment and discontinuance of the vice, while in the other, under the influence of an hereditary instability of brain, the morbid state became fixed. These examples are of interest, inasmuch as it has been held by men of eminence that masturbation is rather a consequence than an antecedent of insanity. The extent to which this disgusting practice prevails in lunatic asylums would lend countenance to the latter opinion.

The volume also contains articles on the relation of cases of injury to nerves, and cases of neuralgia. Dr. Emerson, of New York, remarks upon the treatment of neuralgia by phosphorus. After reciting the observations of many writers, both American and British, he speaks in terms of satisfaction as to its effects, and expresses the belief that a higher degree of success would have been attained

had the drug been pushed farther, either by giving larger doses or by repeating the dose at shorter intervals.

Dr. Froissac addresses himself to the task of disentangling the constitutional causes of neuralgia from its local excitants. "Neuralgia," observes Dr. Froissac, "is, of all maladies, the most obscure and irregular." Regarded from the author's point of view it must inevitably be so; as, to our apprehension, he singles out only the one symptom—pain—from various constitutional disorders. It is, doubtless, an important point to determine the seat of the affection; to determine, if possible, whether it be central or peripheral, and that, with reference to treatment, since, as Dr. Froissac points out, many a severe case of neuralgia of constitutional origin has been regarded as peripheral, and handed over to the tender mercies of the dentist, instead of having been consigned to the scientific therapeutics of the physician. The sovereign remedy of Dr. Froissac is opium or its alkaloid morphia. Subsidiary to this medicine the author makes mention of the use of datura, belladonna, bromide of potassium, spirits of turpentine, acupuncture, and electro-puncture, but does not make mention of phosphorus, the effects of which, as we have seen, are lauded by Dr. Emerson, of New York. Dr. Froissac's essay gives a good *résumé* of various theories on the subject of neuralgia, and if it does not advance our knowledge by notably new views, it, at all events, strengthens confidence in our treatment.

Dr. Webber contributes an article on myelitis, as supplementary or additional to the independent monograph of Dr. Seguin, the object of which is to demonstrate that spinal paralysis and muscular atrophy, acute, subacute, and chronic, in the adult, are essentially the same as infantile paralysis, viz. myelitis of the anterior horns of the spinal cord. This may be either localised or diffused, primary, or secondary to other disease of the cord or brain. A long series of forty-five cases of this form of disease is given by Dr. Seguin, from which he deduces an analysis of the symptoms under the several heads of disorders of movements, disorders of sensibility, disorders of nutrition, general and special symptoms, electro-muscular contractility, concluding with the pathological anatomy, tabulated from twenty-nine autopsies, the most obvious lesion being degeneration of the cells of the anterior horns, together with such lesions of other tissues as are consequent upon inflammatory action.

Cullen on Typhoid Fever[1].—This tract is the production of the Registrar of the Netley Hospital, an army surgeon of large experience. It bears token of having been hastily put together, but contains some interesting notices of forms and outbreaks of fever in different parts of the world.

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[1] *On some Undetermined Points in Typhoid Fever.* By DAVID CULLEN, M.D. Surgeon-Major A.M.D.

Bearing upon the etiology of the disease, the author narrates several histories in some of which its development was supposed to have been spontaneous. But in more than one of them there is ground for question whether the disease was really typhoid. Of the production of the disease by the use of polluted water, or the absorption of sewage emanations further evidence is given.

Dr. Cullen also adduces a considerable amount of testimony in favour of the belief that much of the severe fever prevalent in tropical and eastern countries, as well as in America, is typhoid, masked or modified by concurrent miasmatic conditions. This seems highly probable.

With the clearer ideas now possessed by our younger brethren in the public services, we trust that ere long much of the obscurity still resting on the subject of tropical fever will be cleared away.

McCall Anderson on Tubercular Affections.[1]—This little work consists of two lectures published with the view of raising the question whether true tubercle is so rebellious to treatment as it is usually considered to be.

In the first, the subject is regarded as bearing upon tubercular peritonitis. The author briefly narrates four cases which he believes to have been examples of the disease, and which were successfully treated by him. The details published, however, are so few and insufficient that we are unable to accept either his premisses or his conclusions. In the history first given, the patient, a girl, aged ten years, had ascites of three months' duration, with some condensation of the summit of the left lung. It is stated that there was dulness on percussion, wavy respiration, with an occasional snoring râle. On the strength of these signs Dr. Anderson infers that she had tubercular disease of the lung, and tubercular peritonitis with effusion. She recovered in about two months under the use of pancreatic emulsion, iodide of iron, and cod-liver oil.

That there may have been some low inflammation in both lung and peritoneum is highly probable, but to call this tuberculous, without more proof than is given, is an instance of laxity in the use of language which we regret to see.

The reports of the other cases are even shorter and more unsatisfactory; in two of them the only warrant for calling the attacks tubercular was that they occurred in the children of unhealthy parents.

In the second lecture the curability of acute phthisis is discussed, and three cases are narrated in support of the affirmative proposition. In the first the disease appears to us to have been capillary

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[1] *Clinical Lectures on the Curability of Attacks of Tubercular Peritonitis and Acute Phthisis.* By Dr. McCall Anderson, Professor of Clinical Medicine in the University of Glasgow.

bronchitis, with some pneumonic consolidation of the left apex and right base. Improvement commenced in a fortnight, and complete recovery was effected in a few weeks. The second also seems to have been a case of broncho-pneumonia, there being universal musical râles, with dulness over the right apex and rusty expectoration. Here the acute symptoms subsided in a fortnight, and complete restoration to health took place in about two months.

In the third patient the attack was sudden, its development rapid, and productive of extreme prostration. Here there was from the first some dulness on percussion over the left apex, which quickly extended and was associated with musical râles over the whole chest. There was high fever, which abated in eight days and ceased in twelve. A perfect and seemingly quick recovery was made. Dr. Anderson believes that these cases were examples of acute phthisis, whether tuberbular or non-tubercular he does not pretend to say. The histories fail to produce any such conviction on our mind, but they illustrate well the power and value, in pulmonary attacks, of careful, judicious, and supporting treatment.

Sullivan on Tropical Diseases.[1]—In the preface to this work the author informs us he passed many years in practice in tropical climates; and, from incidental notices elsewhere, we learn that these embraced Peru and Cuba, a field sufficiently wide to afford ample scope for the study of many forms of tropical diseases as they present themselves among the different races to be met with in these countries. These the author describes in the volume before us from his own experience, as well as that of others, with the object of affording a useful compendium of the facts for such as have not themselves the opportunity of studying disease within the tropics.

The work contains sections on anæmia in hot climates, marsh malarial fever, latent or marked malaria, pernicious fever, tropical bilious fever, marsh diathesis and marsh bacteria, dysentery, acute and chronic, diseases of liver, hepatitis, jaundice of different kinds, yellow fever, and Asiatic cholera. The relative frequency of these forms of disease varies very much in different localities, but unless the practitioner in the tropics be familiar with them, more especially with those connected directly or indirectly with the action of malaria, he will often experience the greatest difficulty in detecting the true nature of the cases which come under his care, and may utterly fail in devising measures for their relief. He may have a case, for instance, with distinctly marked symptoms of meningitis or cerebritis, with more or less fever, which appeared to give way to his remedies in the course of twelve to eighteen or thirty-six hours, but, while congratulating himself on his success in checking the

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[1] *The Endemic Diseases of Tropical Climates, with their Treatment.* By J̲OHN̲ S̲ULLIVAN̲, M.D. London, 1877. Pp. 211.

cerebral affection, at his next visit he may find it had returned in greater intensity, to give way again, perhaps, but leaving his patient much weaker and in a more critical state, and, should another exacerbation take place, it would in all probability destroy life. The practitioner may be anxious to avoid giving quinine lest he aggravate the cerebral mischief, but if he do not prevent, or at all events very much reduce the force of the ensuing paroxysm, it will almost certainly prove fatal. If he administer quinine with proper caution, using at the same time other measures suitable to the case, he will most likely find the expected exacerbation much less severe, if not altogether prevented, and a continuance of the treatment will soon remove all danger. As with meningitis or cerebritis, so it is with every other form of local disease met with in these countries; they may arise and run their course uncomplicated by malarial poisoning, but when they are, and this is very frequently the case, it is hopeless to expect to cure the local affection unless the malarial element be first overcome, and often the only indication of that being in operation is the aggravation of the local mischief at regular intervals. Latent or marked malarial affections are of this nature, and, in different cases, may simulate any variety of disease. Pernicious fevers are merely a modification of the same combination, in which the local complication, involving some important organ or function, appears intercurrently, or at least assumes a threatening aspect during the course of an attack commencing as an ordinary intermittent or remittent.

Dr. Sullivan arranges pernicious fever in two groups, the algid, comprising the fainting dysenteric and choleraic varieties, and the comatose, including the tetanic, lethargic, and convulsive, and he gives some interesting illustrations of these. In the comatose form he tells us " *post-mortem* examination reveals no inflammation of the brain or of its membranes, nor any lesion to which might be traced the severity of the symptoms; " to the naked eye, no doubt, this appears to be so, but if submitted to microscopic examination, we believe distinct indications of active exudation into the cerebral substance will be found in most of such cases, especially when they have undergone more than a single paroxysm.

The author limits the designation of bilious fever to a form met with among acclimatised Europeans, Creoles, or negroes, who have already suffered from intermittent, and " in whom the bilious element begins to show itself." Jaundice occurs early and is accompanied by bilious vomiting, and the urine " is of the colour of ink or coffee-grounds," owing to the presence of bile with a large proportion of blood; the latter may recur with each exacerbation of the fever, and diminish or disappear with the remission or apyrexia, and is connected with enlarged and congested kidneys. Those who have practised in other parts of the tropics will recognise the variety of fevers here described, but most of them, we believe, will regard

it as merely the ordinary endemic fever with hepatic derangement, in an old resident, had not a distinct species of fever, for we must add, the affection of the kidneys and hæmaturia are much more rarely met with in several of the other West Indian islands, and on the African coast, than from the author's account they seem to be in Cuba. Indeed, we are inclined to think his theoretical view as to yellow fever being always a continued fever, has induced him to include under bilious remittent many cases which really were yellow fever in the remittent form. It is difficult otherwise to appreciate his position that in yellow fever blood, or more commonly matter resembling black vomit, may exude from all the mucous membranes, including " the surface of the vagina and uterus, but blood never oozes from the kidneys. In the latter character yellow fever differs from bilious remittent, in which blood in the urine is an essential element " (p. 166).

The author approaches, but hesitates to grapple with the disputed question as to the origin of yellow fever, which, he says, " like cholera, must have for its origin a special miasma, unknown to us in its essence and conditions of birth " (p. 157); but he informs us this fever " is a specific infectious disease, capable of reproducing itself, with the same character and intensity as in the endemic focus itself " (p. 156), and when introduced into the West Indian islands, under conditions favorable to its development, it rages with severity, while, under unfavorable conditions, people who have come from the endemic focus die from yellow fever without its extending, unless in very rare instances. " It is imported or conveyed by man by the clothes he wears, merchandise, or trading vessels. It creates for itself a new focus of infection far from the original one " (p. 156). An infected vessel, loaded or in ballast, contain in its timbers all the elements of the disease capable of reproducing it wherever the vessel may cast anchor, supposing the population predisposed by those general and special conditions by which the disease is evolved " (p. 160). When conditions favorable to development are considered so essential to the communication of this disease, it appears to us it would have been advisable to have examined how far these were connected with the independent productions in the locality of the special miasma referred to above, and if this could not be excluded satisfactorily, what proof have we that " yellow fever is a specific infectious disease capable of reproducing itself?" How can we be certain that this miasma may not have been produced in every instance where a fresh case has arisen? That persons who have been in a locality where the miasma was existing might carry away a portion in their clothes is very likely, just as they might carry away a portion of dust or any other material matter that might be suspended in the air at the place, but this has never yet been shown to be the means of actively spreading the disease. Ships, no doubt,

do occasionally carry its cause, but those that do are merely moveable localities, which happen to contain within them that assemblage of conditions necessary for its development; but though there are many instances in which persons in Europe, or elsewhere, visiting them have contracted the disease, there is no well-authenticated case of any individual being affected who had not been on board, or within the range of those emanations from their holds with which this fever is associated.

Dr. Sullivan considers yellow fever is always a continued fever, and that those presenting periodicity are altogether different and distinct from it. His description of yellow fever is neither so full nor precise as is desirable for the sake of those who would consult his work for information regarding it, and a practitioner anxious to decide whether a case was yellow fever or one of those forms of bilious or other fever which sometimes resemble the yellow, will receive but little assistance from his remarks on their differential diagnosis in solving his difficulty. In fact, fevers presenting every characteristic feature of yellow fever are met with in different localities, not only in the continued but in the remittent and even the intermittent forms, as many of the older writers on the disease pointed out, and the experience of recent years, supported as it has been by the test tube and microscope, has borne out these conclusions. Had our author not adopted the theory which defines yellow fever to be a specific infectious fever of a continued form, he might have presented the relations of the different tropical fevers in a clearer and more correct manner.

In the treatment of yellow fever our author has little to recommend from his own experience, in which respect he is in the same position as many who have preceded him. As he informs us that " in mild cases the mortality is as one to three; in the complete form is as seventy to one hundred," we can scarcely avoid the conclusion that the sick from whom those ratios were obtained must have been under very unfavorable circumstances, such indeed as would have told seriously against the success of any treatment. Though sufficiently formidable under the most favorable circumstances, we believe the mortality from yellow fever in the present day will be found considerably under that just mentioned.

It is unnecessary for us to notice the sections on dysentery, hepatitis, and cholera, diseases which, though met with in the West Indies, are much less common than those in other parts of the tropics, as the above remarks on fever which are frequent in the former; and we believe the genuine product of the country will enable our readers to form a fair idea of how far the author has succeeded in effecting his object.

Fleming on the Genito-Urinary Organs.[1]—There are many and various reasons for writing a book—some good and some bad, and some indifferent. A young man may write one as an advertisement, another may, out of the fulness and newness of original work, give the profession something of extreme value. Many a busy teacher puts his lectures into shape for his pupils, or gives the profession some monograph on the subject which has taken up most of his time and thought; and again, in old age and well-won leisure the veteran fights his battles over again, and selects from the ripe experience of the past years of work cases and observations which he trusts may help his fellows.

The work before us is, we believe, one of the last variety and a most favorable specimen. Such works are nearly always clinical and historical, generally ill-arranged or not arranged at all, sometimes showing a little the garrulity and diffuseness of age; often showing here and there that Horace was right, and that the old man is 'laudator temporis acti se puero,' fortunate if he is not ' censor castigator que minorum.'

This work contains, or rather is made up of, a long series of most interesting cases, concisely told and well commented on. A long hospital experience well used has given Dr. Fleming most extensive and varied material, and he and his editor have done their work well, making the book not only instructive but entertaining.

The first chapter on the Pathology of the Urine in its relation to surgery is well arranged for the student, and his memory is aided by two most truthfully executed chromo-lithographic plates, containing twelve different specimens of urine glasses, with sediments, colour, &c., contrasted with each other, and also some excellent sections of calculi.

The second chapter on obstructions of the urethra from injury contains accounts of a large number of most curious cases, illustrated by good chromolithographs, though in some of them the colours are too strongly contrasted.

The salient points of Dr. Fleming's teaching that strike us are, first, his patience—he knows in what cases to wait, to hold his hand, not to pass the catheter, not to incise; and secondly, his belief in the internal use of tartar emetic and opium, and the external use of stupes of tobacco juice. Patience may be, and even by Dr. Fleming is, carried too far, but it is a valuable protest against the early and forcible catheterism so often counselled and practised in inflamed urethras. The tobacco stupes may be often useful, but the tartar emetic and opium may be pushed too far. The reason we find for

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[1] *Clinical Records of Injuries and Diseases of the Genito-Urinary Organs.* By CHRISTOPHER FLEMING, M.D., &c. Edited by WILLIAM THOMSON, M.B., M.D. Dublin, 1877.

Dr. Fleming's love of these potent drugs is that he is afraid of chloroform. He does not seem to use it so often or so thoroughly (see Case 36) as we do.

There is much truth in the following sentences, and such teaching is sometimes required :—

" When the penis is in a state of congestion (in some cases almost bordering on priapism), the operation of catheterism is most objectionable. It should never be had recourse to except under the most urgent and pressing circumstances, every expedient having been previously adopted by the surgeon to dispense with its use. In some cases, especially in those of acute gonorrhœa in young subjects, where retention of urine occurs, venesection may be advisable ; in others, local bleeding, followed by copious fomentations, medicated or otherwise, will be found most effective, and in all, the exhibition of tartar emetic and opium, combined with saline cathartic or diuretic medicines, in doses proportioned to the urgency of the symptoms, and repeated at intervals, will prove most useful adjuncts. I attach very much value to the stuping with tobacco fomentations. I have found them most effectual even in cases of paroxysmal retention of urine identified with stricture." Pp. 111, 112.

There must be something in the cheerful ingenuity and self-confidence of the Irish character that impels them to use such extraordinary means of relieving themselves of symptoms or results of stricture, or we should not find such a number and variety of foreign bodies introduced into the urethra. A thorn-twig figured after its removal by Dr. Fleming is a good example of the national trait. He also records and figures various pieces of bougie and one whole one, which must have demanded in their possessors a faith in their own property analogous to that shown in the rotten reins and string-mended harness of the national vehicle. Surely the same easy-going faith is not found in Irish surgeons to the extent implied in the following caution :—

" I may here make the general observation upon the importance in all cases of catheterism of examining the condition of the stilet before introducing the instrument ; the wire is very often allowed to become rusty and encrusted and the canal of the catheter to be partially blocked. The wire should not be passed in until it is quite clean and smooth, and this is best done (*sic*) by means of a little sandpaper and chamois (*sic*). This is a rule which also applies to all canulæ, for I have seen some awkward surgery result from inattention to these details in tapping hydroceles and the bladder, the trochar being almost immovably fixed in its sheath." P. 188.

In the account of chronic prostatic diseases we fail to discover Dr. Fleming's views as to form, mode, and frequency of introduc-

tion of catheter, and general surgical management.   Such cases are often very perplexing, and involve long and tedious treatment.

Hæmaturia is discussed as an important symptom, the result of very varying conditions.   The cases given illustrate well some of the more frequent causes of it—injury, disease of the liver, cancer of various kinds, and polypus.   Mr. Fleming does not seem to have met with that rare and interesting form of bladder disease called sometimes villous cancer.

Stricture is briefly described.   As regards the position in which the patient should be when the catheter is being passed, Mr. Fleming has found that a very satisfactory plan is to introduce the instrument while the patient is sitting either on the edge of the bed or in an arm-chair.   The patient's feet should be on the ground, and the body be thrown well back, allowing the perinæum to be freely exposed (p. 234).

Suprapubic puncture seems to be a favourite resort of Mr. Fleming when the usual methods of relief have failed ; we agree with him in preferring this method in many cases to the rectal puncture, especially now that in the aspirator we have a means of tapping above pubis both safe and successful.

Under the head of affections of the scrotum a somewhat rare malady is described—

" An affection of the scrotum and penis which I have observed in a particular class of men, viz., wine-bottlers.   It is their habit, in pursuance of their occupation, to place the bottle between the thighs, and then to drive the cork home.   I have seen several cases in which this practice has set up irritation at a point where the scrotum joins the penis in front, just as we know that soot causes epithelioma of the scrotum in sweeps.   In the cases which have come before me the irritation was followed by abscesses, which in time burst, leaving an unhealthy, ragged, lupoid-looking ulcer." Pp. 265-6.

The account of the cases of stone in bladder, treated by lithotomy and lithotrity, is valuable, in that it is not a mere record of successes, but that the failures and errors in diagnosis are told as well with a frankness and honesty not by any means invariably seen. And amongst them is a record, illustrated by excellent chromolithographs, of a case which is probably unique—a fibro-calcareous tumour of the uterus, opening into the bladder by pressure and protruding into it.   The symptoms were those of stone, and an attempt was made to relieve the patient of it, first by the urethra, and afterwards by the suprapubic operation—both, of course, unsuccessful.

The book is capitally got up, and the chromo-lithographs are most telling, though some are rather brilliant in their colouring.   The editor has kept himself entirely out of sight, but has done his part of seeing the book through the press most carefully.

Black on Bright's Disease.[1]—This book cannot be commended either as an original work or as a good compilation. To the former character, indeed, it lays but little claim, although here and there vague statements are made as to original investigation. As a compilation it is not deficient in presenting an array of names sometimes with and sometimes without references to the works and passages quoted. Occasionally a curious compromise between these two methods of reference is effected, as when the author quotes (p. 121) "Dr. Noel Guéneau de Mussy (Union Medicale)," or when he refers (p. 101) to what he calls in a footnote " Deut. Arch. Klin. für. Med.," without any mention of volume or year. These are perhaps trifling defects in themselves, but a certain looseness of statement, with obscure and inelegant expressions, abounds in the volume.

The most startling section is the one on treatment. According to Dr. Black, diluents are dangerous, cream of tartar is deadly, possessed of what he calls " killing properties," and saline purgatives are on no account to be advised. Instead of what he terms " the fashionable waiting-upon-death of modern physicians," he recommends general bleeding to be resorted to at once, " if there be a history of exposure to cold or damp, with albuminous urines, with or without the other indications of nephritis," this to be replaced or followed up by cupping or leeching; in the later stages mercury is recommended. Then we are advised to use counter-irritation by cantharides, or by tartar emetic ointment and croton oil, supplemented, when the skin is tender, with mercurial or iodine inunctions; or, " instead of the ordinary counter irritation, the actual cautery may be applied, or setons may be established," &c. (p. 143).

Dr. Black does well to say, immediately after sketching this plan of treatment, to his students, " Notwithstanding your utmost endeavours, nephritis will prove of the greatest gravity ; " this sentence may perhaps direct the more intelligent of them to search for safer guides and better forms of treatment, by which the gravity of nephritis may be lessened instead of increased.

Transactions of the American Gynæcological Society.[2]—This Society, numbering among its Fellows most of the leading obstetricians of America, was founded last year for the promotion of knowledge in all that relates to the diseases of women and to obstetrics.

The President, in his opening address, expresses the hope that " the Society will exert a marked influence in stimulating inquiry,

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[1] *Lectures on Bright's Disease.* By D. CAMPBELL BLACK, M.D. London, 1875.

[2] *Transactions of the American Gynæcological Society for the year* 1876. Vol. i. Boston and London.

investigation, and recorded observation, and thus be an important agent in contributing to the progress of science and our national reputation in this branch of our profession."

The council intend to publish in each volume of the 'Transactions' a complete bibliography of everything that appears pertaining to obstetrics and gynæcology during the current year, not only in English but in foreign literature. The number of Fellows being limited to sixty, and "no one being eligible for active Fellowship until he shall have submitted to the council a paper on some subject connected with gynæcological science," the approval or rejection of such paper determining the nomination for election or otherwise of each candidate, we may fairly expect that the class of papers will be such as to ensure the success of the 'Transactions,' and help to give the Society a high scientific position.

The opening annual address of the President, Dr. Fordyce Barker, is one of much interest, and contains many valuable suggestions that might with propriety be adopted in this country. He very properly divides into two classes the papers contributed: (1) those which would be useful and interesting to be read and discussed at the meetings, and (2) a second class of papers of such a character that no one could listen to the reading of them with interest or advantage. All subjects relative to questions in pathology and practice which have not yet been settled by the general sentiment of the profession, he considers, will appropriately belong to the former class; all practical questions which involve great statistical research, analysis, and deduction, and all papers based on original, physiological, anatomical, and pathological studies, will come under the second category.

He suggests that the discussion should be the expression of careful study, deliberate judgment, and mature experience; the time of the Society being too valuable to be taken up by listening to the crude, impulsive, badly arranged outbursts of the moment.

Considering that the Society was somewhat hastily constituted, it was to be expected that the first contribution would have little of an original character; in fact, many of them are simply old friends with new faces, others bear evidence of having been very hurriedly put together, and some few would perhaps have been better omitted altogether as conducing neither to the renown of their authors nor to the value of the 'Transactions.' The volume itself is, however, one of great interest, and contains many valuable contributions from well-known physicians.

Dr. T. A. Emmett contributes the result of many years' experience on 'The Etiology of Uterine Flexures, with the proper mode of Treatment indicated.' He enters minutely into the statistics of the frequency of flexion among some 2447 cases, the subject of various diseases and injuries peculiar to women. He considers that "flexures of the cervix have their origin at about the age of puberty, by the

balance being lost between the relative growth of the body and cervix." Anteflexion, he thinks, has its origin after puberty, and observation indicates it to be the result of obstructed circulation from impaired nutrition. Retroflexion he regards as a deviation from a previously existing retroversion.

He never divides the neck of the uterus laterally, except for the treatment of fibroid or for partial closure of the os. He thinks the operation as practised by Simpson, or any modification of it, uncalled for and detrimental.

Dr. R. Battey relates the history of ten cases of 'Extirpation of the Functionally Active Ovaries for the Remedy of Otherwise Incurable Diseases.' Two of the cases proved fatal, the remainder were benefited by the treatment in various degrees. In his opinion the removal of the functionally active ovaries is indicated in the case of any grave disease which is either dangerous to life or destructive of health and happiness, which is incurable by other and less radical means, and which we may reasonably expect to remove by the arrest of ovulation or change of life.

Dr. J. Matthews Duncan has a short paper on 'Central Rupture of the Perineum.' Dr. E. W. Zenk directs attention to the value of Viburnum Prunifolium in all uterine disorders characterised by loss of blood. Dr. Robert Barnes, of London, contributes an interesting article on the 'Relations of Pregnancy to General Pathology,' well worthy of perusal, though difficult to summarise. Dr. W. H. Byford directs attention to the 'Spontaneous and Artificial Destruction and Expulsion of Fibrous Tumours of the Uterus,' in which he refers to the action of ergot in these cases. Dr. T. Gaillard Thomas gives the 'Report of a Case of Abdominal Pregnancy treated by Laparotomy,' with success as regards the mother. The foetus had been dead some little time. The placenta was left alone, a large glass drainage tube being placed in the lower extremity of the incision. The decomposed placenta was found protruding from the wound five weeks after the operation, and was removed. The patient made a rapid recovery.

Dr. H. F. Campbell writes an exhaustive article on 'Pneumatic Self-replacement in Dislocations of the Gravid and Non-Gravid Uterus,'—a subject to which he has directed attention for years. His method is far less frequently practised in this country than it should be. Its simplicity apparently is not sufficiently important to ensure attention, and yet it is one of the most efficacious methods of treatment we know of in many cases of retroversion and flexion of the uterus.

Three important conditions—I might say powerful influences—all applicable to, and bearing upon, the displaced uterus, are found to be coexistent when the woman is placed in the genu-pectoral

position : first, reversal of gravity ; secondly, draft of the viscera ; and, thirdly, external atmospheric pressure."

Most of us are familiar with the postural method of treatment in cases of prolapse of the umbilical cord during parturition—the genu-pectoral position—where gravity alone is sufficient to retain the replaced funis. Surgeons more frequently resort to the expedient of elevating the hip in cases of perineal protrusions, with a view to reduction, " the draft of the viscera " being really the force assisting. In addition to these forces we have a third, viz. external atmospheric pressure, by means of air allowed to enter the vagina.

The whole subject deserves more attention among the profession than has hitherto been accorded to it.

Dr. W. L. Richardson, in a short paper, treats of the 'Advantages of Hydrate of Chloral in Obstetric Practice.' 'Labour complicated with Uterine Fibroid and Placenta Previa' forms a brief but interesting communication from the energetic hon. sec., Dr. J. R. Chadwick. Dr. Emil Noeggerath, again, brings prominently forward the subject of 'Latent Gonorrhœa, especially with regard to its Influence in Fertility in Women.' Since the author first published his work upon this subject, some five years ago, professional opinion has altered considerably. The statement that about ninety per cent. of sterile women are married to husbands who have suffered from gonorrhœa, either previous to or during married life, was scarcely credited by many, and indeed will probably be doubted by most practitioners now ; still, that gonorrhœa forms an important item in the list of producing causes of cervico-endometritis and various forms of perimetritis cannot be denied.

Dr. W. Goodall contributes a short 'Clinical Memoir on some of the Genital Lesions of Childbirth,' in which he briefly discusses the question of immediate operation in rupture of the perineum during parturition. The cases are too few to admit of any reliable deduction, but we quite agree with the author that, while the immediate operation does not prove so successful in complete ruptures as in incomplete ones, yet a far better chance of union is afforded in both forms of laceration by the suture than by the " let-alone " treatment.

In a brief note on 'Hermaphroditism,' by Mr. Lawson Tait, of Birmingham, attention is called to the common mistake of the inexperienced, confounding arrested development for cases of bisexuality, where the actual presence of the organs of both sexes in the same individual has been revealed by a post-mortem examination. In some cases there is distinct evidence of over-development or hypererchesis. On the strength of these observations the author concludes that we must accept Darwin's theory of the descent of man, the occurrence of such malformation being regarded a reversion of type. "This

acceptance at once becomes the explanation of the occasional occurrence of bisexual vertebrates, and consequently of true hermaphroditism in human individuals."

Dr. J. R. Chadwick also contributes an article on ' Rare Forms of Umbilical Hernia in the Fœtus,' illustrated by five woodcuts.

The volume concludes with a brief memoir of the late Dr. Sneider, by Dr. Paul F. Mundé, a well-executed and most expressive steel engraving accompanying the text.

Gout: its Cause, Nature, and Treatment.[1]—It is very difficult to determine how this book should be received by the profession, for while it exhibits a large amount of scientific and literary research and some good reasoning, it is so dogmatic in its tone in many parts as almost to exclude it from serious notice. It is quite true that the treatment of gout has long been a matter of controversy, and is still very often unsuccessful, but yet it is supposed by a great number of the profession that its causes and nature are pretty well known, that its therapeutics and dietetic management are, on the whole, tolerably well understood, and that, making allowance for a certain latitude of practice, the rules by which the practitioners should be guided in this disease are pretty well defined. But Mr. Parkin informs us that all our knowledge on the subject is founded on error, that the cause and nature of gout have been hitherto mistaken, and that the treatment generally pursued is altogether erroneous. According to Mr. Parkin the disease is not due to any specific *materies morbi,* but is caused, in common with various epidemics, by malaria ; that the blood in gout is not acid, but alkaline; that uric acid is not free in the system, but in a state of combination ; that colchicum is worse than useless in the treatment for it—is positively injurious and poisonous ; and that the true specific for gout is carbonic acid gas ! This gas is to be administered in the form of soda water or seltzer water, or still better, in a state of effervescence in the ordinary draught made by combining carbonate of soda with citric or tartaric acid. The efficacy of this mode of treatment is vouched for by a number of cures which Mr. Parkin has performed.

Clinical Studies in the Non-Emetic Use of Ipecacuanha.[2]—Although it would be inconsistent with sound medical practice to treat nearly all diseases with one remedy, inasmuch as experience

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[1] *Gout ; its Cause, Nature, and Treatment, with Directions for the Regulation of the Diet.* By JOHN PARKIN, F.R.C.P.E., &c. Second Edition, pp. 144. London, 1877.

[2] *Studies, chiefly Clinical, in the Non-Emetic Use of Ipecacuanha, with a Contribution to the Therapeusis of Cholera.* By ALFRED A. WOODHULL, M.D., Assistant-Surgeon U. S. Army. Pp. 155. Philadelphia, 1876.

teaches us that a variety of drugs are applicable to different morbid conditions, yet it is perhaps true that ipecacuan is available in a far greater number of cases than is generally admitted, and that its value as a medicine is still somewhat underrated. Hence, although Dr. Woodhull is rather enthusiastic in his eulogies of this drug, and other practitioners might be less successful than himself in its employment, we think he has done good service in offering the present little book to the medical public. It consists essentially of a series of special reports to the Surgeon-General of the United States Army, which were afterwards published, with additions, in an American journal; but Dr. Woodhull has now collected and arranged the materials afresh, and the whole constitutes a really valuable monograph on a very interesting practical subject. Admitting, of course, the great value of ipecacuan as an emetic, the author claims for it also the quality of being a direct nervous stimulant, acting chiefly, if not entirely, upon the sympathetic system. In the treatment of dysentery in its various forms Dr. Woodhull considers it to be extremely valuable, and he gives a series of cases in corroboration of his opinion; but he also recommends it as being more or less efficacious in a variety of very different diseases and morbid conditions, such as cholera, uterine and other hæmorrhages; some forms of dyspepsia, vomiting of pregnancy, nervous and other coughs, drunkenness and delirium tremens, neuralgia, intermittent fever, acute hepatitis, &c. We have already expressed our doubts whether an extended experience would justify all the commendations bestowed on ipecacuan by Dr. Woodhull, but at the same time we believe that much of the author's reasoning is correct, and we may express our own opinion that the medicine is a most valuable one, and that it is not injurious even when it is unsuccessful.

Hospital Mortality.[1]—The discussion upon hospital mortality raised by Sir James Y. Simpson shortly before his death ceased soon after that time, and the term hospitalism became almost forgotten. We believe the discussion at that time did much good, and drew attention to much that was bad in our hospital arrangements, and has doubtless, by making hospital authorities more alive to the necessity of attending to hygienic details, done much to limit the number of preventible deaths. The cessation of the discussion we believe to have been due in great measure to the fact that, instead of talking, hospital surgeons and committees have been acting, and in every way labouring to improve the hygienic conditions of the buildings under their care. As a proof of this we may mention

[1] *An Essay on Hospital Mortality; based upon the Statistics of the Hospitals of Great Britain for Fifteen Years.* By LAWSON TAIT, F.R.C.S. London, 1877.

that thousands of pounds have been expended in rebuilding or reconstructing old hospitals. As examples we may mention Edin-burgh and Manchester, where new hospitals are in process of con-struction; Norwich, where the building is completed; and in London, Westminster, Charing Cross, St. George's, St. Mary's, the Children's, and others, in which either partial or complete reconstruction has been, or is in process of being carried out. Moreover, the remedies proposed by Sir James Y. Simpson were so absolutely impracticable in large cities that practical men felt little inclination to discuss them, and, instead of talking about abolishing all hospitals, were content to do their best to diminish to the greatest possible extent the unavoidable evils of such institutions. At Sir James Simpson's death his papers fell into the hands of Mr. Lawson Tait, and he has apparently felt himself bound to continue the same line of investigation, and to pile up a heap of figures exceeding in amount those of his " great master." Sir James Simpson followed a plan of investigation at any rate calculated to lead to some definite results. By limiting himself to one set of cases only he was able to draw comparisons between hospital and private practice which were certainly of some value, though they only confirmed the universally received opinion that amputations do better in private houses and in the country than they do in the crowded hospitals of great cities. Mr. Tait found that this part of the subject had been so thoroughly done by Sir James Simpson that he "felt that any further investi-gation of the subject must be prefaced by research in another direction." The more he thought on the difficult subject of discover-ing something which should be more than a repetition of Sir James Simpson's work, " the more he became satisfied that the first step was to establish the facts of a total hospital mortality for a definite and somewhat extended period," and he has therefore, with infinite labour, collected the mass of figures contained in this book. In order to arrive at any accurate general conclusion it is, of course, necessary to collect the largest possible mass of particulars, and it is to be hoped that if the mass be sufficient the various causes of error will neutralise each other. If, however, after having arrived at the general conclusion, we attempt to apply it to individual cases, the errors will, of course, reappear. All Sir James Simpson attempted to do was to prove the general proposition that the mortality after major amputations is greater in hospital than in private practice, and that it is greater in large hospitals than in small. He did not attempt to point out individual hospitals, and lay, as it were, manslaughter to their charge because their death-rate might be peculiarly high, without pretending to investigate the causes of such an exceptional mortality. Mr. Lawson Tait has not in this point followed in the steps of his "great master." He is constantly, all through his remarks, holding up two or three hospitals

as models of low mortality, and some others as places in which it
is incumbent on the managing body to show that they are not
killing their patients by bad management and negligence. Thus, he
tells us that as the district mortality of St. Bartholomew's is higher
than that of St. Thomas's, there seems *à priori* no reason why there
should be such a great difference in the death-rate of the two
hospitals. "If the death-rate of St. Thomas's was as low as that
of St. Bartholomew's, 220 valuable lives would be saved every
year." If this sentence has any meaning at all it is that there are
220 preventible deaths annually in St. Thomas's Hospital. Mr. Tait
assumes that it is the duty of the authorities of St. Thomas's Hos-
pital to prove that this statement is not true. We think it was
the duty of Mr. Tait to prove its truth before making so rash and
grave an accusation. Mr. Tait's object being to establish a general
hospital mortality, that is to say, the percentage of all patients
admitted to a hospital that die, he has for this purpose obtained
reports from nearly 300 hospitals and infirmaries. The number
of beds in the institutions from which his statistics are derived
varies from 700 to 5. The statistical tables are divided into
nine columns. In the first is given the full number of beds con-
tained in the institution; in the second the average number of
these occupied; in the third the average yearly number of in-
patients; in the fourth the average number of patients annually to
each bed; in the fifth the mean residence; the sixth and seventh
contain the average mortality per hundred beds and patients; and
the two last columns contain the death-rate of the district in which
the hospital is situated, and the ratio of the hospital to the district
mortality. The average number of patients to each bed is calcu-
lated by dividing the average number of full beds into the yearly
number of patients. Thus, let us say a hospital of a hundred beds
has on an average only ten full, and receives one hundred patients
per annum. Mr. Tait would put the patients per bed as ten. Another
hospital of one hundred beds might have every bed full all the year
round, and receive a thousand patients, and Mr. Tait would still
call the average per bed ten, and would say that one hospital was as
"active" as the other. Thus, while making comparisons between
the high death-rate of University College Hospital and the singu-
larly low death-rate which he attributes to St. Bartholomew's, he
states that "the work of one is quite as active" as that of the
other. On turning to his tables we find that he gives the average
number of patients per bed at Bartholomew's as 11·11, and at
University College as 12·75. If, however, we divide the number
of in-patients by the full number of beds in the hospital we find
that at Bartholomew's each bed receives 4·7 patients, while at
University College each bed receives 11·3. How the work of one
can be called as "active" as the work of the other we do not

understand, and what conclusion of any value can be drawn from such a calculation it is difficult to conceive. On the other hand, if calculated from the full number of beds, the patients per bed is an important indication of the character of work done in the hospital. If, for instance, the margin of empty beds is small, and the number of patients per bed large, it is clear that there must almost constantly be a considerable strain on the accommodation of the hospital, and only the most urgent and severe cases will be admitted, and a high death-rate under such circumstances is not to be wondered at. The mortality per bed is also calculated from the average number of beds full, and not from the full number of beds in the hospital. The mortality per cent. of patients is, however, the chief object aimed at in this work, and we find that 6·24 of every hundred patients admitted into the hospitals of the United Kingdom die ; but having found this, are we much the wiser or better for the knowledge ? The tables show us that some hospitals rise high above this figure, and the others, chiefly country cottage hospitals, fall far below ; but there is not one word in the book or one figure of this vast mass of calculations which can really help us to guess the cause of this variation. We venture to think that if the medical cases were separated from the surgical, if those cases which die within three days of admission were excluded, and if deaths from diseases usually supposed to be attributable to faulty hygienic conditions were separately tabulated, and the chief causes of all the deaths were also stated, some conclusions of value might be drawn. It may be replied that the labour of this would be too great for any man to undertake. Then let the author limit his number of hospitals, and do what he does attempt thoroughly. We do not see that anything but fallacy can arise from comparing the statistics of a small cottage hospital with those of one of the great hospitals of our large cities. These small hospitals might we, believe, have been excluded altogether with advantage, and Mr. Tait would then perhaps have found time to tabulate more fully the results of our large hospitals. If, then, he was able to show that in any one the death-rate was unduly high, and that this was due to preventible disease, he would have rendered real service to his profession and his country. The variations in the nature of the cases admitted in various hospitals is too great for mere statistics such as we have before us to be of any value. Some hospitals take in large numbers of simple cases which in smaller hospitals are treated as out-patients, and amongst whom it is scarcely possible for a death to occur. Some keep incurable patients till death terminates their sufferings, others send almost all hopelessly chronic cases to the parish infirmary. In hospitals to which medical schools are attached a case likely to terminate fatally at an early date is by no means unwelcome, while in many others, especially in small towns, such cases

are not at all desired, for fear of raising the death-rate. Some
hospitals fill half their beds with venereal diseases, whilst others
rigidly exclude such cases, by a rule of the committee. No doubt all
this does not in any way invalidate the truth of the main statement
of this book, that 6·24 of every hundred hospital patients die, but it
renders it utterly impossible to apply the statement in any useful
way to any individual hospital. With regard to the accuracy of the
figures we can only say but little. We have, of course, no means
of verifying the great majority, but the first of all (St. Bartholo-
mew's Hospital) we hope is not a fair specimen of the rest. We
find the number of beds put down at 710, "excluding ophthalmic
beds." The prospectus of St. Bartholomew's Hospital for the
session 1877–8 contains the following statement :—"The clinical
practice of the hospital now comprises a service of 710 beds : of
these, 227 are allotted to the medical cases, 322 to the surgical,
26 to diseases of the eye, 20 to diseases of women, and 81 to the
syphilitic, while 34 are at the Convalescent Hospital at Highgate."
Thus, we see, in estimating the death-rate, Mr. Tait has included
81 syphilitic beds in which death is hardly likely to occur, 26 eye
beds which he says he has excluded, and 34 beds in a convalescent
home four miles away. We must say such gross blunders about a
hospital, of which the prospectus and the printed report can be
obtained by any one, has rather shaken our faith in the accuracy of
the rest of the table. At the end of the book are some remarks
on the statistics of ovariotomy and on the mortality of lying-in
hospitals, but on both these subjects it would be difficult to say
anything new.

In an appendix are some statistics of major amputations, and
here we may remark that these tables show very clearly the fallacy
of many of the conclusions drawn from those at the beginning of
the work. The hospital held up as the type of all that is bad is
University College Hospital—that of all that is good is St.
Bartholomew's. However, we find that in the former the mortality
after amputation is 1 in 4·3, in the latter 1 in 3·7, whereas the
general death-rate is given as 11·91 in the former and 5·12 in the
latter. Mr. Tait's "great master," Sir James Simpson, would
therefore have considered University College to be more healthy
than St. Bartholomew's, and used this as a proof of his statement
that the death-rate varies directly as the size of the hospital. If,
therefore, the pupil and master are at such utter variance, it is
evident further investigation is necessary before either of their con-
clusions can be considered reliable.

Diseases of the Skin.[1]—The recent increase in number and size of

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[1] *A Practical Treatise on Diseases of the Skin.* By Louis A. Duhring,
M.D. Philadelphia, 1877.

the treatises on skin diseases has become a positive source of alarm to the medical reviewer. No sooner has one ponderous volume been digested than another issues from the press, and again have we wearily to examine the tedious pages devoted to the oft-told story of the anatomy of the skin and the varying classifications of its diseases. Surely the time has arrived when professed anatomists and histologists may be trusted to embody an accurate account of the structure of the skin in their proper text-books, and the dermatologist may assume, as do the writers upon most other departments of medicine, that his reader is sufficiently acquainted with the elements of anatomy, physiology, and general pathology. If he is not already well grounded in the principles of medicine he is not prepared to undertake the study of a special department, and detached chapters are not likely to help him. Dr. Duhring is evidently of a different opinion. He begins, in the orthodox manner, with the anatomy of the skin. He even goes further, and assumes that we do not know what the skin is, as he actually takes the trouble to inform us that it is " a covering which invests the body completely, giving it form (*sic*) and also protecting it." He tells us that " hairs, either fine or coarse, are found upon almost all regions of the body ; they are more highly developed in certain parts than others." For whom can such a sentence be written ? Why is not the intelligent reader further informed of the more general prevalence of the whisker among the male sex ? If by the time he begins to study diseases of the skin he is unacquainted with the somewhat obvious fact that a man usually has more hair on his head than on his feet, statements not beyond the requirements of an infant school can scarcely be too minute for him ; he must, in truth, be an absolute ignoramus. To speak more seriously, it is sheer waste of time, paper, and printing ink, for a writer on a special subject to occupy any portion of his book with general information. This either is, or ought to be, previously well known by the reader. What we require of a specialist in the present day is not a ponderous volume, but well-digested individual observation of clinical, histological, or pathological facts, with as little padding as possible. Dr. Duhring's book consists of no fewer than 600 pages octavo, without counting indices, &c. Of these pages the paper is good, the type excellent, and the margins are cut. These are common and praiseworthy features in most American books. Other, but less admirable, marks of nationality are to be found in the peculiarities of diction in which our author not unfrequently indulges. For instance, the word " trouble," in its transatlantic sense, is largely employed. We read of " troubles of the nerve-centres," " kidney-troubles," " troubles of the alimentary canal," and so forth. To the reader unacquainted with Yankee colloquialisms it may be necessary to explain that " trouble " is a word constantly in the mouths of American patients.

A lady, for instance, will speak of having "head-trouble," meaning thereby migraine, and not mental distress. Such slight degree of the latter as may fall to the lot of a young lady on account of freckles is expressed by our author in the uncouth word "worriment."

The matter of the book, though not original, is sound. The author is a follower of Hebra, whose classification he adopts. He is evidently a firm believer in his teacher's system of local treatment. Great reliance is placed upon soft soap, tar, and the litharge ointment; but he approximates more nearly to the English school in regarding the ordinary diseases of the skin as due to more or less remote causes. He insists strongly on the necessity for diligent investigation into the conditions of internal organs, and evidently holds firmly to the doctrine that the majority of chronic skin affections are in intimate relation with diseases or "troubles" of the stomach, nervous system, blood, or uterus. This, we need scarcely remind our readers, is a very important deviation from Hebra, who in his teaching has discouraged all speculation of this kind. He holds that as the skin is a very large and exposed organ it is therefore of necessity liable to be the seat of primary disease. The most direct treatment is, therefore, the best, and the most direct treatment is the local. He passes in review a great number of internal remedies, only to condemn them as worthless. He speaks disrespectfully of the most revered medicinal idols. Dr. Duhring seems to accept without hesitation Hebra's testimony as to the value of local treatment, but seems unwilling in his own practice to trust to it alone. Thus he says—

"Eczema is a perfectly curable disease. For its relief two distinct methods of therapeusis are employed—one directing all its force against the skin itself as the offending organ, trusting by this means alone to restore health to the part; the other endeavouring to remedy the disorder by the employment of internal or constitutional remedies, intended to act against the real or supposed source of the disease. The plan which appears to me to be the correct one, and which in my experience has proved most satisfactory, is that which recognises both local and constitutional remedies as being of equal value. I am confident that, viewing the matter in a broad light, this doctrine affords us the best results in practice."

We must admire the frankness which admits that an internal remedy may be employed against the *supposed* source of the disease, but we are scarcely capable of viewing it in a sufficiently broad light to consider it of any value at all. Now, as this is a very good description of the way in which drugs, in the present state of our knowledge, *must* be applied, it follows that internal treatment cannot be equal in value to local. With topical applications we know exactly what we are doing; we can see for ourselves whether a tract of skin requires soothing or stimulating, and we are able to judge by mere

inspection how far our remedies fulfil the indications. On the whole, however, Dr. Duhring's directions for treatment accord with our views. We are told to correct dyspepsia, remove constipation; that diuretics are of service when there is deficient renal action, and so forth. He does not seem to place undue reliance upon specifics, and shares the prevalent belief that arsenic has been overrated. He gives good rules for its employment in eczema, and mentions Hutchinson's recommendations of it as a specific in some cases of pemphigus. He mentions phosphorus in general terms only, but does not seem to have made any trial of it in psoriasis. We have found no reference to the new therapeutic toy, chrysophanic acid, nor even to the virtues of the more primitive goa-powder. The directions for the employment of topical remedies are generally precise and ample. The student who is unacquainted with the Sydenham Society's translation of Hebra's great work can obtain a good idea of the Vienna treatment from Dr. Duhring's pages. Altogether the book may be fairly recommended as embodying a trustworthy account of the dermatological knowledge of the day. It lacks originality and personal testimony, and greatly needs compression; but though not remarkable for any striking merits, it is commendably free from error, and may be taken as a safe guide to the study of skin diseases.

Transport of Sick and Wounded by Pack Animals.[1]—This work forms Circular No. 9, issued from the Surgeon-General's Office in Washington on 1st March, 1877, and is intended to embody the experience of the medical officers of the United States Army in transporting sick and wounded in localities unsuitable for wheeled carriages; other modes of transport on animals, employed elsewhere, however, are noticed. The operations of the American forces against hostile Indian tribes, of late years, have taken place in mountainous districts without roads, and presenting such obstacles to the employment of wheeled transport that, to admit of free and rapid movement, it has been thought better to trust entirely to pack animals for conveying the necessary supplies. The troops employed have on several occasions amounted to from 1200 to 1500 men, and the Indians they encountered being bold and well armed, numerous casualties have resulted, and such of the wounded as could not proceed on horseback have had to be transported to the base of operations by such means of conveyance as could be improvised on the spot. These, in nearly all cases, seem to have taken the form either of a litter for one person recumbent, carried by two horses or mules, or of the "travail" or "travois," a very old Indian arrangement, resembling the litter in form, but drawn by a single horse or mule, while the hind ends of the side poles are

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[1] *On the Transport of Sick and Wounded by Pack Animals.* By GEORGE A. OTIS, Assistant Surgeon U.S. Army. 4to, pp. 32.

allowed to trail on the ground instead of being carried by a second animal. Opinions are divided as to the relative merits of these two modes of conveyance; the former is admitted to be more comfortable for cases that have to be carried altogether recumbent; but as two animals are required, with a man to guide each, and two others at the sides of the litter to check undue oscillation, or four men and two pack animals for the transport of a single patient, there may be many occasions when the services of so many could not be spared. With the "travail," on the other hand, a single pack animal and a driver are sufficient for the removal of a single case, unless where there is very rough ground or streams to cross, when additional assistance may become necessary. Wounded have been transported long distances in such conveyances with comparative ease, and, when the materials to construct them can be procured, they are well worthy of a trial. Mr. Otis speaks approvingly of the French mule litters, but points out that, however desirable it would be to adopt them in the American service, it would be hopeless to attempt it unless mules of sufficient size properly trained, and with attendants accustomed to manage them, were at the same time available.

What is Vital Force?[1]—An octavo volume of 328 pages bearing the above title is likely to induce a reviewer to search for merit, and a casual encounter with such a work would naturally induce the reader to look for original observations, novel theories, or recently established facts. In the case of the book under review he will find a remarkable conglomeration of a few well-known data gleaned from authors ancient and modern (even from Holy Writ), stated in such an incoherent style, in long, ungrammatical, non-understandable sentences, denied moreover or doubted, till he lays down the work in disgust. No purpose is discernible, no definite conclusions reached, but the work is full of false premises leading to mystified or incorrect inferences. In the very first page we are startled by Mr. Battye rejecting the law of gravitation. A few pages further on (p. 6) we meet with the following paragraph, the purport of which is hard to guess:

"In whatever tissue active vital functions, either of a chemico-vital or cell-destructive power, are going on, there *mucous membrane* is recognised, purely and solely from its active vital functions, altogether irrespective of the form of differentiation it may assume; *i.e.* if in its vitality it includes active vital processes that are not contractile processes, there the functions of mucous membrane exhibit a certain special active property, which declares what is its proper place in the grouping of the membranes in any special *tripartite membrane.*"

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[1] *What is Vital Force? or a short and comprehensive sketch, including Vital Physics, Animal Morphology, and Epidemics; to which is added an Appendix upon Geology: is the Detrital Theory of Geology tenable?* By RICHARD FAWCETT BATTYE. London, 1877.

The largest portion of Mr. Battye's monograph (pages 143 to 298) is occupied by a discussion of the subject of epidemics. We look here in vain for any important facts to elucidate the mysteries still surrounding this subject; certain peculiar, perhaps original, ideas are stated without genuine evidence to support them, and they deserve notice merely as examples of the shallowness of the author's argument :

"But " (he says, p. 232) " in addition to fungi, is it not possible to suppose an *animal sarcode* as perfect and yet as simple in structure as the fungi (bearing in mind in all animal growth the tripartite element), and capable of transportation, as a light and almost impalpable dust, from person to person, as much as we imagine that fungi are, as evidenced in certain skin diseases which are propagated by close contact or actual touch, and much easier of conveyance than the ponderous acari, on the animal side, propagating their brood of ova, to be kindly housed in the next neighbour's hand, who by gentle contact is sufficiently felicitous to be honoured with their presence ?"

It is scarcely conceivable that in 1877 there are published sentences, nay pagefuls, like the above, showing total ignorance of recent microscopical and experimental investigations on contagium. Again, Mr. Battye states the following novel idea in his introduction (p. 28) :—" The secular variation of the compass from east to west is about 320 years, and back again another 320, so that a complete revolution is about 640, or the period here fixed, by observation from history, as being the period or epoch of an epidemic era." But, when we refer to page 209, we find an astonishing numerical discrepancy, or, at all events, a very loose use of numbers. To continue examining Mr. Battye's monograph critically might be amusing, but would certainly not be profitable. Any one desiring to obtain strange information from such works as the Book of Maccabees, Mrs. Mariet's ' History of Astronomy,' Lardner's ' Cabinet Cyclopædia,' 1831, &c., can have their satiety of such in Mr. Battye's reply to the query " What is vital force ;" but any earnest student of the subject will avoid it as undeserving his attention.

The Rational Treatment of Wounds.[1]—This is an exceedingly interesting little brochure by a commission of distinguished Russian surgeons on the rational method of treatment of wounds. Fortunately for us, and for European medicine in general, it is written in excellent French, and with great plainness of speech the commission give the result of their experiments and inquiries.

The sum and substance of the whole matter is that they believe,

[1] *Traitement Rationnel des Plaies. Méthode d'aeration.* Moscow, 1877. Pp. 44.

as many excellent surgeons have done before them, that all bandages and dressings to a wound are evils, perhaps in some cases necessary evils; that free egress to serum, pus, and blood, with freest possible ingress of air to aid in drying up secretions and assisting granulations, are the chief objects of a judicious surgeon. Still, the wound must be protected from other injurious effects, such as dirt, pressure, and the like. Theoretically we fancy the aim of our Russian *confrères* would be best attained by placing the patient, or at least his injured limb, in a sort of wire-gauze cover, such as is to be met with in well-ordered larders, so that flies, dirt, and pressure, could be kept off, while air is let in.

Several ingenious plans are adopted for aerating the wounds, and other modes of dressing are discussed. The Lister plan of swathing the part in multitudinous folds of gauze is regarded as not so objectionable as it would otherwise be, because the gauze admits air and lets out discharges.

It is fortunate that nature has great powers of healing, even without extensive dressings, exorcisms, or poultices of figs, for we fear that in the Shipka Pass many a poor fellow has more chance of having to try the method of aeration than more elaborate or expensive dressings.

Clinical Surgery in the Paris Hospitals.[1] — This book is rather a curious one, in that it is a collection of surgical cases and observations from all the Parisian hospitals, and the practice of many or most of the distinguished surgeons of that city. The cases are selected and arranged at the will of the collector, and their description seems sometimes to be in the words and from the point of view of the observer—sometimes to be in the ipsissima verba of the master who gave clinical commentaries on his own cases. They are arranged in different ways. Now, we have an account of the cases illustrating certain forms of disease from the wards of a single surgeon; again, we have in the same chapter cases treated by different surgeons at different hospitals; and, perhaps, related to each other by some resemblance, real or imaginary. Thus, the account of an aphasic at the Hôtel Dieu is followed by that of an operation for blepharoplasty at the Pitié, and then of blepharophimosis at the Hôtel Dieu, and one of paralytic strabismus at the Saint Louis. Many of the cases are interesting, while again, others, trivial and commonplace, seem to act as padding. On the whole, however, the work gives a fair idea of what may be found to interest and instruct the surgical student in the hospitals of Paris. Some subjects have been better worked out than others; among those which have received most development are traumatic tetanus,

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[1] *Clinique Chirurgicale des Hôpitaux de Paris.* Par le Docteur P. GILLETTE, Chirurgien des Hôpitaux, &c.

and the different modes of treatment; tumours of the eye and orbit, especially those melanotic ones which so often recur; fibrous polypus of the uterus; spontaneous fracture; dislocation of the thumb, and of the astragalus; erectile tumours.

An excellent alphabetical index to the cases enables the reader at a glance to select any subject in which he is interested, and very considerable care has been taken to collect and arrange bibliographical references to rare cases or modes of treatment.

Dr. Gillette is evidently able to read and enjoy our language, as his references to British and American surgery are frequent. This volume refers to clinical work extending over four years. We can fancy that such a work, if published yearly, with the same care and common sense, would make a most valuable record of Parisian surgery. We confess that we do not envy our *confrère* the labour, which is unselfish, and we fear that the business of selection and revision of other men's cases may prove a somewhat delicate task. Are the successful cases only to be reported? or should the results be let alone?

Excision of Elbow-joint.[1]—This is an interesting brochure, containing the results of excision of elbow-joint by Dr. R. M. Hodges during a period of ten years.

First.—As to method of operating: a straight incision behind, and attention to preserving (*a*) the connection of the triceps extensor tendon with the investing aponeurosis of the arm and forearm, (*b*) the attachment of the brachialis anticus muscle to the shaft of ulna and base of coronoid process, and (*c*) the insertion of biceps into radius. A claim of originality in the teaching and practice of this method seems to be well substantiated, so far back as 1855 in the dead subject, and 1866 in the living.

Second.—As to the cases: we are at once struck by the very great proportion which the excisions for injury bear to those for disease — fourteen having been performed for injury, three for anchylosis, and four only for disease.

Third.—As to fatality: five out of the twenty-one died; all of these were excisions for injury, and of these two were complicated by other injuries; two died of blood-poisoning and one of tetanus.

Fourth.—The results as to usefulness were all good; some exceptionally brilliant, especially two or three of the traumatic cases. One is depicted in two positions of the arm wielding a heavy hammer.

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[1] *Excision of Elbow-joint, showing results. From a series of twenty-one cases operated on at the Massachusetts General Hospital.* By H. H. A. BEECH, M.D.

Eustace Smith on Diseases of Children.[1]—In the present work Dr. Smith treats only on diseases of the lungs and acute tuberculosis. Like previous work by the author, the present is characterised by carefulness and thoroughness. The book opens with some admirable remarks on diagnosis and treatment, which are of especial importance and value.

Collapse of the lung is next fully and clearly considered. The mechanism of its production is lucidly explained, and its most frequent causes are set forth. The remarks on treatment are very judicious. Croupous pneumonia is also well described, and the cases given in illustration are good. We cordially commend the chapter on pleurisy, and entirely endorse the sentence with which it opens—" that of all chest diseases in children, pleurisy is perhaps the one in which mistakes in diagnosis are most often made," the error consisting, in our experience, in the circumstance that the disease is frequently overlooked, and even unsuspected.

The author states clearly the causes, symptoms, and treatment of the different forms of pleurisy, and his teaching is sound and reliable. There is one cause of pleurisy and empyema, however, which he does not mention, perhaps only because he may not have met with it, to which we are disposed to attribute some importance, viz. fracture of the ribs. It might be thought that such fractures would be readily detected, but our own experience leads us to think that they not unfrequently escape observation. A fracture of an infant's rib would be of the "green-stick" variety, and might not give rise to symptoms indicative of the injury, as in an adult or older child. The projection inwards of the injured rib would irritate the parietal pleura, and " fret " the visceral pleura and the lung at every inspiration and expiration. We have seen more than one illustration of this—one case proving fatal from empyema, which opened spontaneously at the seat of fracture ; there was no necrosis of bone.

In discussing the next subject (catarrhal pneumonia) Dr. Smith clearly sets forth the differences, pathological and clinical, between that form of inflammation of the lung and croupous pneumonia.

Chronic catarrhal pneumonia and pneumonic phthisis, its frequent termination, are well described.

Dr. Smith writes very ably on all lung affections, and we would especially commend this section of his work.

Another valuable chapter is that devoted to fibroid induration of the lung. This form of lung mischief, though not so common in children as in adults, probably occurs in youth with greater frequency than is generally believed. Dr. Smith has very ably pointed

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[1] *Clinical Studies of Disease in Children.* By EUSTACE SMITH, M.D. London, 1876.

out its characteristics, and the chapter will well repay careful perusal.

The author's intimate practical acquaintance with children's diseases is well shown in the section devoted to acute general tuberculosis. The recognition of tubercular mischief is probably as important to the practitioner as that of any other disease which affects children. Not that it is very amenable to treatment—for, unfortunately, little is to be hoped for in that respect—but because of the importance, for prognostic and therapeutical purposes, of making the differential diagnosis between it and other maladies which are curable.

Dr. Smith gives a very good account of tubercular meningitis, and the truthfulness of the clinical picture drawn by him will be recognised by all who are at all familiar with the disease. There is but one point on which we should be disposed to join issue with Dr. Smith, and that is respecting the duration of the illness. He says, " It seldom ends sooner than seven days, or lasts longer than twenty-one." Our own observation would lead us to the conclusion that the duration considerably exceeds three weeks in not a few cases ; these are the unfortunate cases in which delusive hopes are from time to time excited, leading only in the end to bitter disappointment.

We congratulate Dr. Smith on the present work, and trust that in due time he will favour the profession with a complete and exhaustive treatise on children's diseases. Even were it not already fully acknowledged, Dr. Smith's experience with children's diseases would furnish ample justification for the consideration of infantile maladies in a further treatise.

Atthill on Diseases of Women.[1]—Dr. Atthill does not put forth this work as a complete treatise on gynæcology, but offers it as a contribution to the subject from a clinical standpoint. It embraces a number of important affections, and in the main affords reliable guidance to the diagnosis and treatment of most of them.

In the first lecture directions are given for the examination of patients. They are clear and sufficient. Leucorrhœa and vaginitis are next discussed, but not at all as clearly or accurately as might have been expected. In the midst of this lecture remarks on vascular growths of the urethra are interpolated, which are neither sufficient nor consistent; for if they are called " trifling though very troublesome," " their removal is a matter of difficulty."

The subject of amenorrhœa is pretty fully and judiciously treated, and dysmenorrhœa is also well handled. We are somewhat afraid, however, that Dr. Atthill too lightly estimates the dangers attending

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[1] *Clinical Lectures on Diseases peculiar to Women.* By LOMBE ATTHILL, M.D. Fourth edition. Dublin, 1876.

division of the cervix uteri. They are confessedly so serious that the operation should only extremely rarely be performed.

Menorrhagia is fully, though we think not very satisfactorily, handled. The wisdom of placing nitrate of silver in the uterine cavity appears to us doubtful. Probably Dr. Atthill's plan of using fuming nitric acid is preferable, though that is not an easily applied remedy.

The author frequently makes use of the term " granular ulceration," which he applies to the granular erosion so often met with in gynæcological practice.

The condition in question is not one of true *ulceration*, but of erosion, and such erroneous pathological teaching is, therefore, misleading.

We cordially approve of the author's emphatic recommendation to dilate and explore the uterine cavity in cases of menorrhagia, which are not due to some readily discoverable constitutional or local condition. Many of these cases are due to intra-uterine lesions, and are curable only by local treatment.

The remarks on uterine polypi are practical, and we would especially commend the teaching of Dr. Atthill in respect of treatment. Dr. Atthill recommends the removal of polypi by the wire écraseur, because he has seen such profuse hæmorrhage from the use of the knife and scissors. He also dwells upon the fact that " intra-uterine polypi, in the majority of instances, occur in women who have never been pregnant," a statement that our own experience enables us to confirm.

We commend the lecture on fibrous tumours as sound and practical; but we are surprised that no reference is made in it to the treatment of large growths by ablation of the uterus when relief is demanded and other methods of treatment are impracticable. Such operations are undoubtedly grave, but so also is the disease, and the experience of Péan, Keith, Koeberlé, Wells, and others, has been so encouraging that this mode of dealing with large uterine fibroids merits discussion in any work professing to deal with the surgery of the subject. Inflammation of the cervix uteri is next usefully discussed; but pelvic cellulitis and pelvic hæmatocele are most meagrely and imperfectly dealt with. These are important and serious affections, and we cannot but regard the work as gravely defective in respect of these affections, for other matters might usefully have been omitted to make room for the consideration of them. It would appear that the author has selected for his lectures those subjects with which he is most familiar, or in which he takes the greatest interest. Flexions and versions of the uterus are ably treated. The statements made are accurate and sensible, and the teaching is thoroughly reliable.

Lecture XII, on " enlargements of the womb," is not satisfactory.

This very important subject is dealt with in a disconnected and fragmentary manner, quite insufficient for practical purposes. It might reasonably have been expected that in a lecture devoted to uterine enlargements, a comprehensive and complete survey of the whole matter would have been given, together with indications for the differential diagnosis of the various forms; but such is not the case.

On the other hand, we gladly commend the lecture on uterine cancer as on the whole excellent, though we can hardly agree with the author that in cases of cancer of the body of the uterus we are "utterly powerless." Ovarian disease is but poorly handled; and a practitioner would derive but little help in the diagnosis or treatment of any forms of ovarian disease from the perusal of Dr. Atthill's lectures.

But Dr. Atthill is decidedly strong in his excellent remarks on "uterine therapeutics." The only thing to be wished is, that he had given fuller indications respecting the kind of cases in which the remedies he recommends should be employed.

On the whole, it may be said that in spite of deficiencies, which their able author would have but little difficulty in amending, these lectures form a very useful contribution to an important department of medicine. They excite in us the wish that Dr. Atthill would undertake a more complete and exhaustive work.

Original Communications.

I.—On the Transmission of Syphilis from Parent to Offspring, with an Appendix of Cases. By Jonathan Hutchinson, F.R.C.S., Senior Surgeon to the London Hospital.

That acquired syphilis developes itself with exceedingly different degrees of severity in different persons all will admit. For the most part we are obliged to accept this fact without explanation, and to say simply that in itself it proves that individuals differ much in their degree, or kind, of susceptibility to the syphilitic virus; or perhaps we ought to express our meaning in somewhat different words, and say that the tissues of different persons show differing tendencies under the influence of syphilis. For there are a class of facts which show that it is not so much differing susceptibility as different kinds of reaction. Of twenty persons who may show the disease in exceedingly different phases of severity it may be true that no one differed from the others as regards susceptibility. All contracted the malady with equal ease, all went through the same stages, and all were equally protected by it as regards future attacks; but how different may have been the kind of manifestation. One may have had a sore which lasted, it is true, a month or two, but which gave him no trouble, and which was not followed by anything which could be recognised as a constitutional phenomenon, whilst another may be covered from head to foot with eruption, have lost his hair, had sores in the throat, iritis, and a state of general ill health which laid him up for months. That the one as truly went through the stages of the syphilitic fever as the other is proved by the almost equal liability to tertiary symptoms, and the equal risk in each of transmission to offspring. Some physicians—Dr. Wilks, I think, for one—to whom we are much indebted for investigations as to severe visceral forms of tertiary syphilis, holds that it is exceptional in these cases to be able to obtain any good history of secondary symptoms. Without adopting this opinion, and whilst believing that the secondary and tertiary symptoms are usually as regards severity in direct rather than in inverse ratio, it is yet of importance for my present purpose

to allude to it, and to admit that it is one for which much might be said.

It is not true that this varying severity in the secondary stage is to be explained by reference to the patient's state of health, temperament, age, sex, or diathesis. The most robust are often those to suffer most severely ; and though we may admit that syphilis does acquire certain minor differences in connection with the age, sex, and temperament of its victims, yet these certainly go but very little way in explanation of the startling variations in severity to which I have adverted. That the secondary symptoms are usually in ratio with the primary seems probable, at any rate it is very rare for a patient who has had a chancre of unusual development in respect to size and hardness to escape, excepting as a consequence of treatment, a severe outbreak of eruption, &c. In saying this, however, we say little more than that the manner and degree in which a chancre indurates may be taken as a specimen of the manner in which the cell-structures of the individual are likely to behave when brought under the influence of the syphilitic virus. Apart from this we possess no knowledge which will enable us to predict during the first stage of syphilis or before it as to any given person whether he is likely to suffer severely from syphilis or not. It would be as easy for a physician to pick out those who will have smallpox badly. Both in the case of the medical exanthems and of syphilis it would appear that the difference in constitution which determines severity or mildness may be a very slight one, for it is common enough to witness that, of brothers or other near relatives apparently of similar temperaments and in like conditions of health, one may suffer very severely and another escape with a very slight illness.

I feel sure that it is almost impossible to estimate too highly the importance of this fact, that acquired syphilis is, for rea· sons which we cannot explain, very unequal in its incidence in different persons. From want of giving it due weight we are exposed to endless fallacies in our inferences, both as to the natural history of this disease and the results of treatment.

A precisely similar line of argument is applicable to inherited syphilis. We do not yet know within what limits of severity or of mildness the disease may vary. Our investigations here are even more beset with difficulty and uncertainty than in the case of the acquired disease. We do not, in the first place, know whether inherited taint is always the same kind. It has been customary to assume, as regards inheritance, that the child born nearest to the parent's taint is the one most likely to suffer severely, and the younger, if they do not escape entirely, may perhaps suffer only slightly. But a possible fallacy under-

lies our creed in this matter which it is very desirable to expose. It may be that, after all, there are no degrees of severity in inheritance, no varieties in the potency of the virus received by the child, and that if a child inherits any taint whatever it inherits, as a necessity, the whole disease.   It is possible that the extremely unequal severity with which different children suffer is to be explained rather by differences in idiosyncrasy than by supposing that one received a large dose of the poison and another a small one.   That this is really so may be made probable if we can show that of children born within equally short periods of the parental contamination the range of variation in severity is as great as in those more remote.   It is with the hope of supplying some evidence on this and on many other points in respect to the facts as to inheritance of taint that I have collected the following cases from my note-books.   They are purposely put as concisely as is possible consistently with clearness of statement.   Isolated cases prove but little, and, being firmly convinced that it is only by the most liberal citation of evidence that we can hope to arrive at truth, I make no apology for trespassing on the reader's patience with so many case narratives.

I cannot better illustrate the apparent irregularity with which the taint of syphilis is transmitted to children than by mentioning two cases which were brought recently under my notice on the same day.   On the morning in question a surgeon from the country consulted me respecting his own case.   He had married three or four years after syphilis, having been under treatment in the first instance for about six months, and having believed himself quite well for two years or more.   His wife never suffered, but her first two conceptions ended in dead births, and the next two children, although born alive, died with the usual symptoms of inherited disease.   Seven children now living have all in turn shown symptoms of infantile syphilis.   Thus, it would appear that eleven conceptions have in succession been tainted, the inheritance being from the father only, he being in fair health, and a period which has now reached fifteen years having elapsed since he contracted the disease. Let me mention my next case in contrast.   On the same morning my friend Dr. Fletcher, of Camden Town, brought to me a married lady from the country whose antecedents he was well acquainted with.   This lady's husband had contracted syphilis during her first pregnancy, and been treated for it by Dr. Fletcher.   His wife about six months later showed the usual symptoms—had rash, lost her hair, and had sores on the tongue. Her primary symptoms had been neglected, her husband not choosing to run the risk of exciting her suspicions.   After this

Mrs. —— has remained under Dr. Fletcher's observation at times until the present date, and she still shows numerous sores on the sides of the tongue and at the angles of the mouth, and has a few patches remaining on the skin. It was for these persistent symptoms that she was brought to me. Now, the remarkable fact as to inheritance is this, that this lady has, during the last two years, borne two children, and that neither of them has shown any symptoms. It will be seen that they are the offspring of parents both of whom have suffered within three years, and one of whom (the mother) still shows symptoms indicating considerable activity in the virus. I have been careful to say merely that they have shown no symptoms, not that they have escaped, for I do not believe that they have escaped. I should almost regard it as impossible that children could be born under such circumstances and escape a taint. Without, however, believing that they have really escaped, the two series of facts may serve well enough to illustrate my point as to very unequal severity in the incidence of the disease, and to suggest a source of fallacy in our observations which we ought constantly to keep in mind, and for which it is almost impossible to make too much allowance.

I. *Inheritance from Father and Mother.*—Case of Esther R—, 46, and daughter; (Bag " Syph. Paralysis ").—Mother had an eruption six weeks after marriage, which her husband told her was caused by venereal disease from him. This was sixteen years ago; two years ago she had sudden *left* hemiplegia, and paralysis of *right* fifth nerve; was unconscious for three weeks. Gradually recovered, but had a relapse on same (left) side, and again has nearly recovered power of left arm and leg, and partially sensation of right face; right eye lost. No details as to husband.

1. F., æt. 14, no history of infantile symptoms, but a doubtful account of keratitis.

2. F., died at birth.

3. F., æt. 9, now under my care. Periostitis of left tibia; double keratitis; typically notched teeth.

4. F., died at birth.

5. F., died at birth.

There were also two miscarriages at about three months, the dates of which in reference to the other pregnancies is not noted.

II. *Inheritance from both parents.*—Anne H—, 4, M. F. " O," p. 128, and Honora H—, M. F. "O," p. 140.—Mother had primary and secondary syphilis eleven years after the birth of her first child. No note of father.

1. Reported to be quite healthy. Now mother's syphilis. Since then seven other children. Of these five have died : one

at three weeks; one at one week; two born dead; one at two weeks; two are living, *viz.* Honora (see reference above), æt. seven *years.* Typical aspect and nose. Sores in mouth from sharp teeth. Anne, æt. four years. Scars about anus and on legs ("? small-pox") and strumous ophthalmia.

III. *Inheritance from both parents.*—Sarah G—, 26, and son, M. F. N., Nos. 2896, p. 133, and 30, 58.—Mother married eight years. Good health till then, but failed afterwards. No history of primary or definite secondary disease. Had sore tongue and mouth; then four years after marriage serpiginous eruption on knees; then nodes on tibia, and now enlargement of right femur, rapidly diminishing under iodide. No note of father.

1. Æt. eleven, reported to be healthy and never to have ailed anything.

2. Died, æt. five weeks, with snuffles and rash.

3. M., James G—, æt. 6. Had severe snuffles and rash, and a condyloma on tongue, which was burnt, and scar remains. Now kerato-iritis.

The evidence as to the father is negative; the wife may have acquired it elsewhere after marriage, but her history is more that of disease from conception.

IV. *Inheritance from Father ?*—Case of Flora C—, B. 33, and my book, p. 168.—No history of syphilis in either parent to be obtained from mother. She thinks her husband may have had it before marriage.

1. F., died at one month.

2. M., died at birth.

3. F., died at birth.

4. F., died at nineteen months.

5. F., died at four months.

6. Died at ten months, "atrophy."

Several of these had blotches, &c.

7. Flora C—, 10. No history of infantile symptoms. Now most typical teeth and physiognomy. Lately, extensive choroido-neuro-retinitis in each eye, and partial idiotcy.

8. F., died at ten months.

9. F., seven years old, "healthy."

Remarks.—Persistency for seven years, at least, very probable.

V. *Inheritance from Father and Mother.*—(H— family. M. F., vol. 1, case 207; vol. 3, pp. 63 and 139; vol. O, p. 106; vol. M, pp. 165 and 292 and 107; vol. K, p. 29.)—Father had primary syphilis and suppurated buboes at æt. 20, two years before marriage. Now (æt. 27), has psoriasis palmaris. Wife had no symptoms till after birth of first child, when she suffered from sores and condylomata.

1. M., Edward H., æt. 5, Sept. 29, 1857. Had snuffles for three or four months, and was puny till æt. two years. Then "large patches of psoriasis" on face and chin, which afterwards became "slightly tubercular," and was cured by iodide. Then at æt. 5 condylomata at anus.

2. M., John W. H., at æt. 5 weeks; "beginning to waste," and commencing "syphilitic psoriasis" January, 1856.

3. M., Thomas H., æt. 6 months, (Oct. 1859). Snuffles, severe cachexia, psoriasis, &c. Syphilitic onychia (*nails sketched*).

Remarks.—It is possible from the above statements that the mother contracted primary syphilis after she had borne a syphilitic child. If so we should have an exception to Colles' law. But against such a supposition is the fact that if it were true we should have to suppose that the husband had fresh syphilis within a short period of his first attack. I have thought it best to let the notes stand as I made them twenty years ago, but my present belief is that the wife must have been mistaken in her history, and that probably what she described as her first symptoms were only a relapse.

We have proof of persistence of transmission for seven years, and I believe that it extended much longer. Mrs. H., who suffered most severely, was under my observation for ten years or more, and brought me child after child all suffering with equally definite and severe symptoms. The last that I saw suffered just as severely as the first. All were born alive, and I believe that under treatment all survived. I regret that my notes do not supply details of more than the above three.

VI. *Inheritance from Mother only.*—Bag "Hered. Syph.," drawer 34. Joseph A—, 3.—Mother caught syphilis from a nurse child seven years ago and between her third and fourth pregnancies. She had a sore on nipple and copious secondary symptoms; good health since. No note as to father.

1. M., died at ten years of "phthisis." No history of syphilitic symptoms.

2. Living, 8½ years old; reported good health.

3. Died of "fever."

. Now mother's syphilis.

4. Died at three months, "gastric fever." History of syphilitic symptoms.

5. Joseph Andrews, 3. Had bad snuffles and rash and swellings on head, which did not break. At three months, inflamed eyes resulting in occluded pupil in the right (iritis). Scars at angles of mouth, head large, nose flat, upper incisors decaying.

6. Died at seven months.

Remarks.—Death-rate two in three before the syphilis, and the same subsequently.

This case and the following are alike, in that the mother contracted syphilis accidentally and at a known date. There is every reason to believe that in each the inheritance was from her only. In Mrs. D.'s case I myself saw and treated her chancre. Both women had borne children before the accident. It is a warning lesson against hasty inferences to note that in each the infantile death-rate had been very heavy *before* the disease was introduced.

VII. *Inheritance from Mother only.*—Mrs. D—, 36, and infant M. F. L., pp. 13 and 384, and K., p. 32.—Mother contracted syphilis by nursing a neighbour's child, which shortly afterwards died of the disease.

She had a characteristic hard chancre on the nipple, followed by rash and condyloma. She had weaned her own baby just about the time when she began to suckle the neighbour's child. Her own baby remained healthy. The neighbour's child had a badly ulcerated mouth.

Born before mother's syphilis :

1, 2, 3, 4, 5, 6, 7, four dead (no details) ; three living, and reported healthy.

8. Living and healthy, weaned just before mother caught syphilis.

Now mother's syphilis; chancre, December, 1857 ; rash, &c., to June, 1858.

9. Born dead in October, 1858.

10. M., George D., æt. 1 month, born September, 1859, at eighth month. Was very ill at birth, and soon wasted ; under care for severe syphilis at æt. one month, and died soon after.

VIII. *Father.*—George P—, 47, and son, G. 313.—Father denies ever having had any venereal disease, but admits risk before marriage. He is under care for cycloplegia of one eye (incomplete) and had old choroiditis in the other.

1, 2, 3, 4, 5, and 6. All F., all alive, and reported to be and to have been healthy. None are deaf, and none have had bad eyes.

7. M., died at æt. three years, of " smallpox" ; all the others also had the disease, but the parents did not take it.

8 and 9. Miscarriages.

10. M., æt. 9 ; now under care for severe double keratitis, with a small hypopyon in one eye. Teeth suspicious.

Remarks.—It may be plausibly suspected that the disease was introduced between the seventh and tenth birth.

IX. *Father and Mother.*—Mary B—, 14, M. F. J., p. 1631.—There is here no history of syphilis in father, and only an account of some sores about genitals and anus in mother " after one of her early confinements." The husband, a few years after marriage, was found to be living with another woman, and from the facts

as to the children, it becomes nearly certain that syphilis was introduced by him between his wife's first and second pregnancies.

1. F., æt. 18, well grown, florid, and shows no signs whatever of syphilis.

2. Born dead.

3. Died at nine months, " water on the brain."

4. F., Mary B., æt. 14, extremely marked syphilitic physiognomy, typical teeth, nearly blind from most severe keratitis and iritis, for which she attended hospitals between æt. 6 and 10. Not deaf. History of nodes on head. Very severe rash and illness in infancy.

5. Died at two weeks.

6. Died at ten months.

7. Living, æt. 11 years. Forehead large, but nothing characteristic of syphilis in physiognomy. Is delicate.

8. F., died at eight months.

9. F., died at ten months, with "the head."

10. M., Living, æt. 6 years. Reported healthy.

11. F., died at ten months.

The facts as to the cause of death of several of the infants are wanting, but there is no proof that the disease persisted long in the family. It is not certain that more than one suffered, although the two following deaths may be suspected.

X. *Inheritance from Father* (G. 211).—Father had syphilis at 17, and has since had much psoriasis, benefited by mercury; now has nerve symptoms. (The above is from the surgeon who has attended him throughout.) Mother is the subject of well-marked hereditary syphilis, with keratitis; is eighth of thirteen, nine of which were stillborn. One of the four living ones has fits and is partly out of his mind. She has had no disease from her husband (direct questions).

1. M., æt. 12, now has interstitial keratitis of one eye and choroiditis in other. History of severe infantile symptoms.

2, 3. Born dead.

4. Living; said to have had no symptoms.

5. Born dead.

6, 7, 8, 9. Living, and reported to have had no symptoms.

10. Born dead.

Of the six living five are males, one female.

Of the four still-born two were males, two females.

Remarks.—In this and the following case it of some interest to note that the mother was herself the subject of inherited taint. Had it happened that the history of disease in the father had not been forthcoming it might have been assumed that the child inherited from her mother, and that we had proof of transmission o third generation. All facts seeming to support such a supposi-

tion must be received with much doubt, unless the history of the other parent is very clearly negative.

XI. *Inheritance from Father.*—Case of Sarah F—, F. 374.— Date of father's syphilis not ascertained; has been "very gay," and is now (æt. 53) hemiplegic. Mother married to him thirty-one years, and now 53; has several times had gonorrhœa from husband, but no history of syphilitic symptoms. *She is herself the subject of hereditary syphilis.*

1. Miscarriage at third month, twelve months after marriage. Now the interval of twelve years without a pregnancy; during this time and three years after marriage husband gave her gonorrhœa for first time.

2. M., born dead at full time.

3, 4, 5, 6, 7. All miscarriages.

Remarks.—Not a single viable child, but no proof that any were syphilitic.

XII. *Father, probably.*—Case of Mary Anne B—, 9, G. 319.— Father reported by mother to have had good health till after birth of first child. Between first and second child he became ill of "rheumatism." Was very ill for a year and a half, and never well afterwards, but no history of syphilitic symptoms. He died at 43 of "bronchitis" and "consumption." Mother robust and healthy, appears to have had no symptoms.

1. M., living, 23, always had good health. Now father's illness, probably syphilis, with severe rheumatic symptoms.

2. M., died at fourteen months; a "healthy fat baby," died after six days' illness; "teeth and inflammation."

3. Born dead.

4. M., died at first month. "Delicate."

5. F., died at fourteen months. Weakly; "abscess on shoulder."

6. F., died at five years. "Diseased bone behind ear," probably result of "measles and scarlet fever."

7. M., died at eight months.

8. F., died at eight months.

9. F., Mary Anne B—, æt. 9. Interstitial keratitis of right. Extremely typical teeth. Slight deafness. Moderately characteristic physiognomy. Scar of an abscess under right lower jaw (probably strumous).

It is very probable, but not certain, that the father had syphilis. The great mortality is remarkable.

XIII. *Inheritance from Father* (F. 398).—Father had venereal disease, with bad throat, twelve years ago. Has had multiple round ulcers on right leg since primary disease, and now has paresis of left sixth nerve and noise in left ear of three weeks' duration.

Wife has had seven children, six M., some of which were born dead, and all the rest died within a few days of birth.

1. F., living, æt. 8. She shows no signs of syphilis, but has not yet cut permanent incisors. She is about the middle one of the seven.

Remarks.—But one child living out of seven. All died within a few hours of birth, but there is no absolute proof of syphilis in any.

XIV. *Inheritance from Father.*—Case of Mr. P—, 44. Priv. H. 197.—Father had severe " gonorrhœa " twenty years ago, lasting many weeks (twelve years before marriage). No history of secondary symptoms or of a chancre. He has, however, characteristic white marks on tongue and inside cheeks. Married eight years. Wife appears not to have suffered.

1 and 2. F., both living and healthy; had no symptoms.

3. M., died at a few months. The surgeon said it was syphilitic.

4. M., born prematurely; died at two weeks.

5. M., living and well.

6. M., died at six weeks. Was covered with " syphilitic rash."

Remarks.—This case is difficult of interpretation. It is difficult to avoid the suspicion that the taint was introduced by one of the parents between the birth of the second and third, probably by the father. If this be so we have three deaths out of four.

XV. *Inheritance from both Father and Mother.*—Case of Edward H—, 9. G. 146.—Father " gay; " wife knows he " had venereal disease several times, both before and since marriage." Details wanting. Wife has had vaginal discharge repeatedly, and some history of *recent* secondary symptoms.

1. M., a seven months' child; died at birth.

2. M., Edward H—, 9; a seven months' child. Had bad snuffles; no rash. Interstitial keratitis; typical teeth. Characteristic physiognomy. Badly grown. History of purulent ophthalmia in infancy.

3 and 4. M., Twins, born dead at six and a half months.

5. M., died at eighteen months, "teething." Thrush and snuffles. A seven months' child.

6. M., died at birth. Seven months' child.

7. F., living, æt. 14 months. Had snuffles. Now pale and badly grown.

Remarks.—The taint has lasted through the whole family, unless in one or other parent it has been reintroduced.

XVI. *Inheritance from both Father and Mother.*—Mrs. J—, H. S. D. Daughter at Moorfields, for keratitis, Nov., 1873.—Husband had syphilis between the second and third child; treated and

got quite well, and has remained so ever since. Mother had it from him during her fourth pregnancy; she had it mildly, but five years later began to suffer from severe ulceration of nose, leading to loss of bone and destruction of the alæ. Cured by iodide at Skin Hospital, and has remained well for the last six and a half years. Was my patient at Skin Hospital.

1. F., 16. Good health.
2. Miscarriage.
3. M., 14. Good health.

During next pregnancy the syphilis occurred. It is not stated at what period of pregnancy the mother became infected.

4. F., 12. Had snuffles, and gave her mother much trouble in infancy. At æt. 12 had typical interstitial keratitis; under my care at Moorfields. Typical syphilitic teeth.
5. M., 11.
6. F., died at ten weeks.
7. M., 8. In good health.
8. F., 6. In good health.
9. F., 5. In good health.
10. M., 4. In good health.
11. F., 9 months. In good health.

Several of these had snuffles in infancy, but none ailed much.

Remarks.—It may be doubted whether the younger children have really inherited any taint. The single symptom of snuffles estimated only by the mother cannot count for much, still it is possible that it was of syphilitic origin. It is an instance of a child *in utero* contracting syphilis from its mother, but unfortunately the stage of pregnancy at which the disease was acquired is not known.

XVII. *Inheritance from both Father and Mother.*—Case of Lillian W—, 5. F. 388.—Mother had venereal disease, with spots on thighs, a month or two after marriage. No positive history of symptoms in father.

1. F., born dead.
2. F., Lillian W—, 5. Had snuffles in infancy; now has interstitial keratitis and suspicious physiognomy.

Remarks.—We have here the first born, after recent disease in both parents, killed probably by it, and the second suffering severely, but surviving.

XVIII. *Inheritance from both Father and Mother* (?).—Case of Eliza T—, 2. G. 16.—Mother had sores on genitals and spots on thighs three weeks after marriage; no history of general rash, but her hair came out. No particulars as to date of syphilis in father.

1. F., born dead at seventh month.
2. F., born dead at eighth month.

3. F., Eliza Taylor, 2.   Had snuffles, thrush, and eruption on head and buttocks.   Now has choroido-retinitis in each eye.

4. M., æt. 6 weeks.   Reported healthy.

P.S.—Two and a half years after above notes.

5 and 6.   Born since above notes, and reported healthy.

Remarks.—As in the preceding case, we have first dead births, then a child suffering severely but surviving, and then healthy children.

XIX. *Inheritance from Father and Mother* (F. 347).—Case of Mrs. M—, æt. 36.—There is no statement as to syphilis in the father, but there can be very little doubt.   The mother, who has been married to him fifteen years, and seems never to have had symptoms of syphilis till nine months after the twelfth confinement, when she came under care for paralysis of one third nerve, which passed off in two months under iodide.

1, 2. M., born dead.

3. F., born dead.

4, 5, 6, 7, 8, 9, 10, 11. M., all born dead.

12. M., æt. 10 months.   Had bad thrush, eruption and snuffles, and now has a somewhat prominent forehead, depressed nasal bridge, and sore at angle of mouth.   No condyloma.

Remarks.—A good instance of what is supposed to be very common—a long series of dead births due to taint.   If we may assume that syphilis was the cause, and that no second introduction of the disease occurred, we have proof that transmission may occur twelve or fifteen years after the primary malady.

XX. *Inheritance from both Father and Mother.*—Amelia L—, æt. 9 (M. F. N., p. 174).—Father has now badly cracked syphilitic tongue.   Mother no symptoms, except pains in bones.

Four pregnancies; one miscarriage (date not given), three born alive, as under:

1. F., Amelia L—, æt. 9.   Treated by Mr. Startin in infancy for rash on buttocks, &c.; had no snuffles; now kerato-iritis (began with unusually well-marked iritis, the corneal haze supervening shortly after).

2. Living, and reported healthy.

3. Died at two months.   Was a six months' child.

XXI. *Inheritance from both Father and Mother* (S. H. Letters, 1868-70, 246).—Harriet T—, æt. 44.—During the last four years mother has had several large gummous tumours on left arm and elsewhere.   In other respects has had good health since marriage. Husband reported healthy, but no details as to whether the syphilis was derived from him.

1. M., died at four months.

2. F., æt. 14 ; has notched and screw-driver teeth, but nothing else.

3. F., æt. 13.   No signs of syphilis in teeth or physiognomy.

4. M., died at four months.

5. M., died at two months.

6, 7, 8, 9. All F. Living, and reported healthy ; one who comes shows no signs.

Remarks.—The children were brought up by hand, away from home, and mother knows nothing of the symptoms in the third (M.), who died in infancy.   In this instance the taint appears to have ceased, and the younger children have shown no symptoms.

XXII. *Inheritance from Father only.*—S. H.   Letters, 1875, 375. James L—, 43.—Father had a chancre, for which he took pills till his mouth was sore, eight years ago.   Had no secondary symptoms.   Comes for pityriasis palmaris of one hand, which has rapidly healed under bichloride of mercury.

The primary disease occurred after death of his first wife and one year before he married a second time.   The conceptions terminated as follows :

1, 2. Miscarriages at third month.

3. F., died at birth.

4. F., died at two weeks.

5. F., living, æt. 9 months ; reported to have snuffles.

Remarks.—Here we have two miscarriages, a dead birth, and an infant which died early, and then a living infant which showed symptoms.   An instance of too early marriage after syphilis.

XXIII. *Both Parents.*—L. H. Letters, 1868-70, 468.   Emma P—, 32.—Mother suffering from serpiginous tubercular eruption on forehead, of nearly two years' duration.   It began about five months after first confinement, probably by conception.   No note as to husband ; married three years.

1. F., born nine months after marriage ; died at two days.

2. F., living, æt. 1½ year. Had snuffles, but no thrush. Bridge of nose depressed, frontal eminences prominent, upper incisors have already dropped out.

XXIV. *Father.*—F., p. 154. Case of Maria D—.   Father now in late tertiary stage ; has lost bone from hard palate and had extensive ulceration of soft palate and uvula ; dates of syphilis and marriage not given.   No note as to mother.

1. born dead.

2. M., living and reported healthy.

3. Born dead.

4. M., living and reported healthy.

5. F., comes to hospital at request.   No signs of syphilis ; no history of past symptoms.

6. M., living and reported healthy.

7. F., Maria Duff, 13. Typical teeth and symmetrical interstitial keratitis and iritis; deafness.

8, 9. F.

10, 11, 12. Miscarriages.

13. M.

14. F.

15. M., æt. 6 weeks. Reported to have thrush.

Remarks.—In all probability the disease was acquired by one or both parents between the sixth and seventh births. The subsequent notes are imperfect.

XXV. *Inheritance from Father probably.*—Case of Matilda J—, 19, F. 304.—No conclusive evidence as to source of the syphilis. Mother married twice, and several of the children by the second husband furnish more or less conclusive evidence of the introduction of the disease by one or other parent at that date.

By first husband :

1. F., æt. 27. Shows no signs of syphilis.

By second husband (probable date of introduction of syphilis) :

2. M., died in infancy.

3. F., died in infancy.

4. F., Matilda J—, 19. Interstitial keratitis; screw-driver teeth; scarred angles of mouth; chronic synovial thickening of left knee-joint.

5. F., living, æt. 17.

6. F., living, æt. 15.

Said to be, and always to have been, in good health.

Remarks.—The taint would appear to have ceased with the fourth child.

XXVI. *Inheritance from Father.*—G. 301.—Father had well-marked syphilis about eighteen years ago (date in relation to marriage not stated). Was salivated for the secondary symptoms. Now paresis of right third nerve.

1, 2, 3, 4, 5, 6, 7. Miscarriages.

8. F., æt. 13. Is very stunted; looks only eight. Physiognomy suspicious, but not more. Teeth normal. *A single abruptly defined patch of choroidal atrophy* at periphery of one eye.

9. M., æt. 8. Healthy looking and well-grown.

10. Living, reported healthy.

The choroidal patch is important and interesting evidence.

Remarks.—It is probable that the child was born within five years of her father's syphilis. No facts as regards the escape of the mother are recorded. Four years after the birth of the tainted but viable child we find healthy infants.

XXVII. *Father* (?).—Case of Mary A. S—, syphilitic keratitis.
—Father a sailor. Mother knows of no suspicious symptoms either
in him or herself. He is troubled with "abscesses." Probably
the father had the disease before the birth of the fourth child.

  1. M., 28.
  2. F., 26.
  3. F., 24.

Conjectural date of syphilis.

  4. Born dead at eight months.
  5. Born dead at seven months.
  6. F., 20, Mary Anne. Has still the remains of keratitis, which
occurred at æt. 9. Teeth typical.
  7. F., died at six weeks "from a small bleeding sore."
  8. M., 16. Good health.
  9. M., died at 3 of measles.
  10. F., died at nine months of measles.
  11. M., 9. Good health.
  12. F., died at a year and three quarters, of measles.
  13, 14, 15, 16, 17, 18. Miscarriages at about four months.

None of the children, according to the mother's memory, had
suspicious symptoms in infancy, but the sixth was the most delicate
baby.

Remarks.—We have here a large family only one member of
which shows undoubted signs of inherited syphilis, she being born
after two miscarriages, which had been preceded by three healthy
children. After five others had been born, all of which remained
free from symptoms, we have a series of six miscarriages.

XXVIII. *Inheritance from Father.*—Case of Fairlie D—, 8.
F. 280.—Husband probably had syphilis before marriage. No evi-
dence that the mother ever suffered at all. Married eleven years.

  1. M., a premature birth, died at five days.
  2. M., Fairlie D—, 8, kerato-iritis and characteristic physio-
gnomy. Teeth malformed, but not characteristic of syphilis.
  3. F., died at one week.
  4. Miscarriage.
  5. M., living, æt. 5.
  6. F., died of "wasting" at four months.
  7. Miscarriage.
  8. M., born dead at seventh month.

Remarks.—The taint has probably persisted through eleven
years.

XXIX. *Inheritance from Father, probably.*—C. 313. Case of
Alice B—, 8.—Date of syphilis doubtful. Father had a deep ulcer on
one arm about three years ago. Mother had eruption of red blotches
on her legs after birth of the eleventh child (the patient); these

have left stains eight years after. Thus, probably the mother's disease was due to her fœtus.

1. M., died at six years.

2. F., living, 22. No symptoms in infancy or since.

3. F., living.

4, 5, 6 and 7. Miscarriage at about third month.

8. M., 16; no symptoms at any time; healthy.

9. F., 15;   ,,   ,,

10. M., 11;   ,,   ,,

Probable date of syphilis in father.

11. F., Alice B—. Scaly rash on forehead at three months; subsequently severe symmetrical keratitis, partial deafness, elongation of tibiæ. Teeth good.

12. M., 6. Healthy (not seen).

Remarks.—A single member of a large family suffering from syphilis; predecessors and successor reported healthy. Several miscarriages at a distance from the syphilitic child, and with healthy children intervening. Probably the disease was contracted by one or both parents not long before Alice B— was conceived.

XXX. *Inheritance from Mother and Father, probably.*—L. H. Letters, 1871, 343. Louisa S—, 29.—Mother had sores on genitals before first confinement, but does not remember any rash. Now has ulcers and scarring on one forearm and on front of neck. No history of husband.

1 and 2. Both born dead.

3. Died with snuffles and rash at three months.

4. Living, æt. 5 years. Attended at L. H. for a rash on the face, but no details obtainable.

5. Living and reported healthy and not to have had any symptoms.

6. Died at nine weks with eruption and other suspicious symptoms.

Remarks.—Here the taint seems to have persisted through the family, but to have fallen on different children with very varying severity.

XXXI. *Inheritance from Father.*—Case of Mary Anne S—. F. 356.—Father had some venereal disease when æt. 18, two years before marriage. Was salivated for it, and " afterwards " had ulcers on the left leg for several years, which have left scars. No definite history of secondary symptoms.

Mother died of " decline " at 54.

Family consisted of sixteen children, born alive. Of these only five are now living; eleven are dead; some died at a few days, some at about fifteen months, and one at 26, of " decline."

The five living children :

1. (the first born), æt. 27; married.   No history of symptoms. (Order of the next three doubtful.)

2. F., æt. 21.   Married; no history of symptoms.

3. M., æt. 18.   No history of symptoms.

4. Mary Anne S—, æt. 15.   Well-marked interstitial keratitis of long duration.   Other evidence wanting.

5. F. (the 16th and last child), æt. 12.   No history of symptoms.

Remarks.—It is possible that in this family the tendency to transmit syphilis extended over many years. This depends upon the assumption that the one who died æt. 26, and who had been under my care for her eyes, was tainted. If she were, then it is certain that a sister, ten years younger, suffered also. On the other hand, the disease may have begun either for the first or second time just prior to the begetting of our patient, Mary Anne.

XXXII. *Inheritance from Father and Mother.*—Case of Wm. M—, 18, G. 318.—Father, formerly a soldier, told the mother that he had had venereal disease several times before marriage, but never admitted having it after marriage. It is probable, however, that he had syphilis during or just before his wife's fourth pregnancy (miscarriage). After this event he was in military hospital for four months for an inflamed eye, while immediately after the miscarriage his wife had well-marked secondary syphilis, for which she was salivated. The history of the children makes it highly probable that the father had had syphilis also before marriage.

1. F., living, 23.   Had bad snuffles and thrush.

2. Miscarriage.

3. F., living, 20.   Weak health.   No infantile symptoms.

Now father's syphilis (? second attack).

4. Miscarriage.

Now mother's secondary symptoms.

5. M., Wm. Matthews, 18; history of condyloma (probably) at one year, and about same time severe rash of "round places like burns," "as large as shillings all over him" (? late secondary). Now has interstitial keratitis, typical teeth, and characteristic physiognomy.

6. M., died at three months.   "Convulsions."

7. F., died at nine months.   "Whooping-cough" and "debility."

8. F., died at one month.

9. M., died at five months.   "Measles."

10. F., born dead.

Since the last child there have been two or three miscarriages, also about six or seven between the various children; eighteen or twenty pregnancies in all, and only three children surviving infancy.

Remarks.—The mortality in this family was very heavy indeed.

It is unfortunate that I had no opportunity of seeing Nos. 1 and 3, and making certain whether or not they were tainted. If they were then it is probable that both parents had syphilis twice, the father after chancre-syphilis and the mother after syphilis by conception.

XXXIII. *Inheritance from Father.*—Case of Albert B—, 13, G. 91.—Father had "clap" for three weeks two years before marriage; "cured by three boxes of pills." Had no secondary symptoms. No note as to mother.

1. M., living, æt. 15. "In infancy covered with sores all over."

2. F., living, æt. 14. "Well till vaccinated, then all her teeth decayed."

3. Miscarriage.

4. M., Albert B—, 13. Severe double keratitis; deafness; typical teeth and characteristic physiognomy. Remarkably soft skin. Death at æt. 15½, of "disease of the brain."

5. M., died at four years. "Consumption;" was very delicate from birth.

6. F., living, æt. 9.

7. Miscarriage.

8. M., born dead.

9. F., living, æt. 4.

10. Miscarriage.

11. M., living, æt. 11 months. Is delicate.

It is very probable that the fourth child suffered from a taint contracted before marriage. The facts are imperfect, because none of the other children were inspected.

XXXIV. *Inheritance from Father and Mother.*—Wm. W—, 7 months. M. F. J., p. 421.—Mother had syphilis from husband a month after marriage. No other details.

1. Born dead.

2. Wm. Wooley, æt. 7 months. Born twelve months after first child. Is under care for well-marked snuffles, syphilitic rash, and sores at angles of mouth.

XXXV. *Inheritance from both Father and Mother.*—Mrs. M—, 26, and child, M. F. K., p. 173.—Mother married twice. During first pregnancy by second husband she had syphilis fully. She was not fully salivated.

The mother's children by first husband are—

1 and 2. Living and reported quite healthy.

Now father's (second husband) and mother's syphilis during pregnancy with—

3. Born dead at seven and a half months.

4. Hannah Elizabeth M—l, æt. ten weeks. Healthy at birth, but

soon began to snuffle, and now has in addition well-marked syphilitic rash.

Remarks.—In this case it would appear that syphilis contracted during pregnancy (exact date uncertain) caused the death of the fœtus.

XXXVI. *Inheritance from both Mother and Father.*—Case of Mrs. W—, 33, Priv. H. 344.—Father has had syphilis, but its date is not stated. The mother had chancre and secondary symptoms six years ago, soon after her first confinement. Probably took mercury, but not to salivation. Now has fissures in nostril.

1. Born dead at full time, nine months after marriage.

Now mother's syphilis.

2, 3, 4, 5, 6. All born prematurely at six to seven months, and died a few hours or days after birth. The sixth had spots on skin, and died in half an hour.

Remarks.—It is not improbable that the mother really had her chancre just before her first confinement, and that the fœtus died in consequence. The effect of the taint as regards the subsequent conceptions was unusually fatal.

The foregoing series of facts have been taken, with but little selection, from my note books, and they comprise but a small portion of those which I possess. They are sufficient, however, not alone to illustrate some of the difficulties which attend the inquiry, but to furnish valuable evidence on several of the questions under debate. Amongst the difficulties is to be mentioned, first, the circumstance that in most cases the history has often to be collected backwards from the memory of untrained and uninformed observers. It is but very seldom that the whole history of a syphilitic family can be got from a medical witness who has himself been conversant with all the facts. This risk as regards errors in statement, which applies to almost every individual case-narrative, renders it needful to place a large number in juxtaposition, that thus the fallacies of some may be corrected by others.

I shall not attempt any statistical analysis of my facts, since such could not be otherwise than tedious and unsatisfactory. I must leave the reader to form his own impression as to their teaching, and shall base the few remarks which I append in part upon them and part upon my recollections of others.

One of the most important of the fallacies which we encounter is that of—

The possible influence on offspring of second attacks in one or other parent.

Although second attacks of true syphilis are rare, yet they do

undoubtedly occur. That the course of the disease differs much
from what is usual in first attacks seems highly probable.   In
not a few a second infection ends, or appears to end, with the
local sore, and in those in which constitutional phenomena are
witnessed they are usually mild.   Now and then, however, a
person who has at a distant period been through a well charac-
terised attack of syphilis passes through a second one, which is
complete in all its stages.   It is, of course, very common for
patients to tell us that they have had the disease several times,
and although in the majority of cases we put aside this state-
ment as a mistake, yet it is not safe to do so always.   This
possibility of a second infection makes it unsafe to measure in
all cases the period of liability to transmit from the date of a
known attack.

I have endeavoured, whenever possible, to trace the taint to
the parent from whom it originated, with the object of trying
to answer the question—

*Is inherited syphilis more severe when derived from the
mother than from the father?*

I take for granted (although I know that there are still
some who doubt) that it is possible for a father to transmit the
taint, the mother being at the time of conception wholly free.
I believe, indeed, that in practice this is by far the most com-
mon way in which syphilis is transmitted.   Whether in these
cases it is correct to speak of the inheritance being paternal
only is, as we have just seen, another matter, since it is pos-
sible that in every instance the mother derives an infection
from the fœtus, and may thus in return influence it.   It is,
however, scarcely probable that an embryo in which the disease
existed in such power as to be competent to contaminate the
maternal organism, should be susceptible of further hurt in the
manner suggested.

It is, fortunately, not common to encounter cases in which
married women have had syphilis, either before marriage or
subsequently, independently of their husbands.

We have, therefore, comparatively few observations of fami-
lies born to a tainted mother, the father being free.   The
foregoing list contains two such, in both of which the mother
after marriage was infected through a chancre on the nipple
(See Cases VI and VII).   In each instance the subsequent
children suffered severely, but not more so than in many in-
stances in which the inheritance was paternal only. From time
to time other cases of this kind have come under my notice,
and although I have formed a general impression that it is rare
for the children to escape when the mother has suffered from
chancre-syphilis, yet I know of no facts which would uphold

the belief that they do so more severely than under other circumstances.

The question, *Is inherited syphilis more severe when inherited from both than from one parent?* must be answered in the same manner as the preceding one, there is no proof that it makes any difference whether the inheritance is from one or both.

That the noteworthy differences between chancre-syphilis and the inherited disease are to be interpreted by consideration of the differences in the tissues of the growing child and the adult is made very probable by what is observed when a mother near the end of pregnancy becomes infected with primary disease. In such a case the fœtus, nearly full grown, acquires the disease, without a chancre, directly from the maternal blood. It is acquisition, not inheritance, for at the date of conception both the paternal and maternal elements were free from taint, and during the first six, seven, or even eight months of intra-uterine life the fœtus remained healthy. Yet, as I have proved elsewhere by citation of cases, syphilis obtained in this peculiar method resembles exactly that which comes by true inheritance, and not that which follows a chancre. This important fact goes, with many others, in support of the belief that the poison of syphilis remains identical, however obtained, and that the differences which are so patent in its manifestations are due to differences in the state of its recipient.

For the present, I am certainly inclined to believe that we must abandon the old doctrine as to the possible transmission of less or more of taint, and must hold that whenever a child derives the disease by inheritance, it derives it as a whole, and with liability to its full development. In proportion to the distance from the parental acquisition is the probability that the offspring will escape, and hence the frequency with which we see the eldest in the family suffer, and the others escape. If, however, the others escape, they do so altogether, and not with the liability to the development of the disease in slight or imperfect forms. In close connection with this statement comes the question as to whether it is possible for a parent in whom the disease exists to fail to transmit it; whether, for instance, of twins, one may suffer and the other escape, or whether of three successive births one and three may suffer and two escape. Probably it is not possible when syphilis exists in an active form in a mother for her offspring to escape it, but in the case of the father we can by no means feel so certain. That the virus may be present in the semen on one occasion and absent on another is quite conceivable. Here again, however, Colles' law comes in to our confusion, and here again we have to admit and remember the wide range of varia-

bility in the severity of the disease.   It is not possible to feel sure that any given child has escaped, for we know very well that all infantile symptoms may have been absent, and the growth and development may have been perfect, and yet at puberty, or even much later, for the first time symptoms unquestionably due to inherited taint may show themselves.

NOTE.—The cases are given very briefly, and are mere abstracts of longer narratives.   In most instances I have retained references to their manuscript, partly for my own guidance and partly that I may be able, should it be necessary at any future time, to supply more facts.

Chronicle of Medical Science.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By ROBERT HUNTER SEMPLE, M.D.,

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On the Therapeutical Employment of the Double Cyanide of Potassium and Zinc. By Doctor E. LELU and G. LUGAN, of Paris.—In January, 1875, Dr. Luton published a long account of the employment of the cyanides in the treatment of articular rheumatism, and the success of this mode of treatment was so striking as to draw general attention to the results. But the inevitable alteration, and consequently the variable composition, of the cyanides employed induced the authors of the present paper to endeavour to find a product of easy preparation and, at the same time, of stable composition—that is to say, always containing the same amount of cyanogen, and consequently being identically alike in all pharmaceutical establishments. Dr. Lelu and M. Lugan, in the first place, pass in review the cyanic compounds hitherto employed, and the objections to the use of each. Hydrocyanic acid is very easily decomposed, and, moreover, in its concentrated state it is very dangerous to prepare. The cyanide of potassium is also very easily decomposed, and is hence very variable in composition. The cyanide of zinc is stable, but insoluble, and it is not decomposed but dissolved by acids, and thus no doubt it acts in the human system; but the acidity of the gastric juice differs in different individuals, and hence the efficacy of the cyanide in some cases and its failure in others. From these considerations the authors conceived the idea of having recourse to the double cyanides, and they selected the double cyanide of potassium and zinc already proposed by Gerhardt. This salt is obtained by dissolving cyanide of zinc in a solution of cyanide of potassium. The cyanide of zinc is first prepared by precipitating cyanide of ammonium by sulphate of zinc quite free from iron. The precipitate is thrown on a filter and washed with boiling water, and then separated and dissolved in a solution of cyanide of potassium. After evaporation the double salt crystallises in beautiful octohedra, which are transparent and anhydrous. The cyanide thus obtained has a sugary taste, but without any smell of hydrocyanic acid; it is very soluble in cold water, has a very constant composition, and is easily attacked by acids with the disengagement of prussic acid. The authors prepared 50 to 60 grammes of this salt about two years

ago, and since that time it has undergone no alteration, and although the vessel in which it was kept was frequently uncorked, it never allowed the least smell of prussic acid to be perceptible. One gramme (about 15 grains) of this salt contains ·42 of cyanogen, corresponding to ·44 of anhydrous prussic acid. They have employed this cyanide only in three cases, of which they give the particulars, all being cases of articular rheumatism. The results obtained were cessation of pain, reduction of the temperature, lowering of the pulse, and diminution of the duration of the disease, and were similar to those recorded by Dr. Luton with the cyanic compounds he employed, but which, however, were of three different kinds. Dr. Lelu and M. Lugan consider that the double cyanide now described possesses all the beneficial qualities of the different cyanides formerly recommended, without their inconveniences, for it is unalterable in the air, it is soluble, its chemical constitution is constant, and it is easily decomposed by acids, even the weakest ones. They recommend it to be given with distilled water containing some simple syrup and some essence of peppermint.— *Bulletin Général de Thérapeutique,* May 30th, 1877.

On the Use of Oxide of Zinc in Obstinate Diarrhœa. By Dr. BONAMY, of the Hospital of Nantes.—Dr. Bonamy had occasion, in the year 1876, to observe at the Hôtel Dieu at Nantes a number of patients affected with obstinate diarrhœa, which in most cases had lasted for several months, and was characterised by the abundance and the great frequency of the stools. In general the evacuations had a sero-bilious appearance, and held in suspension some epithelial fragments, but they did not resemble the stools of dysentery, and if they contained any blood it was only in the form of streaks or little clots. The pain felt by the patients was a kind of vague colic along the large intestine, and of moderate acuteness. Some of the cases, in which the diarrhœa had lasted a long time, exhibited extreme emaciation; the tongue had often the *beef-steak* appearance, and the buccal cavity was red and sometimes ulcerated. Dr. Bonamy resolved to try the effect of oxide of zinc in these cases, as was first suggested by Professor Gubler, and the mode of administration was to take $3\frac{1}{2}$ grammes of oxide of zinc with 50 centigrammes of bicarbonate of soda, divided into three or four doses, one to be taken every three hours. Seven cases are recorded in which this treatment was adopted with success. In all of them the symptoms seemed to point to a chronic catarrhal condition of the whole mucous membrane of the intestine. All the patients were persons who had been coarsely fed, and were unable to guard themselves against sudden cooling of the temperature, and one of them had an organic disease of the heart. The advantages of the oxide of zinc in obstinate diarrhœa appear to Dr. Bonamy to be the following, viz.—(1) Superiority of action; for all the cases treated had resisted other kinds of medication, and the diarrhœa reappeared as soon as the oxide was replaced by some other agent; and (2) rapidity of action; for the cases which had lasted for several months were relieved as soon as the first doses of the drug were administered. —*Bulletin Général de Thérapeutique,* March 30th, 1877.

On the Employment respectively of Chlorate of Potash, Cubebs, and Salicylate of Soda in the Treatment of Diphtheria. By Dr. SEELIG-MULLER, of Halle, Prussia, and Dr. CADET DE GASSICOURT, of the Hôpital Sainte-Eugénie, Paris.—A great number of substances, as is well known, have been recommended, from time to time, in various countries, as remedies for diphtheria. Among these substances, chlorate of potash, cubebs, and salicylate of soda have lately been employed, and have been extolled by some writers. Dr. Seeligmuller (whose paper is accompanied by the expression of some reservations on the part of the Editors of the Journal in which it appears) regards chlorate of potash as a specific in diphtheria, and uses it locally as well as generally. He contrasts the great fatality of diphtheria among the patients under his care when former remedies were employed, with the remarkable success obtained when the chlorate of potash was used in large doses and in saturated solution. The proportions he adopted were 10 grammes of the chlorate in 200 grammes of distilled water, and of this solution half a tablespoonful was ordered for children under three years old, and a whole one for the elder children, every two hours, and every hour if the case was serious. He considers that this saturated solution of the chlorate exerts a local and general action on the progress of diphtheria—local, like caustics, in displacing the false membranes, and general, by supplying the oxygen which is carried away from the blood by the bacteriæ and by destroying the latter. This author adopts the parasitic theory of diphtheria.

But Dr. Cadet de Gassicourt, in a comparative survey of the use of chlorate of potash, cubebs, and salicylate of soda in the treatment of diphtheria, takes a wide view of the therapeutic question, deriving his materials from facts observed by himself during the years 1874-75-76, and the first three months of 1877. In the first place, he divides the cases of diphtheria into two groups, namely, *diphtheritic angina* and *croup*, or, in other words, into the cases in which the back of the mouth is the seat of disease, and those in which the larynx and trachea are involved. He explains that the number of the former cases was limited in his hospital, because the slighter cases of diphtheria are usually treated at home, and it is only when the symptoms are very urgent and suffocation is threatened that the parents of the children bring them to the hospital for the performance of tracheotomy. On a rough calculation of the respective efficacy of chlorate of potash, cubebs, and salicylate of soda in the treatment of diphtheria, Dr. Cadet de Gassicourt found that 15 cases were treated by the first, and 15 were cured; that 7 were treated by the second (cubebs), and 6 were cured and 1 died; that 5 were treated with salicylate of soda, and 3 were cured and 2 died. At the first view, therefore, it appears that chlorate of potash is far superior to the other remedies in efficacy, but a careful examination of all the facts by no means bears out this view, for, as the author explains, out of the 15 cases treated by the chlorate, 12 were very slight, and of the 3 others only one was very severe. While, therefore, he regards the chlorate as one of the best remedies in diphtheria, he does not rank it as a specific, nor does he think it advisable to give it in such large doses as those recommended by Dr.

Seeligmuller. As for cubebs and copaiba, Dr. Cadet de Gassicourt finds that out of 7 cases there were 6 cures and 1 death, but of the 6 cures 1 case was very slight, 3 were of moderate severity, and 2 were serious. He employed the salicylate of soda in 5 cases, 2 of which were very slight and were cured, 1 was serious but was also cured, and the other two died. In drawing some general conclusions and tabulating the results of treatment, the author observes that the fortunate or unfortunate issue of each case appears to depend on the severity of the attack rather than on the medicine employed, and he doubts whether any specific for diphtheria has yet been discovered.

In discussing the value of the remedies under consideration in the treatment of *croup*, by which name the author denotes laryngo-tracheal diphtheria, he divides the cases into those in which tracheotomy is performed and those in which it is not performed, and here, he observes, the result of the cases is subject to totally different conditions than those of faucial diphtheria, and the value of any mode of treatment is very difficult to determine. The best medicine is undoubtedly that which would cure croup without having recourse to tracheotomy; the next best is that which would cure the disease after the operation is performed; and all medicines must be rejected which do not cure either before or after the operation. By tabulating the results of cases, it is found that chlorate of potash, cubebs, and salicylate of soda have pretty nearly an equal value when tested in the manner just indicated, and this value, Dr. Cadet de Gassicourt remarks, is not great, and he believes that they are all of doubtful efficacy in the treatment of diphtheria. He does not deny that his results are discouraging, but nevertheless he does not wish to convey the impression that the expectant treatment is regarded by him as the best in this disease; and although he does not believe that diphtheria is a catarrhal disease to be cured by balsamic remedies, or that it can be cured by chlorate of potash, by supplying oxygen and destroying *bacteriæ*, yet he employs various remedies with a hope of arriving eventually at favorable results, and rejects only those medicines and those forms of treatment which are proved to be dangerous. He condemns all medicines and all modes of treatment which tend to weaken the patient or to interfere with alimentation and sleep. It is above all necessary, he observes in conclusion, by alimentation, by cordials, and by tonics, to endeavour to give to the patient the strength in which he is deficient, and thus to supply him with the vigour necessary to resist the attacks of the disease, or to wait for the relief afforded by tracheotomy.—*Bulletin Général de Thérapeutique*, May 15th and June 15th, 1877.

On the Employment of Cold Baths in the Treatment of Typhoid Fever. By Professor PETER, of the Hospital of La Pitié, Paris.— Dr. Peter, in a series of very able papers, denies altogether the efficacy of cold baths in the treatment of typhoid fever, and believes, on the contrary, that this kind of medication is attended with the most dangerous and even fatal results, and he argues the question both on general principles and from the results of actual experiment in the practice of various hospitals.

He observes that the advocates of the cold-bath treatment restrict

their field of inquiry to the contemplation of a single symptom, namely, *hyperthermia*, instead of regarding the numerous circumstances which combine to constitute typhoid fever; and he maintains that the principle of this treatment is not to attack the primary cause of the disease, but one of its effects, namely, the exaggerated heat, or, in other words, not to aim at removing the combustible material, but to subtract the caloric produced by the combustion. Thus the struggle is between *hypothermia* and *hyperthermia*, and, the patient himself being disregarded, the manifold and imperious demands of his organisation are made secondary to a question of a greater or less amount of heat. Dr. Peter regards hyperthermia as only one of the elements of a general and serious morbid condition, the others being delirium, dryness of tongue, hæmorrhage, congestions, &c.; and he thinks it quite as absurd to treat typhoid fever by temporarily reducing the temperature as it would be by moistening the tongue of the patient to expect to remedy the febrile condition indicated by that organ. He distrusts all therapeutical formulæ of a so-called rigorous character, that is to say, when they are exclusively employed, and the more simple they are the more are they deceptive; the operations of the healthy organism are infinitely complex, and those of the unhealthy are still more so, and therefore therapeutical applications cannot be simple, for it is impossible to simplify what is not simplifiable (il est impossible de simplifier ce qui n'est pas simplifiable).

Dr. Peter thinks it is easy to prove by facts that the hypothermic plan does not succeed, by means of cold baths, in diminishing the heat for a sufficient length of time, and hence the necessity of repeating the measure at frequent and short intervals, or as it is termed, " *coup sur coup ;*" and, on the other hand, that sometimes the desired result is too successfully achieved, and hypothermia is realised too far. He gives an instance in proof of this latter proposition, where a patient was so successfully cooled by means of the cold bath, that the temperature in the axilla and the rectum fell to 35·3 and 35·6 C., and it was very difficult to restore the temperature, the patient eventually dying with an enormous mortification on the sacrum and œdema of the lower limbs. When the refrigerant treatment succeeds, Dr. Peter argues that the result is not owing to the mere abstraction of heat, but to the effect on the nervous system, and this is the base of the hydrotherapeutic system which operates by action and re-action, and does not depend only on its power of subtracting caloric, but by its sudden, active, and energetic excitement of the sensitive nerves of the skin. In typhoid fever he denounces this violent mode of treatment as being full of dangers, the chief being syncope, inflammation, and hæmorrhages. Syncope is a kind of death, and is unsuitable as a therapeutic agent in a depressed condition of the system such as exists in typhoid fever, in which the nervous shock cannot be borne without the greatest inconvenience. Hæmorrhages are admitted to be common in typhoid fever under any treatment, but Dr. Peter believes that the cold-bath system aggravates the risk of such discharges, and even causes them from organs which are usually unaffected; while various inflammations are traceable to the same cause, such as capillary bronchitis, broncho-

pneumonia and lobar pneumonia, and renal congestion advancing to albuminuria and Bright's disease. Dr. Peter gives a number of facts in confirmation of his opinion, and he shows from statistical results that the number of deaths in the cases of typhoid fever treated by cold baths is rather in excess of that which is noted in other forms of treatment. The excess of deaths is not great, amounting indeed to only about 1 per cent., but Dr. Peter argues that even if the number of deaths in the new treatment was exactly the same as in the old, there is no reason for changing what he calls the old and rational medication for one which is difficult to impracticability and painful to cruelty.

His general conclusions on the whole subject are the following:—1. Typhoid fever is not the only factor to be regarded by the physician, but the numerous conditions presented by the patient must be taken into account. 2. Still less ought the physician to fix his attention on a single morbid element of the disease, namely, *hyperthermia*. 3. All systematic treatment directed to a single symptom is absolutely illogical and insufficient. 4. The advantages of the refrigerant system, if it has any, are not due to the lowering of the temperature, but to a thorough disturbance of the nervous system. 5. Therefore this modification can be effected by hydro-therapeutic methods different from cold baths, and not dangerous, as they are. 6. There are cases where cold lotions used imprudently or repeated too often are dangerous, for a nervous shock, although slight, may become injurious in an organism rendered susceptible by the typhoid poison. 7. When a serious result is produced by cold baths in typhoid fever, the danger is excessive and out of all proportion to the possible benefit. 8. In conditions which are analogous in all points, the cold-bath treatment has given in Paris returns of mortality higher than those recorded under the rational modes of medication. 9. The best system in therapeutics, above all in the treatment of typhoid cases, is that which admits no special system. And 10, and lastly. When a rational mode of medication (namely, one founded on the indications), supported by the logic of facts and advocated by the highest medical authorities, has been justified by long years of experience, it is necessary to reflect carefully before trying to change it, for every dangerous therapeutical experiment has the double disadvantage of plunging some minds into a painful condition of irresolution, and of committing others to a scepticism still more fatal.—*Bulletin Général de Thérapeutique*, March 15th and 30th, and April 15th, 1877.

On Chinese Materia Medica, and especially Opium.—It appears that early in the present year a collection of medicinal substances was transmitted from Shanghai to the Earl of Derby, the Secretary of State for Foreign Affairs, and by him forwarded for investigation to the London Society of Apothecaries. The specimens were obtained in 1875-76, in the course of a mission to the part of China called Yünnan, and they are found to consist chiefly of such of the vegetable products of this district as are applied by the inhabitants to medicinal purposes. There are no mineral substances in the collection, and the articles consist almost entirely of roots, stems, leaves, barks, and woods; there are but few fruits or seeds, and there are no gums or resins. The

specimens are all carefully wrapped up in paper parcels, inscribed with the name of the contents in Chinese and English characters, and it is from the information thus conveyed, together with the careful examination of the botanical peculiarities of each, that the genera of the plants have been determined. The investigation has been conducted by the present Master of the Society, Mr. Bradford, assisted by Mr. Moore, the Curator of the Society's Botanic Garden at Chelsea, and, as to the chemical analyses, by Mr. A. Stewart, the principal Chemical Operator to the Society. It will, perhaps, be anticipated that the results obtained are rather curious than practically useful, and such indeed appears to be the fact, but nevertheless it is interesting to learn the sources from which so many millions of the human race as are included in the Chinese Empire derive their supply of medicinal substances. It is found that the plants regarded as useful in medicine by the Chinese are little esteemed in this country, although many of them are ornamental or fragrant. Among the last-named are some belonging to the natural order of *Labiatæ*, as the sage, the mint, and the balm; but the smilax, yielding sarsaparilla, the uncaria, from which a kind of catechu is prepared, and elaterium, are among the few which are known to possess active properties. It is found that among the parcels there is one of a coleopterous insect, the mylabris, used by the Chinese as a blistering fly, and there are some nut-galls, apparently the product of some hymenopterous insect. In arranging the specimens in their natural orders, it was observed that those orders which grow in temperate climates, and are known to contain active principles, were almost absent in the collection. Thus, for instance, there were no *Cruciferæ* or *Solanaceæ*, and scarcely any *Compositæ* or *Umbelliferæ*, no *Scrofulariaceæ* or *Euphorbiaceæ*, and only one imperfect specimen of rhubarb.

The most important part of the collection, both in a scientific and commercial point of view, appears to have been the opium. This drug might be obtained to an unlimited amount if a demand for it existed, for the poppy is cultivated to an enormous extent in China, and is said to be probably indigenous in Yünnan. The opium in the collection now alluded to was of two kinds, namely, hard and soft, and Mr. Stewart, the chemical operator who examined and analysed the samples, supposes that the soft specimens are probably watery extracts prepared from the hard ones, with a special view to increase the percentage of morphia in the drug. The soft specimens in fact yield nearly three times as much morphia as the dry ones. The results of the analyses made by Mr. Stewart show that the hard specimens are inferior to Turkey opium in the yield of morphia, which is the standard of the commercial value of opium, the highest result obtained by him being 8·05 per cent., whereas from 10 to 12 per cent. is obtained from fine Turkey opium. The soft specimens contain much more morphia, but the quality in this respect is not uniform, and hence, for general pharmaceutical purposes in England, none of the samples would be of much use, because the British Pharmacopœia requires uniformity of strength. But, with regard to the commercial aspect of the question, Mr. Stewart observes that, looking at the high

percentage of morphia in the soft samples, and their low price (for we may notice that the Chinese affix the price to each of the specimens), it is probable that these might enter successfully into competition, even in England, with Turkey or any other opium for the manufacture of morphia.—*Medical Times and Gazette*, July 21st, 1877.

On the Use of Iodized Chloral-Phenol as a Uterine Escharotic, Alterative, and Local Anæsthetic, with some Remarks on its Use in Other Diseases. By Dr. J. P. Thomas, of Kentucky.—After using carbolic acid and hydrate of chloral separately as adjuvants to iodine for several years, Dr. Thomas conceived the idea of combining them, and he devised a formula for the purpose, consisting of ℥ss of resublimed iodine and ʒj of hydrate of chloral and ʒj of crystallised carbolic acid. The iodine and chloral are to be rubbed together to a fine powder, and after liquefying the carbolic acid by the aid of heat, the ingredients are all to be rubbed together until a homogeneous liquid is formed of the consistence of thin syrup, and of a dark mahogany colour. Dr. Thomas states that this formula has great anæsthetic power, and is anodyne and escharotic; and after an experience of its use for nearly four years, he is enabled to claim for it a very great value in the treatment of uterine disease. In chronic hypertrophy and induration of the os and cervix uteri he has found it to excel all other preparations in remedial power. In several other diseases Dr. Thomas considers it also very valuable, as, for instance, in the treatment of carbuncle, in scrofulous ulceration of the parotid and cervical glands, as a dressing to old and indolent ulcers, and in a few cases of stubborn skin disease. He mentions a case of disease of the scalp, attended with the exudation of fetid pus, and in which, after removing the scurf and scabs by a few applications of acetic acid, the application of the iodized chloral-phenol effected a rapid cure. He finds it also the best remedy in ringworm of the scalp.—*The American Practitioner*, May, 1877.

On the Use of the Seton in Paralysis and Epilepsy. By Dr. T. J. Griffiths, Surgeon of the United States Marine Hospital Service.— Since 1870 Dr. Griffiths has treated eighteen cases of paralysis and five of epilepsy in the United States Marine Hospital at Louisville, Kentucky, by the seton introduced at the back of the neck. In only one case was this treatment altogether unsuccessful, and in the other seventeen, ten recovered, and the others much benefited. Dr. Griffiths is at present cognisant of eight of the ten who recovered, and they have had no return of their former disease. In the seven men who were discharged improved, the paralysis was noticeable in their walk, but they were able to go about and perform a considerable amount of light work. In the cases of paralysis no treatment except the seton and tonics was employed, but bromide of potassium was given in one of the cases of epilepsy, and all had tonics. Since 1870 Dr. Griffiths has also treated eight cases of hemiplegia in private practice by the seton with beneficial results, for six recovered and two improved. Before the date mentioned his practice was to follow the usual routine of treatment by electricity, strychnia, iodide of potassium, &c., but no such results were realised as were obtained by the

use of the seton, and he does not remember a single case of complete recovery treated under the earlier plan. He has long since abandoned the use of electricity and strychnia as remedial agents in paralysis. His plan is to introduce a large seton, fifteen to twenty strands of silk or flax thread, and allow it to remain for months, if necessary, removing it only when the patient seems to be permanently improved or has recovered. He has also used the seton with good results in the treatment of obstinate cases of iritis, whether syphilitic, rheumatic, or from other causes. Chronic cases, which seemed to improve slowly or not at all under ordinary treatment, often showed a marked improvement a few days after the introduction of the seton.—*The American Practitioner*, March, 1877.

On the Influence of Medicines, particularly Narcotics, on the Infant, when administered to the Mother during Pregnancy and Labour. By Drs. Mundé, Barker, Peaslee, Gillett, and Thomas, of New York.—In a series of discussions on the above subject, held lately at the New York Obstetrical Society, the members were divided in opinion, some believing that the administration of narcotic and other powerful drugs to the mother during pregnancy and labour exerted an injurious effect on the infant, while others entertained a contrary view. One member related a case where morphia was administered hypodermically to the mother, who was suffering from puerperal convulsions, and the infant on its birth was asphyxiated, and afterwards convulsed, but eventually recovered. But Dr. Mundé, who appears to have had a large experience, after alluding to the literature of the subject, stated that for more than twenty-five years he had been in the habit of administering chloroform in labour cases without having ever observed any injurious effects to be produced on the child, and as to opium, he states that his experience is of the same nature. He mentions two cases in which the mothers had been habitual opium-eaters, and yet their children were in no way injuriously affected. He concludes from the whole of his investigations, that there is no evidence which can be accepted in science that narcotic drugs administered to the mother ever produce their specific effects on the fœtus in utero, and therefore such drugs may be used without fear of their effect on the fœtus whenever they are necessary for the health or the life of the mother. The most prominent advocate of the opposite view was Dr. Gillett, who declared his firm belief that certain narcotics administered to the mother during parturition may affect the fœtus in utero or the new-born child, and that, in particular, morphia, when administered to the degree of producing its physiological phenomena in the mother, will invariably produce a relative condition of narcotism in the new-born infant. He adduced a number of cases in which morphia was administered to the mother, and the new-born child was more or less affected, though all the children eventually recovered, and he relates two cases in which atropia was administered hypodermically to the mother, but one only of the two children exhibited dilatation of the pupil. Dr. Thomas, who acted as President of the Society, and who wound up the discussion with some remarks of his own, admitted the difficulty of arriving

at a positive conclusion on the question proposed. He related two cases in his own practice where parturient women had taken large doses of laudanum, but no effect was produced on the child. He himself inclines to the belief that the use of narcotics and anæsthetics by the mother has no injurious effect on the child, but he admits that there is evidence on the other side. In Dr. Gillett's cases, where morphia had been used, the child in each case showed the symptoms of opium poisoning, and in one of the cases where atropia had been used, the child's pupils were widely dilated. He was unable to regard these results as mere coincidences, and thought that they tended to prove that large doses of narcotics, especially when used hypodermically for the pregnant woman, might injuriously affect her child. Still, he observes that, although morphia may be administered to the mother during labour and the child may die, it does not prove that morphia killed the child.

It does not appear that any definite conclusion was arrived at as a consequence of the discussion, Dr. Gillett still maintaining that the use of opium produced symptoms of narcotic poisoning in the child, while Dr. Peaslee doubted whether Dr. Gillett's cases really supported his conclusions. He (Dr. Peaslee) could not admit the affirmative of the question till the narcotic was actually detected in the blood or the urine of the new-born child, and the symptoms in the latter were proved to be clearly due to the morphia detected.—*Transactions of the New York Obstetrical Society,* 1877.

Deductions from Three Hundred and Nineteen Observations of the Action of Chrysarobin, a New Emetic Purge. By J. Ashburton Thompson, M.D.—Chrysarobin is another word for Goa-powder, and it has received the new name for the following reasons. The powder is known in Bahia by the name of aroba-powder, but as it is the active part of a whole tree, it is convenient to use the single word *arobin* instead of the compound one, and as the colour of the newly prepared powder is yellow, the prefix *chrys-* is used, and hence the word chrysarobin, or yellow aroba-powder. Professor Attfield's analysis of chrysarobin shows that this substance contains nearly 90 per cent. of chrysophanic acid, and hence there is reason for believing that the latter may be the active principle. The crude powder and the extracted acid have the same locally irritant effects, and either, kept in contact with the skin, produces inflammation and irritation, and when introduced in minute quantity into the eye causes conjunctivitis. Some other plants contain chrysarobin, such as common dock (*rumex*), rhubarb and senna, and the properties of these plants seem to have some connection with their chemical composition. Dr. A. Thompson thinks he has ascertained that chrysophanic acid is a purgative of decided cholagogue powers, and as it might be supposed that it would be useful in medicine he undertook its clinical investigation in 1875, with specimens prepared by Messrs. Young and Postans, of Baker Street. Chrysophanic acid is a granular powder of a fine bright orange colour, has neither smell nor taste, and may be crystallised. The resin which exists in chrysarobin is of two kinds, and Dr. Thompson used a combination of the two in his experiments. He made the first experi-

ments on himself and his brother, and he ascertained that chrysarobin in a small dose had no very active properties, for a dose of sixty grains produced only vomiting and purging of a mild kind. In using it in ninety cases, thirty children and sixty adults, he found that its action was emetic and purgative, vomiting being the first sign of action, but being unattended by any such depression as that caused by tartar emetic or ipecacuan, and the purging being marked by watery stools without griping pain. With regard to the dose of chrysarobin, it was found that twenty grains was a moderate one for an adult, and that, generally speaking, twenty-five grains might be regarded in adults, and six or more grains in children, as a good emetic purge unattended by any inconvenient symptoms. The effect of chrysophanic acid is similar to that of chrysarobin, with this difference—that, while in a suitable dose each will cause vomiting and purging, if the dose is too small chrysarobin is likely to purge only, while chrysophanic acid is likely to cause vomiting only. As to the dose of chrysophanic acid, Dr. Thompson finds that the action is certain for an adult in a dose of fifteen grains, and that a scruple is in general too much ; and that in children the effect is rather uncertain, which is not the case with chrysarobin. Dr. Thompson has made ten observations upon adults with the resin of chrysarobin, and the results were that the action was identical with that of the crude powder and of chrysophanic acid, but was very much more powerful. From the whole series of his investigations Dr. Thompson draws certain definite conclusions as to the action of chrysarobin and its constituents. He regards chrysophanic acid as an emetic purge, having an action as certain, when given in appropriate doses, as that of any other drug acting in either of these ways. He thinks that its action is favoured by the mode of administration, and it is best given in diffusion in water, in the form of pill, and above all, in combination with a strongly alkaline fluid. He sees reason to regard it as a useful addition to our list of remedies, because it affords a means of clearing out the *primæ viæ* with a thoroughness and promptitude not equalled by any other medicine with which he is acquainted, a combination of tartar emetic and ipecacuan alone excepted, while it is at once more certain to produce *both* purging and vomiting than the last-named combination, and is unattended by the serious depression which is often an inseparable objection to its employment, and he thinks that the power of evacuating large quantities of bile, which he claims for it, especially fits it for the purpose named.— *British Medical Journal*, May 19th, 1877.

On Gurjum Balsam and some of its Therapeutical Applications. By Dr. Luc Deval, of Paris.—In this essay, which constitutes the inaugural thesis of Dr. Deval for the doctorate in medicine, the properties of gurjum balsam are considered, both in their application to the treatment of gonorrhœal affections and to that of lepra and some other diseases of the skin. The substance to which the essay relates is furnished by several trees belonging to the genus *Dipterocarpus*, which grows to a great height in some of the islands of the Indian Archipelago and on the eastern coast of Bengal. The balsam is obtained by making large incisions in the trees, and col-

lecting the fluid, which exudes when the part is heated. The gurjam balsam was first employed by Sir William O'Shaughnessy in 1838, as a substitute for copaiba in the treatment of gonorrhœa, and it has since been pretty extensively recommended, principally by British practitioners, for the same purpose. In 1873, however, Dr. Dougall, of the Madras army, found it efficacious also in the treatment of lepra, and this success was confirmed by Mr. Erasmus Wilson, who also employed it in various diseases of the skin. In the treatment of lepra it was used by Dr. Dougall, both internally and externally, the dose given by the mouth being about fifteen grammes, administered with lime water. More lately Dr. Vidal has introduced this substance into France as a remedy for gonorrhœa, and in his wards Dr. Deval was first made acquainted with its therapeutical properties. The author, while admitting that this balsam has been serviceable in the treatment of lepra and some other skin affections, thinks that further experience is required before it can be exclusively employed in such cases. But the success which has attended its use in gonorrhœa entitles it to a well-marked place by the side of copaiba and other balsamic remedies, and its external and internal action in the treatment of vaginitis appears more energetic and rapid than that of copaiba. It must be added that the abundant supply obtained from the trees which produce it, and consequently its lower price in comparison with copaiba, will probably attract the attention of medical practitioners.—*Thesis of the Faculty of Medicine of Paris,* 1877.

On the Therapeutical Importance of the Common Phosphate of Soda. By Dr. Luton, of Rheims.—After glancing at the general therapeutical properties of the phosphate of soda Dr. Luton confines himself, in the present paper, to the consideration of this salt as a respiratory agent employed in the treatment of asthma and pulmonary phthisis. He relates three cases, one of asthma in an old man, and two of pulmonary phthisis, one in a young man and one in a young woman, in all of whom the phosphate was given with great success. Dr. Luton believes that the common crystallised phosphate of soda acts on the human subject as a *phosphorised* principle, and that it presents itself to notice as an agent having the properties of phosphorus without any of its dangers, and that as a phosphorous compound it is suitably employed in those diseases where phosphorised substances are required, such as organic debility, marked adynamic conditions, impotence, amenorrhœa, and perhaps progressive locomotor ataxy; as an alkaline body, also, it is applicable in gout, rheumatism, diabetes, biliary calculi, &c. In pulmonary phthisis there are two morbid elements, namely, the functional respiratory disturbance and the organic debility, the one influencing the other; and it is natural to suppose that the phosphate of soda would be efficacious in this disease, either in combating its essential principle or in counteracting some of its acquired results, and in these characters Dr. Luton regards it, and recommends the trial of its properties by others.—*Union Médicale Scientifique du Nord-Est,* February, 1877.

On an Extract of Ergot of Rye for Hypodermic Injection. By M. YVON, of Paris.—Ergot of rye is a complicated substance, consisting of fatty oil, ergotine, osmazome, mannite; gummy, extractive, and colouring matter, albumen, *fungin*, phosphate of lime, and a few other matters. In 1876 M. Tauret, of Troyes, announced the discovery, in ergot of rye, of a new solid and fixed alkali, which he called *ergotinine*, and in the same year Messrs. Dragendorff and Padwissotzky published some very complete researches on the composition of ergot. According to these chemists the active principle of ergot is viscous and of a colloidal nature, and is called by them *scleromucine*, and the ergot contains, besides, an acid which they call *scleromucic acid,* and some other matters to which they have also given names. M. Yvon thinks that there is still much to be learned in reference to the chemistry of ergot of rye, and he does not himself profess to throw any new light upon this part of the subject but he wishes to introduce to therapeutical use a better preparation than any at present employed. The only certain fact with regard to the active principle is that it is soluble in water, and beyond this fact all is doubtful as to its chemical properties. M. Yvon describes his method of obtaining a preparation for hypodermic use, the process being rather a complicated one. The fixed oil is first removed by sulphuret of carbon, and the residue, dried and powdered, is mixed with tartaric acid; carbonate of lime is subsequently added to neutralise this acid; the active principle is filtered, rendered colourless by animal charcoal; salicylate of soda is dissolved in it, and finally a liquid is obtained equal in weight to that of the ergot employed. The liquid thus obtained is of an amber colour, of a very pleasant smell, and is easily preserved. It is well adapted for hypodermic injections, and M. Yvon has made with it several experiments on the lower animals. M. Dujardin-Beaumetz has also made a series of injections with this fluid on patients in the wards of the Hôpital St. Antoine, in Paris, and he found that they caused no local irritation, and that they produced all the therapeutical effects of ergot of rye.—*Bulletin Général de Thérapeutique,* July 30th, 1877.

On the Use of the Glycerole of Subacetate of Lead in the Treatment of Chronic Eczema. By BALMANNO SQUIRE, M.B. Lond.—Mr. Squire disagrees with those who believe that chronic eczema, because it is a "blood disorder," must necessarily be combated by some internal medication, although he admits that in its acute form the disease may require constitutional treatment. He believes that, after a certain brief duration of the disease, the original cause ceases to operate, and that the malady is perpetuated by an acquired condition or habit of the integument, and on this the chronicity of the affection depends. Hence the object of the dermatologist should be to alter this morbid condition of the surface by appropriate local medication. Mr. Squire, after referring to various applications at present in use against chronic eczema, such as the benzoated oxide of zinc ointment and the unguentum diachyli, gives his reasons for preferring a preparation of lead with glycerine. He first tried the

acetate of lead ointment of the Pharmacopœia, then he thought a mixture of the liquor plumbi diacetatis with lard a more efficient application, and finally he substituted glycerine for lard. The glycerine of subacetate of lead is prepared in precisely the same way as the liqour plumbi diacetatis, only that glycerine is used instead of water. Mr. Squire takes care to explain that he restricts the term "chronic eczema" to its real meaning, and that he does not include in it such affections as lichen and trephulus, prurigo or pityriasis, but only the cases characterised by a colourless viscid sweating from the skin, whether this sweating be abundant or scanty, or whether there be small, raw, scattered, but more or less clustered, weeping excoriations.—*Medical Times and Gazette*, 1876.

Drugs in the Treatment of Disease. By ROBERT HAMILTON, F.R.C.S., Surgeon to the Royal Southern Hospital, Liverpool.— The author of this paper points out the erroneous nature of the popular belief that drugs are the most potent weapons in the physician's armoury, and constitute the true remedies for healing the sick. Drugs, in fact, take a subordinate place in the presence of the curative means now more generally employed, such as diet, rest, heat, cold, light, air, water, and electricity. It is not certain, too, as the author observes, that drugs do not sometimes effect more harm than good by setting up morbid action where none previously existed. Still, Mr. Hammond does not by any means advocate the disuse of drugs altogether, as he is thoroughly convinced of the efficacy of some of them, but he hesitates more in using them than he formerly did. He alludes to the history of homœopathy as showing that this so-called system was merely the result of the dissatisfaction felt with the uncertainty of drugs; and he states that, although the gross absurdities of the views of Hahnemann can excite only ridicule and contempt, yet that the practice of what is called homœopathy—that is to say, letting nature take her own course—is sometimes, and, indeed, often, attended with success. As this conviction is gaining ground in the minds of reasonable people homœopathy is receding, and is gradually passing into the limbo of forgotten things; and it could not be otherwise, for the intelligence of man could never have rested satisfied with the dogma which asserted that the billionth of a grain of any drug could have a greater influence on the system than a grain of the same drug.— Pamphlet, 1877.

REPORT ON PATHOLOGY AND THE PRACTICE OF MEDICINE.

By John T. Arlidge, M.D., A.B. Lond., F.R.C.P. Lond.,

Physician to the North Staffordshire Infirmary, &c.

Progressive Facial Hemiatrophy.—Dr. H. M. Bannister, of Chicago, details two cases of this rare affection in the pages of the 'Journal of Nervous and Mental diseases,' a quarterly journal published at Chicago, of much merit, and to which we are pleased to direct attention. The first case was that of a young German printer. In working a hand-press in hot weather he perspired only on the right side of the face, whilst elsewhere on his body no difference in the cutaneous transudation was discoverable. With this functional disturbance was associated a marked difference between the two sides of the face ; the left side was less full than the other ; the skin appeared tenser and more shining, the subcutaneous adipose tissue was lacking, causing hollowness between the muscles, which on their part retained their volume ; a cut or scratch on the affected side was slow to heal ; the hairs of the moustache fell out, largely, but no paralysis, twitching, or any disorder of mobility was present ; the left eye was a little less prominent than the other, and the temperature of the affected side was lower than that of the right one ; the secretions of the eye and mouth were normal, and tactile sensibility unaltered ; special senses normal, excepting that of taste, which was diminished or lost on the left posterior third of the tongue ; the first upper molar of the left side ached at times, the gum was wasted, so that the root of the tooth was largely exposed ; brown freckle-like blisters occurred equally on each side of the face.

There was no history of accident or of injury of other than slight character, but he had at a previous period of his life acted as a waggoner, and been exposed to cold and bad weather. Pressure on the cervical ganglion was without effect. The pupils were symmetrical.

The second case was that of a merchant, æt. 42, who, some ten years previously, had severe scalp wound over the coronal suture, followed, after the lapse of a day, by delirium, lasting for a month. After recovery from this state a dull pain remained at top of head, with confusion of mind aggravated by exertion, mental or physical, compelling retirement from active business for two years more. Ever since, moreover, unusual mental worry or excitement, or loss of sleep, has caused return of these unpleasant head symptoms. When examined, the left side of face was distinctly smaller—narrower—than the right, the atrophy affecting not only the soft tissues, but also the bones ; the facial muscles were all diminished in volume, but functionally as active as ever ; the adipose tissue was wasted ; skin the same on both sides, but the beard on the left side

thinly developed, and its growth arrested, so that shaving was not required on that side; the cutaneous secretions appeared unchanged, and the temperature on the two sides was the same; nothing abmormal in the mouth.   Tactile sensibility reduced on left side, and so likewise were the hearing and taste; but the eyesight appeared equal on the two sides.   This facial inequality had appeared within ten or twelve years.

Dr. Bannister remarks that the first of these two cases was in an earlier or an undeveloped stage compared with the second, in which, not, only adipose tissue, but also muscles and bones, were atrophied.   No apparent cause was cognisable in the first case; whereas, in the second, there was history of an injury to head and brain, and with it sensory disturbance, not seen in the other instance. A traumatic origin is noticed in the cases recorded by other authors, but in neither of these two examples was a local discoloration, noted by other observers as a phenomenon preceding wasting, and consequent on absorption of subcutaneous fat, present.   No pain seems to have attended the progress of the atrophy in either patient; but with regard to temperature there was a decrease in the former case, but not in the latter; and this want of uniformity in this matter would appear general in similar cases.   Again, there was no perspiration in the first instance recorded on the affected side, whereas, in the second, there was a general absence from the face on both sides. .

It would seem that the secretory nerves connected with the several nerve centres, sympathetic and others, are involved in this morbid state; but an alteration of function, as far as regards sensory nerves, appears to be uncommon.   It is, however, worth while to mention that in the two cases reported the defect of taste had not been recognised by the patients themselves, but was first brought to life by experimental testing; consequently, it may have happened that in some other instances recorded the loss of taste may have been overlooked.

Excepting Laude, who (in 1870) attributed this malady to a primary affection of the connective tissue, all writers upon it have described it as a neurosis, some regarding it as an affection of the vaso-motor system, others as due to an alteration of the trophic influence conveyed mainly through fibres contained in the trunks of other, chiefly sensory, nerves, especially those belonging to the fifth nerve.

Dr. Bannister accepts the latter view, and thus summarises his opinions:

" Progressive facial hemiatrophy is a neurosis.   This is indicated by its limitations to one side of the face, and to the regions supplied by special nerves; by the implication, in some cases, of the sensibiilty, and even special senses; by the merely dystrophic character of the changes it produces in the tissues without any indications of disease, or alterations of the essential structure; by the close analogy in some respects with neuralgias, attended with local atrophies, &c.

"2. As regards the nature of the nervous trouble, it appears, from an analysis of the symptoms in a majority of the cases reported, that the essential lesion is not in the vaso-motor system, but rather in the trophic functions of other nerves. Vaso-motor symptoms, properly speaking, are lacking in a majority of cases.

"3. From the evidence afforded by a large proportion of the reported cases, it seems justifiable to suppose that the trophic functions of the fifth nerve are especially implicated. The facts that render this probable are the frequent limitation of the atrophy to the regions supplied by this nerve, or its separate divisions, the analogy with the neuralgic atrophies, and the occasional sensory symptoms. I may add, also, that the lack of symptoms that would lead to the inference of implication of the facial nerve, is in favour of this view, at least, as assisting in a kind of diagnosis by exclusion. The hypothesis that the atrophy depends upon alterations in the ganglia on the fifth nerve, cannot be said to be very well supported by the facts in our possession, as to the results of disease of these ganglia, but it may be the correct one.

"4. In some cases there are evidences of positive lesions of other cranial nerves than the fifth ; paralysis, or irritation of the sympathetic (Seeligmueller, Brunner) paralysis of the abducens (v. Graefe), of the auditory and glosso-pharyngeal (cases reported in this paper). In some of these observations, where the disorder was apparently due to traumatic causes, syphilis, &c., the facial atrophy is perhaps to be considered as only one of the manifestations of the more general intracranial disease, of which these other phenomena were also symptoms. Still, there are some facts that indicate an alteration of nerve nuclei in the medulla, in many of the cases, the direct cause of which is not obvious.

"5. The symptoms of the disorder indicate a chronic trophic asthenia or paralysis, rather than any irritative action. This is proven by the slow progress of the disease, its usual unirritative character, and by such symptoms as that testified to by one of the patients in the cases here presented, viz. that slight injuries, wounds scratches, &c., were longer in healing on the affected side than on the other.

"6. That the disorder, however, is not merely one of arrest of development is shown by the actual wasting of the tissues, the more solid ones, such as the bones, among them in many cases. In this, it has a resemblance to the atrophy of old age, with which it seems to have other analogies, suggestive in regard to its pathology. The cases of congenital facial atrophy that are not infrequently met with have not generally the progressive character of this affection, and are more properly to be reckoned as due to arrest of development. Still their relations with this disorder may be closer than we think.

"7. There are various peculiarities of this disease which, in the present state of our knowledge, cannot be accounted for in any way. Still they serve to support the neurotic and local theory of the affection. Such is its unilateral character, the greater frequency of its occurrence in females than in males, &c.

" 8. Therapeutic measures have, so far, failed to produce lasting benefit, for the most part. Two or three cases seem to have been benefited by faradisation (two of Baerensprung) and the prognosis, as regards recovery or arrest of the disease, is not altogether hopeless. If, as has been suggested as possible, in one of the cases here related, it ever depends upon a reflex cause, or when it seems due to specific disease, like syphilis, the cutting off of the original irritation or proper specific treatment would seem to be the most promising remedial measures.

" 9. The disorder does not seem to involve life. It is possible, however, that in some cases it might extend to organs that are essential to life, especially if the nuclei of various cranial nerves are involved in the morbid process."

Dr. T. Whiteside Hine, in the 'British Medical Journal' for 26th August, 1876, has furnished a communication on this singular lesion. He partakes the more general belief that it is a neurosis, the terminal nerves regulating the nutrition of the tissues being the agents concerned. He cannot agree with Brunner's views that it is due to alterations in the great sympathetic nerve; for in one case observed by him the signs of such changes were absent. Moreover, in a patient seen by him, in whom perspiration was absent from one half the face, accompanied by hæmorrhagic effusion, and due to an injury, there was no difference between the two halves of the face, although there were clear indications of vaso-motor disturbance.

Coutet likewise narrates a case of unilateral atrophy of the face, differing in many material features from the general history of like cases. It was that of a vigorous young man who had never met with an injury. When about 11, he had many short but violent attacks of dental neuralgia. When 15, a blueish spot appeared over the dental foramen of lower jaw, which in the course of three years attained the size of a five-franc piece, but lost its colour. In this interval, likewise, atrophy had taken place in the subjacent tissues, and he suffered violent pain, closure of the jaws, and muscular tremor in the superficial muscles. In the buccinator, at all times, there was continuous heavy pain, if violent suffering was absent; the muscles in face, and also the tongue, suffered with rigidity. The lips of the right side, though wasted, did not prevent his playing a wind instrument. Paroxysms of pain were induced by exposure to cold. The movements of the face were not attended by deformity. The atrophy was limited between the third cartilage and the upper lip at the median line. The infra-orbital region and the wing of the nose were involved. On the external aspect there was no definite boundary. Probably the pharynx was included in the wasting, but the velum, the tongue, and the lower jaw were unmistakably atrophied. The facial muscles and masseter were wasted, and suffered with tremor. The skin at large retained its normal colour, but the lower lip and chin were of a dull yellowish hue, having the appearance of a cicatrix, but with no adhesion to the subjacent bone. Neither the sensibility of the surface, nor the muscular movements, nor the organs of sense, were affected.

For several months the patient was submitted to the local action of the continuous current without benefit.—*Révue des Sciences Médicales*, tome ix, p. 147.

Plexiform Neuroma.—Dr. Cartaz narrates a case of this unusual form of neuroma, and takes the opportunity to record its history and pathology. The whole number of cases on record amount only to thirteen. It is especially a disease of childhood and early youth; it may be congenital; and its most frequent site is upon some part of the head. It is indolent in its course, and does not attract attention by pain or other circumstance, until it has attained the characters of a definite tumour, covered by a thickened, dense, rugose, or furrowed skin, of a brownish-red colour, and not unfrequently adherent with it by continuity of disease. The sebaceous glands of the skin also become greatly enlarged.

When the neuroma occupies a mucous surface, like hypertrophic changes take place in the submucous tissue, which grows hard and produces a prominent mass.

The growth of these tumours is unaccompanied by inflammatory action, and as pain is absent they may be freely handled. The only exception to this rule occurred in the case recorded by M. Verneuil, where the neuroma appeared later in life on the prepuce, and was the cause of great pain, especially when touched. The rule, indeed is, that the surface of the tumour is of blunted sensibility, so that a needle may be thrust into it without signs of pain. This circumstance is explicable by the pathological character of the growth, which presents either degenerated nerve-fibres, or otherwise a great production of connective tissue around the nerve-fibres, where these still remain normal.

Further, adenitis is a common concurrent condition, though not a consequence, and associated with indications of scrofula. The neuroma is not marked by increased vascularity, excepting that the small subcutaneous, and the deep veins are dilated. However, in a case recorded by Winiwarter, the cellular proliferation had seized upon the capillary vessels generally, causing obstruction and minute thromboses.

When examined minutly the tumour is seen to be made up of a network of contorted nerve-fibres, considerably hypertrophied, and bound together by an excessive growth of connective tissue. It has been noticed in some instances that the nerves fibres, though themselves greatly thickened by hyperplasia of connective tissue, are not bound down by the surrounding mass, but may be drawn out, as if from sheaths, for a greater or less distance. Further, in some cases the overlying integument is involved and adds much to the dimensions of the tumour. Owing to this skin complication some have proposed to distinguish two forms, one cutaneous, the other subcutaneous; but there are not sufficient grounds for the distinction.

The nerve fibres are curiously twisted on themselves, after the fashion of varicose veins, and by means of the excessive growth of connective tissue around and between them, they are pressed upon and become at places obliterated, the myeline undergoing granular

degeneration. A microscopic section shows a transparent fibrous mass, permeated here and there by dark lines, indicating the remnants of nervous substance. It is the persistence of these lines of myeline that distinguishes this form of neuroma microscopically from the ordinary one ; for in this latter there is a hypergenesis of all the constituent parts of the nerves with ulterior atrophy of all nerve elements.

In making a diagnosis, these tumours have to be distinguished from cirsoid aneurisms. This is easy, by reason of the absence of vascularity, of bruit, of expansive movements, and of pulsation. Their difference from old varices, grown thick, and with thromboses on parts, is less pronounced ; but these lesions are commonly found in the lower limbs, are of wide extent, and occur among those advanced in age. Moreover, the history of a past stage, when the vessels were pervious and soft, will determine the diagnosis. Again, lipoma and circumscribed elephantiasis exhibit in some points a resemblance. But though a lipoma may be of slow growth, and its lobulation simulate vaguely the varicose cord feeling of neuroma, the latter does not form so definite a tumour, is less isolated, and has the skin often adherent, and hypertrophied and discoloured. Lastly, plexiform neuroma differs from a localised elephantiasis, by the contorted cord-like feeling of its tissue, and by the early age at which it is found, probably dating from the period of birth.

As to treatment, it has to be remembered that though the evolution of these tumours is slow, it is progressive, and that they likewise may undergo sarcomatous degeneration ; consequently the early removal of the diseased mass suggests itself as the proper course. —*Archives Générales de Medecine*, August, 1876.

Classification of Mental Diseases.—Dr. Sankey, formerly of Hanwell, has essayed a classification of mental diseases, a subject often enough attempted but never satisfactorily realized.   He objects that previous systems have not been founded upon the essential characters of the disease itself, meaning by the word disease the sum of all the phenomena, comprising the first indications of deviation from health, the evolution of the symptoms, their progress, order of succession, and mode of termination.

Taking a general survey of cases of insanity, he makes two divisions :—" A. in which the mental symptoms are primary, or essential, or idiopathic ; and B, in which they are secondary, or *symptomatic*. This grand division (A) or the purely mental cases, may be further divided into two subdivisions, viz. (*a*) in which the cases depend upon a purely *pathological* change, or on what is really disease ; and (*b*) in which the mental symptoms are due rather to *developmental* causes. In the former category (*a*), he can recognise as yet but two species, viz. :—I, insanity proper ; and II, general paresis. In the subdivision (*b*), there are also two kinds of cases : (1), in which the developmental anomaly is an original formation, or idiocy, and (2), in which it is due to decay, or senile imbecility."

" The division B, in which the mental phenomena are only symp-

tomatic, or accidents in the cases, forms, perhaps, a smaller moiety of the inmates of asylums ; and the most frequently occurring kind of cases are the four following : epileptic insanity, alcoholismus, spinal disease, extending to the cerebral regions, organic disease of the brain, as the result of apoplexy, &c.

" The old division of mental disorders into mania, melancholia, &c., rests simply on a symptomatic basis, and the characteristic symptoms are themselves fluctuating, replacing each other oft-times in the history of the same case. But, ' a disease does not change its essential characters during its progress ; it may have well-marked stages, but we cannot imagine that each stage is a distinct species of disease.' "

According to Dr. Sankey, ' a case of ordinary insanity, at its first outset, commences by a stage of melancholy, which is at first considered as acute melancholy. The case has in this stage four modes of termination—(1) in recovery ; (2) in death ; (3) in passing into mania ; (4), in becoming chronic (chronic melancholia.) When, in its evolution, it becomes mania, it has been falsely called a new disease. This stage, too, or acute mania, has various modes of termination—(1) in cure ; (2) in death ; and (3) in chronicity." Termination by the last mode presents many psychological variations to which as many names have been assigned.

The forms of insanity, named with regard to their supposed cause, or pathological connection, or leading psychological feature, such as puerperal mania, phthisical mania, dipsomania, suicidal, homicidal, and syphilitic mania, moral insanity, religious melancholy, suicidal melancholy, melancholy with stupor, melancholia agitans, nympho-mania, and kleptomania, are not to be regarded as distinct diseases. Thus, with respect to puerperal insanity, examples of it have nothing in common except the outbreak of mental disturbance in parturition and its immediately preceding and following stages ;—nothing in the character of its symptoms, progress or termination distinctive from other cases of insanity. So with dipsomania, or oino-mania, the uncontrollable desire is sometimes a vice and the prelude to mental disorder ; at others it is allied with imbecility or idiocy, or it may be a symptom recurrent of insanity, or an accompaniment of the first stage of general paresis. This is not the place to criticise, but it seems to us that Dr. Sankey whilst seeing the weakness of other attempts at classification, is scarcely cognisant of that of his own, although, it must be admitted he modestly puts it forward only as an imperfect scheme. The right basis for classification has, to our mind, yet to be discovered.—*Reprint from the Journal of Pycho-logical Medicine*, vol. iii, Part i, 1877.

Hay-fever, its causes.—Dr. Blackley, who investigated the causes of hay-fever and published his results in 1873, has since extended his experimental researches and published them in the form of a pamphlet. His later observations are confirmatory of previous ones. The causes of old assigned, such as heat, light, strong perfumes, and dust, are entirely insufficient to account for the disease. Benzoic acid has also been put forward as a cause, but considering that it

volatilizes only at temperatures much higher than any we ever have in the atmosphere, it is difficult to conceive what led to the supposition. More comprehensible causes were imagined in the shape of coumarin, the odoriferous principle of the anthoxanthum odoratum (one of the meadow grasses), and of ozone. The former, although a volatile body of a very penetrating odour, had no effect in producing hay-fever, at least not in the experimenter, who has long been a sufferer with the disease. Schönbein suggested that ozone might be a cause, and Dr. Blackley has, therefore, very fully investigated it.

To test the presence and quantity of ozone in the atmosphere, he tried the test paper, made after Schönbein's plan, but could get no satisfactory results with it. Accordingly, he contrived a new kind of test paper (the method of preparing which is given in the essay before us) and finds it to answer for the two objects he had in view, namely, " 1st. To determine in what ratio the quantity of ozone increased in ascending a given scale ; and 2nd. To ascertain what relative quantity of this body would have to be inhaled to bring on catarrh, if it was found to have the power to do so."

To carry on his experiments the writer invented several ingenious instruments calculated to give him correct data. These are figured and described in the pamphlet. At the seaside, ozone, when in the largest quantity, as measured by the accurate meter adopted, " did not (writes the author) at any time bring on hay-fever, and two of our most experienced meteorologists tell me they have never known it to bring on catarrh. Moreover, when we consider that the spot where ozone is most abundant is the place where hay-fever patients are most free from their ailment, we cannot but wonder that this substance should ever have been thought to bring on the malady."

Glass-slides, covered with a thin layer of glycerine and spirit, afford a simple and convenient means of collecting, and determining the quantity of pollen in the air. Besides working at ordinary levels, Dr. Blackley, by means of a kite and a specially constructed instrument attached to it, explored the higher regions of the atmosphere for pollen, reaching the altitude of two thousand feet, and the remarkable fact came out, " that we have more than nineteen times the quantity of pollen in the upper atmosphere than we have in the lower." "Pollen wherever applied, produces disturbance. A decoction of pollen applied to the conjunctiva brought on congestion of the vessels, and this after a time was followed by severe chemosis. When fresh pollen was applied to an abraded portion of the skin œdema of the subcutaneous cellular tissue was produced, but there was an inflammation of the true skin. An important and interesting question is how pollen produces all this disturbance. When placed under the microscope and breathed upon, as it is when in the nares, the pollen grain first begins to swell, then the granular matter alters its position, and eventually escapes by bursting through the *intine* or inner membrane, and whilst this is going on it will frequently move half way across the field of the microscope. My conclusion is, that the influence which pollen exercises upon the

mucous membranes and other tissues is of a mixed kind. The sneezing is, I think, due partly to mechanical and partly to physiological action. The inflammation of the conjunctiva is probably due to mechanical action entirely. The chemosis of the conjunctiva as well as the œdema of the submucous and subcutaneous cellular tissues are, I believe, entirely owing to the physiological action of the granular matter."

Not content with his apparent success in showing pollen to be a cause of hay-fever, Dr. Blackley set himself to work to determine the actual weight of it necessary to bring on an attack of the malady; and by the aid of a delicately contrived machine and of the microscope, has satisfied himself that rather less than $\frac{1}{3427}$th of a grain inhaled in each twenty-four hours will keep up hay-fever in its severest form.

The way in which pollen is distributed on the mucous membrane, when inhaled, may partly account for the effect which so small a quantity produces. Of the two recognised forms of pollen, the coherent and the non-coherent, the former is seldom or never found floating in the atmosphere, and therefore cannot be a cause of hay-fever. The grains of non-coherent pollen are always found floating singly, and may, therefore, be assumed to be distributed singly on the mucous surface, thus enabling each grain to exert its full effect. Whether the pollen acts upon the capillary blood-vessels or on the lymphatics of the mucous tissue, the author is not prepared to say.

On this same subject of pollen as a cause of hay-fever, we have a further pamphlet by Dr. Elias Marsh, read originally as an essay before the New Jersey State Medical Society. It is generally confirmatory of Dr. Blackley's views. Like the gentleman just named, Dr. Marsh has, during his lifetime, been a victim to the disease. The form of the malady best known in the United States is called autumnal catarrh, and according to Dr. Marsh, coincides in the time of its occurrence with the flowering of the ambrosia artemesifolia, known popularly as Roman wormwood or hogweed, and growing nearly everywhere in waste places. Dr. Wyman, who wrote on autumnal catarrh (in 1872), first recognised the property of this plant in inducing hay asthma by its pollen.

Dr. Marsh represents the malady as prevailing in proportion to the frequency and luxuriancy of the plant, and that the catarrhal symptoms never come on previously to the time of its flowering. By means of slides both he and his friends have determined the presence of pollen grains in the atmosphere, and his opinion is that the pollen of ambrosia is a special or specific cause of hay-fever.

He compares the action of pollen upon the mucous membrane with that of the Rhus toxicodendron on the skin. From each substance result heat, swelling, exudation, pain, itching, with little febrile movement, but more or less nervous disturbance. In both cases only a small number of individuals are so affected; in other words, there must be a peculiar susceptiblity of constitution before the respective poisons can act. The sensitiveness, in both cases, is of varying grade, and an equal exposure does not equally affect all.

From rhus poisoning some have a mild attack of erythema, and others a degree of inflammation approaching erysipelas ; some require the immediate contact of the juices, while others are affected by the minute amount of volatile acid wafted by the wind. From hay-fever some have a troublesome coryza only, and others suffocative dyspnœa. In both affections removal from the cause proves curative, for the disease is known in districts only where certain plants grow. The only or chief differences in the character of the two diseases arise from the difference in the persistent or transient duration and application of the poison. In rhus poisoning the irritant is of more or less force during the entire year, and consequently the disease has no defined period of attack. In hay-fever the poison is active for a few weeks only of each year, during the blossoming time of the plant, and consequently the disease is periodical, recurring annually at the same date. In rhus poisoning there is usually only a single application of the poison, and the irritant being then removed the disease seems to yield to treatment. In hay-fever the application of the poison is continuous, hence the disease is persistent, and treatment has no effect in limiting it. If, however, the patient be removed from the cause, then it yields as quickly as in the former case.

Dr. Marsh cannot admit hay-fever to be a constitutional disease, or one of the nervous system, but argues that it is only a local one, dependent solely on a local and special cause, although requiring a peculiar susceptibility for its development. Lastly, it would seem that it is not pollen in all states and stages, but that only which is mature and derived from a plant in full vital activity under favorable atmospheric conditions of light and heat.

No specific remedy is known. Hypodermic injections of morphia usually relieve, as does also chloroform, at times, when inhaled for the asthma. Removal from the locality where the attack occurs, and particularly to the seaside, is the most potent source of relief.

We are not disposed to consider the etiology of hay fever fully explained by this hypothesis of pollen poisoning. The very same symptoms occur where the action of pollen may be ignored, nor can we admit that pollen of all sorts, as seems implied by Dr. Blackley, is a cause of hay fever. Experience shows that a very large number of flowers have no effect ; in seed we can imagine some would be curative.— (Rep.)

REPORT ON MIDWIFERY AND DISEASES OF WOMEN AND CHILDREN.

By Alfred Wiltshire, M.D., M.R.C.P. Lond.,

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Midwifery.

1. *Albuminuria during Pregnancy.—Cure by Jaborandi.* By M. Langlet ('Union Méd. du Nord Est,' No. 6, 1877).
2. *Spontaneous Rupture of the Uterus in the Sixth Month of Pregnancy.* By Dr. Lustgarten ('Wiener Med. Presse,' No. 13, 1876).
3. *On Temporary Glycosuria in the Puerperal State.* By M. Gubler ('Gaz. Méd. de Paris, and Soc. de Biol.,' 1877).
4. *Acute Hyperplasia of the Anterior Lip of the Uterus during Pregnancy.* By L. Kessler ('Dorpat Med. Zeitsch.,' vol. vi, p. 103).
5. *Intra-uterine Injection of Hot Water in Post-partum Hæmorrhage.* By Dr. Runge ('Berliner Klin. Wochensch.,' March, 1877.
6. *Smallpox in a Six Months' Fœtus.* By Dr. Bouteillier ('Union Med. de la Seine Inf.,' April, 1877).
7. *Novel Treatment of Retroversion of the Gravid Uterus.* By M. Massart ('L'Union Medicale,' September, 1877).
8. *On New Forceps.* By Professor Tarnier ('Mémoire,' Paris, 1877).

1. M. Langlet relates a case of albuminuria, accompanied by œdema, which occurred in a multiparous woman when about six weeks pregnant and which he treated by jaborandi.

The patient was threatened by abortion, and had premonitory symptoms of eclampsia. The urine was very scanty and was highly charged with albumen. There was fluid in each pleural cavity.

None of the remedies employed benefited the patient until she took jaborandi. The most notable effects of the drug were a considerable augmentation of the urine and great salivation, sweating, though free, being less marked.

The drug was given for fifteen days, at the end of which time the urine became markedly bloody. There had been a few globules before, but now there was distinct hæmaturia, which Dr. Langlet attributes to the powerful diuretic action of the drug. The patient went to the full term of pregnancy and was delivered of a fine child. The only thing remarkable about the labour was the large amount of liquor amnii which escaped.

2. Dr. Lustgarten gives details of a case of spontaneous rupture

of the uterus at the sixth month in a young woman, æt. 28, pregnant for the second time. The first confinement took place five years previously, and, though laborious, was normal. Without apparent cause she was suddenly seized with violent pain in the abdomen and vomiting.

At the autopsy a rupture of the size of the thumb was found in the upper and left part of the body of the uterus; and on opening the organ it was found that the placenta was solidly fixed on the lower segment of the uterus, entirely occluding the internal orifice of the cervix; it was, in fact, a case of placenta prævia lateralis dextra.

3. The conclusions of Dr. Gubler's memoir on temporary glycosuria in the puerperal state are as follows:

(1.) Glycosuria is not a normal phenomenon of lactation.

(2.) It appears on the suspension or premature suppression of suckling when the mother is well, or at least when the great functions have not sustained any serious disorder.

(3.) In other words, glycosuria appears only as the consequence of a rupture of the equilibrium between production and consumption, giving rise at first to lactosæmia, comparable to sanguineous super-albuminosis whence dyscrasic albuminuria is derived.

But it may be asked why the suspension of the secretion of milk gives rise to the elimination of one only of its immediate principles by the renal glands. For M. Gubler has never seen transitory albuminuria accompany glycosuria in these pathological conditions. It is because the passage of sugar, a crystalloid and dialysable body, is much more easy than that of albumen, which is a colloid body, and will not pass through the dialysers. Albuminuria always supposes at least a renal hyperæmia, which confines itself to the first degree of an inflammation, whilst glycosuria may take place without any anatomical modification of the kidney. Lastly, resorption of milk carries into the blood but a small proportion of the albuminoid materials, whilst it reintroduces into the circulation a large quantity of sugar of milk.

4. Dr. Kessler relates the case of a woman who, a month after a fall, in the thirty-fourth week of her fourth pregnancy, found a mass in the vagina during micturition. It was as large as a hen's egg, and was found on examination to spring from the anterior lip of the cervix uteri. She was in due time delivered by the forceps, and some weeks afterwards the growth was removed, as it did not decrease with the rest of the uterus.

The author remarks that cases of tumefaction of the anterior lip of the uterus are rare. On analysing them he thinks they can be reduced to three kinds.

(*a.*) In the first, the anterior lip is pressed and rubbed by the fœtal head, during labour, against the pelvic walls, whereby more or less intense swelling is caused. This is only a passing tumefaction, which disappears after delivery.

(*b.*) In the second, hypertrophy has existed before pregnancy, but augments with it in proportion to the increase in the volume of

the uterus. Ordinarily this also disappears during the puerperal period, but sometimes it increases, as in the cases observed by Schröder and Schöller.

(*c.*) In the third, the normal anterior lip swells during pregnancy. This tumefaction is transformed into hyperplasia, which may persist or disappear after delivery.

As regards pathology and etiology, the knowledge of the formation of these tumours, and also the histological researches, are defective or insufficient. The author has designated his case as one of hyperplasia, and this hyperplasia may resemble that which Virchow named simple and uniform hyperplasia of the lips; and if one compares this alteration with that which Klebs called "myoma of the uterus," one might denominate it by the term diffuse myoma; for in Klebs' case the hyperplasia was limited and separated from the healthy substance.

This tumefaction, according to Virchow, appears to be due to a species of œdema or inflammation resulting from pressure of the lips between the head and the pelvis.

As regards treatment, the pressure should, if possible, be relieved. If practicable, the lip should be pushed up; if not, the forceps should be applied when the neck is sufficiently dilated.

5. Dr. Runge states his experience of hot water injections in postpartum hæmorrhage. He injected water as hot as from 117° to 124° Fahrenheit. The remedy was successful when ergot, ice, and external manipulation of the uterus had failed.

The injection of water at the higher temperature caused so much pain in some cases that it had to be reduced. Though Dr. Runge cites Windelband, Jakoch, and Landau, he does not state that the injection of hot water for controlling uterine hæmorrhage is practised in America with much success by Dr. Fordyce Baker, Dr. Emmet, and others. The remedy seems to be a powerful one, but probably some caution should be exercised in applying it.

6. Dr. Bouteillier exhibited, at the Medical Society of Rouen, a fœtus showing variola. The mother, æt. 31, had been vaccinated in childhood. She was attacked with smallpox in October, 1876, in her third pregnancy, and aborted about two months later. The fœtus presented a general eruption of umbilicated pustules. It is remarked that "this observation is remarkable for the length of time which appeared to have elapsed between the epoch of invasion of variola in the mother and its probable invasion in the fœtus;" and this is certainly very singular.

Dr. Pennetier related the case of a lady who had smallpox during her pregnancy, but gave birth to a healthy child, who was "refractory to several attempts at vaccination."

Dr. Thierry made the following excellent remarks:

(1.) Variola is rare in the fœtus of six months. It ordinarily declares itself in the eighth or ninth month.

(2.) The pustules in fœtal variola are few on the face, while they may be confluent on other parts of the body.

(3.) Relative to the influence of maternal variola on the aptitude which the child will afterwards have to contract the disease, observa-

tions have in a general manner taught that variola must be contracted by the mother before the seventh month of her pregnancy for the infant to become refractory to the disease. After the eighth month the same immunity is not acquired by the fœtus or child.

(4.) The evolution of variola seems to occur more slowly during intra-uterine life than after birth, and this greater length seems to pertain to the incubation, which may be of a month's duration. Cases are on record of variolous fœtuses being aborted two months after variola in the mother.

The Thesis of Laurens (Paris, 1871) narrates the case of a woman who gave birth to a variolous infant forty-one days after she had been in contact with a woman who had smallpox, and without herself having had the disease. The pustules on the child indicated the seventh to tenth day at most, from which we may conclude that, in this case, the period of incubation was at least thirty-one days. Lastly, he cited cases of twin-birth, in which one only of the children was born variolous, the mother escaping. And Dr. Chantreuil has related a similar fact.

7. M. Massart, of Honfleur, communicated to the Havre Congress of the French Association for the Advancement of Science a remarkable case of retroversion of the gravid uterus, which was cured by a means borrowed from the old Norman bone-setters, which consisted in applying a " pôt de chambre" as a cupping glass on the belly of the patient in such a way as to diminish the pressure which the intestinal loops exercised on the displaced uterus.

8. Professor Tarnier, of Paris, has recently introduced some new forceps for which he claims superiority in certain important respects over those in common use. Briefly stated, the chief advantage gained is that of being able to make traction on the fœtal head in exactly the proper axis of the pelvis without any guidance or control on the part of the operator. The mechanism whereby this is secured consists, in addition to forceps similar to those of ordinary construction, of a pair of movable traction stems, which are inserted one into the lower edge of each blade close to the fenestrum, and which must be kept parallel to the handles of the forceps, the latter acting as indicators of the direction in which the head is descending. The forceps have not handles of the usual form, because traction is not to be made through them but through the traction stems, by means of a transverse bar attached to their extremities. The operator, after properly applying the forceps to the fœtal head, has only to make traction, maintaining always complete parallelism between the traction stems and the handles of the forceps. Competent observers have stated that Professor Tarnier's new instruments appear to be very efficient, but more extended experience is required to completely establish the claims of the learned professor.

DISEASES OF WOMEN.

1. *The Significance of Pus in Ovarian Fluids.* By Dr. CHADWICK ('Reports of Boston City Hospital,' 1877).
2. *On a New Cause of Vaginismus.* By O. JOHANNSEN ('Petersburg Med. Wochensch.,' No. 9, 1876.
3. *On Erythema Exudativum.* By G. LEWIN ('Berliner Klin. Wochensch,' No. 23, p. 321).
4. *On Urethral Affections in Women.* By A. BLUM ('Arch. Gen. de Méd.,' August and September, 1877).
5. *Double Uterus and Vagina with Fibroid Tumours and Absence of one Kidney.* By Mr. JOHN CLAY ('Lancet,' July 21, 1877).
6. *On the Treatment of Vegetations of the Mucous Membrane of the Interior of the Uterus.* By M. GALLARD ('L'Union Méd.,' Sept., 1877).
7. *Sanguineous Infiltration of the Lower Limbs ; Cutaneous Ecchymoses and Purpura coinciding with Menstrual Suppression.* By M. LEDOUBLE ('Annales de Gynécologie,' April, 1877).
8. *On Hysterotomy for Uterine Tumours.* ('Annales de Gynécologie,' April, 1877.)

1. Dr. Chadwick gives an interesting note on the significance of pus in ovarian fluids, based on the observations of a case in which tapping was repeatedly performed.

At the first tapping the fluid was clear and mucilaginous, but at subsequent tappings it was found to contain pus. There were, however, no symptoms pointing to inflammation of the cysts, "no general malaise, no loss of strength or flesh, no fever, no chills, the pulse and temperature were normal on the only occasion on which they were recorded. The case, therefore, suggests that pus in the fluid taken from an ovarian cyst is, in the absence of symptoms pointing to acute inflammation, pathognomonic of ulcerative action in the cyst. The *perforation* of the cyst wall, in the course of this process, indicates the imminent danger to which patients are subject under these circumstances, and calls for quite as prompt operative interference on the part of the surgeon as do the symptoms of acute inflammation."

2. Dr. Johannsen relates the case of a young married woman who had had no children, who for a long time suffered severe pain during coitus which was not relieved by treatment. Micturition was painful, and as pressure of the urethra against the pubis was extremely painful, Dr. Johannsen examined the canal with a speculum, and found on the floor of it two yellow points about the size of pins' heads. These points were the orifices of two fistulæ. The larger of the two fistulæ was split up, and the other touched with caustic, after which the vaginismus disappeared.

(3). In an important work on the influence of the vaso-motor nerves in the production and development of cutaneous affections, Dr. Lewin states the results of the observation of thirty-nine cases

of erythema nodosum. Though generally occurring without pro-
dromata, the eruption may be preceded by fever; it is mostly
symmetrical on the two halves of the body, and the seat of predi-
lection is where bones are covered by inconsiderable layers of
muscle, as the backs of the hands and feet, the extensor sides, and
front of the legs, &c. Ultimately, various other parts of the body
may be invaded. The infiltrations occupy the subcutaneous tissues
and may assume various forms, as tubercular, papular, and nodular.
The disease attacks by preference young persons, and especially of
the female sex. In severe cases, inflammatory affections of the
joints may supervene, which may be attended by serous or purulent
effusions; and occasionally, in the latter case, anchylosis may ensue.
There may also be valvular endocarditis, leading to disorganization
of the valves. Lewin thinks that some of the cases of valvular
endocarditis of doubtful origin may be explained by an old attack
of exudative erythema.

As regards the etiology of exudative erythema, exposure to cold,
which is so generally admitted, is only one of the factors. In a
large proportion of cases, especially in females, an inflammation or
ulceration of the urethra presents a direct causal relation with the
erythema. The cutaneous affection must be considered as the mani-
festation of a reflex irritation proceeding from the urethral mucous
membrane, and acting on the vaso-motor nerves of the skin; it is,
in fact, comparable to blenorrhagic rheumatism.

This view is based on clinical and experimental observations.
Thus a great number of the women treated by Lewin for erythema
had at the same time a blenorrhagic inflammation, ulcerous or not,
of the urethra. One patient, cured by Dr. Lewin of the urethritis
and erythema at the same time, had a recurrence of erythema on a
relapse of the urethral affection.

An experimental proof was furnished by another patient, in
whom, after the cure of the urethritis and erythema, irritation of
the urethra by the sound, or by the introduction of savine ointment,
was sufficient to establish a severe attack of erythema.

Lewin states that sometimes exudative erythema appears epidemi-
cally, and it appears then to be produced by a volatile contagium
similar to those of other acute infectious exanthems.

4. Dr. Blum contributes a copious essay on urethral affections
in women, chiefly condensed from various periodicals, as he finds but
little relating to the subject in classical works. The essay will
repay perusal, though it shows how much is still wanting to com-
plete our knowledge of this exceedingly troublesome class of
affections.

5. An extremely interesting case of double uterus with two vaginæ,
complicated with fibroid tumours and absence of one kidney, occurred
in the practice of Mr. John Clay, of Birmingham. The nature of
the tumours was doubtful, and as aspiration failed to make any im-
pression, an unsuccessful attempt was made at enucleation per
vaginam. The patient died of peritonitis, and at the post-mortem
examination it was found that there were two uteri and two vaginæ.

Both uteri contained fibroid growths. No kidney was found on the left side, but there was a ureter on that side which was obliterated some distance up. There were only two ovaries, one to each uterus, so that the case must be regarded as one of bipartite uterus and vagina.

6. M. Gallard made a communication to the French Association for the Advancement of Science, at its late meeting at Havre, on the nature and treatment of vegetations of the intra-uterine mucous membrane. The subject had been well handled by Recamier, but the practice he recommended had suffered reverses. Dr. Gallard, however, thought the method of treatment by the curette capable of rendering real service, and he related a remarkable case in proof of this assertion.

[We have certainly ourselves seen some very remarkable instances confirmatory of Dr. Gallard's views.—REP.]

7. Dr. Ledouble records a most interesting case in which blood was effused into the lower limbs as the immediate consequence of acute suppression of the menses. The limbs swelled and were painful; large ecchymoses and petechiæ rapidly formed. The legs were nearly twice their natural size, and the swelling was hard and did not pit. Regarding these symptoms as due to suppression of the menses, Dr. Ledouble ordered sinapisms to the breasts, and gave fl ₃ss drachm doses of ether every two hours. On the following day the menses re-appeared, and from that moment a sensible amelioration was apparent. A month afterwards acute suppression of the menses took place from fright; the same symptoms appeared and yielded to the same treatment. After that the menstruation became, as before, perfectly normal.

[Dr. Ledouble's case has an important bearing on the question of the occurrence of general vascular disturbance during menstruation. He is evidently of opinion that something of this kind occurs, for he concludes his reflections on the case as follows:—" It is in a part already weakened by an anterior organic lesion, or by excess of functional work, that pathological manifestations are by preference produced. Under the influence of vasculo-nervous erethism the fluxionary movement due to the suppression of the menses determines hæmorrhage in the part of the body which, owing to an anterior lesion or an excess of work, even physiological, has lost the greater part of its resistance."—REP.]

8. At a late meeting of the Academy of Medicine of Paris, M. Péan exhibited specimens from a case of hysterotomy, the nature of which not being clear, the parts were referred to a committee consisting of MM. Depaul, Robin, and Laboulbéne.

Dr. Péan had previously presented to the Academy a group of observations of twenty-seven hysterotomies practised with the view of relieving the patients from voluminous uterine tumours. These observations were thus distributed:—18 operations for uterine fibromata: 12 successful, 6 unsuccessful; 6 operations for fibro-cystic tumours: 4 successful, 2 unsuccessful; 2 for ablation of utero-cystic (?) tumours: both successful. Since that time he had

practised 7 new hysterotomies : 3 for ablation of fibrous tumours, 2 for utero-cystic tumours, one for a carcinomatous tumour of cystic form, and lastly the case in question.

[The success which has attended the operations of Péan, Kœberlé, Wells, and others, for the removal of fibroid and fibro-cystic tumours of the uterus, is so great, seeing the extreme gravity of the disease in the cases which demand this form of operation, that it is to be hoped that other competent operators will not neglect the operation in suitable cases.

A success equal to that which has happily attended ovariotomy is hardly to be anticipated; but many sufferers who now succumb without an attempt being made to relieve them by operation may thus be rescued from a lingering and painful death.—REP.]

DISEASES OF CHILDREN.

1. *Renal Sarcoma in a Child aged Seven Months.* By Dr. BA-GINSKY ('Berlin Klin. Woch.,' No. 18, p. 249).
2. *On Dyspeptic Asthma.* By Dr. HENOCH ('Berliner Klin. Wochensch.,' No. 18, p. 242).
3. *A Case of Spina-Bifida cured by the Elastic Ligature.* By Dr. COLGONESE (' Bull. delle Scienze Medici,' April, 1877).
4. *Erysipelas in Infants caused by Rancid Fat.* By M. SIREDEY (' L'Union Médicale,' September, 1877).

1. In Dr. Baginsky's case there had been hæmaturia for several weeks. On examination a tumour was found in the left side extending from the costal margin to the median line and down to the iliac crest. As dyspnœa arose from increase in the size of the tumour a puncture was made, which gave exit to 470 cubic centimètres of dark beer-coloured fluid which contained much blood, and some urea and uric acid. The child died on the following day without having had convulsions. At the *post-mortem* examination the left kidney was found to be transformed into a cystic tumour composed of two parts, an enormous cyst and a solid mass, nearly as large as a fœtal head. The growth was adherent to the abdominal walls and to the diaphragm ; and the ascending colon was found on its surface. The tumour was examined by Virchow, who found it to be a fusiform-celled sarcoma, with but few remains of kidney structure. The right kidney showed a marked specimen of parenchymatous nephritis.

2. Henoch, describes cases of dyspeptic asthma, two of which occurred in children aged respectively nine months and nine years. Both were very severe. The dyspnœa was urgent and a fatal issue was in each case feared. Henoch first observed the affection in company with Traube, and whilst he could not renounce the idea of an affection of the circulatory apparatus, in spite of the negative results of examinations, Traube, relying on the experiments of Mayer and Pibram (' Sitzungsberichte der Wiener Akadamie der Wissen-schaften,' 1872), considered them to depend on irritation of the

stomach. He explained the chain of phenomena in the following manner; reflex irritation starting from the stomach, vaso-motor cramp in the small arteries, whence coldness of the extremities, insensibility of the pulse, stasis in the venous system and right heart, cyanosis, accumulation of carbonic acid in the blood, and, as a consequence, frequent and dyspnœic.

Emetics and dry cupping gave most relief.

3. Dr. Colognese's case of spina-bifida was one which sprang from the level of the ninth dorsal vertebra. It was of the size of a large orange, had a pedunculated base, and contained transparent fluid. The skin covering it presented a normal appearance. The tumour moved with respiration and when the infant cried :—a hydro-rachidian tumour was accordingly diagnosed ; serous fluid was evacuated by puncture. An elastic ligature was placed around the pedicle, care being taken to avoid tightening it too much at first, and also to avoid dragging on the spinal canal or skin. The child was comfortable and slept well at first, afterwards it cried a great deal and refused the breast, but ultimately, on the ninth day, the mass dropped off, leaving a healthy wound, perfectly closed, and from which no fluid escaped.

4. In an unaccountable endemic of erysipelas among the infants in the Lariboisière Hospital, at Paris, M. Siredey discovered that the disease was due to the application of rancid lard, which was employed to anoint the buttocks of children who were suffering from, or threatened by, diarrhœa. As many as fifteen cases occurred in the half year, and only two of these recovered. Healthy as well as cachectic children were attacked, and the erysipelas did not start from the umbilicus, but from the buttocks, genital organs, and parts covered by napkins. The disease ceased to be endemic when fresh lard was used.

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PUBLISHERS' NOTICE.

It is with much regret that we have to announce to our readers that this is the last number which it is intended to publish of the ' British and Foreign Medico-Chirurgical Review.' For a period of thirty-eight years the *Review* has been looked upon by the profession as holding a position above that of the weekly and other medical periodicals, the contents of which, from their more frequent publication and the haste in which they must often be compiled and printed, are necessarily more ephemeral in character, while the standard position of the *Review* has been again and again attested by the importance which anxious authors have been wont to attach to its critical remarks upon their literary productions. But in the present day he who would succeed in life must mark the signs of the times. For several years we have witnessed the gradual decline in sale of that which once was a good property, and, in spite of all our efforts to infuse new life into our old friend, we have been obliged to stand by and see it languish, so that the period has at last arrived when to continue to publish it would be to incur an annual loss which would rather increase than diminish as time went on. The reason of all this is that the day for quarterlies is gone by, and, in the face of the daily and weekly periodicals, a quarterly, with its thoughtful articles and well-digested reviews, is no longer appreciated as formerly. This remark applies, we may state, not less to general than to medical literature.

The remarks we make are few, as in writing our farewell words we feel as though taking a last· leave of one we have known for many a year. We cannot, however, conclude without reminding our readers of the list of names of those we have had the happiness to secure as editors of this time-honoured publication. Such men as the late Sir John Forbes and Dr. Parkes, as Dr. Carpenter, Dr. Sieveking, Dr. J. W. Ogle, and our present esteemed friend Dr. Arlidge, are a proof that the literary excellence of the *Review* has been. of the highest order, and to them and to the able collaborateurs who so well seconded their efforts we shall always look back with feelings of the greatest gratitude.

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