Part Second.

REVIEWS.

On the Pathology and Treatment of Scrofula; being the Fothergillian Prize Essay for 1846. By Robert Mortimer Glover, M.D., Lecturer on Materia Medica in the Newcastle Medical School. London: John Churchill. 1846. Pp. 315. Plates.

We have no hesitation in pronouncing this work to be one of the best which has lately appeared on the important subject of which it treats. It is conceived in the true spirit of rational medicine; it is based upon observation and experiment; it has nothing to do with fallacious statistical data, and the subject is sought to be investigated in the only legitimate manner, namely, by anatomical, chemical, and clinical researches. The following passage in the introduction at once prepossessed us in favour of the book, and prepared us for the valuable observations which follow:

"The great difficulty to be contended with in medicine is the obscurity in which the connection of observed facts is veiled. Hence the great value of anything approaching to experiment. The function of experiment must be distinguished from that of mere observation: experiment has in view the nature of the connection between ascertained or observed facts, in order to test the constancy and essentiality of their relation; in other words, it is the bringing out of what Bacon terms prerogative facts. For instance, iodine is a remedy supposed capable of producing the absorption of scrofulous tumours. But a mere case in which a scrofulous person recovers under the use of iodine is of most moderate value; for patients have got rid of scrofula, while submitted to every form of treatment as proposed by generation after generation of so-called practical men; and remedy after remedy, thus used with apparent success, has fallen into oblivion. Suppose we were able to show that the use of iodine promotes not merely the flow of the urine, but also an increase of the solid contents in this fluid, and especially of the amount of urea? In other words, iodine excites the secondary digestion of the tissues; and as urea is the product of the albuminous tissues thus converted, and tubercle is composed chiefly of albumen, we learn that the connection between the giving of the remedy and the absorption of a scrofulous tumour is not accidental, and thus we may derive a confidence which the blind empiricism, so absurdly denominated practical, could never properly give. The sequel will prove that there is no wish here to undervalue observation of any kind; it is only intended to put prominently forward the great value of rationalism in medicine; and the reflections just written have been dictated by a review of the numerous failures in generalisation presented by the history of the medical literature of scrofula—failures which have chiefly arisen from too hastily grasping at the sequence and relationship of facts, without a sufficient consideration whether the connection observed was essential and constant, or merely accidental."—P. 6, et seq.
Dr Glover has divided his work into two parts—1st, the pathology, and 2d, the treatment of scrofula. Each of these parts is subdivided into chapters, and we shall now proceed to give an analysis of each seriatim. As we have already stated our views regarding the general excellence of the work, we trust that the author will not consider us as in any way disparaging his labours, should we venture occasionally to differ with him in opinion, and to criticise rather closely some of his statements.

In the first chapter Dr Glover describes the structure and chemical composition of scrofulous or tuberculous matter, which he understands to be a peculiar morbid formation, the product of a particular modification of the inflammatory process. Tubercle is a term which includes all scrofulous formations, in whatever tissue or organ they may be found. He agrees with Lugol in considering scrofula—that is, the actual process of disease—to be always revealed by the development of tubercles; and thinks, with Barthez and Rilliet, that the distinction said to exist between scrofula and tubercle is useless, if not incorrect. He believes that tubercles and scrofulous matter are not essentially vascular in their ultimate stage of conversion, but says that scrofulous glands at an early stage can be injected. No one, we presume, has ever denied the possibility of this; but, then, it is not the vessels of tubercle, but of the glandular structure, which are injected. After quoting the observations of Canstatt, Guillot, and others, the author concludes that the vascularity of tubercle is a non-essential phenomenon. We think he might have said more plainly, that tubercle is never vascular, although tubercular glands are. We presume this is what he means, when he says subsequently, that tubercle itself is beyond the normal influence of the circulation—an expression he has placed in italics, and in which we can in no way agree. He calls this a fact, and says it is confirmed by the results of microscopic examination. But it does not follow that although tubercle be non-vascular, it should therefore be beyond the influence of the circulation. Far from it, for tubercle is capable of being affected by the admixture of serous effusion, or liquid plasma exuded from the blood-vessels, and, when reduced to a molecular condition, of being also absorbed through their agency.

As regards the minute structure of tubercle, the author says that he agrees generally with Dr Lebert and Dr Hughes Bennett, according to whom, corpuscles of an irregular angular form, varying in diameter from 1-200dth to 1-300dth of a line, constitute the characteristic element of this substance. He believes that Lebert has mistaken the minute granules which are scattered both between the spaces occupied by the larger corpuscles, and over the larger corpuscles, as component parts of the latter. We beg to assure Dr Glover, however, that Lebert has done nothing of the kind, and that his description of these corpuscles is singularly true to nature. That the granules are really contained within the cell.
Dr Glover confirms the observations of other histologists, who have found tubercle in all its forms, and taken from various tissues and organs, to be identical in structure. He agrees with them also in the idea, that the tubercle corpuscles are cells which are not fully developed, owing to a want of formative power in the scrofulous deposit.

The present state of science renders it of all things important to ascertain the exact chemical composition of tubercle and other morbid productions. Of this Dr Glover seems to have been well aware; for we find that the section on this subject not only contains a lucid account of all that has been done in this field of inquiry, but that it is enriched by thirteen laborious and careful analyses of various kinds of tubercle. For the details of these proximate and ultimate analyses we must refer to the book itself. The following are the general conclusions arrived at:

"1st. The results of the chemical analysis of tubercle and its after products, of scrofulous bones, &c., although they may not as yet warrant very decisive conclusions, yet furnish some useful information, which will be found to bear upon the pathological propositions advanced concerning the essential nature of scrofulous and tubercular affections.

"Thus the large quantity of fat and extractive matters in tubercle, has a direct bearing upon the theory supported by many of the advocates of the use of cod liver oil in the treatment of these diseases. The existence of pyin is important, and could we be sure of that of caseine in quantity, we might to a certain extent explain the unorganizability of tubercle. But we have never been able to satisfy ourselves that the proteine constituent of tubercle, as examined by us, approaches much nearer to caseine, than to albumen. Nevertheless, the remarks of Preuss, Boudet, Scheerer, and others, must be held decisive of the existence, at least in some cases, of caseine; although the last named observer is far from confirming former writers in the statement of a large proportion of tubercle matter being composed of this substance. We have made other examinations for caseine than those recorded, and have never been able to detect its presence. Whoever considers the very doubtful power of the tests which we possess for distinguishing these different substances in the animal body, will be very doubtful of the precise nature of the proteine basis of tubercle. Nevertheless, we may perhaps conclude, that there is great probability of this proteine compound having a certain approach to caseine, or at least of a portion of it, exhibiting a tendency to take on the characters of this latter substance.

"2d. The ultimate analyses which have been made, can lead to no very definite conclusion, although Scheerer infers, from a comparison of his formulae of lung and liver tubercle, that the difference which exists between them may arise from the substance in the latter situation being less exposed to the air. Thus he says, making the azote the fixed quantity, we have

The lung tubercle, C_{43} H_{70} N_{8} O_{13}

The liver tubercle, C_{45} H_{8} N_{12} O_{13}

showing an excess of carbon and hydrogen.

"Our analyses would give to the proteine compound of tubercle a much smaller per centage of azote in general, than those of Scheerer; only 12:31 in the first analysis of mesenteric tubercle, while the proportions of carbon and hydrogen show that the substance had been completely freed from fat. But when we find analyses of normal proteine, differing almost as much from the
ordinary standard, what inference can we draw? Since we wrote as above, Liebig has called in question the proteine theory, but the use of the word proteine, in this essay, is not hypothetical; it is used to signify a basis of an albuminous, or perhaps, partly caseous nature, which undoubtedly is at the bottom of the constitution of the greater portion of tubercle.

"It does not, however, follow that these ultimate analyses are useless, because we cannot at present draw many inferences from them. We see, at least, the close approximation which they enable us to make between the basis of these morbid formations and proteine compounds.

"3d. The analyses of the concretions which we have made, does not bear out in the least the statement of M. Boudet, which makes these bodies contain 70 per cent. of soluble salts. On the contrary, even in the tubercle, which may be supposed to form a transition stage, on the way towards the conversion into the calcareous substance, we find only about one-third of the ash composed of soluble salts; and in the perfect concretion there was merely a fraction of these substances. In this respect, our results agree with those of Scheerer and Mulder.

"A doubt has arisen whether these concretions should be regarded as the remains of absorbed tubercle; and this doubt is supported by Rayer, who maintains them to be oftener the residue of pus. The presence of these concretions may not improbably serve to attract towards them, once formed, more osseous matter.

4th. The analysis of scrofulous bones requires no comment.

5th. Scrofulous pus appears to differ from ordinary pus, chiefly in the fluid part being thinner and mixed with albuminous granules, proceeding from a decomposition of scrofulous or tubercular matter. The pus globules appear also, as stated by Mr Gulliver, to be fewer and less distinct than those of healthy pus; we have found them also more irregular in their form."—Pp. 86-90.

We must confess, that we are in no small degree disappointed at the unsatisfactory conclusions to which Dr Glover’s laborious researches have led. None but those who have engaged in such observations can form an idea of the time, labour, and care which are necessary to complete them. Yet the thirteen accurate analyses, added by Dr Glover to those previously made by Simon, Scheerer, and others, do not seem to have extended materially our knowledge of the nature of tubercle. This, we are satisfied, is owing to no want of labour and accuracy on the part of the author, but rather to the difficulties inherent in the subject, and to the present imperfect state of organic analysis. Fully agreeing, as we do, with Dr Glover, in thinking that tubercle is an exudation from the blood vessels, and therefore a form of inflammation, the question presses upon us, what constitutes the difference between a tubercular and an inflammatory exudation? Why in the one should we have imperfect, and in the other perfect cells? Why is the progress of the one slow, of the other rapid; and why is the one so seldom and the other so frequently amenable to rational treatment? Again, in what does tubercle differ from a cancerous exudation, which is even more incurable; and yet, instead of presenting miserable broken down cells, furnishes the most perfect and beautiful corpuscles which the animal tissues can furnish? These are mysteries which we have always hoped that organic chemistry would one day explain; and we yet anticipate, that as science advances, some labourer, following the footsteps of our author, will earn undying reputation by their solution.
Not satisfied, however, with experimentally investigating into the chemical composition of tubercle itself, our author has extended his researches to the blood and urine of scrofulous and tubercular individuals. The results he has embodied in the second chapter, under the head of humoral pathology of scrofula.

Dr Glover acknowledges, that the methods hitherto pursued of analysing the blood are faulty. He has adopted the method of Andral and Gavarret; not because it is absolutely correct, but because it serves the purpose of comparison.

"Had Simon, or some other observer, given us tables of the composition of healthy blood sufficient to serve our purpose, another method than that referred to, might have been, and with advantage, adopted; but we cannot compare results obtained in another way with the results of the method of the French observers."—P. 99.

If the method of analysis adopted by Andral and Gavarret be faulty, as we believe it is, we regret that Dr Glover should have adopted it, however important it may be to obtain a standard of comparison. By such a proceeding, science can never hope to proceed in a right direction. It does not follow that because the first step taken has been wrong, that we should make other false steps, with our eyes wide open to the errors of the path we are pursuing. We would rather have had made accurate analysis of the blood in cases of scrofula, and left to other and later chemists the task of comparison, than receive vitiated results from the adoption of a faulty process.

The author, with a pains that deserves all praise, has determined the amount of water, solids, fibrine, solids of serum, both organic, and mineral, and blood globules, in eighteen cases. The following are the general results:

"According to Becquerel and Rodier, the proportion of water in the blood of females is 791/2, that of blood globules, 127/2; the fibrine is the same as in the blood of males; the solids of the serum are 79/5, of which 7/695 are salts; the fats are 1/620; and the extractive matters, with their salts, 7/4. In the analyses above given, in only one instance do the blood globules approach very near the normal standard, i.e. in case 18. This was a case, however, in which the scrofulous affection was slight, and, if we make abstraction of the old disease, of recent appearance. Perhaps a more decided illustration of genuine struma than case 12 could scarcely be found; and, accordingly, we find considerable diminution of the blood globules, with a greatly increased quantity of solids of the serum. The salts are throughout nearly normal, oftener below the standard than above it. The fats were only examined in No. 15, and then were found rather below the standard. The fibrine was generally above the standard given by Becquerel and Rodier. The great diminution of the blood globules, in the case of goitre, was remarkable, not being accompanied by any very apparent emaciation. The solids of the serum, above the healthy standard in five of the cases, were below it in the remaining number." "In women the means will be, excluding the goitre case, solids, 203-845; fibrine, 3-585; solids of serum, 85-28; globules, 114-98."—Pp. 113, 114.

These analyses, therefore, are in favour of the opinion, that in scrofula the blood contains an excess of albumen, with a diminution of the red globules. Dr Glover also says, that so far as the analyses
go; the fats are not deficient in the blood; but we cannot understand how this statement can be borne out by the facts recorded. The quantity of fats in the blood was only ascertained in one case, and then, according to the author, they were rather below the normal standard. Yet, insufficient as one analysis must be to decide such a question, even supposing the fat was in excess rather than as it actually was—diminished in the blood, he seizes the opportunity of stating that the well-known theory of Ascherson is incorrect. We can discover no facts advanced by the author to show this. It is true he observes that this theory is "too mechanical, and vitiates itself by giving a too easy explanation of great difficulties" (p. 116); but we do not know where the line is to be drawn between mechanical and vital explanations of phenomena occurring in the body; and we are far from thinking that a theory, because it is easy and simple, is therefore objectionable. We do not maintain that the doctrine of Ascherson is correct, as he promulgated it, although we feel satisfied that it contains the germs of truth. Assuredly it cannot be reasonably set aside by the author's facts, and still less by his arguments.

Dr Glover, however, argues that—

"We find a large portion of fats in tubercles: in one analysis, conducted on a sufficient scale, almost one-fourth of the tubercular matter was found composed of fats! And this result is not much in discordance with other analyses, and with analyses by other persons. If, then, the diseased matter be expelled by a supposed effort of the vis medicatrix, it would appear as if the sanative materials were also expelled."—P. 115. "Again, we by no means find a uniform deficiency of fat in scrofulous persons. We have two females (sisters) now under our care, thoroughly scrofulous. One sister was treated by us a year ago for scrofula of the neck; symptoms of phthisis came on; the scrofulous glands disappeared, but the phthisical symptoms continued. The other sister has now scrofula of the neck, and is the case referred to in No. 18 of the analyses just given. Both sisters were plump, and the latter aged nineteen, weighs a pound less than nine stones, being five feet four and a half inches high. We remember examining the leg of the person referred to in analysis 14 of the preceding, with our friend Mr Potter, who had removed the limb, and we were both struck with the large quantity of firm fat beneath the skin. This person had been almost constantly confined to bed for nine months; and the leg was removed in consequence of the discharge of blood which daily took place from a diseased surface; yet there was no deficiency of fat. Some have asserted that the fat in scrofula is different from ordinary healthy fat, but this is probably the most pure conjecture. When the disease has continued long, we must, of course, in general expect deficiency of fat; but this characteristic of chronic diseases seems rather absent than present in scrofula."—Pp. 115, 117.

To these arguments we think it may be fairly opposed, that the existence of a certain quantity of fat in tubercle, proves nothing. Fat exists in every organized tissue and product, and according to the theory of Ascherson himself, tubercle could not present the imperfect organization it does, unless it contained this element. The question ought to be, is there a greater or less amount of fat in tubercle than in lymph, pus, cancer, or other exudation? It is not whether fatty matters be present, but whether they are in suffi-
cient quantity to constitute perfect formations. We know of no analyses sufficiently extensive to decide this point. Again, those who are favourable to the views of Ascherson, are well aware that scrofula is often present in plump persons. Thus Dr Hughes Bennett, who was the first to make these opinions known in this country, and to whose work the author more than once alludes, describes two forms of scrofula. In the one he states the individuals are fat and well nourished, in the other are thin and cachectic. He further points out that cod liver oil is not adapted to the first class, and that experience has shown that it is in the latter its good effects have been best exhibited; healthy local accumulations of fat whether in the leg, or in the liver, prove nothing as to the existence of a proper proportion of this principle in the chyle or blood, so as to enable the latter to form the tissues, and a simple inflammatory exudation.

In pursuance of this line of argument, the author does not agree with many German writers when they say the stomach is commonly deranged in scrofula, and that there is unusual acidity of the primæ viae. But here he deals in nothing but assertion, whilst we will undertake to say that no accurate observer of the course of the disease will fail to be struck with the remarkable derangement of the digestive powers which accompanies it from first to last.

Dr Glover has made several analyses of the urine, one in nine cases which came under his observation. He has not, however, favoured us with any results derived from these investigations. In two of the cases in which the urine was analysed before and after the commencement of a treatment by iodine, it was ascertained that the amount of urea was greatly augmented in the latter analyses. If iodine really have the effect of increasing the amount of urea, and the secondary digestion of the tissues, which from previous observations of the author we gather to be his opinion, we must consider it a very important fact. This subject, however, is more fully discussed in the second part of the work.

The third chapter is on the scrofulous diathesis, in which the author states his belief that the only positive sign of scrofula is the existence of tubercle, and that there is no necessary connection between tubercular diseases and a peculiar habitus or facies. In this opinion we entirely concur.

We pass over the fourth chapter on the comparative pathology of scrofula, in order to arrive at the fifth, which treats of the identity of scrofulous and tubercular diseases. Here the author expresses his conviction of the essential resemblance of both diseased processes. He shows that microscopic examination and chemical analysis fail to distinguish any difference between tubercular and scrofulous deposits, and ably combats the arguments advanced in support of the opposite doctrine.

The sixth chapter is on the essential nature of scrofula, and the following is the author's view of the subject:
"Scrofula is (speaking of the actually diseased process, not of the diathesis which has been elsewhere described) a peculiar modification of inflammation, whereby the usual, or, as they may be termed, the normal products of this process are not evolved, but instead of them other materials, incapable of passing into the regular cell forms, and which constitute the substance already described, under the name of scrofulous or tuberculous matter. The peculiarity of this formation, and the continuance of the scrofulous diathesis, are the causes of the characters assumed by the various after processes which result from the existence of tubercle."

Although this view of the nature of scrofula does not present us with anything new, we believe it to be essentially correct. Indeed we believe that this chapter presents us with one of the best accounts of the pathology of scrofula which has been published, and we strongly recommend its careful perusal to our readers.

Passing over chapters seventh and eighth, on the etiology and complications of scrofula, we arrive at the second part of the work, in which the treatment of scrofula is considered. The subject is introduced by some very judicious observations on the general principles which should guide the treatment. In most of these we perfectly agree, especially in the repudiation of the absurd proposition of M. Lugol, namely, the legal prohibition of marriage in the case of scrofulous people. The author, however, again seizes the opportunity of denying that the symptoms are in any way to be ascribed to a deranged digestion, and once more opposes the theory of Ascherson, on the sole ground that it is "too mechanical." As we have already endeavoured to show that the author's opinions on this subject are not founded on fact, we need not again attempt to show their fallacy. He observes that "the action of cod liver oil is, in all probability, as a tonic, from the resinous principle which it contains." (p. 243.) This doctrine was maintained by Falkner in 1840, but we do not think that it is at all likely to be the true one.

In chapter tenth, Dr Glover treats of various remedies which have been used in scrofula. These we shall notice in the order he has placed them.

*Digitalis, Walnut Leaves, Vegetable Tonics.*—Although the diuretic properties of digitalis may be useful in scrofula, Dr Glover correctly states that its use is combined with so many inconveniences as to justify its abandonment. He regards the evidence adduced by M. Negrier, in support of the virtues of walnut leaves, as of very doubtful character. The treatment was, in many of the cases, of such long duration, and the apparent physiological operations of the remedy so obscure, that he does not believe it a substance of any great power. There is scarcely a vegetable tonic which may not be used in scrofula, but he considers this class of remedies most useful in the intervals of the iodine treatment.

*Chlorine, Bromine, Iodine.*—The author points out that there is a strong analogy between the physiological and medicinal proper-

NEW SERIES.—NO. VI. DEC. 1846.
ties of chlorine, bromine, and iodine, but that the different forms in which we are obliged to use them, give rise to difference in action. Thus iodine being only slightly soluble cannot be used in solution, whereas the convenience with which solutions of bromine can be prepared, renders this body peculiarly adapted to form lotions for external application. It forms an easily prepared, elegant, and cleanly lotion; 8 or 12 minims of bromine being used to a pint, half a pint, or 8 oz. of water. Reasoning analogically, and from experiments on animals, the author thinks it probable that chlorine is the most active, bromine the next, and iodine the least potent physiologically. Of the therapeutic effects of chlorine in scrofula, however, he says nothing, and it is only stated of bromine, that the lotion just noticed has been found useful as an external application.

With respect to iodine, Dr Glover observes—

"Great discussion has arisen as to whether iodine should be given in small or in large doses. Baudelocque, Lugol, and Tyler Smith support the administration of small doses, while Dr Buchanan has been the chief advocate of the system of large doses. But, in fact, scarcely any one administers the iodine pure in one way or another. It is the iodurered solution of the hydriodate of potass, or the compound iodine tincture, which is always given. We prefer a medium system. We always give iodine in the form of the compound tincture of the London College, or a simple solution of hydriodate of potass. We commence with adults, by giving 25 drops of the compound tincture thrice a-day, which we augment gradually to 30 or 40 drops if the patient can bear it; but in general, when the dose becomes above 35 drops, nausea, pains in the stomach, and sometimes vomiting and purging are occasioned, in which case the dose is immediately reduced. When the patient can bear a good dose without the remedy disagreeing with him, in the way of its primary action, we do not find any inconvenience from its secondary symptoms; but we do not give iodine in a very cachectic habit, preferring in such cases the use of iodide of potassium alone, or the syrup of the iodide of iron, or cod liver oil.

"The compound tincture of iodine, given in this way, improves the appetite, acts as a general tonic, and increases powerfully the quantity of urine, and also, according to our experiments, the amount of solids and of urea. Some of the facts already recorded in the second chapter will support these conclusions. All authorities agree with regard to the increase of urine, and the existence of iodine in this fluid in a combined form, when the element or any of its compounds is given internally."—Pp. 251–3.

Dr Glover now describes the results of a series of observations made on the chemical constituents of the urine, in an individual aged twenty-four, extremely healthy, and regular in all his habits. Analyses are given of the urine both before and after taking iodine; but on comparing these, we cannot observe that the data agree with the author's conclusions, either as regards the solids or the urea. Thus, in the three analyses made before the iodine treatment, the solids amounted on the first, second, and third days respectively to 52, 34, and 50·5 grains; and the urea to 13·50, 9·50, 14·30 grains. The two analyses made after the iodine treatment give 46 and 48·4 grains of solids, and 13·40 and 18·00 of urea. Here, then, the solids cannot be said to be augmented, although
the urea is but in the last analysis only. Dr Glover himself in this place tells us that "further investigation of the subject is very desirable, and may possibly show that this action on the urine is not always an accompaniment of the use of iodine. Indeed, the substance will not produce the same effects on all individuals," &c.—(p. 256). A close analysis of the author's facts, therefore, does not in any way satisfy us that his favourite theory regarding the action of iodine is established. Besides, if the good effects of iodine in scrofula are dependent upon its power of increasing the discharge of urea, other medicines, as colchicum, for example, which possess this property in a high degree, ought also to be beneficial in this disease.

The author concludes his observations on this subject with the following theoretical remarks, concerning which our readers will, from what we have said, be able to make their own comments:

"When we consider the probable connexion of the secondary digestion of the tissues of which the principles of the urine were the chief results, with the state of the blood and the respiration, we may understand the important part, which the use of a remedy like iodine may play in the treatment of such a disease as scrofula: 1st. In quickening the powers of absorption and getting rid of the effused albumen, where this is not in such a form as to preclude all action of the kind; and, 2d, in removing the excess of albuminous substance in the blood. Again, we deem it by no means an improbable supposition that the chief seats of the formation of urea, may be in the lymphatic glands of the general system. This substance is not formed in the kidneys, as we know by the experiment of Prevost and Dumas. Now, is it not very probable that the lymphatic glands may play such a part on fluids absorbed from the digestion of the tissues, as there is reason to attribute to those of the mesentery and others in the course of the chyle, upon this fluid?"—P. 257.

Alkaline and Earthy Oxides and Salts.—The following observations are valuable in a practical point of view.

"The chloride of potassium is not used, so far as we are aware, in the treatment of scrofula; but might, in all probability, be given now, with advantage, when the iodide is so dear. The experiments, which we formerly published, prove this substance to be much more energetic than the corresponding compound of sodium, although not in accordance with the terms of our general law, so powerful as the iodide of potassium. There is scarcely a doubt but that the chlorides, bromides, and iodides of the same basis produce effects most identically similar in kind, differing only in degree.

"The bromide of potassium is, in accordance with the same law, more powerful than the chloride, less active than the iodide. Not being so apt to occasion nausea as the latter substance, it may be used in cases where this might disagree; and our researches show with similar physiological and medicinal effects. As far as our observations, made since the publication of the paper referred to, have gone, they corroborate these former remarks.

"The iodide is, as far as our inquiries go, best given in doses of three or four to eight grains, in solution, three times a day to adults. We generally begin by dissolving a drachm in solution, in 8 oz. of water; sometimes we dissolve four scruples, or five or six scruples in the same quantity of water; and give a tablespoonful of the solution thrice a day: cases occur in which the hydriodate of potass can be borne where the compound tincture disagrees.

"The effects of hydriodate of potass are principally manifested, 1st, as a
tonic; 2d, by increasing the quantity of urine; 3d, sometimes by acting as a purgative. An increased tendency to perspire, is also a common symptom, as with the iodine itself. The salt is readily absorbed, and may be detected in the various excretions, by the well-known test for iodine. It is, probably, as has been already stated, partially decomposed in the system."—Pp. 259-260.

The author further observes that the same general law warrants the belief, that chloride of sodium possesses nearly the same properties as the iodide of potassium. In many mineral waters found beneficial in scrofula, the active ingredient is, in all probability, the common salt which exists in large quantity.

Metallic Preparations.—As regards antimony, Dr Glover tells us that he has obtained beneficial results in the treatment of impetigo, in scrofulous subjects, by combining the exhibition of mercury and antimony. His general plan is to give three grains of the hydrarg. c. creta, along with five or six of the golden sulphuret of antimony twice a day.

Of the preparations of mercury, the author enumerates a long list as being beneficial to scrofula. The bromide, he says, may be used with success, giving it in small doses, precisely as if it were corrosive sublimate.

Of the preparations of iron, he considers the iodide the best, which should always be given in the form of the syrup. It is particularly indicated in cases where the menstruation is defective.

Sea Bathing, Mineral Waters, &c.—The admitted use of sea bathing in scrofula, our author attributes to the stimulation of the skin and the circulation. He says further on, that the internal use of sea water is not unlikely to be as capable of curing scrofula as almost any of our therapeutic means. He justly ridicules the idea of the minute quantities of bromine and iodine found in certain mineral springs, having any importance attached to them, and denies the possibility of their existing in them in a free state. He concludes this section, however, by recommending the use of sea bathing and mineral waters, should the circumstances of the patient admit of such means, with which ordinary medicinal treatment may, of course, be combined.

Cod Liver Oil.—Dr Glover observes, that the chief precaution to be employed in selecting the remedy for use, seems to be to take care that the specimen has not been made from stinking livers, or that it is not the common cod oil which is used by carriers, and is made from the refuse of the cod generally. The oil he employed had a fine briny odour, and was taken by several patients without inconvenience, although others rejected it. When taken internally, no odour can be perceived in the secretions and excretions; and in one case in which the urine was analysed during its use, a quantity of oil was obtained from that fluid. He recommends its employment in cachectic cases of scrofula, both because he has seen its good effects, and because its use is now a fair subject of experiment, for which the treatment of this disease frequently furnishes a legitimate field. He adds, in a note, that
patients who take cod liver oil, almost invariably get stouter under its use. On weighing phthisical patients and others who were taking it from time to time, they were sometimes found to grow stouter, even where the disease was unchecked.

We have only one remark to make on this otherwise valuable chapter, namely, that Dr Glover does not sufficiently indicate the class or kind of cases which demand one remedy in preference to another, and that in consequence we have no guide to the application of many of the preparations he has spoken of. This fault, however, is in a great measure counteracted by the details of twenty-three separate cases, which constitute an appendix to the work. Many of these are very valuable in a practical point of view, more especially in reference to the indications which should lead us to the respective administration of iodine and cod liver oil.

The work is illustrated by four plates. Plate 1 represents the appearance of tubercle as seen under the microscope. We have no doubt that these are exact copies of the demonstrations which the artist was directed to draw, but unfortunately not one of them can be considered as exhibiting the characteristic structure of tubercular matter. They represent rather the appearance of broken down or granular tubercle, than of the corpuscles it contains. Fig. 3, representing a slice of lung-tubercle, magnified four hundred times might represent any coagulated albuminous matter, whereas the magnifying power employed in the other demonstrations not being stated, leaves us in doubt whether we have to do with corpuscles as seen under a low, or granules as seen under a high, power. The remaining plates appear to us unnecessary, although the fourth represents a very characteristic specimen of scrofulous disease of the ankle joint, which is admirably executed.

Notwithstanding we have ventured here and there to question some of the author's opinions, we cannot conclude without expressing the great satisfaction we have obtained from its perusal. It is the publication of monographs of this character that tends to the advancement of medicine, and the one we have now noticed, is in itself sufficient to confer honour upon the institution of the Fother-gillian medal. We confidently recommend it to the perusal of our readers.

Quarantine and the Plague, being a Summary of the Report on these subjects recently addressed to the Royal Academy of Medicine in France, with Introductory Observations, extracts from Parliamentary Correspondence and Notes. By GAVIN MILROY, M.D., &c. London, Highley. 8vo, 1846. Pp. 71.

For several years past the conviction of the non-contagious nature of plague has been gaining force and extension; the quarantine
laws have in consequence become more and more oppressive, and the loud complaints of those engaged in commerce, have at length succeeded in rousing from their apathy, the governments of France and England, and directing their attention to the mighty interests involved in this great medical question. In 1844 the government of France, with that enlightened spirit which pervades all its scientific proceedings, called upon the national academy of medicine to direct its attention to this matter. In the August of that year accordingly, a commission composed of twelve men of the highest scientific attainments, was appointed to examine all the varied questions connected with the plague and with quarantines. The report was read in March and May of the present year, and constitutes a work of which the medical profession in France may be well proud. It is rich in facts and data, admirably digested under distinct heads, and contains several conclusions which have been discussed at nine sittings of the academy. The British government, on the other hand, contented itself with despatching Sir William Pym, inspector of quarantines, to the various lazarettos in the Mediterranean to obtain information upon every subject connected with them. It is not to be expected that the researches of one individual, could be undertaken with that freedom from prejudice, and with that acuteness, which was to be expected from a body composed of twelve scientific persons. Certain it is, that the English report will not bear the slightest comparison with the French one. Indeed, Sir William Pym commenced his inquiries apparently with his mind made up as to the contagiousness of the disease, whereas the French commissioners examined into the truth of this fundamental point, and have recorded facts, which to our mind, constitute sufficient proof that this ancient opinion is a mere delusion.

It must be evident that there are three important questions to be decided in connexion with this subject. 1. Is plague contagious, in other words, is the disease capable of being communicated by contact? 2. Is it infectious, that is susceptible of producing from the body an effluvia or miasm, which being diffused in the atmosphere, and inhaled into the lungs of other individuals, occasions the disease? and lastly, How are its extensive ravages to be explained, and what is its real origin? There are many other minor points and considerations well worthy of investigation, but it is to the three leading questions just noticed, that we shall for the present confine our attention.

The following are facts which we think are sufficiently positive and exact, to enable us to answer the first question in the negative,—and first, with regard to inoculation—

"It should be noted as an important fact that, if all the diseases which are indubitably contagious—Small-pox, Hydrophobia, Glanders, and Syphilis, for example—present us with a palpable liquid which contains the poisonous principle, such is certainly not the case with the plague. Hence the medical men
have operated, by turns and almost indifferently, with the pus of a bubo, the serosity of a carbuncle, or even with the blood itself of a pest-patient. In 1835 the effects of inoculation were tested at the Cairo Hospital, in the presence of Gaetani-Bey, Clot-Bey, and Drs Lacheze and Bulard. Five criminals, who had been condemned to death, were the subjects of the experiments. A lancet, wetted with the blood drawn from a pest-patient, was passed under the epidermis on the inside of the arm of one of these criminals, at two different points. On the third day afterwards, the man was affected with confirmed plague—so, at least, says Dr Lacheze, who reports the experiment; Clot-Bey thought the case doubtful. Three days subsequently, the man was convalescent. In three other cases, no effects followed the inoculation of the blood. In two cases the serosity from a carbuncle, and in one the pus from a bubo, was used for the purpose of inoculation; in none of these cases was the disease induced. With respect to the single case, in which the disease (mild indeed) occurred after inoculation with the blood of a pest-patient, it must be kept in mind not only that the man was exposed, as a matter of course, to the epidemic atmospheric influences then existing in Cairo, but also that, for three days before the performance of the experiment, he had been living in a pest-hospital, which was necessarily a focus of pestilential infection.

Clot-Bey inoculated himself, in six different punctures, with the blood of a pest-patient: no constitutional effects followed. A few days subsequently, he inserted some pus from a bubo on the inner part of his left arm: this was followed by a slight indisposition, which he attributed to the absorption of the purulent matter, but which bore no resemblance to the symptoms of plague. The results of certain trials made by Professor Pruner in 1829, and by Dr Rossi in 1841, were altogether similar.

"The general conclusion of the Commissioners upon the important point under consideration is to this effect: 'The results of the inoculation of the blood drawn from the vein of a plague patient, or from the pus of a pestilential bubo, have been equivocal; the inoculation of the serosity taken from the phlyctene of a pestilential carbuncle has never given the disease. It is therefore not proved that the plague can be transmitted by inoculation, even under the influence of a pestilential constitution.'"—Pp. 37-38.

Then as regards direct contact with the sick:

"All the medical men who accompanied the French expedition to Egypt, Assalini alone excepted, were of the opinion that the plague is propagated by contact with the infected. For nearly forty years after their return, this opinion has been universally received and acted upon. It was not till 1835 that a change of sentiment began to be manifested among medical men on this most important subject. In the course of that year, as we have already seen, a number of European physicians had an opportunity of studying the terrible pestilential epidemic that ravaged Egypt. Impressed at first most firmly with the belief of the transmissibility of the disease by contact with the sick, they have all, with scarcely one exception, completely changed their opinion; as, indeed, MM. Brayer and Cholet, who had observed the epidemics of 1819, 1826, and 1834, at Constantinople, had previously done. The writings of these last named gentlemen, and subsequently of Clot Bey and Aubert Roche, have mainly contributed to effect this very remarkable revolution in medical doctrine. We shall briefly note a few of the most interesting facts which have been of late years made public.

"During the pestilence of 1824, upwards of 30,000 persons died in Cairo, while not more than two or three cases occurred in Alexandria, although the communication between these two cities was constant and uninterrupted. In 1834, on the other hand, the plague broke out and continued in Alexandria for a very considerable time, before it made its appearance at Cairo; and it had existed for fully eight months in the former city, before there was any sign of
it in Mansoura and Damietta, although the daily intercourse between these places remained entirely free. Dr Coch, principal physician of the Egyptian fleet, mentions an interesting fact observed by him in 1835. Ten men had gone from Sakkarah, a populous village, to Cairo, where the plague then existed. On their return home, every one of these men sickened and died; yet not a single member of their families, who had assiduously waited upon them, took the disease. 'Such a fact,' it is emphatically added, 'was observed hundreds of times during the course of this great epidemic.' The same gentleman states, that the Viceroy having ordered that all vessels in which the plague appeared should be subjected to a quarantine of eleven days, the sick were immediately disembarked and carried on shore by the sailors of the fleet; and although these sailors returned on board and communicated freely with the rest of the crews, not a single case of infection was the result.

"Every year pilgrims depart from all parts of the country, subject to the laws of Mahomet, to go to Mecca. Caravans from Morocco, Darfour, Egypt, Constantinople, Persia, Asia-Minor, and Syria, converge at Djedda, at Medina, then at Mecca, the central point. They carry merchandise with them, for this pilgrimage is also a fair. Has the plague ever broken out at the place of meeting of all this population and all this merchandise, which have often, be it remembered, come from places infected by it? No. On the contrary, it is proved that, from time immemorial, the plague has never been seen in Arabia. The epidemic plagues which desolated a great part of Lower Egypt in 1825 and 1835, had not one victim in Arabia, notwithstanding the daily and perfectly free communication which existed between these countries. This has also invariably been the case with respect to the pestilential epidemics of Constantinople, Smyrna, or Syria. The Arabian historians pretend that their country owes this immunity to the protection of the Prophet. Nubia, Sennar, and Abyssinia, notwithstanding their close connexion with Egypt, are not acquainted with the plague.

"Clot Bey observes,—During the five months that the epidemic of 1835 lasted, MM. Gaétani, Lacheze, Bulard, and myself at Cairo, MM. Duvigneau, Scisson, Perron, Fischer at Abouz-Abel, and MM. Rigaud and Aubert at Alexandria, visited the infected in the hospitals and in private houses. None of us took the least prophylactic precaution. We were in immediate contact with the sick during all the stages of the disease. We received, upon our clothes and upon our hands, the matter that was rejected by vomiting; the blood of those who were bled, the pus from the thousands of bubos which we opened. More than a hundred dissections were made at Cairo, and we passed whole hours in endeavouring to detect, in the bodies of those who had just expired, the pathological alterations, which had hitherto been so little attended to. The same researches were made with equal care at Alexandria. Dr Rigaud is the only one among us who fell a victim to the reigning epidemic. It is remarkable, that many physicians who scrupulously avoided all contact with the sick, and with suspected objects, were attacked with the plague and died. Of this number are Dr Mannucchi, sen., Leopold and Lardoni."—Pp. 39-41.

The conclusion of the French Commission is, that

"On the one hand, immediate contact with thousands of plague-patients has not been followed by any dangerous consequences to those who have been exposed to it in the open air, or in well ventilated chambers; and on the other, that there is not a single fact which indisputably proves the transmissibility of the plague by mere contact with the sick."—p. 43.

Such being part of the evidence regarding the non-contagious nature of plague, we shall pass over what is said about its being communicable by fomites. There is not one fact which proves that the disease can be spread in this manner.
Our next question is, is plague infectious? The answer of the French commission to this is in the affirmative. It is true that there is great difficulty in determining with exactitude the infectiousness of any disease, while a pestilential constitution of the atmosphere exists, and when consequently a whole population is exposed to the morbid influence. When a vessel, however, carries one or more affected persons beyond the focus of infection, she cannot take along with them all the causes, past and present, which are necessary to the development of an epidemic. If, then, infected vessels have carried the disease into previously healthy ports, and surgeons or other attendants upon the sick have caught the disorder without any other appreciable cause, we are not warranted in the present state of our knowledge in denying its infectious nature. That such has frequently been the case will appear from the following statement:

"Since the year 1720 down to the present period, 25 vessels having the plague on board, have arrived in the ports of France or Italy; 10 at Marseilles, 5 at Venice, 8 at Leghorn, and one at Genoa. We shall confine our remarks to the circumstances connected with the arrivals at Marseilles, the official documentary evidence upon these being much more complete than in the other cases. The years in which these arrivals occurred are in 1741, 1760, 1784, 1785, 1786 (bis), 1796, 1819, 1825, and 1837. The entire number of cases of plague (omitting all the doubtful ones), treated in the lazaretto of this port since 1720, is 32; and of these, 18 have proved fatal. Three of the quarantine surgeons caught the disease during their attendance on the infected; they all recovered. A fourth surgeon, who had arrived on board an infected ship, and subsequently acted in his professional capacity in the lazaretto, died. Four of the health-guards, who had been (most improperly) put on board infected ships, contracted the disease in the lazaretto; two died. A sailor, who acted as assistant in the lazaretto infirmary, was taken ill and died. Two other sailors, belonging to an infected vessel, but who seemed to have caught the disease in the lazaretto where they had been confined for more than 12 days, died. In the 11 cases therefore of plague, which might have been contracted in the lazaretto, 6 of the patients recovered, and 5 died: all the latter cases occurred in men who had been on board infected vessels. Of the three health-guards, who had caught the disease on board, only one recovered. Indeed it would seem that, in all the fatal cases, the patients had been for a longer or shorter period of time on board infected vessels. It appears, also, that not one of the cases, which occurred on board a vessel at sea during the voyage to France, recovered;—a circumstance that very emphatically shews the malignancy of the disease when it occurs in a crowded confined space, and the great advantages of treating it in a large open lazaretto."—P. 51.

It would seem from all the facts recorded, that as regards infection, plague, in its mode of propagation, resembles the typhus of our large towns; and, like them, is capable of being mitigated or entirely removed by hygienic regulations. This conclusion is borne out by numerous other facts, among which the following are very striking:

"In 1834, in the month of June, during the insurrection which broke out in Judea, the insurgents pillaged and sacked Jerusalem. A number of Roman Catholics took refuge in the convent of St Saviour in this city. "At the end of ten or twelve days of close confinement, I remarked," says
M. Delong, "cases of plague among this distressed population, huddled together in their dormitories, upon and under the stairs, in the courts and other chambers of this vast building. After twenty-five days of expectation, Ibrahim Pacha at length arrived, and the city was relieved. The holy Fathers, full of alarm, hastened to clear their dwelling of all this mass of people, and shut themselves up in most strict quarantine. What happened? Of all those who left the convent three only died four or five days afterwards. But, out of 63 priests, who thought to save themselves by isolation, no fewer than 22 died."

"What occurred in the musical academy at Kanke in 1835 is still more deserving of attention:—The plague having broken out in this school, although it was kept in the strictest quarantine, the pupils were sent into the desert, where they continued for upwards of a month. In the mean time, all the rooms were well cleansed and purified; and no person had remained in the building. Not one case of plague occurred in the desert; but no sooner had the boys returned to their old quarters, than several were taken ill; and each day several fresh cases were reported. Again were the boys sent into the desert; and again the disease ceased to spread. While they continued in the desert, 15 soldiers were employed to go daily to the village, where the plague was raging, for provisions; but none of these men caught the disease themselves, or gave it to the boys."—Pp. 46, 47.

"Dr Mead mentions that at Rome, during the plague of 1657, Cardinal Gastaldy prohibited any infected person, and even any person in health who was suspected, to remain in their houses. They were promptly taken to the hospital, built on the island which divides the Tiber. With respect to those who had occupied the same house, they were placed in other hospitals near the city, from whence they were removed into the island if the disease showed itself. During this time, the Cardinal was very careful to have all the furniture taken out of the infected houses, exposed in the open air, and the apartments left open, in order to purify them. By these means the Cardinal, in two months, caused the plague to cease, after it had raged at Rome for two years.

"But that which deserves most attention, adds Dr Mead, is, that, before these regulations, it was constantly observed that the disease rarely appeared in a house without attacking all its inhabitants; whereas, after they had been put in force, scarcely five out of a hundred of those who were removed from the proximity of the infected, were subsequently attacked with plague.—Mead on Pestilential Contagion, 1720.

"The Board of Health at Constantinople has, for the last eight or nine years, followed out the prophylactic method recommended by Gastaldy and Mead, removing the infected to a hospital, and emptying every house, in which a case occurs, alike of its inhabitants and furniture, having it well cleansed and purified, and not allowing any one to occupy it for the space of a month. It is to the adoption of these means that the Board attributes the exemption of Constantinople and the principal ports in Turkey from the plague, since the year 1839. If, in place of acting in this manner, the houses of the infected were condemned with their inmates to a severe quarantine, the result would necessarily be to create fresh foci of pestilential infection, and thus increase the very evil that is vainly sought to be extinguished."—P. 49.

These facts require no comment. The last point to which we shall direct our attention is, how are the extensive ravages of plague to be explained, and what is its real origin? In endeavouring to answer this question, it must be observed in the first place, that although plague may arise spontaneously in a number of different localities, it is no doubt true that in recent times, Egypt, Syria, and Constantinople—more especially the first—have been the principal foci of the disease. This leads us to inquire whether there are any circumstances peculiar to these places, which may
rationally be considered causes of the disorder. The following are the observations of a gentleman, who long resided in Egypt:—

"The inhabitant of the Delta, says M. Hamont (Destruction de la peste et des quarantaines. Bulletin de l'Academie Royale de Medicine. Paris: 1844, t. x. p. 40), has prepared the causes of his own destruction. The destitution, filth, and misery of the poor inhabitants are extreme. Their wretched hovels are so horribly disgusting as almost to defy description; they are not only surrounded by, but are actually receptacles of, heaps of ordure and putrid matters. Not unfrequently the dead are buried immediately under the mud floors of these dwellings of the living; and many of the graves in the cemeteries (which are always within the villages), being left open, are continually exhal- ing a stench that is utterly intolerable to any stranger. Then, again, the food of the Fellah is always of the worst description, and often too of the most scanty supply. Rotten cheese, decayed vegetables, semi-putrid flesh or fish; such are the articles that he lives upon. The very water that he drinks is filthy and impure. And then think of his mental and moral condition; the brutal degradation of all his faculties and affections, his hopeless servitude, his blank unmitigated wretchedness.

"The hygienic state of the cities and larger towns in Egypt is not much better than that of the villages. Cairo, with its 200,000 inhabitants, is a very hot-bed of the most disgusting and pestiferous impurities. From the canal, which traverses it, there is constantly steaming forth a cloud of intolerable offensiveness; and yet this is the supply of water for the use of its people! There are no fewer than 35 cemeteries, of which 25 are within its walls. In the Copt quarter of the town, the dead are buried under the floors of the houses; and nothing but a few boards separate the living from the putrid bodies of the deceased. From 80 to 90 corpses have been known to be huddled together in these horrible sub-domal receptacles. Can we therefore wonder that Cairo should be a generating focus of pestilential disease?"—Pp. 22, 23.

The neighbourhood of the Euphrates, in Syria, and that of the Danube, in Turkey, present the same endemic causes of insalubrity:

"The poorer classes in Moldavia and Wallachia live in the greatest misery and filth. After the heats of summer, almost all the prevailing diseases assume a character of marked gravity. Malignant intermittent fevers are always more or less prevalent in autumn; these generally precede the appearance of the plague, which in these countries is usually only sporadic."—L. 24.

"When Dupuytren inquired of the young Egyptian students, who had been brought by Clot-Bey to Paris for medical education, what was the opinion of the most enlightened men in Egypt respecting the origin of the Plague, the answer they gave was, 'la peste vient de la terre.' All that is conveyed by such an expression is merely that a humid and marshy soil, more or less covered with decaying vegetable and animal matters, is a powerful cause of the alteration of the atmosphere, and consequently of the disease. Now nothing can better serve to show the importance of the conditions of the soil, in reference to the production of the plague, than the comparing together of two localities in the same country, inhabited by the same people, and governed by the same laws and customs, in one of which the disease is endemic, while the other remains entirely exempt from its attacks, even although the infected may die within its walls.

"Fayoum is elevated above the level of the sea: Damietta borders upon the shore. At Damietta, the air is hot and damp; at Fayoum, it is hot, but dry. Fayoum is free from marshes; Damietta is surrounded with ponds of fresh and salt water. While at Damietta the cemeteries are in the town itself; at Fayoum, they are at a distance from the dwellings. Here, the water, although not very pure, may be drunk without inconvenience, owing to the quantity of nitre it contains; at Damietta, the fresh water is either mixed with sea-water, or it is rendered impure by excrementitious products, and by animal and vege-
table matter in a state of putrefaction. Fayoum is surrounded by the desert of Lybia; Damietta is enclosed by rice-fields, and situated in front of the pestiferous Delta."—P. 81.

These and like facts and considerations have led the French commission to the following conclusions:—

"In all countries where the spontaneous plague has been observed, its development may be reasonably attributed to certain determinate conditions acting upon a large portion of the inhabitants. The principal of these conditions are, residence upon marshy alluvial soils near the Mediterranean or near certain rivers, as the Nile, Euphrates, and Danube; the dwellings being low, crowded, and badly ventilated; a warm moist atmosphere; the action of putrescent animal and vegetable matters, unwholesome and insufficient food, and great physical and moral wretchedness."

"All the producing causes of the plague being found united in Lower Egypt, the disease is endemic in that country, where it is seen every year in the sporadic, and about every tenth year in the epidemic, form.

"Whenever the plague has raged with violence in Africa, Asia, and Europe, it has always exhibited the principal characters of epidemic diseases."

The ravages of plague, therefore, are principally dependant on local causes, and the essential nature of this disease is endemic. It seems to us that no other conclusion can be derived from the facts above narrated, than that the quarantine laws ought to be abolished, and that our efforts to prevent plague, instead of being carried on at the ports of healthy countries, should commence in the districts where it originates. To this end, the influence of our Government should be directed, to impress upon eastern European nations the importance of good hygienic regulations; the necessity of pure air and cleanliness, of appropriate diet, and an improvement in all those circumstances which tend to elevate the social position of man. By so doing, there is every reason to hope that this fearful pestilence, if it could not be altogether annihilated, would be so far lessened in intensity, as to be rendered comparatively innocuous. The panic and dread with which it is even now regarded by the ignorant, would then be abolished, and thus, probably, another important cause of its production removed. Lastly, the free intercourse which would exist among commercial nations, would react favourably upon the inhabitants of Egypt, Syria, and Moldavia; the arts of civilization would teach them to drain their pestiferous swamps, and reap copious harvests and the means of supporting life, from those lands, which at present tend only to originate and diffuse desolation and death.

The pamphlet of Dr Milroy, which has furnished us with the facts embodied in this article, is very ably executed, and we strongly recommend it to the members of the profession, who, from the position they hold in society, may be expected to be consulted on this subject. Lastly, as the quarantine laws are to be brought under the consideration of Parliament next session, it is worthy of the perusal of every legislator who is anxious to make himself acquainted with the important facts of the case, divested of circumlocution, and condensed into a readable form.
Lectures and Observations on Clinical Surgery, by ANDREW ELLIS, Fellow of the Royal College of Surgeons of Ireland, &c., &c. Dublin: Fannin and Co. 1846.

This work contains the substance of a series of clinical lectures, delivered by Mr Ellis at the Jervis Street hospital, Dublin. It is one of a class which we consider eminently useful, as serving to extend the benefits of hospital experience to the profession generally; and we think it is matter of regret that in this country so few lectures on clinical surgery are published in a collected form such as this, to which surgeons might refer for practical suggestions in troublesome or doubtful cases.

The plan and execution of the work are exceedingly good. Mr Ellis seems to have been well supplied with important cases, and has spared no pains to make his discourses both interesting and instructive. These lectures contain much valuable matter; but as our limits prevent us making lengthened extracts, we must confine our remarks to the few points of our author's practice which we consider objectionable.

In reading the cases of suicidal wounds of the throat, we notice that immediately after describing the nature and circumstances of the wound, Mr Ellis proceeds to mention the closure of the divided parts by sutures supported by compresses of lint and adhesive plaster. Now, if (as we are led to infer from the manner in which the cases are detailed) Mr E. advocates immediate closure of the wound, we consider the practice as at once dangerous and useless. Dangerous, because under any circumstances there is risk to be apprehended from swelling of the divided parts from infiltration, and this is necessarily increased by stitching the wound closely. Besides, after the active hemorrhage has been arrested, there is always more or less oozing of blood, which, if it does not escape readily by the wound, is apt to trickle down the air passages, and may prove fatal by suffocation. Mr Liston relates a case in his work on Practical Surgery where the patient, except for his timely aid, would have been suffocated from the pressure caused by confined coagula, although the air passages had not been opened into. The addition of compresses and plasters must of course add to the danger, on the interruption they cause to the breathing and circulation. And the practice is useless, because the constant separation of the deeper seated parts of the wound, caused by the slightest motion of the head, by attempts to swallow or cough, together with the passage of air and mucus between the divided surfaces, all render immediate union of such wounds impossible. Sewing up the wound, then, can only serve to render the appearance of the unfortunate patient less frightful, whilst it greatly increases his real danger. The insertion of a single point of suture near each end of the incision, in extensive transverse wounds, for the purpose of
diminishing the exposed surface, is not liable, of course, to these objections.

In the details of an operation for securing the carotid, mention is made of a practice which we thought had been dismissed from modern surgery—we mean the use of a blunt instrument for opening the sheath of the vessel preparatory to passing the ligature. "The sheath was now cautiously opened with a blunt silver knife (a fruit-knife)." Now the degree of force necessary to tear open the cellular sheath with such an instrument is very injurious, as tending to destroy to a greater extent than necessary the vascular connections between the artery and its sheath, thus increasing the risk of secondary hemorrhage; whilst the small portion that does require to be prepared for the ligature can never be so fairly cleared as when a sharp instrument is used. For these reasons we have always looked unfavourably on blunt instruments in general; whilst, perhaps, the economical feelings characteristic of our country incline us to regard silver fruit-knives as articles of luxury which might safely be dispensed with in surgical armamentaria. In the author's remarks on this case, we are at a loss to understand why he should think "it might be fairly supposed" that the symptoms of hiccough could have depended on some injury done to the phrenic nerve during the operation. We cannot "suppose" the possibility of injuring the phrenic nerve in an operation for tying the common carotid artery.

Some of the expressions which Mr Ellis uses in laying down his rules regarding cases that demand the operation of trepan, are scarcely definite enough; especially in rules 5, 6, and 7, the expression "bad constitutional symptoms" is too vague, and might lead to practice very different from what he means to inculcate, if we may judge from his excellent remarks in the concluding part of the fifth lecture.

In conclusion, we would again recommend this book to the profession as a useful and instructive work; the chapter on injuries of the head, and wounds of the abdomen, will be found particularly interesting to the practical surgeon, and worthy his perusal.

The Microscopic Anatomy of the Human Body in Health and Disease. Illustrated by numerous drawings in colour. Parts I. II. and III. By Arthur Hill Hassall, &c. London: Highley, 1846.

What are we to understand by the term, Microscopic Anatomy? Are we in future to make divisions in a science according to the instruments employed in its prosecution? If so, why not speak of a scalpel and forceps anatomy? If we applied this rule to the other sciences we think the effect would be very curious. We
might then have an ocular and a telescopic astronomy, a spade and
a plough agriculture, a retort and a blow-pipe chemistry; or, to
draw illustration from branches of our own profession, we could
speak of a stethoscopic medicine, a knife and saw surgery, and a
lever and forceps midwifery. We need not say that all such dis-
tinctions are unscientific and absurd, and that we regret our
literature should be tainted with works based on such frivolous
distinctions.

It will be seen from the following statement in a notice attached
to the second number, that this work is put forth with no small
pretensions:

“When the paucity and incomplete nature of the works which have
hitherto appeared on this subject are remembered, as well as the numberless
papers and memoirs on special portions of it lying scattered through various
publications, many of them rare and difficult of access, and the results of
which papers too often contradict each other, its necessity will be unhesitatingly
admitted.

“The design of the work, if not altogether original, is almost unique, the
only work extant which embraces the entire range of microscopic anatomy,
and this not exclusively human, is that of Mandl.”

Now, it is true, that with this one exception, works on “micro-
scopic” arts or sciences are very rare. The reason of this is, we
presume, that they have no existence, and that the cultivators of
general anatomy and pathology employ every instrument and
method of research, simply as means to an end. When, however,
as is stated above, it is endeavoured to be maintained that works,
on the subject treated by the author, are few and incomplete, or
that the design of the one now before us is unique, we feel bound
to observe that the assertion is incorrect. The Treatises of Henle,
and Todd and Bowman on General Anatomy, and of Müller and
Wagner on Physiology, certainly are not designated “Microsco-
ic,” neither did Vogel entitle his book “Microscopic Pathology.”
Such men could not have employed terms so ridiculous. But as
regards the nature of the subject we can distinguish no difference
between the so-called “Microscopic Anatomy” of Mr Hassall,
and the general or physiological anatomy of other writers. A
glance at the plates are sufficient to prove that it is in no way
more “human,” in short, that it differs from them only in name,
and that is a bad one.

It is said, however that—

“Further, one great feature of the book, it is hoped, will be the fidelity of
the drawings; a principal fault in most of the published works on minute and
microscopic anatomy being that the figures do not exhibit the characters
described in the text.”

Here it may be asked to what published works on microscopic
anatomy does this passage allude. We know only of one, that by
M. Mandl, and his figures are very exact. But we are willing to
confess that the illustrations given by Mr Hassall are sufficiently
characteristic. They are representations which do not bear the stamp of high art, on the contrary, some of them are very coarse, still they are as mere drawings true to nature, and as we have said, characteristic enough for the purpose of illustration. But here again we must find fault with the colour. In the first number the blood corpuscles are represented of a beautiful rose pink—in the second they are a lively red, whilst in the third they have degenerated into a kind of dirty brown. If Mr Hassall will study the blood corpuscles attentively by day light, he will find that they are of a yellow colour, and not pink, red, or brown, as he has erroneously represented them.

We seriously advise the author to change the title of his production. Why not call it on the structural or general anatomy of man, or more scientifically on the histology of man. We would further suggest that the colouring of the lithographs be rendered less gaudy, and more natural. If our hints be attended to, we may on the completion of the work notice the contents more at length. To do so at present would be unjust to the author, and useless to our readers.

---

**Liebige's Question to Mulder tested by Morality and Science.** By Dr G. T. Mulder, Professor of Chemistry in the University of Utrecht. Translated by Dr P. F. H. Fromberg. London and Edinburgh. 1846.

At length the rumours of approaching war which have long been current in relation to the great chemists of Giessen and Utrecht have found fulfilment in actual hostilities commencing; and appropriately enough, the *casus belli* is that same sulphur which has so much to do with all modern wars. A great sulphur question brought Britain and Sicily to the verge of actual battle a few years ago, but happily one of the belligerent powers, was so notoriously the better fighter of the two, that the other came to terms before hostilities began. Unfortunately in this minor sulphur question, Utrecht and Giessen are so well matched, that neither thought of compromise, and already the first round has been fought of an unhappy duel, which will vex the spirits, ruffle the tempers, and embitter the affections, besides wasting the time of the two combatants, long after the original ground of quarrel has been satisfactorily disposed of, and the science of the matter in dispute, for ever settled.

Liebig and Mülder are at issue as to two points. 1st, Is there such a substance as the latter calls protein? 2d, Does protein contain sulphur? They form in truth, however, but one question. Does Mülder's protein contain sulphur? If it do, it does not deserve its name from the Greek πρωτος, first, as being
the organic basis or primary constituent of the azotised tissues. In short, it is not proteine. Liebig would probably be inclined at present to derive it from Proteus the changeable.

The following extract from Professor Johnston’s excellent preface will more fully indicate the point in dispute:

"The main point assailed by Liebig will be understood by the following statement:

"Mulder, many years ago, examined the fibrin of blood, the albumen or white of the egg, and the gluten of wheat. When these substances are dissolved in caustic potash, with the requisite precautions, and the solution is then made slightly acid by the addition of vinegar, a white precipitate falls, which Mulder collected and carefully analysed. To this substance, for certain theoretical reasons, he gave the name of protein. It was free from sulphur and phosphorus, —both of which are contained in the albumen of the egg and the fibrin of the blood. It consisted of carbon, hydrogen, nitrogen, and oxygen only, and was represented by him by the formula

\[
\text{C}_{40} \text{H}_{31} \text{N}_5 \text{O}_{12}
\]

By degrees, as his researches multiplied, and were enlarged and confirmed by others, his own views extended, and he arrived at the following general conclusions:

"1 mo, That this protein formed the basis of a large group of animal substances —the albuminous group—comprising fibrin, albumen, casein, the crystalline lens of the eye, hair, horn, &c.

"2 do, That in these substances the protein was combined with oxygen, sulphur, or phosphorus, or with two of these bodies, or with all the three,—and that the proportions of these several elements determined the special qualities of each compound of the albuminous group.

"3 to, That the sap and leaves, but especially the seeds, of plants, contained protein in combination with sulphur and phosphorus, as it is found in the animal body,—and that the gluten of wheat, the legumen of the bean, and the nitrogenous substances generally, which are found in the seeds of plants, were compounds of this kind. Lastly, he ventured to announce in a brief manner,

"4 do, That these substances were formed by the plant out of the food drawn by its several parts from the air and from the soil,—that it produced them for the purpose of diminishing the digestive labour, so to speak, of the animal —of supplying it with food fitted directly to form and nourish its muscular and albuminous parts—and that the animal derived its whole supply of the raw material out of which these parts were to be built up, from the vegetable food on which it lived.

"This beautiful train of research and reasoning naturally attracted the attention of scientific physiologists, and gave to protein an importance in the history of Organic Chemistry to which scarcely any other known body can lay claim.

"It is a remarkable fact, that up to January last, the researches of all other chemists only tended to confirm Mulder’s experimental results, and to strengthen and widen his deductions. The laboratories of Paris, of Giessen, and of Stockholm, had been employed upon them at intervals for several years, and yet the existence and characters of protein, as described by Mr Mulder, were only everywhere confirmed.

"But in January last, Liebig announced, in his Annalen, that he could no longer obtain protein possessing the composition and properties assigned to it by Mulder. He intimated his belief that the so-called protein always contained sulphur,—that without sulphur it could not exist,—and that, when perfectly freed from sulphur by the action of caustic alkalies, it ceased to possess the characters or composition of Mulder’s protein. He threw doubts, in like man...

NEW SERIES.—NO. VI. DEC. 1846.

F F
ner, upon the existence of the so-called oxides of protein, and invited Mulder to explain away the difficulties which he now professed to meet with.

"Subsequent to this, in the May number of the Annalen, a paper appeared by Dr Laskowski, one of Liebig's pupils, detailing the experiments to which Liebig had previously alluded, and drawing similar conclusions. Two other short memoirs in the same journal, and by pupils of the Giessen School, showed that the quantity of sulphur previously assigned to albumen and fibrin by Mulder was too low; a fact which the newer researches of Mulder himself has established, but which does not at all affect the existence of protein, or the value of the so-called protein theory."

The pamphlet before us is occupied in showing that if Mulder's directions be followed, an unsulphureted product is obtained, identical with what he described in his original papers as proteine; and in explaining how others have in some cases procured results differing from his.

One would conceive that two chemists might have quietly enough discussed a problem so unexciting, as, does a certain substance contain sulphur, or does it not? without finding it necessary to indulge in taunts or evil speaking on either side. It has, however, seemed otherwise to the Dutch and German chemists. According to Mulder, the first offence was given by Liebig in his paper in the Annalen der Chemie for January, 1846, p. 132, 133. In this, he tauntingly begs that it may please Mulder to mention with every possible detail in what way he obtained protein, and affirms that he (Liebig) never has been able to prepare it with the properties described by Mulder. The Utrecht chemist appears to consider this query as containing an implication against not only his scientific accuracy, but also his moral honesty. Moreover, he reproaches Liebig with duplicity, in as much as the latter formerly professed, through his pupil Scheerer, to have confirmed all Mulder's analyses of proteine, and now he declares, that he never succeeded in making it. The motive which Mulder supposes to have influenced Liebig in acting thus, was indignation at the former for teaching views different from Liebig's, in his work on physiological chemistry.

"Freedom of scientific opinion has never been understood by Liebig. For years past a tribunal has been established in Giessen, before which Liebig is at the same time accuser, witness, public prosecutor, advocate, and judge. Before this tribunal a case is rapidly terminated, but grace or justice can never, never, be obtained there. From this tribunal even the purest innocence is never dismissed without being whipped and branded; and for this purpose again Liebig holds also the office of executioner, and is never fatigued with whipping and branding. Truth! he exclaims, and goes on whipping; Truth! and down comes the rod; Truth! he repeats once more, while he is heating his branding-iron red-hot; Truth! finally, and he presses it on the forehead of the chemists of the day, and rejoices in the ascending vapour. If, perchance, this whipping and branding is discontinued for some days or weeks, then he talks of forbearance (Annalen, January 1846.) He is, in his own estimation, a hero in the empire of morality, because he has deigned for some days or weeks to leave the rod at rest."

Again,
"Almost every one who moves on the same scientific ground with him self, has been attacked by him with a fury, such as was never exhibited in science. He leaves no one at rest; and pronounces judgment upon men and things in a manner which grows bolder, more severe, and more afflicting, as his circle of knowledge expands.

"I shall quote here a few out of the hundreds, nay thousands of instances, in confirmation of the above statement.

"In the Annalen for January 1846, p. 105, he calls Laurent 'one of the most talented chemists of our time, and the most rich in genius;' and yet, on the same page, he represents him as 'a stage-hero, who covers himself with gilded paper, made out of the undervalued labours of others;' while in p. 112, Laurent and Gerhardt are said to be 'two conceited self-complacent cocks strutting about on the top of a dunghill.'

"It is true that some individual is now and then highly praised, but only with a view to the rule: *Tolluntur in alium, ut lapsus graviore ruant.* Laurent first was persecuted for a succession of years, then he was allowed to teach at Giessen, and now he is a cock on a dunghill. Gerhardt was first praised for his excellent translation of the *Chimie Organique* (see the preface to that translation); now he is accused of having mutilated that book (Annalen, January 1846, pages 106, 107). Formerly, Mitscherlich's language was scorned as *old wife's babbling* (Ann., 1841, p. 358); now he is quoted as an authority against Gerhardt (Ann., January 1846, p. 118), and is again 'Herr Professor Mitscherlich.' And the man who, with all the wicked purpose of wounded self-love, could sneer at one whom he formerly called the *most experienced chemist of our time, and probably of all past times*—this same man now dares to write (Ann., January 1846, p. 117), 'that words fail him to express his indignation against Gerhardt's behaviour towards Berzelius,—a behaviour which certainly deserves to be met with the same indignation as that of Liebig towards Berzelius (Ann., May 1846, and Comptes Rendus Complet. Febr. 1845). This man (Liebig I mean) ought first to feel indignation against his own actions before he expresses it so warmly against those of others.

"Four years ago, I myself was held up as an example to Dumas (Ann., Bd. 38, p. 202); five years ago everything I did was right, and all I had done had been confirmed in his own laboratory (Letter of June 1841); but now I have contradicted him on almost every page (!) of the latter parts of my work on physiological chemistry—I hurry towards an abyss, and most of my results are false. Now my hunt after numbers has mutilated the science, and through me physiological chemistry has become unworthy of confidence." (Letter from Liebig, 29th March 1846.)

"In this manner Liebig has got into quarrels with every one; and a legion of pamphlets has appeared against him, in reply to his unwarranted attacks, which are always made under the mask of truth. Men, such as Mohl, who never before used the pen for any other purpose than for the quiet advancement of science, were forced to shake off the odium that was thrown upon them and their labours; and though numbers have called out to him, in their deep conviction of his errors, 'Cease your injustice'—he still goes on like a madman, seizing one after another, and always under the detestable war-cry of love for truth."

Such are the imputations which two of the greatest European chemists cast upon each other. What can we expect from the younger men of science, when such an example is set them by these illustrious seniors. The accusations are true, or they are false; but the adoption of either alternative lowers our estimation in the one case of the accuser, in the other of the accused. We can by no means sympathise with the spirit in which Liebig demanded from Mulder a full account of his process for *Proteine*, although the request, if
couched in becoming language, would have been reasonable enough. Neither was it just of the Giessen chemist to omit all reference to his previous success in obtaining proteine. On the other hand, we must differ from Mulder, when he affirms that Liebig is not a truth-loving, truth-seeking man. We are satisfied that he is both. He is also, however, gifted, or perhaps we should rather say afflicted, with a satirical, sarcastic spirit, which prompts him, often with no very severe intention, to say bitter and most provoking things. A nickname, or a thoughtless epithet, stings often very deeply, and leaves sometimes an incurable wound. It is in gratification of this temper, not from jealousy of their discoveries, that Liebig makes so many enemies, by his scoff and derision. Mulder, moreover, had given some provocation, though we believe unintentionally. In a private letter sent to Liebig, in 1844, which he reprints in his pamphlet, he expostulates with the latter in reference to his attacks on other chemists, in a strain which, however well meant on the part of the honest Dutch chemist, could not but be very provoking to the more impetuous German. We quote one sentence as a sample:—"Believe me," says Mulder, "once more, your (Liebig's) life is full of troubles. Your old age will be full of vexations, and your death-bed full of remorse."—P. 5. Prophecies such as these would provoke a milder person than even his greatest admirers represent Liebig as being.

We cannot, nevertheless, but commend the general tone of Mulder's pamphlet. Considering himself as wronged and insulted, he writes indignantly, but not offensively. He is manifestly no lover of controversy, or morbid hankerer after reputation. There is an honesty, sincerity, and simplicity, as well as a suavity and forbearance in his whole statement extremely pleasing, and we rise from it with the conviction that Mulder is a man accustomed to guide himself by a high and pure standard of morality. As for the scientific question—sulphur or not sulphur? we offer no opinion on the matter. Some third distinguished chemist who shall compare the results of Liebig and Mulder, must decide between them. Meanwhile we trust that neither sulphur, nor any other simple combustible, may again occur in such circumstance as to kindle an unprofitable war between two chemists.

Notes on the Epidemic Cholera. By R. Hartley Kennedy, M.D., &c., Late Physician-General and President of the Medical Board, Bombay. Second Edition, revised. 12mo, pp. 279. London 1846.

The first edition of Dr Kennedy's work on Epidemic Cholera was published at Calcutta in 1826; of that work the present edition is merely a revised reprint. Though the author has had extensive
opportunities since that time of witnessing "the ravages of cholera under every aspect, and in its worst epidemic form," he has seen no reason to alter any of the views which he took of the disease in his first edition. It is to be regretted, we think, that Dr Kennedy should have taken the resolution merely to reprint a work composed twenty years ago, during which so much additional experience of the ravages of cholera has been accumulated, not merely in India, but nearly throughout the whole world. The work has somewhat of a popular character, and undoubtedly contains much interesting matter on the subject of cholera, though it hardly belongs to that rigorous class of works by which alone we can expect the obscurities of such a disease to be dispelled or diminished. In the introduction and first chapter various details are given of the progress of the disease in India after its appearance at Jessore in August 1817; in the second chapter, Dr K. discusses the theories proposed in explanation of the nature of cholera. Into this subject we abstain from following him, farther than to notice the view which he himself has adopted. "I consider," he says, "a nervous derangement, similar to concussion of the brain, to be the disease, how induced I know not, following the above inexplicable shock sustained by the constitution; and the collapse and spasms to be symptomatic of the disorder of the brain; and, finally, I consider the purging and vomiting to be no part of the disease, but the struggle and effort of nature to relieve the constitution, and cast off the noxious principle which is destroying it. For the treatment of such a disease, the indication is distinctly apparent to relieve the brain by bleeding, and to induce the sanitary process of vomiting and purging where they do not exist, or to moderate them when violent. Into these brief injunctions may be resolved all that has been written on respectable authority; and the only difference in my theory is, that I would propose a regular systematic procedure, in preference to the uncertainty, hesitation, and undecidedness, which, in spite of every thing which has yet been written, continues to prevail, in a case where, of all others, the patient's safety most mainly hinges on the promptitude of treatment."—Pp. 56, 57.

In a note on this passage, the author quotes from a letter of Mr Connell's, a statement to the effect that cases in which there is much vomiting, commonly terminate favourably; that cases in which there is little or no vomiting, and much purging, are very apt to prove fatal; and that the most speedily fatal cases are those in which there is little of either vomiting or purging, the whole alimentary canal being as it were paralysed.

The third chapter is on the contagiousness and epidemic character of cholera, in which the author shows himself a decided contagionist, concluding with the following words: "I know no character belonging to any contagious disease, which cholera does not possess; and if it be not contagious, I know no other disease which I should be inclined to consider so."
The fourth chapter turns on a comparison between the virulence and malignity of cholera, and those of other epidemic diseases, with the purpose, almost superfluous at this time of day, of showing how greatly it exceeds in these bad qualities.

The fifth chapter is on concussion of the brain, containing a number of details designed to support the view which Dr K. has adopted of the nature of cholera.

The sixth chapter is on the nature and character of critical discharges, with the like object of upholding the author's views of cholera.

The seventh chapter is entitled, On the Nosological Application of Cholera, in which the author brings a good deal of learning to bear on his subject.

The eight chapter is on the varieties and anomalies which appear in cholera, and on the cholera spontanea of Cullen, and in it the reader will find some interesting details.

The ninth and last chapter is entitled, On Epidemic Cholera, and here full details are given of the symptoms and treatment of the disease. The work concludes with an appendix, containing some additional information on several parts of the subject.

Although we cannot pronounce Dr Kennedy's work to be of a very high character, as a monograph on cholera, yet we must confess we have read it with much satisfaction. It contains no small amount of information on this frightful disease; rather, indeed, of a desultory kind, yet conveyed to the reader in an agreeable manner, and in such a way as strongly to impress us with the sterling honesty of the author's character. We must repeat, however, that we cannot forgive him for having sunk twenty years, fruitful in experience, and having served us up a twice told tale, notwithstanding the profusion of materials to make his tale new, lying at his feet.

---

**Part Third.**

**Periscope.**

**Physiology.**

On the Absorption of Narcotic Poisons by the Lymphatics. By Professor Bischoff of Giessen.

It is now many years since Emmert, in consequence of a series of experiments, came to the conclusion that narcotic poisons were either not taken up by the lymphatics at all; or that, if so taken up, they became so altered that they lost all their poisonous properties. Henle, on the other hand, gave the