myself, and of which, indeed, my paper is in some sort a running summary. Our knowledge of syphilis has, indeed, been set in the light of many minds. If we were to pass over in silence all excepting those who have contributed recent additions to our stock, and of them take only those who have worked at the departments most interesting to this Society, the list would still be very long, and the difficulty of doing justice very great. In France, Ricord, Diday, Lagneau, Lancereaux, and Fournier; in Germany, Virchow, Wegner of Berlin, Dittrich of Prague, Von Bürensprung, and Zeissl; in Scandinavia, Bergh, Boeck, and Bidentkap; in America, Bumstead—have taken foremost places; but their names do not even tithe those who have a claim on our gratitude. At home, Dr. Wilks, I believe, led the way as regards visceral syphilis; and the earliest contributions were most of them made before this Society. Dr. Murchison, Dr. Moxon, Dr. Weber, Dr. Payne, have also contributed most valuable facts. Our knowledge of the diseases of the nervous system, to which I believe Dr. Reid of Belfast was the first of our countrymen to contribute, was early helped on by Dr. Meadows, who, from the practice of the late Dr. Todd, published some cases in the *King's College Hospital Reports*; and more recently by Dr. Hughlings Jackson, Dr. Buzzard, Dr. Russell, and Dr. Broadbent. Nor must I omit to mention, as zealous and successful workers at the general subject, the names of Lee, Berkeley Hill, De Méric, and Gascoyen; nor, last, the author of the most recent and, I think, most comprehensive and able treatise on the disease that has yet appeared—Dr. Bäumlér—who we would gladly claim as, at any rate in training, half an Englishman.

The regulation of the discussion which is to follow, and the decision as to the precise scope which it shall take, will, I am glad to know, rest in much abler hands than mine. That I may not, however, fail of any part of my duty as its introducer, I have endeavoured to epitomise under the following heads some of the chief points raised in my paper. It will be seen that they are by no means closely connected; and that it will be quite competent for any speaker, if he prefer, to confine his attention to a single one, upon which he may be in possession of special facts, without adverting to the others.

Are there any facts which favour the belief that syphilis continues to be a blood-disease after the cessation of all tendency to produce symmetrical symptoms?

The dearth of facts illustrating the internal pathology of the secondary stage of acquired syphilis.

The importance of all facts or arguments for or against the belief that the gummata of the tertiary stage are purely local, and result from renewed growth in formations left over from the exanthem stage.

The value of facts as to the pathology of the late periods of inherited syphilis.

THERAPEUTIC MEMORANDA.

TREATMENT OF EPISTAXIS.

HAVING read Mr. John Cochrane's communication in the *JOURNAL* of January 15th on the above subject, I am induced to record two cases of severe nose-bleeding treated by the local application of liquor ferri perchloridi by a simpler and more easy method than by plugging with lint soaked in a solution of iron, or by injecting the solution up the nostril. The first case was in a woman suffering from cardiac disease, with pulmonary and renal complications, in which loss of blood to any great extent would be likely to induce serious results. The bleeding had been going on for some hours when I saw her, and was almost running from both nostrils, a large clot of the size of an apple hanging over the mouth. I removed the clot, and the blood fairly gushed out. I then injected up both nostrils a solution containing one part of liquor ferri perchloridi fortior and three parts of water with a simple glass syringe, with the result of immediately sealing up the nostrils and stopping the hæmorrhage. A quantity of blood had escaped through the posterior nares, and this also stopped at once. I used about three ounces of the solution altogether. There was not the slightest return of hæmorrhage, and in two days I removed the plug, which was as perfect as one made with lint or aught else.

The second case, of a child, was also one of great severity, and all usual remedies had failed. Half an ounce of the solution (1 to 4) immediately checked the bleeding. I think the injection preferable to making a plug with lint soaked in a solution of iron, for two reasons: 1. It is much more easily applied; 2. It is more likely to reach the seat of hæmorrhage, and consequently would be more effectual.

WILLIAM COX, Winchcombe.

REMARKS

ON

A NEW ATTEMPT TO ESTABLISH THE TRUTH OF THE GERM-THEORY.

By H. CHARLTON BASTIAN, M.A., M.D., F.R.S.,

Professor of Pathological Anatomy in University College; Physician to University College Hospital; and Senior Assistant Physician to the Hospital for the Paralysed and Epileptic.

THE abstract of Professor Tyndall's communication made to the Royal Society on January 13th, and of his Friday evening's discourse at the Royal Institution in the following week, has been published in two journals; and it is evident that he seeks to exert a powerful influence on public opinion generally, as well as on the medical profession, against the possibility of what is termed "spontaneous generation", and in favour of the germ-theory of disease. Any views on this subject, coming in a very emphatic form from so distinguished a cultivator of physical science, were likely to attract much attention, so that it especially behoved him to look carefully into the present state of knowledge concerning it. It is, moreover, incumbent upon others to weigh his views and conclusions upon their own merits, and not to be unduly influenced by his great reputation and the authoritative mode in which they are uttered. The formation of correct opinions upon this subject is of enormous importance, both as regards the science and practice of medicine.

It will be found, however, more especially necessary to scrutinise Professor Tyndall's conclusions on this subject, because he has based them almost wholly upon his own experiments, and has made a very insufficient reference to, and apparently a very insufficient study of, the previously recorded work of other investigators.

His own experiments are divisible into two categories. First, he studies the results of exposing previously boiled organic infusions to the influence of ordinary air; and, secondly, the effect of exposing similar infusions to pure air—that is, to air deprived of the particles which it usually contains in suspension.

The determination to abide by the isolated result of his recent investigations is seen in a remarkable manner even in reference to those of the first series. Organic infusions were gravely placed in the hands of the President of the Royal Society, of Mr. Darwin, Mr. Hirst, Sir John Lubbock, and other friends not very far from London, to be exposed by them to the air of the places in which they lived. Though Professor Tyndall "had no reason to doubt the general diffusion of germs in the atmosphere, he thought it desirable to place the point beyond question". Noting, by the way, that for the word "germs" the cautious reader should substitute "atmospheric particles", it may be stated that the infusions entrusted to these eminent persons became turbid in the ordinary manner. Thus Professor Tyndall had the satisfaction of getting additional confirmation of a fact which is almost as well known to those whom it concerns, as that the letter B follows the letter A in the alphabet. He, too, now regards it as established, that previously boiled organic infusions will, when the weather is not too cold, sooner or later putrefy on exposure to ordinary air.

We come now to Professor Tyndall's second series of experiments. In these, infusions similar to those of the first series, as well as others of a different nature, were exposed to air which had been deprived of its particles by allowing them to subside within certain rather elaborate closed chambers. In all cases, such protected infusions remained pure; hence Professor Tyndall concludes, as M. Pasteur concluded fifteen years ago, that atmospheric particles commonly known as "germs" are the sole causes of putrefaction in boiled organic fluids. It follows, therefore, as a corollary from this doctrine, that such fluids, when effectually guarded from contact with atmospheric dust, will remain permanently pure and free from living organisms.

So far as the results themselves go, they are quite familiar, and their occurrence had been thoroughly well attested by numerous antecedent workers. It is widely recognised that many organic infusions will remain barren when, after previous boiling, they are kept from contact with atmospheric dust; and any experimenter could without much difficulty, in the course of a few months, obtain five thousand of such results, and add them to the five or six hundred recently obtained by Professor Tyndall. But what end would be gained thereby? The

explanation of these experiments is easy. The fermentability of the infusions yielding such results is destroyed, because the process of boiling kills all the living germs they may have contained, and at the same time impairs the virtues of the dissolved organic matter. Hence it happens that, in the absence of exciting agencies, in the form either of mere unheated organic fragments or of actual living germs dropping into them from without, they will remain for an indefinite time free from any very appreciable changes. The germ-theorists attribute the barrenness of these infusions solely to absence of living germs, whilst their opponents say it may be owing merely to the absence of the unheated organic particles forming part of the organic *débris* entering largely into the composition of atmospheric dust.

Now Professor Tyndall believes that the potencies of previously boiled organic fluids are limited in the manner above stated, and that none of them are self-fermentable. The incitors of fermentation in a previously boiled fluid are, he maintains, invariably derived from atmospheric dust; and he further maintains that the only constituents of it capable of exciting fermentation or putrefaction are actual living germs. Because I have obtained results contrary to those of his second series, and tending to show that many previously boiled fluids are self-fermentable, he avers that such results must be due to errors of experiment or of observation.

In contrasting his own results with mine, Professor Tyndall further says, more or less explicitly, that he has strictly adhered to the experimental conditions prescribed by me; and it is because of his failure to get positive results in the experiments of his second series (which he regards as comparable with mine), that he now proclaims spontaneous generation to be a chimera, and my experimental results to have been vitiated by error. But, looking to my works, *The Modes of Origin of Lowest Organisms* (p. 67), and *The Beginnings of Life* (vol. ii, p. 30), where directions are given concerning the manner of conducting the experiments, I find it stated very prominently that, after the infusions had been boiled, they were subjected to "a temperature of 80-95 deg. Fahr. during both day and night". Now, Professor Tyndall's mode of strictly imitating these conditions is to maintain his own infusions at "a temperature of 60 deg. to 70 deg. Fahr." Yet the maintenance of the infusions at a sufficiently high temperature after boiling is of immense importance. I have, indeed, recently found that some infusions, which remain sterile within hermetically sealed vessels at 90 deg. Fahr. or higher, not unfrequently may be made to putrefy if exposed for a few days to the still higher temperature of 115 deg. Fahr.

In the preface to the smaller work above mentioned, I said, in 1871: "In these experiments with heated fluids in closed flasks, nothing is easier than to obtain negative results. The same kinds of infusions which—if care has been taken to obtain them strong enough—will in a few days teem with living organisms, often show no trace of living things after much longer periods when the solutions are weak." Now, looking to the very striking manner in which Professor Tyndall has departed from the experimental conditions which he professes to have followed in respect of temperature, how is it possible to have full confidence that he has prepared the infusions in the manner prescribed? For my own part, I feel no assurance that he has bestowed sufficient care upon them—that is, so as to obtain them of adequate strength.

It will have been seen that one of the principal differences as regards facts existing between Professor Tyndall and myself, is as to whether there are or are not certain organic infusions which, after boiling, can be made to putrefy on exposure to pure air. He, arguing from the basis of his own imperfectly conducted experiments, declares that he has not met with such fluids; whilst I affirm that many fluids of this type exist, some of which are in the strictest sense self-fermentable, since they will ferment or putrefy, not only when protected from atmospheric dust, but when shut in a sealed vessel, away even from contact with pure air.

From my work entitled *Evolution and the Origin of Life*, published in the winter of 1874, Professor Tyndall quotes the following passage from p. 94: "We now know that boiled turnip- or hay-infusions exposed to ordinary air, exposed to filtered air, to calcined air, or shut off altogether from contact with air, are more or less prone to swarm with bacteria and vibriones in the course of from two to six days." Then he himself adds: "Who the 'we' are who possess this knowledge is not stated." Professor Tyndall thus tends to convey the impression that the possibility of obtaining such results is not known and acknowledged by well informed men of science. Immediately afterwards, he refers to experiments of his own in which (doubtless from such causes as I have already indicated) he was unsuccessful in obtaining similar results; and, from the doubly insecure basis of his own lack of learning and want of attention to prescribed conditions, he comes to the conclusion that I have been deceiving myself and others in making such

statements. He refers, moreover, to a statement of mine made last year in opening the debate on "The Germ-Theory of Disease" at the Pathological Society, to the effect that carefully prepared infusions of muscle, kidney, and liver, would, after a time, become corrupt within vessels which had been hermetically sealed whilst the fluids were boiling (see *Transactions of Pathological Society*, vol. xxvi, p. 272). Professor Tyndall made, in his own fashion, many experiments of this kind; and he records one hundred and thirty-nine failures to obtain such results as I have indicated. He says, with crushing severity, "not one of this cloud of witnesses offered the least countenance" to the truth of my assertions.

Now, if it did not concern so grave a question, all this would be truly laughable. It certainly seems far more like burlesque than the sober pursuit of science. But since Professor Tyndall says he does not know who can be comprised under the "we" which I employ in his quotation above given, and as he consequently does not tell his readers, it seems desirable, now that the question is asked, for me to try to enlighten him—they may not require it.

Some of the various methods of testing the behaviour of boiled organic fluids when kept free from atmospheric particles are mentioned in the passage quoted by Professor Tyndall; and his own method of subsidence belongs to this series, which, more fully stated, would stand as follows:

- a. Making air pass through red-hot tubes—calcination.
- b. Making it pass through cotton-wool or a plate of porous porcelain—filtration.
- c. Allowing atmospheric particles to gravitate—subsidence.
- d. Making air pass into flasks through long and bent necks—filtration and subsidence combined.
- e. Getting rid of air and its particles by sealing the neck of the vessel during ebullition of the fluid—expulsion.

I now subjoin a list of investigators who, by experimenting in one or more of these ways, have come to the conclusion that certain fluids exposed to such conditions will putrefy, although Professor Tyndall thinks they ought all to remain pure. What explanations these investigators severally give of the facts they record are questions which need not now detain us, since it is the facts themselves which Professor Tyndall ignores and in effect denies.

1. Schwann, *Isis*, 1837; Poggendorff's *Annalen*, 1837.
2. Mantegazza, *Giorn. dell' R. Instit. Lombard*, 1852.
3. Schroeder and Dusch, *Annal. de Chimie*, tome xli, 1854.
4. Schroeder, Liebig's *Annalen*, cix, 1859 and *Chem. News*, vol. v, 1862.
5. Pouchet, *Hétérogénie*, 1859, and *Nouvelles Expériences*, etc., 1864.
6. Pasteur, *Ann. de Chimie*, 1862 (see pp. 60-62).
7. Joly and Musset, *Compt. Rend.*, 1861 and 1862.
8. Victor Meunier, *Compt. Rend.*, tome xli, 1865.
9. Hughes Bennett, *Ed. Med. and Surg. Journ.*, 1868.
10. Cantoni, *Gaz. Med. Ital. Lombard.*, tome i, 1868.
11. Bastian, *Nature*, 1870; *Modes of Origin*, etc., 1871; *The Beginnings of Life*, 1872.
12. Burdon Sanderson, *Nature*, January 8th and June 1873, *Med. Times and Gaz.*, October 22nd, 1873.
13. Huizinga, *Nature*, March 20th, 1873, and Pflüger's *Archiv*, vols. vii and viii.
14. Lankester and Pöde, *Proced. of Roy. Soc.*, 1874.
15. Roberts, *Phil. Trans.*, vol. clxiv, 1874.
16. Samuelson, Pflüger's *Archiv*, vol. vii, 1874.
17. Gscheidlen, quoted by Dr. Sanderson in *Academy*, July 10, 1875.

I have set down the names in order of time, and included my own amongst them because those mentioned after me have all confirmed my results with regard to the putrefaction of some fluids in hermetically sealed vessels, from which the air has been expelled by boiling; the very experiments, in fact, which Professor Tyndall (like one waking from a three years' sleep) now endeavours to impeach by his own one hundred and thirty-nine failures.

Dr. Burdon Sanderson's well known corroboration of the accuracy of his results may be here reproduced. He says (*Nature*, January 8th, 1873):—"The accuracy of Dr. Bastian's statement of fact with reference to the particular experiments now under consideration has been publicly questioned. I myself doubted it, and expressed my doubts, if not publicly, at least in conversation. I am content to have established—at all events, to my own satisfaction—that, by following Dr. Bastian's directions, infusions can be prepared which are not deprived by an ebullition of from five to ten minutes of the faculty of undergoing those chemical changes which are characterised by the presence of swarms of bacteria, and that the development of these organisms can proceed with the greatest activity in hermetically sealed glass

vessels, from which almost the whole of the air had been expelled by boiling.”*

And, if Professor Tyndall and others wish to know how far these results have since been generally recognised as correct, reference may be made to a review of my work, *Evolution and the Origin of Life*, by Dr. Burdon Sanderson, in the *Academy* of July 10th, 1875. There, in reference to the confirmation which these experiments had received, and in relation to other work in connection with the question generally by Samuelson and Gscheidlen, Dr. Sanderson writes:—“As regards the trustworthy character of the experiments themselves, it will probably be a sufficient guarantee to most readers that they have been conducted under the immediate supervision of men like Pflüger and Hoppe-Seyler, who occupy the foremost rank as vital physicists. Those who are more especially interested in the subject will best satisfy themselves of the exactitude and completeness with which all the investigations have been carried out by reading for themselves the original papers.”

Although Dr. Sanderson thus thoroughly recognises the fact (and knows that others do the same) that many boiled fluids will putrefy in closed vessels from which air has been expelled by boiling, it is well known that he is not willing to regard such facts as the proof of the occurrence of “spontaneous generation”. He admits, indeed (BRITISH MEDICAL JOURNAL, February 13th, 1875, page 201) that I and others have shown that bacteria in their “ordinary state” are killed by a temperature of about 140 deg. Fahr.; but, instead of accepting “spontaneous generation” as an explanation of the occurrence of living organisms in the vessels above referred to, he pleads in favour of the only other possible explanation, viz., a “latent vitality” in some bacteria—germs not extinguishable by exposure for ten minutes or so to the influence of boiling water. I felt it my duty to refer to this hypothesis in my address at the Pathological Society last year (BRITISH MEDICAL JOURNAL, April 10th, 1875, page 473); but, whatever its worth may be, Dr. Sanderson, whose learning and knowledge of the whole question few will dispute, knows that this, or some such hypothesis, alone stands in the way of the acceptance of “spontaneous generation” as a proved reality.

Professor Tyndall, however, seems to regard this hypothesis as beneath his notice. He makes no sort of reference to it, and agrees with me in thinking that bacteria and their germs are decidedly killed by five minutes’ boiling in organic infusions. He still further supports the view held by me, in opposition to that of M. Pasteur, that such a result follows both with alkaline and with acid infusions.

This may seem to many of my readers a rather farcical finale when compared with Professor Tyndall’s own anticipations; but I feel thoroughly assured that those who understand the subject will see that, in the present stage of the controversy, no other conclusions of value are deducible from his recent experiments. He appears to have completely misapprehended the present state of knowledge on the question; he has uniformly failed to obtain results which are now firmly established; and, as regards the only question which is at present in dispute, he unhesitatingly coincides with me. If, as Professor Tyndall believes, and as his experiments seem to show, bacteria and their germs are killed by boiling them for five minutes, nothing remains but for us to shake hands over the establishment of the occurrence of “spontaneous generation” and the overthrow of the Germ-Theory of Disease. I shall be heartily glad to take leave of the subject, as I have now little leisure available for following up questions of this kind, upon which I have already bestowed much time and labour.

* It seems desirable, in order to prevent misunderstanding, to make some reference to Dr. Sanderson’s cautious use of the word “almost”. It would be difficult to ascertain how much air would remain in a small retort, nearly full at first, and in which the fluid was boiled continuously for five to ten minutes before sealing. The expulsion of air goes no further in the experiments with bent-neck flasks, in those whose necks are plugged with cotton-wool, or in those in which the re-entering air is calcined. In my experiments above referred to, instead of purifying the re-entering air, it is shut out by hermetically sealing the flask whilst steam is still issuing.

BEQUESTS.—By the will of Mr. William Bishop, late of Chepstow Villas West, Notting Hill, the following bequests are made:—St. Mary’s Hospital, Paddington; St. George’s Hospital; Middlesex Hospital, Charing Cross Hospital, St. Bartholomew’s Hospital, the Royal Free Hospital, the Hospital for Consumption and Diseases of the Chest at Brompton, St. Thomas’s Hospital, and the Small-pox Hospital, £1,000 each.—Mr. Simon Magnus, late of Chatham, has left £19 10s. each to the Kent County Hospital, the Asylum for Idiots, the British Home for Incurables, the Metropolitan Free Hospital (for the benefit of the Jewish Ward), and St. Bartholomew’s Hospital, Rochester.—Mr. Simon Waley Waley, of the Stock Exchange, has bequeathed £100 to University College Hospital.

SOME REMARKS ON THE MEDICO-LEGAL EXAMINATION OF HUMAN REMAINS, AND ESPECIALLY THE UTERUS.*

By J. BRAXTON HICKS, M.D., F.R.S.,
Obstetric Physician to Guy’s Hospital.

FROM what fell from various speakers at the last meeting of the Society last year, I have thought that it may be interesting to the Fellows to examine the uterus referred to at the trial of Harriet Lane. I, therefore, through the kindness of Mr. Larkin, produce it for your inspection. It may be asked why I have brought it rather than others; but the fact is, that long before the trial, while it was in the condition in which it was removed from the remains, it was shown to me by Mr. Larkin; but in this state it was so difficult to make out the relative size and position of parts, that I suggested for the consideration of the medical gentlemen of both sides that it should be placed in water with some disinfectant. This was done, and the result appears in the specimen now present. It appears as if recently removed from a body which has undergone some slight decomposition. It has all the characters which writers have been wont to attribute to the uterus of a multipara; and I was prepared to say as much, had I been called on to give evidence at the trial.

But there are other reasons than curiosity for which I have considered it useful to bring forward this specimen. They are two, closely connected with jurisprudence: and both have a bearing on the identification of human remains.

The first is the advantage of soaking the dried specimen in water. This applies not only to the uterus, but to many parts of the body, such as the heart; for it may in certain cases be necessary to distinguish between the remains of man and those of other animals. I am not aware of any other trial requiring proof of a similar kind as in this, where the drying of the remains was so complete; yet it is not so unlikely a case but that it may again occur. It is well that facts should be examined which may assist in elucidating the subject.

The idea which will probably strike thinkers on the subject is, whether the process of drying and re-soaking produces any alteration in the shape of the organ, and especially if a certain amount of decomposition have occurred also. Although the time has been too short to inquire into the effects of decomposition, I have, assisted by my son Dr. Edward Hicks, made some experiments on the other points, the results of which I have tabulated. I obtained six uteri; and, having measured them, I had them dried. They were then again measured. Then, after soaking them in carbolic water, they were a third time measured. The drying caused most of the measurements to be lessened, according to the amount of desiccation; but it also made it more difficult to define the outline of the organ; and, owing to the agglutination of the sides, next to impossible to measure the internal length. This subject is worthy of further investigation, both in regard to the uterus and to other parts of the body. Indeed, it appears to me that the whole body, and notably the whole head, might be submitted to the same process. Whether, however, the identification of the features of an Egyptian mummy may be made out, I will leave to future experiments to prove.

In the case of the uterus now before the Society, a certain amount of decomposition had occurred. It will be necessary in any experiments of a parallel nature, to allow a certain amount of putrefaction to proceed before complete drying. No doubt there is loss of tissue and consequent thinning of the walls. It is a subject of regret that the measurements of this uterus were not taken before it was placed in water. The relative form of its parts, however, do not appear to be different.

The second point connected with this subject is as to the probability of saying whether any given uterus is that of a woman who has borne children; and this is the point which has been alluded to by Dr. Meadows. Perhaps we might ask another question: Can we say positively that a given uterus has never conceived? I think that all will concede that the uterus of a woman who has been delivered within a twelvemonth will be very distinguishable from that of a woman who has never conceived. The characters of globosity are not readily effaced; but at a later period, or when the atrophy of age has set in, will such an uterus revert to the virgin type so as to be indistinguishable from it? Upon this point, I consider, we require farther knowledge. Again, supposing that in a virgin there has been obstruction to the exit of the secretions, and a certain globosity has been produced, with hypertrophy of the walls, can such an organ be distinguished from

* Read before the Obstetrical Society of London on February and.