AN ADDRESS ON THE PATHOLOGY OF SYPHILIS.

BEING THE

*Introduction to a Debate at the Pathological Society of London, on Tuesday,
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IN engaging in a discussion on Syphilis, with a view to the advance of our knowledge of it, there are certain facts as to its nature which we may, I presume, take as generally accepted. It is, I suppose, now a matter of general belief that the phenomena of this disease, notwithstanding their great variety in detail of character, are due to one virus, which, having been introduced into the body by contagion, develops within it, with tolerable uniformity as regards stage and time, certain processes of inflammatory growth. The pluralistic doctrine of Carmichael never obtained any wide acceptance; and it is now, I think, generally acknowledged that what was called dualism had not really any claim to such a name, since but few ever believed that there were really two rival forms of virus capable of producing constitutional effects. Further, even in its much more restricted sense, as implying that there are two distinct kinds of virus, to which are attributed two different forms of chancre, I think we may say of dualism, that it is dead, and that the far simpler creed which attributes the soft chancre to contagion with inflammatory products produced by syphilis, but not, as a rule, containing its germs, is the one which now obtains general acceptance. We have, then, in syphilis, but one malady and one virus.

The objections which, on account of its apparent irregularity, were at first urged against the claim of syphilis to take position amongst the specific fevers, have been greatly diminished of late by facts from two different directions: first, by the observation that the short-lived exanthemata are by no means so regular and uniform in their career as it had been customary to account them; and that, in point of fact, they do vary quite as much as to length of stage, degree of severity, and occasional omission of some of their phenomena, as does syphilis. Secondly, we have come to appreciate more correctly the antidotal power of our specifics, and have remembered that it is not fair to allege against a malady that it is irregular in its course, when our observations are made on cases in which we have done our best, by means of a most potent antidote, to arrest its career. I have no doubt that, if mercury were entirely put aside, we should soon see that syphilis is quite as regular in its stages as variola, and also that it varies quite as little in its degree of severity in different persons. When mercury does not cure, it delays; and the retardation of stages sometimes witnessed under its use is very remarkable.

Thus, then, what I shall have to say this evening will be based on the assumption that in syphilis we have to deal with a specific fever of prolonged but definite stages, which is produced by contagion only, which has a period of incubation, a period of outbreak (known as primary symptoms), and a period of efflorescence or exanthem (known as the secondary stage); and which, in exceptional cases, differs somewhat from its more short-lived congeners by being followed by sequelæ to which we give the name of tertiary symptoms.

Had it not been for this unfortunate tendency to recur and become protracted, syphilis might have remained a surgical malady. If it always ended, as it ought to do, with its exanthem or secondary stage, there would have been no need to trouble physicians as to its treatment, nor is it very probable that the Pathological Society of London would have thought it worth while to invite a detailed discussion of its life-history problems. To the annoyance and misery of thousands, however, syphilis does not always end with its apparent death at the expiration of the secondary stage. The patient seems then to be well; he loses his fever; his rash disappears; his throat heals; his hair grows again; and his blood is renovated; but he is not safe. There remains ever after a risk—comparatively a very small one, but still sufficient to cause much anxiety—that he may become troubled with a tendency to the growth of tumours, which will be in direct relation to his bygone disease. These may occur in important parts, may at-

tain large size, and will probably show very little tendency to spontaneous cure. To these, we give the name of tertiary symptoms or sequelæ; and we recognise a most important distinction between them and those of the earlier stage, in that they are never general, and only by accident symmetrical. They do not constitute another stage of a blood-disease; but, by their constant non-symmetry, appear to prove that now, at least, the blood is not concerned. We no more witness returns of the syphilitic exanthem—a symmetrical and general eruption—at long periods after the first, than we do recurrences of the small-pox eruption after the patient has recovered. Most unhesitatingly may we characterise as errors in observation a certain small number of cases which have been supposed to illustrate this occurrence. In all probability, syphilis ceases to be a blood-disease at the date when symmetrical manifestations cease to be usual. The precise period at which this takes place will vary with the treatment pursued and the idiosyncrasy of the patient.

The correct determination of the period at which syphilis ceases to be a blood-disease could, perhaps, be determined with certainty only by inoculation experiments. That there is a period at which it does so cease without in the least exempting the patient from the risk of fresh local symptoms, appears highly probable. Whilst the secretions from all forms of secondary ulcerations and the blood of patients in the secondary stage are abundantly proved to be contagious, the negative evidence as regards both in the tertiary stage is very strong. The secretion from a phagedænic sore in a tertiary case may spread phagedæna, but will not spread syphilis; and hence, probably, one frequent source of what are called soft sores. It would be rash to assert that it is impossible for tertiary syphilis to prove contagious, but at present I know no evidence which would support the affirmation. The test of contagion is probably the best that we possess as regards the existence of a blood-taint, and guided by it there seem strong reasons for suspecting that risk of hereditary transmission may persist long after the cessation of blood contamination. At any rate, the risk of contagion appears to cease long before the risk of hereditary transmission. I admit that there is difficulty in conceiving of this, or in understanding how a taint no longer active in the parent's fluids can be reproduced with virulence in his offspring, but the facts seem to point in that direction. In speaking of the possibility of local syphilitic phenomena occurring long after the cessation of blood-taint, it is intended to imply very definitely the belief that many of the late phenomena of syphilis are purely local; and that, if they exist in parts which are accessible to local remedies, they do not imply any necessity for internal treatment. If I may be pardoned a statement which refers to clinical rather than to pathological matters, I may say that the long persistent and frequently relapsing sores in the tongue, and many forms of skin-disease, palmar psoriasis, lupoid tubercles, etc., are local and not constitutional, and are cured quite as easily by local as by internal treatment. The internal gummata are also probably often only local, but you cannot bring mercury or iodine into contact with them, excepting by introducing these remedies into the blood. It is scarcely necessary to remark that success by internal treatment by no means proves that the disease was more than local.

Before proceeding further to speculate upon the true relationship between the secondary phenomena and their sequelæ, it may be best to glance briefly at some of the peculiarities of the syphilitic inflammations. So peculiar are the products, that one is almost tempted to speak of new growth rather than of inflammation. From beginning to end—from the chancre to the latest tertiary gumma—the tendency to cell-growth is most remarkable; and the production of a solid palpable mass, often very firm, is a characteristic feature of the inflammatory process when lighted up by the virus of syphilis. It is, however, by no means the only one. An avoidance of proclivity to suppuration (not constant, but still very marked), a tendency to cause death of the tissues affected, and thus produce phagedænic ulceration, or even sloughing, and a proneness to undergo rapid and complete absorption, especially when attacked by certain metals or their salts, are features which characterise and distinguish the new growths due to syphilis. Let us add that, whilst absorption may easily be procured, relapses are very prone to occur, especially if the treatment be arrested too soon. It is to be insisted that these peculiarities are the common property of syphilis in all its stages. A gumma in the tongue may be almost as hard as a chancre, and either of them may be, and have been many a time, mistaken for scirrhus. A chancre, when very large and dense, may perish and slough out just like an overgrown gumma. The chancre, the secondary rash, and the gumma are all alike liable to undergo molecular death and to be attended by phagedænic ulceration. To attempt to distinguish between the secondary and tertiary stages, by saying that the latter only is attended by liability to deep ulceration, is to set facts at defiance.

The attempt to found distinctions as to the stage of syphilis upon the tendency or otherwise to deep ulceration, has been the means of introducing much confusion into the subject. It cannot be too strongly insisted, that this may be shown at any stage. It may be remarked, also, in passing, that phagedænic action constitutes an exception to the statement that the inflammations due to syphilis in an early stage show a tendency to spontaneous cure.

When the wizard Syphilis has once called up the demon Phagedæna, it has evoked that which it is powerless to control. No law of spontaneous cessation and recovery will now be observed; nor will the specifics for the parent malady avail much as regards the offspring. Phagedæna, once started, exists on its own behalf, and spreads by the contagion of its own pus. To arrest its spread, it is necessary to destroy its secretion.

So marked is this liability to phagedæna in syphilis, and so rare in connection with any other cause, that with a few exceptions we may count syphilis, either directly or indirectly, as the parent of all phagedæna. The discussion as to whether rupia and its allies are secondary or tertiary symptoms may thus be easily decided, by explaining that there is nothing in the "rupial sore" characteristic of either, and that its features may be assumed in both. If rupia occur as symmetrical eruption, it will always be found that the period which has elapsed since the contagion is only short, and that the disease is consequently secondary; but if it be unsymmetrical, then in all probability it is tertiary. Histologists, I believe, admit that the differences in cell-structure between an indurated chancre, a secondary tubercle, and a tertiary gumma, are only very slight, and that essentially all three are constructed on the same plan.

Thus, in examining the question as to the relationship between the several stages of syphilis, it is, I think, a matter of necessity that we admit that many features and many tendencies are shared by the phenomena which occur in all. Whilst, however, their similarities are marked, so also to some extent are their differences. First, and as by far the very most important of the differences between the stages, we place one which has been already mentioned, the tendency to general and symmetrical development in the secondary stage, and to local, restricted, and unsymmetrical formations in that of sequelæ. This, however, after all is merely a question of abundance and not so much of character. In the secondary stage, the blood and all the tissues are involved; whilst, in the later ones, only certain regions, or it may be only single spots, are affected. The second difference is, that a spontaneous tendency to resolution of the new growths and to absorption is constantly witnessed; whereas it is exceptional in all tertiary products. It is, however, by no means certain that spontaneous disappearance does not often occur in the case of tertiary growths. Still it may be fairly granted that a proneness to persist, to grow, to spread, and to contaminate adjacent parts, is far more frequently witnessed in the tertiary than in the early formations. I am puzzled to find for mention any other characteristics which distinguish these two classes of symptoms. It may probably be suggested that there is difference as to the parts liable to be attacked, and as to the depth to which ulceration may go; that the symptoms in the secondary stage are all superficial—skin and mucous membranes—and that they do not spread deeply; whilst those of the tertiary stage are apt to occur in cellular tissue, periosteum, bones, viscera, or meninges. I can, however, only admit with great limitation these assertions. Deeply spreading ulceration occurs not unfrequently in the secondary or even in the primary stage; and, as to the immunity of the viscera, cellular tissue, and periosteum in the secondary stage, it is far more easily asserted than proven. Some able writers on syphilis have proposed the term "period of gummata" as a substitute for "tertiary stage"—but really gummata may occur in the secondary stage also. It is true that the large gummata of the testis, cellular tissue, liver, etc., are very rare, excepting in the late tertiary periods, and hence a general impression on the subject, which is, I suspect, very false. Nor is it absolutely true that even the large gummata are not seen in the secondary stage. Abundant facts on record disprove such an assertion. Distinctions which relate only to size may easily mislead.

The visceral pathology of the secondary stage might form a chapter in the history of syphilis which has not as yet been written, and for which we possess but few data. It is, however, I feel sure, a great mistake to suppose that there are none to be obtained; and this Society would, I think, be doing a most useful work in encouraging the production of carefully sifted evidence on this point. It is of course rare for patients to die during this stage, and opportunities for *post mortem* examinations are not frequent. The widely spread belief that secondary syphilis, in the acquired form, has no internal pathology worth working at, has probably led to the waste of not a few opportunities which have chanced to occur. I venture to foretell that, when the facts are forthcoming, we shall change our opinions on this point very definitely. There is surely

no *à priori* probability in the creed that a blood-disease so severe as syphilis should produce lesions on the skin, in the mouth, and in the eye only; that it should, in fact, affect all the visible parts, and avoid all the concealed ones. The fact that the latter are concealed is probably the only reason why we believe so. The real difference between the secondary and tertiary manifestations, let me repeat, concerns probably some minor facts as to their course and tendencies, rather than the parts attacked. In the secondary stage, all the morbid processes are transitory, and tend to spontaneous cessation; whilst the tertiary ones are more persistent, and tend to destruction of the parts concerned. But, apart from mere inferential conjecture, there are certain facts already ascertained which seem to discredit the opinion that certain tissues are attacked in the secondary stage, and certain others in the tertiary one. Time will not permit my mentioning cases, but I may briefly enumerate the following points.

1. In the inflammations of the eye well recognised as early and secondary, we have various tissues involved: the iris, which is fibromuscular; the hyaloid, the retina, and usually, I think, at a somewhat later period, the choroid. In the iris and in the choroid, little tumours, analogous, I suspect, in all respects to gummata, are seen.

2. When the internal ear is attacked in acquired syphilis, a very rare event, it is usually during or immediately after the secondary symptoms.

3. In two cases of death from syphilitic disease of the heart which have come under my own notice, the event in both occurred in the secondary stage (myocarditis with gumma).

4. In one of the latter cases (examined *post mortem* by Dr. Sutton), definite gummata were found in both testes and in the spleen, although the secondary rash was still out in the patient's skin. A similar case in an infant, three months old, with secondary rash, has recently been brought before this Society by Dr. Coupland. Parallel facts have been recorded by French observers.

5. It is very common for patients during the secondary stage to complain of symptoms which would imply disease of the same tissues which are attacked later on in the tertiary period. Rheumatoid pains often with slight swellings over bones, represent probably the nodes of a later stage. Febrile disturbance, severe headache, loss of appetite and strength, are all symptoms, which the eruption on the skin scarcely suffices to explain. Transitory albuminuria is not uncommon, and some authors assert that jaundice may occasionally occur. We should probably see more of such symptoms, were it not for the common practice in England to assail the disease with its antidote.

6. The strongest reason for believing in the frequent occurrence of visceral and periosteal lesions in the secondary stage of acquired syphilis is, that they are very common in the corresponding stage of the inherited disease. In the latter, the secondary stage is far more severe than it is in adults, and often ends in death, and abundant opportunities are afforded to the pathologist. In infants, it is well known that visceral and bone lesions occur not very infrequently and simultaneously with the skin-eruption. The best example of gummata in the liver that I have ever seen was from an infant. Mr. Canton has described in our *Transactions* a case in which these growths attained a large size, and showed all the characters which are displayed in those of the adult. The able work of Dr. Taylor of Boston, just published, contains abundant evidence on the same point. To Wegner of Berlin we are indebted for important investigations proving the frequency of periostitis in infants; and when syphilitic sarcocele (gumma, and often large) occur in inherited disease, it is always in young children and sometimes in infants. Nor is it possible to dismiss this class of facts by suggesting that many infants begin with the tertiary stage, and that probably those who show internal lesions are born of parents who have suffered from the disease long ago. It is, I believe, almost invariable for infants to begin with the secondary stage, whatever may have been that which their parents may have reached. And, further, in not a few cases in which infants have displayed phenomena ordinarily ranked as tertiary, they were the offspring of parents who had but recently suffered, and they themselves showed at the same time the usual symptoms of the secondary one. It is certain, then, that, in infantile syphilis at any rate, visceral and periosteal lesions often occur in the early periods. Let it be added that precisely the same kind of differences are observed between the periostitis of infancy and the periostitis of some years later, that we see between the transitory bone-pains of the secondary stage in adults and the more lasting nodes which follow years afterwards. In the infant, the epiphyses and adjacent bones swell and become tender; but, after a while, all trace of enlargement passes off, and unless, as sometimes happens to a growing bone, deformity is caused, no trace of the malady remains. Five or eight years later, very probably the same child's bones may become thickened by permanent osseous nodes.

The theory suggested as the relationship between the secondary and tertiary stages would amount to the abandonment of the latter as any true stage at all. I would emphasise the teaching of those who regard tertiary symptoms merely as sequelæ. Many authors, impressed with the difficulty of arranging in any orderly manner the various sequences of syphilis, have been willing to give up the use of the term tertiary; but it has, I think, always been found necessary to substitute it by some other in no respect more satisfactory. In Lancereaux's able treatise, we have the term "period of gummy deposits". My argument is, however, that gummy deposits occur also in the secondary stage, and that in this they differ only from those of the tertiary in that they rarely attain any large size, and usually subside spontaneously. The gummata of the early period are usually small—miliary as they have been called; those of the tertiary sometimes attain a large size. On this admission of fact follows closely an hypothesis which may prove important. Are not the tertiary gummata probably regrowths in structures left behind from the secondary stage? Such an hypothesis, if plausible, would undoubtedly help much to the simplification of the subject; it might also in the future assist in giving definiteness to our plans of treatment. The ingenious doctrine of residual abscesses, ably propounded by Sir James Paget, may aid us in the appreciation of this suggestion. We need not, indeed, go far in any direction of pathological observation for proofs of the tendency of parts once diseased, or the site of morbid products, to relapse even after very long periods of quiet. An especially curious and apposite one is afforded by the rare cases in which indurated chancres relapse. The fact that indurations sometimes recur in parts where formerly chancres existed, without any fresh contagion, has been noticed by several writers, and, amongst others, Mr. Lee has recorded an example of it. It has, however, I think, scarcely received the attention which it deserves as an illustration of pathological law. I have under occasional observation at the present time several patients who are subjects of the relapsing chancre. Indurations, in all respects like primary sores, occur repeatedly, and without any fresh contagion, and present features which would defy the most experienced observer to diagnose them excepting by the history. They may remain for a few weeks, or even months, and may occur many years after the patient has passed through his secondary stage. Mr. Lee has suggested that they are to be regarded as tertiary gummata; but it is to be noted that they are as hard as cartilage, never slough, often disappear spontaneously, and are always located exactly in the site of the original sore. It is this latter fact which seems to me so important in reference to the theory just suggested. It may be well to add, that the facts that these relapsed sores are never followed by secondary symptoms, and that they may occur not once or twice, but half-a-dozen times to the same individual, conclusively support the patient's assertion, that they are not produced by fresh contagion. If an indurated chancre may, in virtue of something which has been left behind, develop afresh in its old site ten years after the primary disease, why may not a like event happen to the cell-products of the secondary stage? It is not to be forgotten that a difficulty may be suggested in reference to this hypothesis, in that the tertiary gummata occur often in parts which are not proved to have been involved in the secondary, and are absent in others in which they frequently are so. For instance, secondary iritis is common, but tertiary iritis or tertiary gummata in the iris are very rare. This objection is probably more apparent than real; and we must always keep in mind that tertiary phenomena of any kind are exceptional; and, further, that it is quite possible that regrowth is the most likely to happen where the original development has been but slight, and the cell-growth has been checked before its power was exhausted. It may, also, be fairly urged that, although exceptions happen, yet, as a rough rule, the tertiary neoplasms are met with in or near the parts which are most commonly affected in the secondary stage. The skin, the subcutaneous cellular tissue, the palate, and the throat, are their more frequent sites. It is true that the brain, nerves, muscles, and viscera, claim our attention much more; but this is on account of the greater importance and interest of the symptoms produced, not because they are really the most frequent sites of tertiary gummata.

In connection with this theory, that the tertiary growths occur in all structures which have remained over in a quiescent state from the secondary period, it is matter of justice that I should mention the great name of Virchow. Amongst the many suggestions for which we are indebted to his labours, is his well known theory that relapses of symptoms are caused by stores of poison which have remained latent in various parts, especially in lymphatic glands, and, undergoing development after an interval, cause fresh contamination of the blood. This, however, is not the same idea as that the tertiary growths result not from blood-contamination in any way, but simply from local re-activation of long-resting germs.

In contrasting the course of inherited syphilis with that of the acquired disease, several remarkable features at once arrest our attention. Amongst these, are the severity of the secondary stage, often fatal; and, on the other hand, the frequent omission of all early symptoms; the remarkably long periods of latency which ensue after the cessation of the infantile symptoms; and, lastly, the great rarity of most of the conditions, which in the acquired form we rank as tertiary. I am warned that I must be exceedingly brief in what I have to say on these topics, and to the first two I will not further allude. The periods of latency which are often witnessed constitute however a phenomenon too remarkable to be passed over. As is well known, most syphilitic infants suffer during the first few months of life from affections of the skin and mucous membranes, which are clearly analogous with the early secondary symptoms in adults. These pass off after a time, and at the end of a year, if the child have survived, it usually appears to be quite well. It may now remain for five, ten, twenty, or even five-and-thirty years without any further indication of its taint, and then may occur some definite and most peculiar affections. That there is a form of interstitial inflammation of the cornea, which when well characterised can be recognised as most certainly due to inherited syphilis, is a fact which, I believe, is not now doubted by any English observers. It is usually symmetrical, and, in a large majority of cases, it runs its course and disappears spontaneously, and as completely as does a secondary syphilitic rash. Nor are the facts as to the long period of latency which may precede it more in dispute than its own character, and I shall scarcely be blamed for exaggeration, if I say that they are amongst the marvels of pathology. Not only may this form of keratitis occur to a grown-up person who has had no symptoms of syphilis since infancy, but it may happen also to one who has never had any symptoms before, and may yet be itself most characteristic. It is difficult to offer any explanation of the cause of this long delay, other than that it has, perhaps, something to do with the gradual development of the tissues. In connection with it, we must remark that, however long may have been the interval of latency, interstitial keratitis is still plainly one of the secondary group. Its symmetry and its tendency to spontaneous disappearance both prove its position in this respect, and we note, besides, that certain other phenomena which sometimes occur at about the same age show similar characters. The choroiditis and the disease of the ear which in these patients so often leads to deafness, are both of them usually symmetrical, and so also very often is the more chronic form of periostitis. I do not mean that the symmetry is absolute. It is not the Dutch-gardener sort of symmetry, which insists on an absolute correspondence, both as to time and features, in the two halves; but still it is sufficiently well marked to assure us of the fact. Very often one eye takes precedence by a short time of the other, and often one suffers more severely than its fellow. I have yet another and a stronger reason to urge for placing the keratitis and the inflammation of the internal ear, which are so frequent under the circumstances described, in the secondary class; namely, that when met with in acquired syphilis they are distinctly secondary. Both are, then, very rare. Of the internal otitis I have seen but few examples, and of the keratitis but one; all the cases, however, occurred to those who had but recently acquired their disease. Until within the last three months, I have always been inclined to deny that the interstitial keratitis common in inherited syphilis had any analogue in the acquired disease. There is, however, at present in the Moorfields Hospital, under Mr. Wordsworth's care, a case which, as far as my own observation has gone, is unique. It is an instance of acute symmetrical keratitis in connection with acquired syphilis, and it occurs simultaneously with a general papular eruption, and without doubt within a few months of the primary sore. Thus it would appear to be a fact in hereditary syphilis, that conditions which in the acquired disease would occur within the first year, may be delayed for ten or twenty. Next let us note that the conditions which chiefly attract attention in the tertiary stage of acquired syphilis are almost wholly absent in the subjects of inherited taint. Large gummata in the cellular tissue, in muscles, in the tongue, in the viscera, or in connection with the cerebral meninges, are almost unknown. The paralysis of single muscles of the eyeball, or of those of the face; the attacks of hemiplegia, and of optic neuritis, with which we are so familiar in subjects of acquired disease, are, I think, never seen in the inherited form. The able work of Dr. Buzzard on *Syphilitic Nervous Affections* does not record a single example of such in connection with inherited taint; and although Dr. Hughlings Jackson has with his usual zeal availed himself of almost unequalled opportunities, he has succeeded in getting together but few cases—and none exactly of the kinds I have specified. It is true that epilepsy and some forms of idiocy and insanity, and some other anomalous spinal symptoms, are met with in heredito-syphilitic patients; but, I repeat, we have none

of the cases in which symptoms are supposed to be due to gummata, whether of nerve-trunks, of the arterial coats, or of the coverings of the brain.

To the last-named author we owe most of our knowledge as to the discrimination of the different anatomical lesions which cause nerve-symptoms in syphilis. It is mainly in consequence of the elucidation of the subject due to his labours that I am enabled to make the statements which I now record.

It is clear that we have yet much to learn respecting the laws of hereditary transmission. As just remarked, it is exceedingly difficult to assign reasons why phenomena which in acquired disease are crowded together in the first year or two, should in the inherited form be separated by ten or twenty. Nor can we offer any explanation of the fact that, in the late periods of the inherited disease, there is no tendency to the formation of the neoplasms frequently seen in the acquired form. I said that the chapter describing the internal pathology of the secondary period in acquired disease had yet to be written, and I may now say the same as regards that of the tertiary period in the subjects of inherited taint. With but very rare exceptions, the latter do not appear to suffer from any maladies which tend to shorten life. I made this statement in print many years ago, and nothing has occurred since to induce me to modify it. Although I have had constantly under observation numerous adolescents and adults well known to have suffered severely from inherited syphilis, neither in them, nor in their brothers or sisters, have I encountered any special disease of important organs. They have suffered in their eyes and ears, and sometimes in the skin; but here, for the most part, their liabilities seem to end. I do not think that I have made, or known made, more than half-a-dozen *post mortem* examinations in such subjects, and in these for the most part no very noteworthy lesions were found. In two such, death was preceded by long continued albuminuria, and suspicion occurred that possibly the very long continued use of iodide of potassium might have had something to do with it. So meagre is our knowledge of this department of the pathology of syphilis, that our Society would, I think, do well to encourage the production before it of all evidence which may in the future be obtainable, with a view to its permanent record in our *Transactions*.

I now come to the last of the questions which is on my list for consideration. Is there any reason to believe that a taint of syphilis may mix itself with other causes of disease, and produce results of a hybrid nature? It is to scrofula and scrofulous disorders that chief reference is here made. Does syphilis in any way predispose to scrofula or tuberculosis? Vague suspicions have been entertained on this head almost since the earliest date of recognition of constitutional syphilis; and opinions, often of a very sweeping tenor, have been expressed with great confidence by many authorities. Formerly, indeed, it was a question very difficult of investigation. I would venture, however, to submit to the Society, that the facts which have been accumulated during recent periods, and the new symptoms which have been placed at our disposal, justify us in believing that syphilis produces in all its stages special and wholly peculiar lesions; and that although these may easily be mistaken for struma, they have in reality nothing whatever to do with that state of constitution. It is undeniable that syphilis, and sometimes the treatment required for it, may for a time enfeeble the system, and that during this state of debility a person predisposed to phthisis or struma may experience an increase of his proclivity. In this, however, there is nothing more than what is common to all causes of enfeeblement, whether special or otherwise; and I do not think that in the instance of syphilis it amounts to much. Syphilis may be contracted by persons of the most various states of health, and by those who are the subjects of special diatheses. Strumous or tuberculous subjects, those who suffer from gout or from psoriasis, and those of extreme enfeeblement of circulation, may all in turn present us with examples of this specific disease. As a rule, we do not observe any modification of the one by the other. A common psoriasis rash may persist during the treatment of constitutional syphilis, and may remain for years afterwards, just as it did before; and the same is, I think, true in the general way of lupus, leprosy, and other chronic maladies which show their chief symptoms in the skin.

In investigating the relationships between scrofula and syphilis, we might conveniently take as type-examples of strumous manifestations the common and fairly definite conditions of ulcers in the cornea and lupus. We have chiefly to consider the hereditary form of syphilis. A large majority of examples of chronic ulceration of the cornea (strumous ophthalmia) occur in those who show no signs of hereditary taint, but who, either in themselves or near relations, betray a tendency to other forms of scrofula. When such ulcers happen to those who are known to be the subjects of inherited taint, for the most part they run their usual course and require their usual remedies, while syphilitic

keratitis is the same malady in the strumous and in the healthy. I do not say that this is invariable, but I think it is the rule. Respecting the common and interesting disease of the skin known as lupus, grave suspicions are still entertained by many who are well informed. To my own mind, however, the evidence is clear. True lupus has nothing whatever to do with syphilis, either acquired or in the first or second generation of inheritance. The evidence on this point is of two kinds. In the first place, we hardly ever, as far as my own experience goes—I might, I think, say never—meet with common lupus in those who are obviously the subjects of inherited taint. It must be granted, however, in the fullest manner, that not all, or nearly all, of those who really inherit a taint betray it either in physiognomy, teeth, or by concurrent disease of suspicious character; and, further, that it is precisely in those who do not so betray it that we must expect the history of symptoms in infancy to be wanting. In these cases, however, it is seldom that the patient is an only child, and it is very rare for a whole family to escape. If the surgeon will widen his inquiry, and, instead of contenting himself with the original patient, inspect carefully all the brothers and sisters, he will generally find evidence that is conclusive. In this way the recognition of signs of inherited taint in one individual very commonly reveals its existence in possibly a quite latent form in three or four others. We identify not merely an individual but a whole family, and thus very much extend our area of observation. Making use of the symptoms of physiognomy, teeth, keratitis, and choroiditis, I do not think that it would be difficult in any large out-patients' institution to soon make the acquaintance of several hundred individuals concerning whom the observer might be sure that they all, in greater or less degree, inherited a taint of syphilis. Anyone who had done this would be in a position to say whether lupus occurred in more than average proportion amongst this class; and I have no doubt as to what his verdict would be. It is difficult to leave this part of our subject without suggesting that most valuable work in this direction, not only as regards lupus and scrofula, but in reference to the late results of inherited taint, might be done by those of our members who have the advantage to be engaged in family practice. If all the cases in which family advisers are aware of the fact, either of the existence of syphilis in parents or its actual presence in offspring, or both, would carefully note all that occurs to all the children in such families, not only in childhood, but in after life, a mass of facts might soon be collected which would help greatly to the solution of many important questions. It is far more difficult to follow out these inquiries in hospital practice.

There is yet another line of argument as regards the non-connection between syphilis and lupus. We have said that lupus does not occur frequently in the hereditarily syphilitic, or their brothers and sisters. Next, I assert that amongst those who have lupus, and their relations, you will find, as a rule, not the slightest reason for suspecting specific taint. No one can, I think, doubt for a moment that lupus may occur under conditions which make the existence of syphilis either in parents or grandparents most improbable, nor, indeed, that it is, for the most part, under those conditions that we usually meet with it. Now, as lupus is a definite and peculiar malady, the probability is that it has a definite cause, and it is not due in one case to the tubercular diathesis, and in the next to that of syphilis.

There is no doubt that syphilis may produce skin-diseases which superficially very closely resemble lupus. This occurs most frequently in the acquired form of the disease, and, in a few of these, the diagnosis is really very difficult. The essential difference in nature is, however, usually proved at once by the results of treatment. In inherited syphilis, we seldom or never see anything which at all resembles the tubercular or common form of lupus. We do meet not unfrequently, however, with a disease which formerly was called phagedænic lupus, but which, like other varieties of phagedæna, is always and wholly the result of syphilis. It spreads far more rapidly than true lupus, and is easily curable by measures which would avail but little against the latter. Above all, it is easily distinguished after cure by the fact that its scars remain perfectly sound without the least tendency to the relapses which are so constant after cures of lupus.

Here, Mr. President, I must draw to an end. In attempting to fulfil the responsible duty with which the kindness of your Council has honoured me this evening, I have thought it better to avoid all subjects of mere detail, such as might seem to be more appropriate for special communications. I have kept two points chiefly before me: to try, if possible, to simplify and make more orderly our general view of the subject; and to direct attention to those parts of it which seem most to need further investigation. To my great regret, I have, under compulsion as to brevity, found but little opportunity for mention of the names of those to whom we are indebted for the facts and suggestions of which I have freely availed

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, Dittich 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.,

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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