AN ADDRESS

ON

CERTAIN POINTS IN THE PATHOLOGY OF SYPHILIS.

Delivered at the opening of the adjourned Discussion at the Pathological Society of London, on February 15th, 1876.

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I DESIRE to begin by expressing my agreement in the main with the doctrines of syphilis which Mr. Jonathan Hutchinson propounded at the last meeting of the Society. I agree with him in the belief, if he will allow me to quote his own words, that in syphilis we have to do with but one malady and one virus; that in syphilis we have to deal with a specific fever of prolonged definite stages, and that it is the sequelæ of the fever to which we give the name of tertiary symptoms.

Mr. Hutchinson has rightly indicated some of the points to which he would suggest that the discussion of the subject should lead; and I will take one of these, namely, the relation of the tertiary symptoms which he regards as the sequelæ of the syphilitic fever, if I may use the term. In this view, I do not pretend to add any new facts to those which Mr. Jonathan Hutchinson has himself stated. If I can add anything to strengthen the arguments he has adduced, I think it can only be by pointing out the very close resemblance which exists between the tertiary symptoms and those which may be regarded as sequelæ of other forms of fever. The very characters which Mr. Jonathan Hutchinson has pointed out as indicative of the tertiary period, the sequel of the syphilitic fever, are, first, that the symptoms which it presents are not symmetrical; then, that it is a disease which is not contagious, nor yet, so far as experiments have yet proceeded, distinctly communicable. Moreover, I suppose it may be added that it is very seldom transmissible by inheritance.

Let me take occasion to speak personally of my gratification that the doctrine of the symmetry of disease is upheld upon so strong grounds as those which Mr. Jonathan Hutchinson has introduced; for, so far as I know, the full significance of symmetry as characteristic of a blood-disease has scarcely been dwelt on since it was the subject of papers communicated to the Royal Medical and Chirurgical Society by Dr. William Budd and myself; and if, in speaking of syphilis as a blood-disease, Mr. Jonathan Hutchinson has raised some strong questions in modern pathology as to whether the term blood-disease can, under any condition, be justly used, I will recommend those who object to the term to read carefully the paper by Dr. William Budd in the twenty-fifth volume of the *Medico-Chirurgical Transactions*. I think they will find there something as near to demonstration as can be arrived at in pathology, that diseases which are symmetrical owe their existence to a morbid material in the blood; and that those which are regarded as the morbid products of the disease shown symmetrically, are really the morbid material combined for a time with the natural texture of the parts affected. To that doctrine I still hold; and I may have occasion to refer to it in that meaning many times in what I say. The years that have passed—more than thirty now—have shown, doubtless, that the nervous system has a larger share in the government of symmetry in disease than either Dr. William Budd or myself at that time supposed. Nevertheless, the main facts hold true that, as Mr. Hutchinson has illustrated, a symmetrical disease does, by its symmetry, indicate the presence of a morbid material in the blood. Moreover, I would add to what he has himself demonstrated, that the product as we call it, the appearance of the disease, is due to the combination of the morbid material with the natural textures of the parts.

Mr. Jonathan Hutchinson asks, as one of the first questions which he submits for the consideration of the Society—if I may again use his own words—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? Now, I think, that to answer that question we must inquire what are the characteristics of the sequelæ of other fevers in reference to symmetrical disease. It is clear that the sequelæ of all other fevers are, with very rare exceptions, not symmetrical. There are the sequelæ of pyæmia, the pyogenic fever; its phenomena, in its acute stage, are rarely symmetrical, but when it passes into its sequelæ in the chronic stage its phenomena are perhaps still more rarely symmetrical. They indicate a singular

similarity to the tertiary symptoms of syphilis, in that they very commonly affect always the same textures, as, for example, a large number of them are shown only in suppuration in the connective tissue; or in other persons they appear only as slowly successive instances of necrosis, herein indicating one of the most marked features of resemblance between the sequelæ of the best marked acute fever, and of that which some may deem the least marked—the syphilitic fever. But they are not symmetrical.

Again, intermittent fever is a fever well to be studied in its likenesses to syphilis, because it passes even beyond syphilis in the duration in which it will hold itself, as I suppose, in the blood of the patient. Thirty years are referred to as the time that syphilis may continue without intervening symptoms. It is no uncommon thing for ague, with very rare appearances, to be repeated in its phenomena thirty years after the time of its probable infection; but it is notable that what may be regarded as the sequelæ, or the latest symptoms, of ague, are not symmetrical. They are best of all marked in the various neuroses, which are recognised under the terms of brow-ague, migraine, and others, for the most part not symmetrical affections of the nervous system.

Then I may instance the sequelæ of typhoid fever, a subject which, I venture to think, has not sufficiently attracted the attention of physicians, probably because the cases more frequently fall into the hands of surgeons. Within some years past I have been struck with the great variety of surgical sequelæ of typhoid fever, and amongst them I have most rarely indeed met with instances of symmetrical disease. Among the instances of necrosis as the sequelæ of typhoid fever, ten or twelve in number, I have seen only one symmetrical—a well-marked case of symmetrical necrosis of the lower ends of both tibiae; but in all others, whether affecting the tibia, the ulna, cranium, or other bone, there has been always an absence of any sign of symmetry. So, in the case of phlebitis, which is common in the femoral vein as a sequel of typhoid fever, I have never seen symmetrical phlebitis. Nor have I seen a single instance of symmetrical affection of the nervous system as a sequelæ of typhoid fever, except in one or two cases, where complete paraplegia was produced; but in all those which are more common, paralysis of the anterior muscles of the leg, or of part of the muscles of the arm, or any other, I have not yet met with one instance of symmetrical paralysis. Lastly, with regard to an affection which is not unfrequently seen as a sequelæ to typhoid, suppurative periostitis of the ribs, six or eight of those cases that I have seen have shown no sign of symmetry.

Therefore, I think we may hold that in this absence of symmetry in these sequelæ, that is, in its tertiary symptoms, the syphilitic fever, if I may so call it, shows a remarkable feature of resemblance with all those other more acute fevers which we can trace in producing any sequelæ. And in this I suppose it only falls in with what is generally true concerning symmetrical diseases, or diseases of the blood, that in their later periods of existence they all become irregular, that is, they not only lose the regular character of symmetry as affecting both sides of the body alike, but they lose many of the regular characters of form and size in external appearance with which we are familiar. For example, if we watch lepra, or psoriasis, perhaps the best marked instance of symmetrical eruption, if after a time it spread from the limbs over the trunk, or from the upper extremities over the face, in its later periods it commonly ceases to be distinctly symmetrical. Or again, if I may cite an instance of what is not properly, I believe, to be called a blood-disease, yet is distinctly symmetrical (that is, the so-called pityriasis versicolor): it is symmetrical in all its early stages: when it is declining, it does not decline or fade with symmetry; or if it relapse, it does not relapse with symmetry. So with rheumatic gout, or with cancer: in the condition in which I suppose, even in this room, it will be generally understood to be a blood-disease, when it has thoroughly infected the blood in its later stages, and appears in multiple forms, they are not commonly symmetrical.

Therefore I think the rule may generally be, if we are to answer Mr. Jonathan Hutchinson's question as to whether there be indications that in the absence of symmetry a blood-disease has ceased, that the cessation of symmetry as a characteristic of syphilis is exactly accordant with its absence in the sequelæ of other fevers, and generally with its absence in the periods of decline of nearly all diseases that begin by being symmetrical, in other words, in nearly all blood-diseases. And I apprehend that the reason of this decline may be assigned not only, as Mr. Jonathan Hutchinson assigns it, to the influence of antidotal remedies, but, besides, to the constantly increasing prevalence of the natural nutritive forces over the morbid ones. For we may understand that syphilis is a disease which does not usually tend to a continual increase and fatal termination, that it is constantly exposed to the strife with the natural nutritive forces, which will not, indeed, simply diminish it in intensity, but, if I may so speak, gradually break it up

by making it irregular, and amongst the first signs of its irregularity make it not symmetrical.

But, to revert to Mr. Jonathan Hutchinson's question, whether the absence of symmetry may be taken as the indication of the arrest of blood-disease, I must observe that in the hypothesis which follows in his paper, in which he assigns the occurrence of the tertiary symptoms to changes of structure ensuing in the residues of the secondary deposits, he would have to admit that in those residues the syphilitic disease shows itself as secondary, and therefore, as it should be, symmetrical; and he would then have to explain why, being symmetrical in the forms of the secondary period, it becomes unsymmetrical in the manifestations of the third. If he will let me venture to improve his hypothesis, I would suggest that those of which he speaks as the residues of the secondary period are only such as are formed in the period of irregularity which follows the secondary and precedes the tertiary: a period of irregularity in which we should expect that, after the common manner of all blood-diseases, symmetry would cease to be observed, or would, at least, be gradually less and less distinctly observed. In that case, I can fully adopt his hypothesis of the deposits which reappear in the tertiary period being produced in the secondary.

But as to whether the tertiary symptoms may be regarded as not due to blood-disease because they are not symmetrical, we must look to further characteristics for decision; and I would venture to believe still that syphilis, so long as it exists in any manifestation at all, is a blood-disease. It may be supposed that the secondary deposits, as the results of blood-disease, may be renewed into disease by any external cause, but this would hardly explain several features of tertiary syphilis. First of all, local diseases we scarcely understand to be entirely of spontaneous origin; yet the outbreak of a tertiary syphilitic attack is to all appearance as spontaneous as anything that we see in disease. Then, in local diseases, we commonly suppose that the evidence of disease will bear some kind of proportion to the force with which it has been produced; and yet certainly tertiary syphilis in great severity may be produced or made manifest by an external force very small in amount. Then, again, most of the tertiary syphilitic symptoms have their distinct, or, as they are commonly called, specific, forms, manners of nutrition, methods of disease, by which they are sufficiently distinguished in our diagnosis from anything that could fairly be called common disease. We can usually distinguish a syphilitic ulcer of the tertiary period from any ordinary ulcer. Very commonly we can distinguish a syphilitic joint from a rheumatic or a gouty, or a simply inflamed joint. Very commonly, too, we can distinguish a syphilitic periostitis from a common one.

Then, lastly, as another sign of specific character, and therefore, so far, probably of persistent blood-disease, we can discern that, in certain cases, the tertiary syphilitic disease requires specific treatment. I entirely agree with the ingenious expression of Mr. Jonathan Hutchinson that it may be local treatment which we address to a disease, even when we put the remedy into the blood, that we only—if I may so say—use the blood that it may carry the remedy into the substance of the diseased part; and yet it is plain that there are certain tertiary syphilitic diseases accessible to external treatment, as well as those which would be considered internal, but not curable by external treatment. The annular and crescentic ulcers of the tertiary and syphilitic periods are, so far as I know, not curable by any external treatment. We may bring iodide of potassium, or mercury, or any other treatment, to their surface, and give it every opportunity of reaching their very substance, but we do not thereby cure them; we need for the cure of them, as much as for primary syphilis, either iodide of potassium for their passing cure, or mercury for their final cure.

Therefore, out of these facts, I should answer, so far as I can, that question of Mr. Jonathan Hutchinson, by holding that, to the last, so long as there can be any evidence of its existence, syphilis must be regarded as a blood-disease in its last stages, illustrating in the best way the manner in which a blood disease gradually ceases, whether diminishing in intensity or in the quantity present in the blood, becoming irregular, and at last ceasing to be a blood-disease, and losing all specific character.

I come, next, to that to which I have already adverted, the hypothesis—a most ingenious one, and one in which I am nearly sure there is some real value—namely, that the gummata of the tertiary stage result from renewed growth in forms left over from the exanthem stage; that, if I may express it otherwise, the exanthem stage of syphilis leaves behind it certain residues in which the disease may be renewed by any casual external conditions. I cannot cite instances of this from my own observation in syphilis; but instances may be cited in general pathology and from common diseases which, at least, will go far to render it not improbable that that may be a true hypothesis, even although there may not be discernible in the parts which become seats

of the tertiary syphilitic disease any change of structure whatever. I published, some years ago, an instance which occurred in my own person, in which a residue of disease may be left unobserved, and yet ready for outbreak upon any fit occasion, in regard to some scars I had from the application of leeches. I had applied leeches to my wrist for a sprain of it, and four or five years elapsed with no trouble whatever in the scars, except that I could count them on my wrist. At the end of five years, when the scars might have been deemed to be as sound as any other texture of the body, I was for the first time exposed to heat and other conditions of travelling in Italy, and then at once every scar inflamed and ulcerated; and so it occurred for every year in succession when I was exposed to the same influence; every scar and every leech-bite inflamed and suppurated again, and it took four years of similar exposure to those causes of disease before they settled down into a condition in which they could resist even that.

And, if I may tell another personal experience, it will bear with some force on the question of the necessity of studying residues of disease. Four years ago, I had an attack of acute erysipelas following dissection-poison. It affected only my left arm and part of my left side, the right arm being wholly unoccupied by it. My left arm recovered, as I supposed, without a blemish in it. For years, I went on without the smallest consciousness that there was anything in that arm which could be affected by any force which would not equally affect the other arm. This winter—I hardly know why—I took cold baths instead of warm ones, and, on every application of cold water to the arm, in which, let me repeat, I am wholly unconscious of any difference from the right arm, there ensues lividity; there is a marked difference between the left arm and the right; such a difference as would spoil the symmetry of any constitutional disease with which I may be affected; such a difference as might be marked by a greater tendency to production of disease in that limb than in the other: a clear residue, not discernible by any other means than those applications of cold; a residue very similar to that which may occur after any specific disease to which any man may be exposed. I am disposed to parody the well known expression, "Let no man be accounted happy until he is dead", by "Let no man be accounted healthy until he is well examined after death".

There is only one point more in Mr. Jonathan Hutchinson's paper to which I will refer, and that is one in which I feel bound for the first time, not materially, yet in a measure, to differ from him. He enters upon this question—one of the greatest interest—in reference to the relation which syphilis bears to other diseases, and asks, Does syphilis in any way predispose to scrofula or tuberculosis? In answer to this, I should agree with him absolutely that it does not so predispose in any other sense or degree than any other fever does. Syphilis, by long-continued illness and deterioration of health, may let in scrofula or tuberculosis; but so may rheumatism, so may gout, so may typhoid, so may any other disease which, to use the plainest word, in any degree impairs the health of the person disposed by inheritance to scrofula or tuberculosis. Then he speaks of the relations of lupus to syphilis. And there, too, I agree with him wholly that lupus is in no proper sense a syphilitic disease. It is definitely a disease of tuberculosis. I doubt whether any persons the subjects of lupus can be found in whom there might not be traced all their well marked inheritance of tuberculosis; and, if there be a syphilitic lupus, I should hold that it can be found only in persons who become syphilitic after having inherited the constitution out of which lupus is likely to occur; but, when Mr. Hutchinson says that, as a rule, we do not observe any modification of one by the other—referring there to syphilis as it may occur in strumous or tuberculous people—in those who suffer from gout or psoriasis, and those who have extreme enfeeblement of circulation, I cannot but differ from him. I think that one of the things we have most to study, both in the pathology and treatment of syphilis, is the modifications which it undergoes in persons of different constitutions in whom it may be inserted.

I should hold this even in regard to the primary syphilitic sore. Agreeing with Mr. Hutchinson that there is but one syphilitic virus, I yet cannot but feel that we have to explain the singular variety of forms in which that virus, or the pus in which it is contained, produces its effect in different persons. We may suppose that in some persons the pus is inserted without the virus, and in others not; but it would be an extreme difficulty to tell any means by which the virus can be separated from the pus. I think we should look for facts which would indicate that, out of the constitution of each person into whom the syphilitic virus is inserted, there may come a different—not essentially different—but a modified result. The facts, indeed, should be more like those which may sometimes be discerned even now in the insertion of the vaccine virus, to which some are less and others more

subject, and some absolutely indifferent, or such affections as we have on record from the old histories of inoculation, where the disease produced was the same in kind, but very far from the same in degree, in all persons in whom it was inserted. So, too, in the secondary period, I think we may yet see, however obscurely, that there are some hints of differences due to the difference of the constitution of the person in whom the secondary disease occurs, but much more in tertiary syphilis, in that period in which the syphilitic virus seems to be losing its power or intensity, and the natural constitution of the person comes to have more and more share in the general product of the disease.

I would not venture to call the disease that may occur in a scrofulous person, become syphilitic, a hybrid one; and yet, perhaps, the term is not altogether wrong; but, at least, I would call it a mixed disease, and hold that syphilis inserted in a scrofulous person will, in its tertiary period, produce signs which it may be very hard to distinguish from scrofula—signs in which the characters of scrofula and of syphilis are mingled, and—which is very important—which require that the treatment of scrofula should be combined with the treatment of syphilis, in order to produce a fully successful result. So, too, in gouty persons, and in the rheumatic and the neurotic, I think we can discern mingled characters of syphilitic disease with the diseases to which their constitution might be liable; and I think I can be sure, too, that all these several persons require for their treatment that we should have in view not only the specific disease, but the constitutional disease with which it is mingled; so that the treatment for the constitution natural and the constitution acquired may be combined.

And this is quite confirmed by what we observe in the sequelæ of other fevers; for clearly, when we watch the sequelæ of typhoid fever, we have to explain why one person has phlebitis, another necrosis, and another suppurating disease of a rib; and I believe that those differences may be explained in discerning different constitutional or even different local peculiarities in the persons affected. I am acquainted with an instance in which five members of one family, having suffered from typhoid, have all had affections of the nervous system as its sequelæ; and the worst case I have yet seen of crural phlebitis following typhoid is the case of a gentleman in whose family I know of four other persons all suffering from disease of the veins of the lower extremities. These, it may be said, are instances in which the sequelæ of the disease falls only on a part which may be considered weak; nevertheless, that which is true of a part would also probably be true of the constitution; and, at least, we have to keep in mind that, among the many diseases which we have to study, few are so variable in their phenomena, taking them altogether, as syphilis.

Now, we have cast aside—at least, I have, with Mr. Jonathan Hutchinson—the belief in the multiplicity of poison. But, if there be one poison only, how is it that that one poison produces so many various effects? We cannot ascribe it to the various external conditions in which the patients are placed; neither can we reasonably ascribe it to the variety of treatment; for the treatment of syphilis, as now carried on, is, for the most part, very similar in all cases. It seems to me that the only thing to which we can fairly look is to discern the differences produced in the one disease by the variety of persons in whom it is inserted—the variety of soils, if I may so speak, in which it has to develop and to grow.

These are all the observations that I would make in regard to Mr. Jonathan Hutchinson's paper; but I would not sit down without expressing my admiration of the paper as a whole, and of the thorough, profound view which Mr. Hutchinson has taken of the whole subject. I venture to make myself proud in calling Mr. Jonathan Hutchinson one of my pupils—not my pupil in syphilis. With regard to that, I say nothing, but that, at least, I did not interfere with his studies while he pursued them with myself in the out-patients' room of St. Bartholomew's Hospital, where I saw nothing but confusion, and he saw light come; and I venture to say, if any of us think that in this instance Mr. Jonathan Hutchinson is looking into outer darkness for more light in the pathology of syphilis, we shall be very unwise if we neglect his signals when he says that light is come.

SALICIN AS AN ENTERIC TONIC AND ASTRINGENT.—Dr. W. H. Vail extols (*New York Medical Journal*, October 1875) salicin as an enteric tonic and astringent. "It combines," he says, "the tonic element of quinine with strong astringent properties. In cases of enteric atony, especially in chronic cases, from chronic diarrhoea or other debilitating diseases, where quinine is inadmissible from its tendency to irritate the bowels, in my hands salicin has given the most flattering results." He always prescribes it in powders with sugar and water. It is readily taken by children, he says—more easily than quinia, though more bitter. The dose is the same as of quinia.

THERAPEUTIC MEMORANDA.

INTERNAL ADMINISTRATION OF TAR IN PSORIASIS.

IN last Saturday's number of the *JOURNAL*, two cases of psoriasis treated by means of the internal administration of tar, and without success, are reported by Dr. Clay, and he concludes with the following remark.—"In a previous number of the *JOURNAL*, Dr. McCall Anderson, while upholding the usefulness of tar internally in psoriasis, states that perhaps the reason why his cases were more successful than Mr. Balmanno Squire's might be because of the difference in the dose and mode of administration. In the above cases, the same method as Mr. Balmanno Squire's has been followed, and both large and small doses have been given without any benefit." I think it right, therefore, to say that it is only a certain proportion of cases to which the tar treatment is applicable (e.g., cases in which there is functional derangement of internal organs which would be aggravated thereby); that the mode of administration adopted by Dr. Clay is not that which I have employed; for I prefer giving the tar mixed with treacle; and that no number of cases treated unsuccessfully can alter the fact, which I have over and over again demonstrated clinically, that tar is sometimes successful after arsenic and other remedies have failed.

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THE TREATMENT OF PSORIASIS BY INDIA-RUBBER UNDERCLOTHING.

THE two cases published by Dr. Clay in the *JOURNAL* of February 12th have given value to the cases I summarised in a previous number of the *JOURNAL*, as contradicting the belief that tar administered internally was any assistance to outward tar in the treatment of psoriasis. The mode in which Dr. Clay's experiments were conducted is an example of the manner in which hospital practice may be advantageously utilised by the employment of one remedy, and that in one manner, at a time when it is desired to ascertain its positive value.

I desire to lay before him and others who may be actuated by the same tone of investigation of a simple kind into the action of remedies, two cases of psoriasis, now in-patients of the British Hospital for Diseases of the Skin, treated by me in a simple manner by an agent of a different sort.

CASE I.—A woman, aged 30, had been affected with chronic psoriasis in a moderate degree for twenty years, and, in addition to this, had for the past five weeks been the subject of an acute general attack of guttate psoriasis, which freely occupied every part of the skin, with the exception only of the fingers and the palms of the hands, and the toes and soles of the feet, but most copiously the front and inner surfaces of the thighs, the lower half of the abdomen, the loins, and the front of the legs and the back of the forearms. On February 1st, 1876, she commenced wearing a complete suit of vulcanised India-rubber underclothing, together with an oil-skin cap, washing herself daily all over with warm soap and water. On February 8th, the scabiness, the thickening, and redness of the skin at the spots had quite gone, and the eruption consisted merely of faded yellow stains, with the exception of a few spots on the legs. On February 12th, the stains had become much fainter, those on the forearms requiring to be looked at "across the light" in order to make them out.

CASE II.—A girl, aged 18, had been affected with chronic psoriasis for twelve months. The eruption was scattered in patches of varying size over the trunk and limbs (the larger patches occupying the front and outer surface of the lower part of the thighs and the front of the upper part of the legs and the back surfaces of the forearms and lower part of the arms), and on the scalp. On February 1st, 1876, she was clothed like Case I, and washed herself daily in the same way. On February 8th, half the patches had disappeared, leaving only faint yellow stains. All the others had in part disappeared, some (for example, the two largest patches situate on the knees), having become only broken rings, which, a week before, had been large, filled in, figurate patches. On February 12th, quite three-fourths of the whole number of patches had disappeared, and the vestiges of the remaining fourth had become still scantier.

REMARKS.—The device is not my own, but that of Dr. Colson of Beauvais. Experiments of this kind have since been pursued by Dr. McCall Anderson of Glasgow, Drs. Hardy, Lailler, and Besnier of the Hôpital St. Louis, Paris, and Dr. Hebra of the General Hospital, Vienna. No distress is produced by this kind of underclothing, nor, indeed, inconvenience of any kind, save only that, if the wearer exert himself or herself at all actively, a moderately profuse perspiration is the result.

BALMANNO SQUIRE,
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