the cells of the auditory perceptive centre by means of fibres connecting together the two: an explanation, perhaps, of the fact that fibres from the corpus striatum and thalamus pass to all marginal convolutions, whether in the motor or sensory regions of the hemispheres. And, as the motor nuclei of the cord can still be employed in reflex action through the sensory nuclei, as well as in voluntary motion by means of descending fibres from the corpus striatum, so may the word-groups in the corpus striatum be reached imitatively through the auditory perceptive centre, as well as through the third frontal gyrus.

We come now to an important consideration. The auditory sense-centres and perceptive centres concerned in the formation of the word-groups being bilaterally associated, and the motor nerve-nuclei concerned in the utterance of words—those of the chest, larynx, tongue, and lips—being also bilaterally associated, the word-groups must be formed in both corpora striata, although the left hemisphere only is educated for speech. Were it otherwise, and were word-groups formed in the left corpus striatum only, we should have aphasia equally from damage to this ganglion as from lesion of the third frontal gyrus of this side, which is not the case. Right hemiplegia is common enough without aphasia, and I know of no case on record in which lesion of the corpus striatum not extending higher has given rise to loss of speech. When the left corpus striatum is destroyed, the direct way out for words being destroyed, a way round is taken, starting from the third left frontal to the third right frontal gyrus, passing thence to the word-groups in the corresponding corpus striatum; thence, of course, to the bilaterally associated nerve-nuclei in the pons, medulla, and cord. This, together with one or two other points, will be better understood by reference to the accompanying diagram, in which the parts concerned are indicated by

Fig. 9.

their respective initials. The left third frontal gyrus (L. 3rd F.) being considered as the instrument of the brain for speech or intellectual expression, loss of speech or aphasia may be produced (a) by destruction of its grey cortex; (b) by its isolation through damage involving, say at r. c., the fibres connecting it with the corpus striatum and those crossing in the corpus callosum (C. C.) to the corresponding gyrus on the right side (R. 3rd F.); (c) by simultaneous lesion of the left corpus striatum (L. C. S.) and of the corpus callosum (C. C.)

The left corpus striatum (L. C. S.) being destroyed, aphasia is avoided, as stated above, by speech crossing from L. 3rd F. by C. C. to R. 3rd F., and descending thence to R. C. S. and the nerve-nuclei. The difficulty in taking this new route explains the great temporary embarrassment of speech often met with in right hemiplegia. It should not be understood that the corpus callosum, right third frontal gyrus, and corpus striatum are habitually passive. I should conclude that they are normally employed in every act of speaking; my idea of the co-ordination of centres on the same level being, that co-operation is obligatory; that, in fact, centres so connected hold each other in mutual tension, discharge of one implying discharge of the other. A higher centre controls altogether a lower centre, and discharges or inhibits it, so to speak, at will. Stammering I suppose to be due to want of accord between the discharges of the two corpora striata, so that the bilaterally associated nerve-nuclei in communication with both receive discordant impulses; and not to want of accord between the muscles of phonation and those of articulation.

There are still many points of great interest which ought to have been considered. It has been difficult, indeed, to decide what should be discussed and what passed over. Fortunately, there are limits of time and space, or, in the attempt to make this lecture comprehensive, I might have made it tedious.

TESTIMONIAL.—Dr. Robert S. Archer, on retiring as House-Surgeon to the Weston-super-Mare Hospital and Dispensary, has been presented with a gold pencil-case by the resident staff, and a handsome oak-cased tankard and goblet mounted with silver, and an address, by two hundred poor out-patients.

REMARKS ON THE PATHOLOGY OF SYPHILIS.

BEING THE

Concluding Reply in the Debate at the Pathological Society of London, Tuesday, April 4th, 1876.

By JONATHAN HUTCHINSON, F.R.C.S.,

Senior Surgeon to the London Hospital, and to the Blackfriars Hospital for Skin-Diseases; Surgeon to Moorfields Ophthalmic Hospital.

It will, sir, I think, have been obvious to most that two principal ideas ran through the remarks which I had the honour of addressing to the Society in introducing this discussion. The first and most important was, that syphilis depends upon the introduction into the system of a living material which is capable of self-multiplication, which breeds in the blood and tissues, and which is destined to pass through various stages of development, and finally to die. My second idea was, that, in order to the correct clinical comprehension of all that follows on syphilitic poisoning, we must admit, in connection with the phenomena which rank as its direct results, certain less closely associated peculiarities of inflammation and of cell-growth. In this way, I tried to show that soft chancres might result from contagion with pus produced by syphilitic inflammation, but not actually containing the syphilitic spores; that phagedæna in like manner might produce a contagious secretion, but not a syphilitic one, and that it might wholly escape the restraint of the specifics for syphilis, and that a gumma might grow long after the patient had ceased to possess either in his blood or tissues the living virus of his original malady. These various events were, I suggested, to be reckoned rather as adventitious to the syphilitic fever than as true parts of it; and to the later forms of gumma, etc., known as tertiary symptoms, I thought the name *sequelæ* the most appropriate, meaning by that term that they came after the true syphilis was over, just as we know that as a sequel to war may come a famine, though the war itself is ended. It will, I think, be readily granted that the correct appreciation of the relationship of the soft sore, of phagedæna, and of tertiary symptoms, to syphilis itself is all important as regards our insight into the kinship of the latter to other more short-lived exanthems. If the tertiary symptoms are not, in a strict sense, a part of syphilis, and if in reality the latter be usually over in a year or two from its outset, we are able at once to realise much more easily the kinship referred to.

Now, in looking back on the very important criticisms which have fallen from the various speakers who have so ably sustained the debate which ends to-night, I find, on the whole, a very considerable amount of concurrence in the views expressed; by no means, however, without some expressions of misgiving and dissent. By one or two it has been hinted that there is no proof of the existence of a germinal poison as the cause of syphilis; others have thought that the analogy with specific fevers has been pushed too far; that the stages of syphilis are so irregular, that they can scarcely count as such at all, and that it is wiser to speak of the disease as one whole; whilst many have expressed doubts as to whether it be possible for any results to persist, unless the blood still continue tainted. To take the first of these objections, I have to reply to Mr. de Méric—who reminded me that the germ-poison of syphilis had never been put under the microscope—that surely there are cases where deduction amounts almost to proof. With regret I admit that I have never seen with my outward eye the cryptogamic germ-poison of syphilis; but to my mind's eye it is as certainly present as if I had. Someone will see it some day, for it is beyond doubt that it must be there. I would even venture to suggest that it might be wise to anticipate discovery a little further, and to speak of this *contagium vivum* as the *syphilitic yeast*, so that we may force our minds to keep clearly in view the possible developments of this theory both as regards the inherited and the acquired disease. When a better theory is forthcoming, we may lay this aside, together with the forms of phraseology which it has brought in; but I cannot but note, as a remarkable result of this discussion, that, despite certain objections to

this, there seems to be no other theory in the field. For the present, to abandon the germ or yeast hypothesis of syphilis would be to throw the subject back into confusion; for it, as far as I can see, is the only clue which we possess to the orderly arrangement of our facts. Whether or not it will, when carefully developed in detail, be proved sufficient to explain all the facts, is the problem which confronts investigators. For myself, I may admit that the result of this debate has been to strengthen my belief that it is not probable that it will ever be supplanted. I have failed to see what those who suggest that the analogy with the exanthems has been pushed too far have advanced in support of their criticism. I can only again express my conviction that the more the facts as to syphilis are examined, the more clearly it will appear that, when allowed to develop without interference by specifics, its stages are very fairly regular, whilst the more we know of the other exanthemata, the more willingly shall we admit that theirs are by no means so definite and precise as we are accustomed to assume. Whether or not it results from our habit of prescribing remedies from which no very definite effects are expected, I cannot tell; but certainly it seems wonderfully difficult to persuade ourselves that mercury really does interfere with the development of syphilis, and that is is hopeless to attempt to get correct ideas of the natural course of the malady, if we investigate cases in which it has been used. Amongst the facts which chiefly impressed on my own mind the lesson that syphilis, when let alone, is really a very orderly disease were the two now well known series of cases in which, some years ago, I had the opportunity of examining a number of patients who had all at certain definite dates been made syphilitic by vaccination. In the first of these, about ten persons had indurated chancres on their arms at the same time, and due to the same cause. I do not think that any two of their sores varied more than a week in stage of progress from each other. The similarity in date of induration, etc., was most remarkable, and quite as definite as would have been the phenomena of vaccination or of small-pox in a like series of cases. In my second series, the stage of the disease was further advanced before the accident was discovered; and here, again, we found syphilis very regular, since case after case was hunted up and found to be in almost exactly the same stage as its fellows.

If, however, mercury be given, the course of things is wholly altered, and it depends upon circumstances whether the exanthem stage be wholly prevented or only very much deferred. In a case in which mercury was begun early and continued for six months, I have known the rash, which would otherwise have come within six weeks, deferred till the end of the period named. Here I am obliged to interpolate that I did not exactly see Dr. Farquharson's meaning, when he said that mercury makes syphilis more regular. That it makes it milder is undoubted, and that it makes its stages, as a rule, much shorter is most undoubted; but I feel sure that, in certain cases in which it fails to cure, it may greatly protract the malady by increasing the distance between its stages. The theoretical explanation of this is, that it probably prevents the development of the yeast in the blood; and, if it do not wholly kill, it leaves it still able at some future time, when the antidote has been laid aside, to resume its growth. In connection with this theory, I wish to direct attention to a most important suggestion which fell from Dr. Broadbent, and which was especially welcome as a striking exception to the remark I have just made as to the general incredulity now prevalent as to positive therapeutics. Dr. Broadbent remarked that, now that it has been proved that mercury is an antidote for syphilis, and made probable that the syphilitic virus is the same in its nature as those which cause the exanthems, we ought to again make careful trials of that and other specifics for them. The same thought has often occurred to my own mind. If mercury can kill or retard the development of the yeast-plant of syphilis, it is very probable that it or the iodide, or some similar remedy, may do the same for the yeast of typhus, typhoid, or small-pox. We must not assume that this question has been set at rest by any trials which have as yet been made; for they have not, I think, been carried out with sufficient care. It will be desirable to ascertain carefully, beginning at the very earliest possible period, whether the evolution of the disease, as tested by the temperature and other symptoms, can be modified; whether the stages can be protracted; and, this point having been set at rest, we must then inquire whether such retardation is, on the whole, for the good of the patient, and determine the clinical details to which it may be necessary to attend. It is quite possible that the same remedy may be very useful in one mode of administration and very hurtful in another, and thus the statistics *en gros* of recoveries and deaths under mercurial treatment in typhoid which have been collected hitherto may have no close application.

The discussion which has taken place as to when syphilis ceases to be a blood-disease has been very important; but it has been in part

based upon a misapprehension, and it has elicited expressions of opinion rather than facts. There has been, I think, a general tendency to hold that it is probable that, so long as any manifestations whatever exist, there must be a blood-taint. Several speakers have seemed rather to deprecate the attempt to mark out stages at all. Thus one to whom the subject is very greatly indebted—Dr. Wilks—said that for him, when a patient had syphilis, he had syphilis; and another of high position in the profession informed us, pithily but apparently without regard to stage, that syphilis was not so much a blood-disease as a flesh-and-blood disease.

Now, in reply to these criticisms, I cannot but still think that it is consistent with fact to divide syphilis into stages; and that the degree in which the blood and tissues are relatively affected by it differs very much at the different periods. No one will probably deny that syphilis, whilst breeding in the early periods of the chancre, is a tissue-disease only, its yeast is not yet free in the blood; then follows a period when the blood is its home, as may easily be proved by inoculation experiment. The blood cannot be infected many days—probably not many hours—before the poison finds its way into the tissues; and the misapprehension to which I have just adverted consists in that some speakers have seemed to suppose that by blood-disease was meant one in which the blood alone shares. I really cannot admit that the terms I used afford any ground for such misapprehension; for the very pith of my argument as regards the symmetry of the phenomena in the secondary stage was that the tissues became infected by the germ-carrying blood. Of course, in its secondary or exanthem stage, syphilis is a flesh-and-blood disease, or rather, to denote sequence accurately, a blood-and-flesh disease. But, in the primary stage, it was local; and it is very possible that in the tertiary stage it may be local again. In other words, the poison or yeast may have died out of the rapidly changing blood; whilst it, or perhaps its results, are still present in the solid and less mutable tissues. To determine this, we want not opinions, but facts.

Now, there are several kinds of facts by which the state of the blood in syphilis may be estimated. First, we have its contagious properties, or inoculability. This is conclusive; and it is quite certain that, during the secondary stage, the blood is contagious. The numerous accidents in vaccination and otherwise which have occurred during this stage have fully proved, both for the acquired and inherited forms, that the blood may be rich in contagious material, even when the patient displays no external symptoms. Although, as a rule, when the blood contains the poison it will produce an eruption, it by no means invariably does so; and it may be admitted as highly probable that the blood continues to be contagious for a certain period after the external phenomena have ceased. How long is that period, and within what limits may its duration vary? That is the question before us. Most unfortunately we are precluded from experiment; for syphilis is with great difficulty communicable to the lower animals, and it is not easy to find conditions under which, in the human subject, such procedures would be justifiable. Availing ourselves of such facts as accident throws in our way, I believe we can produce but little evidence in favour of prolonged contagiousness of the blood. All the accidents occur during the year or eighteen months which we count as the secondary stage, and most of them in the early part of it. There is every reason to believe that, in the tertiary stage, neither the blood, nor even inflammatory secretions produced by sores which still bear the specific type, can reproduce the disease. Our next test is the possibility of transmission to offspring; and I note that almost every speaker has been inclined to assert that the production of a tainted child must be regarded as proof of blood-poisoning still extant in the parent. This may be so; but I cannot help the conjecture that it may be possible for the germs to still hold possession of cell-structures in the ovary or testis when they no longer exist free in the blood. That such is the case cannot be proved, and must rest for the present as mere conjecture. It becomes, then, of great importance to answer the question—How long after the secondary stage is it possible for syphilis to be transmitted hereditarily? During the last few weeks, I have gone through the notes of a great many cases, in order to get data for a safe reply on this head, and with the result of a strong impression that we have much exaggerated our estimates of the time. The cases in which syphilis is transmissible by inheritance for more than a year or two after its secondary stage appear to be very exceptional. I possess notes of a few in which successive children, during a period of seven or even ten years, have presented evidence of taint; but the ordinary course certainly seems to be that the first two or three children suffer, and that the others escape. Unless the risk of hereditary transmission did really cease early in the vast majority of instances, infantile syphilis would be far more common than it is. It will be readily seen that our decision on this point is not a mere matter of speculation or

of transcendental pathology, for upon it must rest the advice which we give our patients in reference to marriage. Now, I have for long made it a rule when consulted on this point to insist that, before marriage, a period of two years should elapse from the last of what I have considered blood-symptoms. I have given this opinion to a great many persons, and may confess that it has been a constant source of anxiety, lest some day some one should bring me a snuffling spot-covered baby, and say: "See here; you said I might marry; just look at this!" Such an occurrence has, however, never yet happened to me. I may strengthen the bearing of this fact by adding that I have been cognisant of not a few cases in which the marriage took place at a much shorter interval than had been advised, and yet healthy children were produced. I repeat, then, that there is reason to believe that the instances of liability to transmit to offspring extending over periods of several years are exceptional, and are by no means to be dealt with as if they illustrated the rule. There are, moreover, numerous fallacies to be carefully kept in mind in investigating cases of the supposed unusual prolongation of this risk. There is the almost certainty to which I shall have to allude directly, that the mother becomes contaminated by her foetus, and thus, if healthy before, supplies a new starting point for the infection of future children; and there is always the risk that one or other of the parents may have contracted the disease a second time.

Thus, then, we have clearly a period of syphilis during which the original sore is contagious; a period during which the blood and tissues are contagious; and a period during which transmission to offspring is possible. It is doubtful whether or not the two latter cease simultaneously, but there is some probability that the last remains the longest; both, however, in almost all instances, end within comparatively short periods.

Let us ask, next, how it is with the symptoms which we rank as tertiary. Here we find a totally different law. The liability to them persists after the longest periods of apparent immunity, and after a whole family of healthy children have been produced. Nor are such cases rare; they are almost the rule. I am obliged, then, to again ask the question—Is it not far more probable that such symptoms result from changes in the solids which have taken place during the protracted secondary stage, than that they are consequent on a blood-taint still in activity?

At this point, the argument receives, I cannot but think, great support from the general fact that everything in the secondary stage is symmetrical, and everything in the tertiary unsymmetrical. The inference suggested from this is that the phenomena of the tertiary stage, not unfrequently single, are of local origin, and possibly not unfrequently acknowledge accidental causes; whilst those of the early are due directly to the circulation of poisoned blood. Now, I must confess that I have been a little astonished at some of the opinions which have been expressed during the debate on these facts as regards symmetry and the inferences from them. One speaker, if I did not misunderstand him, thought it a novel statement that symmetry after all did not mean anything more than that the poisoned blood circulated equally on the two halves of the body. Now, so far from this explanation being novel, it is precisely that which gives the symptom its value. Others have alleged that, after all, secondary syphilis is not so very symmetrical, and Dr. Moxon has explained to us that, although it is symmetrical, yet its symmetry has no meaning, since it is invalidated by the "fallacy of universality". To those who doubt the fact of its usual symmetry, I have to say simply, strip your patients and inspect them in a good light; and, if this be done, I cannot conceive that there can be any doubt on the matter. Allowance must always be made for slight deviations from exact parallelism, for neither the spots on the wings of a butterfly nor the markings on a spider are ever absolutely symmetrical. It is recorded of a Dutch gardener that he caught a boy stealing apples, flogged him, and shut him up in one of his summer-houses; and that, having done this, his mind was so much disturbed by the interference with the symmetrical plan of his garden, that he could get no peace until he had flogged his own boy and locked him up in the corresponding building. I was careful in my introductory remarks to say that we must not expect from Nature symmetry of the Dutch garden type. That in secondary syphilis the sameness in extent and location of the phenomena on the two halves of the body is such as ought to be quite conclusive as to its meaning I yet fearlessly assert. To Dr. Moxon, I reply that, if an eruption be so universal that you cannot judge of its symmetry, that the fact of universality teaches pretty much the same lesson, and cannot often be explained on any other hypothesis than that of blood-contamination. Those, however, who are familiar with the syphilitic exanthem, know that it is not often universal, and that its symmetry is constantly seen in the most definite manner in cases in which it is only scanty. Sir William Jenner, directing his argument in the same direction,

has told us that the rash of typhoid fever, a blood-disease, is not symmetrical; whilst common psoriasis, the patches of which are usually symmetrical, is a skin-disease only, and may be produced by local causes. Now, there are few authorities to whom I would with greater pleasure yield my opinion if I could than to Sir William Jenner, but in this instance I am obliged to dissent somewhat from both his statements. If the rash of typhoid be really not symmetrical in any other sense than that it is often ill-characterised and difficult to see, it is a very extraordinary fact in pathology, and is well worthy the most attentive scrutiny of this Society. On the other hand, taking psoriasis as the very type of a symmetrical eruption, I must assert that it is probably not a mere skin-disease, and that it cannot be evoked by local causes. The influence of varying states of health and, age of lactation, etc., in producing it, and its cure by arsenic seem to prove that it is constitutional in origin; whilst, if it were ever due to local causes, it could certainly never, under such circumstances, become symmetrical unless the irritation were equally applied on the corresponding parts.

Amongst those who have produced evidence in favour of the assertion of the phenomena of tertiary syphilis are, as a rule, non-symmetrical, my thanks are chiefly due to Dr. Moxon, whose facts derive additional value from the circumstance that he is an unwilling witness. Could anything be more conclusive as to the general fact on this subject than that a pathologist so able and zealous as Dr. Moxon, with Guy's Hospital as his field, mentions to us four cases of symmetrical tertiary syphilis, and four only. If the facts were as he wished to imply, that no great difference in this respect could be established between secondary and tertiary symptoms, he would easily have been able to give us four hundred. I have myself repeatedly published cases of symmetrical tertiary syphilis, but the interest of the cases consisted in the fact that they were exceptions to the rule. It must be obvious that there is no law to prevent tissue-disease, such as I hold tertiary symptoms to be, from being symmetrical; on the contrary, the wonder is that they are not more often so, resulting, as they do, from long standing disorder of nutrition which was caused at a time when the blood was tainted. So much the more significant is the clinical fact that, as a rule, they are not symmetrical. In repeating this assertion of their almost constant non-symmetry, I am speaking of the later tertiary phenomena, and chiefly of such as paralysis of the nerves of the eyeball or face, gummata in the tongue, muscles, or cellular tissue, and periosteal nodes.

Having mentioned Dr. Moxon's clever and amusing speech, it may be convenient here, perhaps, to conclude my reference to it, and to say that I am glad to find that he supports the exanthem doctrine of syphilis. I spoke of syphilis as a slow-staged or long-staged exanthem, and he calls it an "exanthem diluted by time". As to his suggestion that tertiary growths have their seat in portions of new matter, which, by some means, have got introduced into the system without parentage, and, therefore, without inheriting any degree of immunity, I shall prefer to avail myself of its author's permission to say nothing about it, and shall await with interest its further development.

It has been prominently mentioned, as if it constituted a real difference between the exanthems and syphilis, that the former are not capable of transmission to offspring. The reply to this is, that such transmission is possible only during the period when the specific poison is living in the blood and tissues; and that this period in the more acute exanthems is very short, whilst the patient's condition during it is such as to utterly preclude the possibility of his becoming a parent. We cannot too clearly keep in mind this fact of extreme difference in length of stages, for by it are to be explained not only the possibility of hereditary transmission, but the subsequent liability to tertiary disease.

The statement made by a high authority, that syphilis differs from the other exanthems in having no pyrexia, I may conveniently answer by quoting from Dr. Buzzard's speech at an earlier meeting. Dr. Buzzard, apparently as familiar with the earlier or surgical stages of syphilis as he is well known to be with those in which the nervous system suffers, spoke of fever as being "so significant a part". In point of fact, the pyrexia in the exanthem stage of syphilis sometimes runs high, and is probably always present more or less. I have known a patient almost die during it.

Amongst the important matters brought prominently under notice during this discussion, is the fact that there exist two schools of opinion as regards what takes place in the transmission of syphilis from parent to child. In the past, some, perhaps most of us, have held in a muddled sort of way both doctrines, and have allowed them to mix themselves up in a somewhat incongruous manner in our expressions and forms of belief. I cannot but regard it as a great gain that the two are now fairly confronted, since, without presuming to assert that they are incompatible, it seems very improbable that both are true.

The one, which is the creed of those who think that syphilis is due to a sort of yeast germinal matter, holds that transmission is effected only by transference from parent to child of germs; that it is, in fact, contagion to the ovum. Thus, unless such infectious matter pass with the sperm or germ, no transmission of the disease will take place, and a child perfectly healthy, *quoad* syphilis, may be born. Transmission thus becomes to a certain extent a matter of chance, and by no means subject to the laws of ordinary heredity. The other view, which with the clearness and ability which characterise all that he teaches, was expounded by Sir William Jenner, makes the transference of syphilitic taint from parent to child but an example of heredity, in the same way that colour of hair, texture of skin, etc., are transmitted. "Potentiality of development, then, and not the state of the blood; not any appreciable change, is the real pathology, it seems to me, of infantile syphilis." I am quoting Sir William's exact words.

Let me try to state as briefly as I can a few of the facts in reference to these two doctrines. When syphilitic parents procreate, a not uncommon result is death of the ovum at very varying periods of intra-uterine life, a circumstance, probably, to be explained more often by disease of maternal structures (of the placenta) than by breeding of syphilitic virus in the infant. In another group of cases, the infant is born apparently healthy, but sickens rapidly, and without any of the ordinary evidences of syphilis, but with possibly a pemphigoid eruption on the hands and feet, dies in spite of all treatment within a few days. It is very difficult to say what happens in these cases, or in what way death is caused; they are rare. Lastly, we have the common well known result that a fine healthy infant is born at full time, which at the end of a month begins to snuffle, and a few weeks later shows a symmetrical rash closely similar to those seen in the acquired syphilis of adults. These symptoms are, as in the adult, amenable to treatment, and also subject to the law of spontaneous disappearance after a definite duration. It is remarkable that the period which intervenes between birth and the full development of the exanthem stage, is exactly that occupied by incubation in the case of acquired disease, and the conjecture is forced upon us, that the development of the yeast in the child's blood is restrained during intrauterine life, and commences only with the function of respiration. The phenomena which ensue are those of a specific fever, and in no respect resemble those of development. If aberrations from normal development are ultimately produced, peculiar physiognomy, malformed teeth, etc., they are always in ratio with the preceding infantile inflammations, and are probably directly produced by them. If a syphilitic child in infancy escape syphilitic inflammations, its development will probably be in no respect peculiar; it may grow well, and present every appearance of excellent health. Thus far, then, we may assert, that what happens to a syphilitic infant is closely similar to what occurs in the acquired disease, and suggests rather the breeding of specific poison in the blood and tissues, than the inheritance of "potentialities of development". It may be added, that were the latter creed the true one, we ought to find children inheriting the disease at the special stage to which it had attained in their parents; whereas, this old opinion is now generally discarded, and it is acknowledged that, whatever may be the stage in the parent, the child will begin again at the early secondary period. If the doctrine of yeast-contagion to the ovum be the true one, we ought to find no shading-off in degree of severity of transmitted taint—the child must inherit the whole of syphilis or none; whereas, if the other view be correct, we should expect the most various gradations, the severity being in relation with the parent's state. Now, I believe that the general—perhaps the universal—opinion held at the present time is, that inherited syphilis does really shade off, so to speak, in the younger children. On former occasions, I have myself used expressions favouring this view, and, if I now discredit it, I fear I may encounter some suspicion of allowing my theoretic belief to influence my interpretation of fact. I may, however, assure the Society, that, although I have a limitedly changed my opinion on the suggestion of theory, I have not allowed myself to do so without collating very carefully the evidence afforded by the histories of some scores of syphilitic families. It appears to me, as the result of this study, that there is no proof of the transmission of minor degrees of syphilis, and that what we witness in children is rather exactly parallel with what we observe in the acquired disease, *very variable severity in different individuals*. If the real facts as regards the natural history of syphilis could be obtained, we should probably find that a considerable proportion of those who pass through the syphilitic fever after chancre-contagion, do so without rash, without sore-throat, without, indeed, any visible symptoms, and that this is by no means dependent upon the treatment, but rather upon some peculiarity in the patient's organisation, or some arrest in the development of the yeast poison. If this be the case in the acquired disease, we need not wonder that many

infants who yet show evidences of taint in later life, escape the ordinary rôle of symptoms during the first few months of the first year. I have notes of many families in which some suffered and others appeared to escape; and although, as we should expect, it is the rule for the eldest to suffer most severely, there are so many exceptions to this, that I do not think we can infer anything as to diminished intensity of the poison. We ought rather to say, that it is the rule for the eldest to suffer most often, and that the chance of total escape is increased with each successive year. As the rule, the younger members of syphilitic families show no traces of the disease whatever, and appear to have escaped entirely. Lastly, it may be remarked, that if the inheritance of syphilis were an inheritance of "potentialities of development", and not, as Mr. Simon has well expressed it, of "a material something which passes absolutely and bodily into the infected ovum", we should expect to have mixed and ill-defined results. We should see the syphilitic potentiality mingling itself with others and producing hybrid diseases. And, further, one scarcely sees how on such an hypothesis a syphilitic parent should ever have healthy children, or why his younger ones should suffer less than his early ones. Admitting that the subject is one of much difficulty, and that further investigations of fact are needed, my conclusion still is for the present a tolerably confident one, that the transmission of syphilis to children is like that from person to person in the acquired disease, a communication of special germs, and that there is no such thing as the transmission of less or more, that the foetus either gets syphilis in the full or not at all.

I confess that it seems very difficult, taking this view of the mode of transmission, to believe it possible that the germs received *in utero* can survive long enough to be transferred to a third generation. For this to take place, a period of vitality of nearly twenty years must in most cases be presupposed, and this is, I believe, far longer than can be proved in the case of the acquired disease. Facts bearing on the point are very few in number, and most of them negative. Mr. Simon mentioned briefly the other night one which seemed to favour it. Some years ago I recorded a number in proof that those who had suffered severely might yet bear healthy children, and with them one which might be considered to bear in the other direction. The fallacies are, however, almost insurmountable.

Whilst urging that the kind of poison communicated in parental transmission is probably just the same as that conveyed in chancre-contagion, we must not forget that there are possibilities of difference, to fully appreciate which a knowledge of the laws of cryptogamic life, rather than of heredity, is needed. To this important topic Mr. Simon and Dr. Greenfield both alluded; but I shall be best able to illustrate what is meant if I may be allowed to pass back to the speech of Dr. Broadbent. Amongst certain difficulties in the way of accepting the theory that tertiary deposits occur only where secondary disease has preceded them, Dr. Broadbent mentioned the well-known opinion that a healthy woman pregnant by a syphilitic husband may acquire the disease from the foetus, and pass at once to the tertiary stage, having never suffered from any secondary symptoms whatever. My reply to this would be, that in such cases the "tertiary symptoms" are never displayed early; that they follow, as in other instances, after long intervals; and that the cases are probably examples of the secondary stage being passed through without external signs. This subject is, however, far too important and too curious to be thus dismissed. If what seem to be facts respecting it be really credible, we appear to have a wholly novel phase of the disease opened out to us for investigation. It has long been believed by many that the mother might become diseased by absorption direct from the blood of the foetus, and without the intervention of any chancre. It is just twenty years since I wrote a paper on this subject, and collected a good deal of evidence concerning it. My conclusion was, as stated by Dr. Broadbent, that in these cases the woman never suffers from secondary symptoms, and experiences nothing during the pregnancy which infects her, excepting perhaps a little loss of strength and slight indefinite ailments. In further proof that she does really contract something, I made use of the well-known observation of Abraham Colles that syphilitic infants nursed at the breast often infect wet nurses, but never their own mothers. I did not, however, until within the last few weeks, fully appreciate the bearing of this fact. If Colles's law be one which has no exceptions, it follows that all women who bear syphilitic children contract syphilis; for how else can they obtain immunity? And, since it is notorious that women under such circumstances scarcely ever show secondary symptoms, it follows, further, that we have here a form of syphilis which is protective, but which is unattended by any cutaneous outbreak. Thus syphilis acquired by blood-contagion from the foetus would appear to be, for the mother, a parallel with vaccination in regard to small-pox: she gains immunity without suffering from any severe form of disease. The botanist will at once suggest that probably in

both cases the explanation is to be found in heteromorphism or alternation of generations on the part of the fungus. To him the clinician might aptly rejoin, that really one might almost have expected it; for, when the mother gets syphilis from the fœtus, she obtains it from fluids in which the plant-life is evidently under some very special restraint; for in the fœtus itself, as a rule, no development of it takes place during the nine months of intra-uterine life. We have only to suppose that the same condition of the yeast which existed in the fœtus is perpetuated in the mother, and the thing is done. We had no right to suppose that infection by inoculation of solids (or chancre-contagion) would be exactly the same in its results as direct imbibition by the blood.

As we may call vaccinia undeveloped yet protective variola, so we may consider syphilis, derived from the fœtus, as an undeveloped yet protective form of that disease; and we have here another most interesting point of analogy between syphilis and the exanthems.

Before, however, we accept as probable such a possibility as that just hinted at, it is desirable to look at the facts with the utmost incredulity. Let us doubt unsparingly at every stage of the reasoning. First, is Colles's law true? I cannot see any escape from the conclusion that it is. It was announced in 1837, and has received, I believe, the assent of every authority who has written on the subject since. It has attracted attention both at home and abroad, and I am not aware that a single exception to it has been recorded. We have all of us seen chancres on the nipples of wet nurses. They are, indeed, not very unfrequent. We have, however, none of us seen such on those of the mothers of infected children. Let us remember that it is very unusual to put a syphilitic infant out to wet nurse—a thing which no prudent surgeon would ever permit—and that probably, for one so nursed, a hundred are suckled by their mothers; and we shall appreciate the weight with which this entire absence of proof that mothers ever suffer bears. It amounts, I think, all but to proof that they are absolutely insusceptible. It is as strong in that direction as is the rarity of small-pox within short intervals after successful vaccination. We must remember also that these mothers of syphilitic infants not only nurse one infected child, but often several in succession; that they not only suckle them, but handle them, dress their sores, and in various ways through long periods expose themselves to risk. If it be granted that it is proved that these mothers—a very numerous class—have really in some way had syphilis and acquired immunity, I do not think there can be much dispute as to the next fact, that they do not during pregnancy show any of the usual symptoms of the disease in its secondary stage. This is a matter of every-day experience. There remains, however, the possibility that the syphilis may have been gone through prior to pregnancy; and I am well aware that the few remaining writers who teach that syphilis can be inherited only from the mother will hail this confirmation of Colles's law as a strong support for their opinions. Here, however, again I must appeal to every-day experience. Is it not the fact that women bear syphilitic children without having ever themselves, either before or during pregnancy, had any symptoms, either primary or secondary, of that disease? If this happened only once or twice, we might reasonably doubt the histories given us. But it is not so; it is in hundreds of cases; and few, I think, of much experience, can doubt that, as a rule, syphilis is inherited from the father; and that the mother never shows any external signs of the malady. I purpose shortly to publish the evidence which I have collected on this and some kindred subjects; and the assertions just made will, no doubt, receive the scrutiny of other observers. If the argument should in the end be thought to be substantiated, some other very interesting questions will suggest themselves. We shall have to ask, What are the ulterior liabilities of a woman who has thus acquired the modified form of syphilis? Can the taint be transmitted from her to offspring borne subsequently to a healthy father? Is her blood at any period contagious; and what would be the result of inoculation with it? Does the taint in her last as long as when the disease is acquired in the ordinary way? I have already, in speaking of the difficulty of measuring the length of time during which it is possible for the tendency to transmit to offspring to last, adverted to the fallacy introduced by the fact that usually the mother acquires the disease from her first pregnancy, and may herself become the source of contamination in the second. We must not, however, take it for granted that a taint acquired in this peculiar manner is transmissible, or, at any rate, that it is so for any long period of time. It is quite possible that its stages may be far shorter than those of the common type of the disease.

Here, Mr. President, I will ask permission to leave the proper topics of reply, and to make a suggestion. It is this, that our next debate should be on the laws of evolution of the exanthemata. Surgeons have been asked to make a clean breast of it as regards syphilis; and we have told all that we know, and much, perhaps, that we only half

know. Let us next solicit of medicine a similar declaration of faith in reference to the most interesting part of her proper domain. There are a host of questions upon which we surgeons, out of pure scientific curiosity, and with no wish to intrude into medical practice, are longing to be enlightened. By what right does diphtheria claim its place as a specific fever? If the rash of typhoid be not symmetrical, on what pattern and by what influences is it arranged? What is the interpretation of the curious phenomena of corymbiform variola? If a pregnant woman suffer from an exanthematic fever, what are the possible events as regards the fœtus? and does it ever happen under such circumstances that the infant develops the disease, as in the case of syphilis, soon after it gains access to the light? Are not the stages of the exanthemata far less accurately determined and much more liable to variation than is generally admitted? May not varicella, for instance, vary in its incubation period from fifteen to twenty-seven days? and in its exanthem period from four (the average) to fourteen or twenty days?—limits of variation wider than we need claim for syphilis. Is it possible to convey the disease by inoculation with varicella-fluid?—a fact denied by most writers, but affirmed by one of the latest without the citation of any evidence. Is it not probable that cases are very common in which patients go through specific fevers and acquire protection, without having had any rash or shown any external symptoms? On these and many other important topics medical opinion is, I believe, very far from being settled; and their investigation might, perhaps, have the collateral advantage of diverting the zeal of our medical friends (excepting in the *post mortem* room, where they are always welcome) from the one specific fever, which is all that the surgeon can claim as his. To speak seriously, it has sometimes struck me forcibly that, in comparing syphilis with the exanthemata, the large amount of uncertainty which still surrounds the latter is a most serious bar to progress.

Amongst the more important of the questions which have been put to me in the course of the debate, I find one in the very suggestive speech of Dr. Greenfield, as to whether there may not be some after-results of syphilis which are to be classed rather as those of malnutrition than as consequences of the specific poison. He illustrates the question by asking whether all the children who suffer from keratitis have really had syphilis, and whether it may not, in some cases, rank rather as a defect in nutritive power. To this I may give a clear reply, that I believe that all the conditions which we have as yet recognised as syphilitic, whether in the acquired or the inherited form of the disease, are the direct results of the disease. A severe attack of syphilis may of course damage a man's nutritive power, but as the result of such damage we shall not encounter anything in the least special; and it is quite impossible that he should, in virtue of such, transmit to his children a state of cachexia rendering them liable to such a disease as interstitial keratitis. The latter malady cannot, I feel convinced, occur to any one who has not had syphilis. Interstitial keratitis also obeys the usual law of syphilitic inflammations in always showing tendency to spontaneous cure, which would scarcely be the case if it resulted from defect of nutrition. There is, however, a rare form of chloroido-retinitis, which is steadily and slowly aggressive, much like a degenerative affair; but I suspect that it is secondary to damage done by previous inflammation, otherwise it might seem to be an example of what Dr. Greenfield is seeking.

The peculiar relationship in which the gumma, the soft sore, and phagedænic action, stand to syphilis, have perhaps been sufficiently discussed. I may note, however, that, with the exception, I think, of Mr. de Méric, no speaker has attacked the dogmatic assertions which I very purposely made as to my belief in the essential alliance between phagedænic and syphilitic inflammations. I was very anxious to attract attention to this subject, believing it to be of great clinical importance. I have long held, and publicly taught in the most positive manner, that, certain very rare cases excepted, all well characterised phagedæna may be traced to syphilis; and that it is from the contagion by pus from syphilitic sores (mostly tertiary, and not containing the specific virus) that hospital phagedæna takes its rise. You, Mr. President, can, I know, give us some valuable evidence on this point; and as I hold that its knowledge is of great importance, in order to the prevention of epidemics of phagedæna, both in military and civil practice, I rather regret that it has not, on the present occasion, had the benefit of more criticism. Mr. de Méric had somewhat misunderstood my meaning, and seemed to think that I held syphilitic phagedæna to be incurable, which was very far from what I intended to say. I should be very glad if I could believe that the silence of other surgeons implied acceptance of my creed.

As a curious illustration of the difficulty in making one's meaning clear, I find that I have been criticised by Mr. Wood and others for attempting, as they think, to define too closely the stages of syphilis, whilst Mr. de Méric complains that I have shown too great a tendency to put the

whole disease in one lump, and expresses his preference for the doctrine of stages. I cannot defend myself on this point from either critic without recapitulating almost the whole of my address—a task which the Society will, I am sure, gladly excuse me. Briefly, however, I may say that I consider the several so-called stages of syphilis as in part natural and in part conventional, and that I have not the slightest wish to make them more definite than is required by clinical convenience or suggested by pathological probability.

Amongst the chief contributions to facts, we must place Dr. Buzzard's valuable statistics as to the average age of those who suffer from disease of the nervous system, Dr. Hilton Fagge's statements as to the frequent alliance between syphilis and amyloid disease, and Dr. Greenfield's account of certain pathological details. Dr. Moxon has also mentioned some interesting examples of symmetrical tertiary growths; and Dr. Greenfield has recorded a *post mortem* examination in the secondary stage, in which gummata were found in the dura mater. I am sanguine that, hereafter, many of the topics which as yet have been only debated will be examined by the light of accumulations of new facts, otherwise some regret might perhaps be felt that, following my own bad example, most speakers have contented themselves by expressions of opinion.

The possibility of diagnosing syphilis by the odour of the patient was adverted to by Sir William Gull. The late Mr. Wormald used to teach that this was possible, and was fond of saying, "They can't deceive me; I nose 'em." It probably requires a very good sense of smell to make much use of this symptom. For myself, I may confess that I have rarely been able to distinguish anything peculiar in the odour of adults suffering from syphilis; but in infants I think it is often very perceptible and quite special.

In answer to the numerous surgeons who have requested further details as to my disbelief in dualism, I may briefly say that, although of course we must all fully recognise the differences between hard and soft sores, yet I do not think that the soft is a special form of disease. On the contrary, I believe that it is usually produced by contagion with the results of syphilitic inflammation; and that, if we could get rid of true syphilis, we should after a while cease to see examples of the soft sore also, with the exception of those which result from common inflammation, balanitis, and the like. My views on this point are explained in detail in a lecture published in the *Lancet* six months ago. To those who were inclined to complain that I had said that dualism was dead whilst it is still mentioned in several standard and excellent works, I can only reply that I was careful to say only dead, and that I did not say that it was buried. Had I done so, I should have been a little premature.

The observation made in the address as to the extreme unfrequency of affections of the brain and nerves in inherited syphilis has been confirmed by Dr. Buzzard and all speakers who have adverted to it. As none have mentioned facts in opposition, I think we may now regard it as generally admitted. I am not sure but that some statements which have been made may even help to explain it. Dr. Buzzard and Dr. Greenfield have both called attention to the probability that the tertiary growths take place in perivascular spaces and other adjuncts of the lymphatic system. Professor Sigmund of Vienna has also, during this discussion, in a paper published in the *Practitioner*, drawn attention to the circumstance that syphilis, from its beginning, shows an especial tendency to develop in lymphatic structures. Now, we must remember that this is true only of syphilis when acquired by a chancre. It is not the case when it is transmitted from parent to child, nor when a foetus infects the blood of its mother. In these, there is in the beginning no implication of glands. This may possibly be the reason why, in neither of these classes of cases do we find in the later stages of the disease that liability to perivascular growths and gummata which are not unfrequently witnessed as results of the chancre form of syphilis. It appears highly probable that we have to deal with syphilis in two or three (perhaps more) different moods, varying according to the precise manner in which the transference of the yeast-poison is effected, and the structures into which it is first introduced. In connection with these differing moods, different classes of sequelæ must be expected.

Dr. Wilks, Mr. Berkeley Hill, Dr. Buzzard, and others, have adverted to the interesting fact that serious tertiary symptoms are not unfrequently met with in those who have suffered but very little in the earlier stages. This is to be explained probably by several different considerations. In the first place, it is true only of a certain class. Some of the very worst examples of tertiary disease in which multiple manifestations occur, happen often to those who have suffered throughout very severely, who have perhaps scarcely ever got rid of their symptoms. Such at least has been my experience, and these are often patients with whom the specifics have disagreed, or who show in par-

ticular an idiosyncrasy as regards mercury. They are often very seriously out of health. There is, however, another class in which the tertiary symptoms attract much attention, and become very serious, not so much on account of their own extent or severity, but because located in an important position. A patient apparently in perfect health may have a single gumma on his brain, which may cause him most serious distress, whilst he has not about him another symptom of syphilis, either present or past. These are the cases to which I suspect allusion was made, and in these not unfrequently the patient knows but little about his early symptoms. I suspect that the real explanation of the frequency of such cases as these, and of the converse fact that those who have had well marked secondaries usually escape sequelæ, is that the latter are well treated by mercury, whilst the former neglect it.

I am painfully aware that in this reply I have done very scant justice to the numerous speakers who have taken part in the debate. I have, however, I hope, profited by all, and have endeavoured to incorporate in my statements to-night the hints they have given. That I have not even alluded to the splendid speech of Sir James Paget, is to be explained by the fact, that we agree on almost every point; and this, again, finds its solution in the further fact, that he was my teacher, not alone, as I was proud to be reminded, in general surgery, but also in the special doctrines of syphilis.

In conclusion, Mr. President, I have to express my sense of obligation for the many valuable suggestions which have been addressed to me both in public and in private, during the course of this prolonged debate. I should be ungrateful, indeed, if I did not very warmly appreciate also the kind and very flattering manner in which my introductory remarks have been received as well by my friends in this room as by my critics in the medical press. It is not for me to say whether the debate will add greatly to our knowledge of its subject; but I may confess with pleasure that I have myself learned much. If it have not been a harvest, we may, I think, hope, at any rate, that it has been seed-time, and that syphilis, long ago named by one of our great masters "the key to all pathology", may in the future be found yet more useful.

ON THE NUMERICAL RATIO OF DISEASE IN THE ADULT MALE COMMUNITY,

DEDUCED FROM THE SANITARY STATISTICS OF HER MAJESTY'S CUSTOMS, LONDON, FOR THE YEARS 1857-74.*

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AN acknowledged desideratum in the public medicine of this country is the registration of disease, and this Association has long urged the importance of making some attempt in this direction, and, through its recognised organs, has more than once brought the subject prominently before the administrative authorities. There can be no doubt of the great public utility of such a scheme, were it practicable to carry it out with even a moderate degree of efficiency and completeness.

The statistics of death, although confessedly far from perfect, yield a great amount of most valuable and trustworthy information with regard to the comparative mortality of age, sex, and place. They present a very fair index of the relative prevalence of those diseases which are normally fatal, as consumption, cancer, organic disease of the heart and other vital organs, and they also give some clue to the progress and epidemic intensity of zymotic diseases, the ratio of the mortality of which to the recoveries is, from the accumulated experience of many years, approximately known. The influence of extreme atmospheric conditions, more especially on the old and young, can also be estimated with tolerable accuracy from the Registrar-General's returns, those of last winter being a striking instance. Their great importance in the innumerable calculations, social, economical, and political, which are based on the probability of human life, and which furnish us with the knowledge to prevent its waste, is too obvious to require comment here. Our country has enjoyed a laudable pre-eminence in this department of statistical science from the time, two centuries ago, when Graunt and Petty utilised the London and Dublin bills of mortality, and the celebrated Dr. Halley submitted to the Royal Society a life-table calculated from the birth and death-rates of Breslau (*Philosophical Transactions*, 1674-93) until now, when the labours of the Registrars-General of the three kingdoms, always progressing and improving, have become the models for other countries to imitate. They place before us

* Read before the Section of Public Medicine at the Annual Meeting of the British Medical Association in Edinburgh, August 1875.