LETTSONIAN LECTURES ON SOME MOOT POINTS IN THE NATURAL HISTORY OF SYPHILIS.

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LECTURE I.

The Mutual Relationships of the Different Forms of Primary Venereal Sore.—The Causes of Phagedæna in Primary Sores.—Hospital-gangrene.—On Recurring Chancres.—On Different Forms of Bubo, and on Questions in reference to Primary Syphilis.—Syphilis after Vaccination with Clear Lymph.

Mutual Relations of the Different Forms of Primary Venereal Sores.—I have long thought that, if the question in debate could be once clearly stated, the duality or unicuity of venereal poisons would soon cease to be a moot point. We are pretty much agreed as to the facts, and the controversy is mainly as to what they imply. A dualist is, I suppose, one who holds that there exist two quite distinct and independent contagia, one of which produces a non-infecting sore, and the other syphilis. An unicist holds, somewhat differently, that the poison of the soft sore is a product of syphilis, and by no means independent. The difference after all is not great, nor clinically is it of much importance. No one thinks that there are two forms of syphilis, and no one doubts that there are two kinds of sores. Are they related and independent? that is all that we dispute about.

The fact which chiefly favours the creed of those who think that they are independent is, that the secretion of the chancreoid is very contagious, and always produces a sore like itself. Bassereau, in the first instance, proved this by confronting his patients who had got chancreoids with the persons who had infected them, and he found that the giver usually possessed a sore just like that which had been given. Since Bassereau's time, numberless experiments, especially those done in the course of what was called syphilisation, have abundantly proved his point. What was named syphilisation consisted in inoculating the secretion of a chancreoid on the skin; the result was that a chancreoid formed. It must be remembered, however, that it was done almost exclusively on those who had had syphilis.

Here was clearly a fallacy, for the patient might, in consequence of his prior syphilis, be insusceptible of fresh contagion. Danielsen, however, in Bergen, tried the practice on a number of lepers who had never had syphilis, and with similar results. He found that he could reproduce a soft sore over and over again, and that it was never followed by syphilis. In further proof that no syphilis was conveyed, it may be stated that one patient, who had undergone many inoculations without ill result, finally by accident received virus from a true chancre, and had, as a consequence, an attack of true syphilis. It might seem that the proof of specific distinctness was here given. It is necessary, however, at this stage to insist that there is an important difference between a specific contagium and a specialised contagium. By specific we denote that which is always, and under all conditions, the same, and producible only by its own seed, distinct in the same sense that wheat and clover are distinct. There may easily be many morbid poisons which are specialised, that is, which may, during a certain number of generations, produce conditions similar to those in which they had their origin, and which yet do not rise to the dignity of species.

All inflammatory products are probably, under favourable conditions, contagious. The gonorrhæal secretion produces gonorrhœa, that of erysipelas erysipelas, that of diphtheria diphtheria, and so on. It is probable, however, that each of the diseases may originate spontaneously, and quite independently of contagion. The contagia are, therefore, the products of inflammation. Further, it is highly probable that, in each of the diseases mentioned, the contagium may vary much in virulence, and that it is by no means always the same. Probably it is quite possible to breed them up to higher degrees of power and of special peculiarities. It is possible then that the poison which produces the chancreoid is, after all, only a specialised product of inflammation, and not a specific virus.

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Many facts seem to support the conclusion just hinted at, and to imply that soft sores are, after all, an appanage of syphilis. When care is taken in inoculation, unquestionably they seem to breed true; but this is not the case in those which we see in practice as the results of accidental contagion. If we place in one group as "soft" all the venereal sores which do not harden, and which do not infect the system, we shall find that but a very small proportion of them present what are considered the typical characters of the chancreoid. We encounter a great variety of conditions and great differences in course, and are obliged to conclude that they agree in one feature only—the absence of hardness. The rounded form, punched out and ragged edges and grey base, are conditions not present in my experience in one of five of the venereal sores which do not harden.

It would be waste of time to attempt to describe the multiform character of non-indurated sores. Many of them are small, almost level with the surface, and have shelving edges. How rarely do we witness the inflammatory bubo tending to abscess which is said to accompany them. How short, as a rule, is their duration. Whilst the typical chancreoid goes through stages, and usually lasts six weeks, a few dressings with iodoform suffice to cure in a week almost all the "soft sores" that we meet with in practice. Now and then, I admit, we encounter the true chancreoid as graphically depicted by Mr. Lee, but it is very exceptional. This want of uniformity in conditions is a strong argument against specificity. Another equally strong argument is that the true chancreoid on the genitals is seldom seen, excepting in those who have had syphilis already. If a person who has never suffered before contracts a venereal sore of any kind, it is highly probable that it will lead to syphilis. This fact, which I think many observers will confirm, is placed in a strong light by Mr. Morgan, of the Dublin Lock Hospital, who states that, of fifty-four patients who came under care for first attacks of venereal sores, fifty proved to have true syphilis. Thus it would seem that insusceptibility is an usual cause of non-occurrence of induration. In using this argument, I by no means wish to deny that the typical chancreoid is sometimes seen in those who have never had syphilis.

Very important evidence as to origin of the chancreoid, and of all non-indurated venereal sores, from syphilitic secretions, is afforded by at least two experimenters. Mr. Morgan, of Dublin, whose paper I have just quoted, inoculated with purulent vaginal fluid from those who had had syphilis, and found that he could with it produce the typical chancreoid. From the sores thus produced, he could inoculate repeatedly and with sameness of results. With praiseworthy caution, he never inoculated excepting in those who had previously had syphilis; and thus, whilst his facts are conclusive as to the production of soft sores, they do not prove that syphilis might not very possibly have been produced at the same time had the soil been suitable. Mr. Lee had previously recorded the possibility of producing from indurated sores, by artificial irritation, a secretion which is purulent, and which is inoculable on the patient, producing a sore not distinguishable from chancreoid. Mr. Gascoyen, Bidekap, and others had, I believe, done the same. It is surely very difficult to get over these facts; whilst, to push the argument further, so far as I can see, all *a priori* probability favours the suggestion that non-indurated sores are produced by the secretion of true chancres, which have been changed in character either by the inflammatory process, or by the non-susceptibility of the tissues of the recipient.

Phagedæna.—A parallel of much importance might, perhaps, be drawn between the chancreoid process and phagedæna. All will admit that syphilitic inflammations have a remarkable tendency to become phagedænic. This may occur in all stages of syphilis, and to all kinds of sores. It is not necessary that there should be any contagion of material from phagedænic sores; it is sufficient that there is syphilis, for syphilis in its purest form often leads to phagedæna. Phagedæna, as we see it in connection with syphilis, is almost invariably of spontaneous origin, or, in other words, caused by syphilitic inflammation, and not by phagedænic contagion. Its existence puts an end to all possible sexual exposure, otherwise we should probably often see it on the genitals as the result of contagion. There is every reason to believe that its products are contagious, and that they would probably produce phagedæna, and not syphilis. The specific virus of the latter is probably destroyed in the gangrenous process. When phagedæna spreads as such by contagion, we encounter it, as I shall have to assert directly, under other aspects, and not as a venereal disease. Now, the chancreoid type of inflammation is possibly only a sort of minimised phagedæna, and differs from it only in degree. Its virus is probably produced under similar conditions, and it is curable under the same methods of treatment. The fact that a chancreoid in a woman does not absolutely disqualify for sexual congress, makes it possible for it to be transferred as such by direct contagion. This fact it was which

mised Bassereau and his followers into the belief that the virus of these sores possessed specific individuality. Probably it is not so; and it is still likely that many chancroids originate spontaneously in the same sense that phagedæna does; that is, they result not from contagion from a sore of the same kind, but from a modification of a syphilitic inflammation by peculiarities of the individual. Be this as it may, it is to be freely admitted that chancroids are very contagious, and that they reproduce themselves with closely similar features. Their virus, if not specific, is at any rate well specialised.

Hospital-Phagedæna.—Closely connected with this topic, and of great clinical interest, we have the question of the origin of hospital-phagedæna from syphilis. It is not uncommon to see the disappearance of hospital-gangrene claimed as one of the triumphs of antiseptic practice. I hold this to be a mistake, and I have the less reluctance in saying so, because I feel sure that no one estimates the legitimate victories of antisepticism more highly than myself. Its great teacher and his school need no borrowed plumes. The truth respecting hospital phagedæna is, that it did not exist in one in ten of our hospitals at the time when antiseptics came into vogue. It is not a disease which is always with us, but rather one which comes occasionally, prevails extensively, and then disappears. It is not due to neglect of cleanliness, nor to atmospheric infection; it does not occur from overcrowding; but it is caused by a special form of contagious pus. All the facts as to its history support this creed. During our last epidemic of it at the London Hospital, one large ward alone of the whole surgical department remained exempt. It was not less crowded than the others, and it received precisely the same class of cases; but it, unlike the others, never took patients from the infected wards. Hence, its escape from contagion. That epidemic ended in 1864, and from that time onward there have been no cases whatever in the hospital. The origin of that epidemic was, I had good reason for believing, the admission of a case of syphilitic phagedæna into a surgical ward. The first case occurred in the next bed to this patient. Mr. Pollock told me that he had arrived at the same conclusion as to an epidemic of the disease in St. George's Hospital. I have mentioned these facts before on more than one occasion; but the following is a new one, and it gives them strong support. A few years ago, a boy was brought into the London Hospital in consequence of his having been attacked by phagedæna in a workhouse-infirmary. He was suffering from acute periostitis of the tibia. A free incision had been made, and this wound it was which had become gangrenous. Its edges were swollen, ragged, and discoloured, its surface covered with pultaceous secretion. It presented, in fact, a good example of what I had often seen during the epidemic referred to, and never since. I had commented at the bedside of the boy on the case, and I had mentioned my belief that hospital-gangrene usually began from syphilitic cases. We could find no suspicion of syphilis in the boy. A house-surgeon present afterwards gave me what was probably the correct information. There had been admitted under one of my colleagues a very bad case of syphilitic phagedæna from the same workhouse, only a little before the lad. On inquiry, I found that in the workhouse the man's prepuce had been slit up with the same instruments which, some time later, had been used for the boy's leg; it was possible even that the same sponges had been used. Here, then, to say the least, was a possible source of contagion. The suggestion that hospital-phagedæna takes its origin from syphilitic phagedæna, would fit well with the fact that it often prevails in military hospitals, especially when crowded, in time of war. These are just the places where we may expect to encounter occasionally neglected and unhealthy venereal sores.

If we admit that what I have stated is very probable, it becomes of interest to glance at the facts of the malady in question. Hospital phagedæna is very contagious, and it spreads by contagion only. The previous health of the patient matters nothing; nor the salubrity or otherwise of the ward. Excepting in varying degrees of severity, all its cases are alike; they tend to the same results, and are to be cured by the same means. It is a well specialised disease. It never leads to constitutional syphilis. Accepting the hypothesis of its syphilitic origin, we have then a parallel fact to what is observed in the case of the chancre. A specialised contagium (pus) has been bred up, which can produce its like wherever inoculated, but which does not contain the virus of syphilis. Both the chancre and the phagedæna are the products of a poison originating in a syphilitic inflammation, but which in neither case can induce syphilis. It is easy enough to see that, if once the particulate virus of syphilis have died out of a secretion, the latter may then be propagated over and over again without the slightest possibility of reproducing the defunct specific elements. It is not, therefore, to be wondered at, that neither the chancre nor hospital-phagedæna, although appa-

nages of syphilis, ever, when once negatively specialised, by any chance produces that disease.

It is a question about which there is still some debate, whether the infecting or the non-infecting sore is the more liable to phagedæna. My own experience would lead me to a very definite opinion, that almost all sores which are attacked by this process are true chancres, and that it occurs at a stage too late to prevent absorption. It is, in fact, a concomitant of a true syphilitic inflammation, and does not usually happen until induration has taken place. It denotes unusual susceptibility to the influence of the virus, and it is often followed by very severe secondary symptoms. I will by no means deny that the retention of irritating secretions, as in phimosis with concealed sores, may give rise to gangrene of the foreskin in cases where no syphilis exists. If, however, a typical phagedænic process be set up, and spread, I believe that it will almost invariably be in association with true syphilis. I have suggested that the chancre process is allied to that of phagedæna; but it appears to be well specialised, and quite capable, under most circumstances, of maintaining its individuality. When once its peculiarities have been declared, the sore seldom deviates much from its type. If it do become aggravated, and spread at its edges, such spreading is only of the very mildest form of what we mean by phagedæna.

A knowledge of the fact that phagedæna usually goes with true syphilis is of much importance for purposes of retrospective diagnosis to those engaged in medical practice. Not unfrequently, with symptoms of visceral or nerve-disease, an examination of the genitals is made in order to seek for scars. Whilst some have assumed that scars on the penis, or its extensive malformation by bygone phagedæna, imply the probability of syphilis, others have asserted that they rather favour the belief that the disease was not true syphilis. My vote would go with those who regard them as important, though not conclusive, evidence of constitutional disease. I have very seldom seen scars on the penis in patients who had not had syphilis, and still more seldom the evidences of phagedænic action.

I am compelled also, as the result of personal observation, to deviate yet further from the popular creed, and to say that I should regard scars in the groin as also presumptive evidence of syphilis. Our rules of diagnosis have been, I cannot but think, far too definitely laid down on these matters. In private practice it is very rarely indeed that we have to deal with inflamed bubos. It so happens that, of late years, almost all the cases of suppurated bubo which I have seen were cases of syphilis. It is not, I believe, on the other hand, very exceptional for the typical chancre to cause no enlargement of the glands at all. I really fear that I may be suspected of differing for the sake of it, but I am compelled to record the result of unprejudiced observation. That an uninfamed indurated sore will be attended by uninfamed indurated glands I fully admit; but the fact remains, that a great many infecting sores do inflame and suppurate, and when that is the case the glands will follow suit. Nor is this inflammation always the result of a mixed contagion; it often, I feel sure, results from personal proclivity in connection with a fairly pure syphilitic virus.

A series of cases of syphilis from circumcision, which I have recently, in association with my friend, Mr. Charles Macnamara, had an opportunity of investigating, is of much interest in reference to the question just discussed. We were shown a group of six infants, all of whom had constitutional syphilis, having been infected by the same operator in the rite of circumcision. In all the operations, the wound had reopened, and assumed the condition of a chancre. Two out of the six had double suppurated buboes in the groin, and two others had large masses of agglutinated glands. The children had all been healthy before the operation; and I cannot but think that their age had probably much to do with the unusual tendency to suppurative inflammation displayed. So also in the case of adults, is it not probable that age and personal peculiarities often exercise great influence on the tendency to inflammatory action? In the children just referred to, it is to be admitted that the sores had much inflamed also, and thus it may be fairly suggested that the contagion was probably mixed. I am not, at the present moment, inclined to dispute that point, my reason for adducing the cases being simply the practical one of showing that suppurated bubo and constitutional syphilis often go together, and that, as a consequence, we are on unsafe ground in believing that scars in the groin imply absence of constitutional infection.

The recognition of non-infecting venereal sores on other parts than the genitals, whether on the hands or elsewhere, is a matter of great difficulty. It is, indeed, so difficult, that the recognition is seldom or never attempted. There are, in fact, no characters by which, apart from the history, such a diagnosis can be made even probable. We

know that unhealthy ulcers may result from a variety of causes, and that they may easily cause gland-enlargements. It is not likely that our non-professional patients will ever volunteer, respecting any sore, on the hand or elsewhere, that it may possibly have been caused by venereal contamination. In the case of the hands of midwives and medical men, however, the case is different. In them, there is nothing disgraceful in the admission of exposure; and we ought, perhaps, looking at the facts as generally believed, to expect to see non-infecting sores as frequently as infecting ones. I suspect, however, that they are very rare. I have myself very seldom indeed seen sores on the fingers of surgeons which could be reasonably supposed to be due to vaginal infection, which did not prove to be true chancres. I do not recollect a single instance in which a sore on the hand, which was not a true chancre, produced a bubo in the armpit. Although I have treated possibly a hundred cases of chancre on the finger, I never yet was concerned with a suppurated bubo in the armpit in association with a venereal sore on the hand. This is a very remarkable fact, and may be held to indicate either that the so-called "soft sore" is rare on the fingers, or that it but rarely causes bubo. Probably both explanations are in turn true.

The following narrative bears in an important manner upon the above general statements. A surgeon in good health pricked his fingers severely in several places during an operation for removal of the cervix uteri. He knew at the time that he was pricking himself on the teeth of the vulsellum, but his patient was bleeding profusely, and he was obliged to persevere. One finger inflamed under the nail within a day or two, and, during the next week, five or six sores had formed on different parts of several fingers of his right hand. Various remedies, black wash, iodoform, etc., were used, but the sores became larger, and, at the date of a month after the accident, he came to me. The conditions were then very suspicious indeed. Although there was no definite induration, most of the sores had elevated swollen edges, and looked, so far as my experience went, just as if about to indurate. They were inflamed, rather deep, and very painful. One of them, under the nail at a finger-end, did not present an actual ulcer, and, in this respect, differed from four others, all of which were exactly alike. They were big enough to have allowed the pulp of the little finger to be put into them. Their edges were not in the least ragged; all were suppurating, but not freely. There was no bubo. I advised the free use of iodoform to some, and of black wash to others. We agreed to abstain from mercury, and wait events. The sores dressed with iodoform became healthy much more quickly than the others, and we soon laid aside the black wash, and used the former to them all. Dr. P. took his temperatures regularly, and carefully watched for eruption. No eruption ever appeared, and, one month later, all the sores were healed, and there was not the slightest hardness or duskiness of the scars. I then ventured to express, for the first time, a confident opinion that there was no syphilis in the case.

Looking at all the facts of this case, it seems very probable that the sores resulted from a quasi-specific venereal poisoning, and were non-infective chancres. The patient from whom the poisoning occurred was a young unmarried woman, who had borne a child, and who suffered from papillary growths in the canal of the cervix, attended by much discharge. She died after the operation, and no investigation could be made as to whether she had ever had syphilis. The sameness in the conditions of the various sores, their steady persistence for a certain time, and their final satisfactory disappearance under the persevering use of iodoform, are facts which all fit with the idea that they were non-indurated venereal sores (that is, chancroids). The absence of gland-enlargement goes, I think, for very little; since, as I have stated elsewhere, it would seem that such sores are but rarely attended by buboes of any kind.

Second Attacks.—In 1839, Ricord made the important observation that a person who had once had syphilis was not liable to have it again. Although he believed that exceptions to this law were possible, and was anxious to admit them, yet, up to 1858, he had met with none which satisfied his mind. In the following year occurred the first case in which he himself witnessed and treated two attacks of undoubted constitutional syphilis in the same patient. The interval was nineteen years. Although with the profession generally, and even with the public, the good news that no second attack was possible spread widely, I doubt whether it ever received the unquestioning acquiescence of any authorities. Diday recorded many exceptional cases, and thought that the second attack occurred when the first was incomplete, and in some sense supplemented it. Thus, if the patient's skin had suffered in the first and his mucous membranes escaped, the reverse would be the case in the second. Later on, Gascoven and Fournier published many exceptional cases. I have myself seen many in which the patient's narrative was clear that he had had a former

attack; and several in which I myself attended the patient in both. It is, I think, now generally accepted that second attacks after considerable intervals are not very uncommon; but, at the same time, that Ricord's law holds good in reference to a very large majority. The exceptions—that is, second attacks—are probably not more frequent than in the case of variola and measles. Diday's supposition that they supplement the first has not been confirmed by other observers, nor does there exist any trustworthy evidence as to the nature of the modifications which they manifest. Sometimes they are very slight, and sometimes very severe; but exactly the same differences are observed between attacks which are the first which the patient has had.

As a rule, when a patient contracts syphilis a second time, it is after an interval of many years, and after, apparently, very perfect recovery. Neither of these statements is, however, absolutely true; I have seen a well characterised indurated chancre due to fresh contagion, within a year of the first, and before the patient was well rid of his symptoms. I have repeatedly seen them in those who still suffered from reminders of their former attack. It has been proved by experiment that in occasional instances fresh inoculations on patients suffering from syphilis may produce a certain degree of induration, although as a rule they fail. On this point, Mr. Lee has some valuable observations.

The Lettsomian lectures were delivered before this Society on the same topic as that I have now ventured to take, nearly thirty years ago, by one whose memory is regarded, I am sure, by not a few present with feelings of affectionate regret. Mr. De Méric, in dealing with the question of second infections, then spoke very strongly, and although he did not deny their possibility, affirmed that no reliable case was on record. This statement led to the publication, by Mr. William Allingham, of a case which had been recently under his observation, so extraordinary that I might have failed to give it the attention which it deserves, were it not that I have had, as just stated, in my own practice, one almost exactly parallel. A gentleman contracted a chancre in February, and took mercury until the hardness disappeared, but no longer. In May he had rash and sore throat, and again took a short course of mercury. Having left it off for a month or more, he returned, in July, with another chancre, which he believed to be the result of fresh contagion, which was not in the site of the former one, and which presented the most characteristic induration. This sore yielded but slowly to mercury, and was followed by rupia, and eventually by perioritis. It is obviously, in such cases, impossible to say whether or not the second sore is the cause of constitutional symptoms, or whether it in any way modified or augmented the effects of the first. It is always quite possible, after such a short period, that all that follows may be the result only of the first sore. I do not, however, see any reason why we should doubt that second attacks may complicate previous ones; indeed, I think I have witnessed facts which very strongly indicated that such a mixture had occurred. If it be objected that these second attacks, especially after such very short intervals, constitute a feature of clear departure, in the case of syphilis, from what we witness in the exanthemata, I must rejoin that we must not feel too sure of that. It is by no means certain that closer observation may not show that in these latter the specific poison of each is, in rare instances, capable of breeding again in the blood after very short intervals. Some of the facts as to vaccination certainly favour that suspicion. Be that as it may, however, we must take the facts as to syphilis as we find them; and candid observers will, I feel sure, not unfrequently encounter most startling exceptions to general rules.

I am precluded by want of time from mentioning cases in proof of second attacks; for I should have to produce a considerable series, and their details are long. I may, however, venture to produce one, and may assure my audience that I have several in which the evidence is just as good. I attended, almost twenty years ago, a young surgeon for syphilis. The disease hung about him, and it was two years before he finally left off mercury. He then married, and he had one or more healthy children. After an interval of about eighteen years, he came to me with a chancre on his upper lip, contracted, as he supposed, from a scratch by a broken vaccine-tube. This chancre was a very large one; and it was attended by a large bubo, and was followed by a most copious eruption. It was, indeed, one of the most severe attacks of syphilis which I have seen for some time.

Incubation-Periods.—It may seem strange that, after the amount of attention which the natural history of syphilis has received from many excellent observers, the length of the incubation-period of chancre should still remain a moot point. The differences of opinion are, however, very great. Thus, Ricord said that induration occurs

most frequently during the 7st or second week after contagion; never before the third day, nor after the third week. Sigmund, of Vienna, dealing with 261 observations, found only three with an interval as long as three weeks, and none with longer; whilst in as many as seventy-one it was only nine days. Other observers have given longer periods; and Mr. Berkeley Hill, with unwearied love of accuracy, and making use only of experimental inoculations, has constructed for us a table which seems to prove that the average period is twenty-four days, the extremes being ten and forty-six. This table comprises thirty-seven cases. Fournier and Clerc give it as twenty-one days. It is admitted that neither differences in the source of contagion nor in the part affected make any difference in the length of the period during which the poison remains quiet. If I were to speak from my own experience only, I should be inclined to make the incubation-period longer than any of the observations just quoted, and am obliged to admit that the statements of Sigmund and Ricord are almost inexplicable. I can only suppose that there has been some misunderstanding as to what phenomena constitute the limits of that period, or that it has even been counted, not from the date of the contagion, but from the first appearance of a sore. In this last supposition I am countenanced by Dr. Taylor, of New York, the very able editor of the last edition of Bumstead's work. If by incubation-period we mean, as I contend we ought to do, the interval between contagion and the production of an induration which can be diagnosed, then I believe we shall seldom find it less than five weeks, and more often six. If we date to the first appearance of a sore, then it will be a week or ten days shorter, for the development of hardness takes that time. In these statements, we of course put aside the very numerous cases in which a sore is present almost from the first, the chancre having been a mixed one. To this class I would unhesitatingly assign all in which the period is said to be short, for it is not the fact that the soft sore always shows itself directly after contagion. I believe that the incubation-periods of uncomplicated infecting sores are far more regular than is supposed. When the data are obtained from the statements of patients, they are obviously exposed to much fallacy. Many and many a time has a patient, who had assigned a period so short as to be, to me, incredible, admitted, in cross-questioning, that he had also exposed himself to risk a few weeks earlier than the occasion he had mentioned. As regards unusually long periods, there is always the same doubt as to correctness of testimony. It will be seen that, in Mr. Hill's tables of intentional inoculation, in no case was the period longer than forty-six days, or rather more than six weeks. No aberrant case, either in the direction of very long or of very short periods, that would bear investigation, has ever come under my own notice. I will briefly cite the following facts from my own observation, and leave them to make their own impression on your minds.

In the case of a medical man who vaccinated himself in the forearm from a syphilitic infant, the punctures, which had quite healed, became irritable on the twenty-first day, and were well characterised chancres on the forty-first.

In another series of cases of vaccination-syphilis, eleven patients received the virus on the same day. In all, the punctures, or vaccine-vesicles, healed; and in all they became irritable at the end of the fifth week, and were well indurated at the end of the eighth. In a father and son, it was especially noted that the irritation at the site of puncture began on the same day.

The following cases are also from my own note-books.

1. A married physician, Dr. A., on one single occasion went astray. He carefully observed all that followed, and it was not till the forty-second day that a pimple under the prepuce was noticed. A chancre developed itself, and syphilis followed.

2. A surgeon of much experience gave me the following fact; the circumstance occurred to himself. He had intercourse of a suspicious nature on one occasion only. He observed nothing whatever on the penis until five weeks and three days had elapsed, when he found a small papule. This soon after became indurated, and was followed by secondaries.

3. About the same time I had another patient, who had a chancre after a single intercourse, and who alleged that he was certain that there was no visible sore until five weeks after the exposure.

4. A well-trained observer (M. B. Lond.) exposed himself to the risk of syphilis on a single occasion, on March 4th, and then anxiously noted the results. On the morning after connection, he had a little abrasion on the prepuce close to the glans. He used lead-lotion, and in three days it healed. It remained quite sound until the second week in April, when it began to look a little dusky. On April 17th it was decidedly swollen, and just beginning to ulcerate. On April 23rd it was definitely indurated, and showed in the centre a group of

minute ashy-grey ulcers. At this date he had no rash, and no appreciable enlargement of the glands. There being not the slightest doubt that the induration was specific, I now directed him to take mercury. The course of events here illustrated is, I think, a very usual one. A small sore was noticed almost immediately after exposure, which healed in a few days. Then followed a four weeks' period of rest, and then inflammation about the little scar, and specific induration. It will be seen that seven weeks had passed before the induration was marked.

5. A young gentleman was brought to me by his uncle, suffering from syphilis. The first ailment had been gonorrhœa, and soon after this was cured, and whilst he was still under medical treatment, a chancre had shown itself. His guardian said that he could forgive the lad having been led astray, but that he felt keenly his untruthfulness, for he persisted in saying that he had exposed himself to risk only once. The surgeon who had treated the gonorrhœa had said that this story must be false, because he had during a whole month frequently inspected the penis, and was certain that there had been no trace of a chancre until five weeks after the advent of the urethral discharge. I was obliged to explain that I did not think sufficient allowance had been made for the incubation period, and that, in all probability, the lad's statement was correct. My assurance on this point not only did the lad an act of justice, but was a source of real gratification to his guardian.

6. In another case a young gentleman was exposed to risk of contagion but once. He caught a gonorrhœa, which developed immediately. Nearly five weeks after the exposure two sores showed themselves on the skin of the penis, and one on that of the abdomen, between the umbilicus and pubes. They all assumed the condition of large indurated chancres, and severe secondary symptoms followed.

Recurrent Chancres of False Indurations.—In connection with the doctrine as to second infection, it is very needful to appreciate the fact that chancres may recur. I believe that I was myself the first to draw attention to the very curious group of cases which illustrate this fact but two years after my brief notice of it; and, quite independently, M. Fournier wrote a far more complete account of the phenomena. Briefly, it is quite possible, and not a very rare occurrence, for indurations to develop, in the retrocoronal fold of the prepuce, which assume the most exact resemblance to hard chancres, but which are not consequent on any fresh contagion. They occur to those who have had syphilis, and usually, but not invariably, on the site of former chancres. They may happen repeatedly to the same individual, and cases in which this occurs afford the clearest proof that they are not newly contracted sores. They may occur at very various periods after syphilis, but usually within five years. Thus they are not to be associated with the phenomena which are definitely tertiary; at any rate, not so in many instances. Nor do they, as a rule, resemble tertiary gummata in the tendency which the latter have to grow irregularly, and to a large size; nor do they usually break down or slough-like gummata. For the most, they retain throughout the most exact resemblance to the ordinary collared chancre, and they are often wholly without ulceration. For myself, I have never, with one exception, seen them in any other position than that mentioned, the fold of mucous membrane just behind the corona, the most ordinary position for the best characterised primary sores. No doubt the development has something to do with the anatomical peculiarities of this part. Under mercurial treatment they melt away very quickly, and they are, I think, rarely attended by enlargement of glands, and never followed by constitutional disease.

The case in which a chancre, not on the penis, recurred, was one in which disease had been due to vaccination. In this instance, about four years after the first disease, one of the scars, which had for long been perfectly sound, again inflamed, and became dusky and slightly hard at its edges. Mercury very quickly, as a rule, but not always, takes away these recurrent chancres, and they are not, I believe, usually attended by any other proofs of tendency to recrudescence of the constitutional taint. I have known at least one instance in which a gentleman had his chancre indurate again repeatedly during several years, and generally with about a year's interval between the attacks, and yet he remained otherwise in perfect health. I am not sure that, in some cases, the induration may not subside spontaneously, but I have never tried the experiment of leaving them without treatment.

I will relate, as an illustration of this occurrence, a case which I have seen this morning, and with the particulars of which you, Mr. President, are cognisant, for it was on your kind suggestion that I saw the patient. This gentleman, in 1882, had warts, which were cut away. He inoculated the wounds before they had healed, and

had sores, which were followed by sore throat, loss of hair, and sores at the anus. For these he took specifics very mildly. Then followed three years of good health, and then a return of the chancres. At the present time, he has two small collar-chancres in the reflected prepuce. They are as hard as cartilage; one of them, he is certain, is exactly in the site of a former sore, but respecting the other he is not so sure. Circumstances, into the detail of which I need not enter, make him feel sure that he has not contracted fresh sores.

On Induration as a Symptom and on Syphilis without Chancre.—That we have been in the habit of attaching far too much importance to the condition of induration as an almost essential characteristic of the initial lesion of syphilis, the observers of to-day are, I think, pretty well agreed. When a sore takes on induration, it is, provided, first, that the patient has never had syphilis before, and, secondly, that no caustic has been used, a certain indication of coming syphilis. But the absence of induration goes for very little in the way of evidence, and it may vary in degree and in duration within very wide limits indeed. In many cases, it lasts only a very short time, and is only very doubtfully marked; in others, it may, in size and duration, simulate a new growth. In women, it is often very ill marked, and its characters vary much in relation to the special tissue affected. Such being the admitted, I may say, the every-day facts, it is necessary to use this symptom with great caution in the diagnosis of syphilis. How variable in character, for instance, are the initial lesions as we observe them on the fingers of surgeons. I have more than once seen severe syphilis follow a midwifery chancre, which was never more than a little dusky, scaly spot, not so large as a threepenny-bit, and never in the least excoriated.

If, however, we admit all this, we may still hesitate to admit that syphilis can begin without any chancre whatever. Yet for practical purposes that is the conclusion to which we must come. In other words, there are cases in which the closest scrutiny, aided by a patient who is not only candid but skilled as an observer, wholly fails to discover any initial lesion. These cases divide themselves into two groups, those in which an attack of gonorrhoea preceded the constitutional symptoms of syphilis, and those in which no local disease of any kind was observed. Both of these groups are, I believe, fully recognised by most authorities. Respecting the last, it is undoubtedly possible, indeed, in most instances, probably true, that a chancre had been present and had escaped recognition. Thus in the mouth, and especially on the tonsil, a sore, which was really the primary one, may not have been noticed until other symptoms appeared, and may then have been counted as part of the secondary group. I have seen several instances of this. On the genitals in women very frequently, and in men sometimes, a small indurated sore may cause such slight irritation that its existence is never discovered. But, making every possible allowance for such sources of fallacy, there still remain a few cases in which careful observation from the beginning has quite failed to find a sore, and in which every possible region has been searched. I recollect several of this kind in which medical men were the patients. In these, there was no cause for concealment, since exposure to risk was fully admitted, and the nature of the final disease recognised, and yet no clue to the original sore could be made out. If we still, as a matter of hypothesis, cling to the belief that there must have been a sore, these cases, in their practical bearing, remain very important. Is it possible that intra-urethral chancres may occur without pain, without signs of obstruction, without external hardening, and without discharge? Such is the suggestion of some, but it does not seem very probable.

Gonorrhoea-Syphilis.—The frequent occurrence of cases in which syphilis follows what was considered to be only gonorrhoea, suggests the suitability of recognising what we might call gonorrhoea-syphilis. It is known to all that Hunter regarded the poison of gonorrhoea as identical with that of syphilis, and, no doubt, it was the occurrence of cases such as I now refer to which had caused his belief. There is no danger now that the name I have proposed should mislead any into adopting again his erroneous generalisation. Cases of gonorrhoea-syphilis must be familiar to all who have opportunities for observation. The urethral inflammation is exactly like that of gonorrhoea, and by no means suggests a urethral chancre; and, in many cases, the urethra has been examined carefully with the hope of discovering local induration or a tender spot without result. I could cite several cases of this kind from my own note-books, but I shall probably employ our time more convincingly by reminding you of the observations of others.

Mr. Lee relates, from Mr. Marston, a case in which two soldiers contracted gonorrhoea, on the same day, from the same woman. Both remained under observation from the second day, the discharge having commenced immediately after exposure. One of them, who had had syphilis before, recovered without any other symptoms than those

of gonorrhoea, but in the other syphilis followed. In the latter, Mr. Marston passed sounds, and tried to discover a local induration, but without avail. The woman who had infected these men was the subject of a cutaneous syphilide, and had vaginal discharge, but no sores could be discovered in her.

In connection with these cases, Mr. Lee quotes the opinions of Mr. Pearson and of Swediaur, both of whom held strongly that syphilis not very infrequently followed a urethral discharge indistinguishable from gonorrhoea, and wholly without the occurrence of chancre.

Mr. Hill has recorded an interesting case, in which the only initial lesion discovered was a general hardening of the whole penile urethra (presumably with gonorrhoeal discharge). In explanation of these facts, it may be admitted at once, that there is nothing in the least improbable in the supposition that the particulate virus of syphilis may exist in gonorrhoeal pus. If a patient, the subject of secondary syphilis, should contract gonorrhoea, no doubt the virus would pass into the discharge, since we know that it is present in the blood, and finds its way into all products of inflammation. Witness its presence in the transparent lymph of the vaccine vesicle. Given, therefore, a person suffering from both gonorrhoea and syphilis, what would be the probable result of contagion? Very likely, as is often seen, a gonorrhoea immediately and a chancre four or five weeks later; but if the latter were omitted, it is still conceivable that the gonorrhoea might allow the absorption of the virus. Possibly, the acute inflammation of the urethra may act in preventing the local adhesive inflammation, which constitutes the conspicuous part of a chancre. This seems a more probable hypothesis than that the virus is absorbed directly, without the intervention of any sore at all. It is to be noted that in gonorrhoea-syphilis there occurs usually definite induration of the inguinal glands. A certain number of experiments have, I am aware, been tried with negative results, in the inoculation of gonorrhoeal pus from the male urethra, the patient being the subject of syphilis. M. Basset, as quoted by Hill, inoculated in this way six persons, without result. It is not improbable that the poison of syphilis is under such circumstances, much diluted, and perhaps placed under disadvantages. It is impossible to accept the evidence of such experiments as conclusive, when we remember how frequently vaccination from syphilitic infants proves innocuous, whilst it succeeds with virulence in a few. Tarnowsky (also quoted by Hill) inoculated eighteen times with the purulent discharge from the vagina of a syphilitic woman, who had no local sore. Only once did he succeed in producing a chancre, which was followed by syphilis. Mr. Morgan, in his experiments, very properly declined the responsibility of inoculating non-syphilitic subjects; and I should certainly, for one, regard a repetition of the experiments of Basset and Tarnowsky as wholly unjustifiable.

Syphilis conveyed in Vaccination with Clear Lymph.—A question which was a few years ago in dispute, but which has, I may say unfortunately, been finally set at rest, is the possibility of conveying syphilis by translucent vaccine-lymph. The belief that it was necessary to draw blood, or, at any rate, to allow the vesicle to drain after emptying it, and thus permit the escape of fresh leucocytes, can no longer be entertained. One of our own profession, with that enthusiasm for knowledge which Hunter displayed in a parallel experiment, made himself the victim, and placed the facts beyond the reach of doubt. The facts of the case are probably known to many present; but as they may be new to some, I may be permitted to relate them. They came under my personal cognisance, but, for obvious reasons, I do not mention names. The gentleman to whom I refer vaccinated his own arm repeatedly, and in many places, from syphilitic infants, being very careful on every occasion to use only clear lymph. On the first two occasions he failed, but on the third he succeeded, and three indurated chancres were the result, followed in due course by constitutional symptoms. The incubation-periods I have already mentioned, the punctures inflamed on the twenty-third day, and were well indurated on the forty-first. It is impossible not to admire the self-devotion which prompted to this experiment, and especially to the perseverance and repetition of it. Had that repetition not taken place, and had a report of results been given to the world after the first two trials, how strong would have been the conviction of all in the truth of the creed that pure lymph, even from infected vaccinifers, is safe. Not often, probably, has our science had so near an escape of being encumbered by a false fact.

The interest of this demonstration does not end with its relations to the practice of vaccination. It proves that the virus of syphilis may exist in a perfectly clear fluid, and in company with that of another specific fever. We know from experiments that if the purulent secretion of soft sores be filtered so as to get rid of pus-cells, it is no longer inoculable. The converse is probably true of the virus of

syphilis. The contagium of the one is pus, that of the other the particulate micro-parasites of a specific fever.

Now, Mr. President and Gentlemen, permit me, in conclusion, briefly to recapitulate. We have concerned ourselves this evening almost solely with the more exceptional of the phenomena which occur in connection with primary sores. We have glanced at the topics of incubation-periods; of recurring induration; of the occasional absence of the usual conditions in primary sores; and at the explanation of the differences presented by different sores of venereal origin. I have tried to reconcile the doctrines of the dualist and the unicist by showing that both are in a sense right, with, however, an abiding protest that there is but one syphilis. I have spoken also of the relations of phagedænic action to syphilis, expressing a belief, which I have often expressed before, that syphilis is in truth the parent of almost all phagedæna. By reference to this doctrine, I have tried to explain the origin of hospital-gangrene. Lastly, I have mentioned briefly the sad, but important and final, proof, that clear vaccine-lymph may contain the virus of syphilis. On most of these topics, it has been my pleasant duty to rest my conclusions quite as much on the observations and opinions of others as my own. Although I have not often, under the exigencies of the occasion, found time to mention names, yet I may now say that the excellent works of Henry Lee, Berkeley Hill, Fournier, and Bumstead, have been laid under constant requisition in preparing the statements which I have made this evening. Although I cannot pretend that there are no differences of opinion, it has been a great pleasure to note a very general unanimity of testimony on most points.

A LECTURE

ON

THE EXCITING CAUSES OF DISEASE.

Delivered before the Edinburgh Health Society.

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(Concluded from page 6.)

In order to understand the measures which should be taken for the prevention and stamping out of these specific infectious and contagious diseases, it is essential to realise that the germs which produce them are not, so far as we know, generated *de novo* (afresh), but that they are always transmitted from some pre-existing case of the same kind. The poison of scarlet fever always comes from a pre-existing case of scarlet fever, that of small-pox from a pre-existing case of small-pox, and so on. If, then, you can destroy all the poison which is given off from every case, for instance, of scarlet fever, and not allow any of it, while still active, to come into contact with any susceptible person, you will ultimately succeed in stamping out the disease. It must, however, be remembered that the poison of some of these diseases is very tenacious of life, that it may lie long dormant; and that, for this and also other reasons (notably, that the poison is invisible), it is not always possible for us to trace the exact source of infection, or to put our finger on the pre-existing case.

The most important means of stamping out the specific infectious and contagious diseases are as follows.

1. *Isolation of the Sick.*—Thorough isolation can seldom be properly carried out in a private house, and is simply impossible in the houses of the poor, where all the members of the family live in one or two rooms. Unless thorough isolation can be carried out, it is generally advisable, in the case of such diseases as scarlet fever and small-pox, which are both very infectious and very fatal, to send the patient to hospital. I am speaking generally; the special nature and circumstances of each case must be taken into account.

To provide the necessary hospital-accommodation, most of our large towns have fever-hospitals. As you are probably all aware, our Edinburgh fever-hospital, which was, until quite recently, connected with the Royal Infirmary, has been—most properly, I think—taken over by the town, and will henceforth be managed by the Public Health Authorities, and supported by the rates. I do most sincerely hope that, whatever additional rate it may be necessary to impose, there will be no complaining or demurring at it. Had time permitted, I would have liked to have read to you a lecture by Sir James Paget, in which he shows the costliness of sickness and the enormous money-

loss which the nation annually sustains from sickness due to preventable disease.¹ Now, if this is true of the nation, as a whole, it is surely true of each one of its corporate parts; in short, it can, I think, be conclusively shown that any sum, however large, which is legitimately spent in the arrest and prevention of infectious disease, is well invested and well spent money. I strongly hold that it is a sound and good financial policy to provide for cases of fever and infectious disease the best hospital-accommodation, the best medical skill, the best nursing, and the best feeding and medical appliances which can be procured. Unless a fever-hospital be conducted on the most approved principles, and officered by the highest class of officials (and that, I need not say, necessitates a large expenditure), the public will not feel full confidence in it, and will not make use of it; and the very object for which it is provided, namely, the isolation of the sick and the arrest of infectious disease, will be defeated.

Satisfactory isolation in a private house is only possible when that portion of the house in which the patient is placed can be thoroughly cut off from the remainder. There must be no passing backwards and forwards between the general body of the house and the sick-room. Those who nurse the patient must submit to be isolated too. The sick-room should be cleared of all unnecessary articles of furniture; curtains and bed-hangings should be removed; free ventilation is essential, and care must be taken that the air from the sick compartments does not blow into the rest of the house.

2. *Destruction of the Poison which is given off from the Body of the Patient.*—This is a very important point. When, as in whooping-cough, the poison is given off in the breath, free ventilation, which dilutes it, and the frequent use of the steam carbolic spray in the sick chamber, are the best means which can be employed. When the poison is given off, as it is in cholera and typhoid fever, from the bowel, a powerful antiseptic—a solution of corrosive sublimate is probably the best—should be freely mixed with the evacuations before they are discharged into the house-drains. When, as in scarlet fever, the poison is contained in the particles of skin which are thrown off from the surface of the body, frequent washing with an antiseptic solution, and the inunction of carbolised oil, are probably the best means which can be employed.

The patient must, of course, be confined to the sick chamber until all risk of infection is passed. The medical attendant is naturally the person who decides this point; and in forming an opinion it is always right to err on the side of caution, and to keep the patient strictly isolated for several days longer than may at first sight appear absolutely necessary.

After the patient leaves the sick-room, it, together with all articles of clothing, furniture, bedding, etc., must, of course, be thoroughly disinfected. Books, playthings, etc., which have been in contact with the patient, must be either disinfected or destroyed. But to go fully into this and other points would require much more time than we have at our disposal this evening.

I cannot, however, leave this part of the subject without reminding you that we citizens of Edinburgh are fortunate in this respect, that we have only to make a request to the local health-authority, and we can have our houses, rooms, furniture, bedding, etc., disinfected for us. I would strongly advise any of you who are so unfortunate as to have cases of infectious disease in your houses, to avail yourselves of this assistance. You must remember that to carry out the process of disinfection thoroughly and satisfactorily, special knowledge and skill—which few private persons, unless they be medical men, possess—are required. If the assistance of the proper authorities were always called in, we should not hear complaints, as we now not unfrequently do, of bedding and articles of clothing, which have been used by persons suffering from infectious disease—such as scarlet fever—being aired and shaken close to the windows of a neighbouring dwelling, a most dangerous proceeding, which cannot be too severely condemned.

3. *Individual Sanitation.*—The third great means of preventing and arresting the spread of infectious disease is the most careful attention to cleanliness, drainage, and all other sanitary matters by each individual for himself.

The wise saying, "If you want a thing well done, do it yourself," is specially applicable to health matters. Public sanitary authorities and sanitary associations are all very well in their way—and far be it from me to say one word which would appear even to lessen their value and importance—but they can never take the place of individual effort. Unless each member of the community attend, so far as his knowledge enables him to do so, to the sanitary arrangements of his own house and premises, we shall never have that im-

¹ An Address on the National Value of Public Health (BRITISH MEDICAL JOURNAL, June 21st, 1884).