

**Lectures on syphilis : delivered at the Harveian Society, December, 1876 /
by James R. Lane.**

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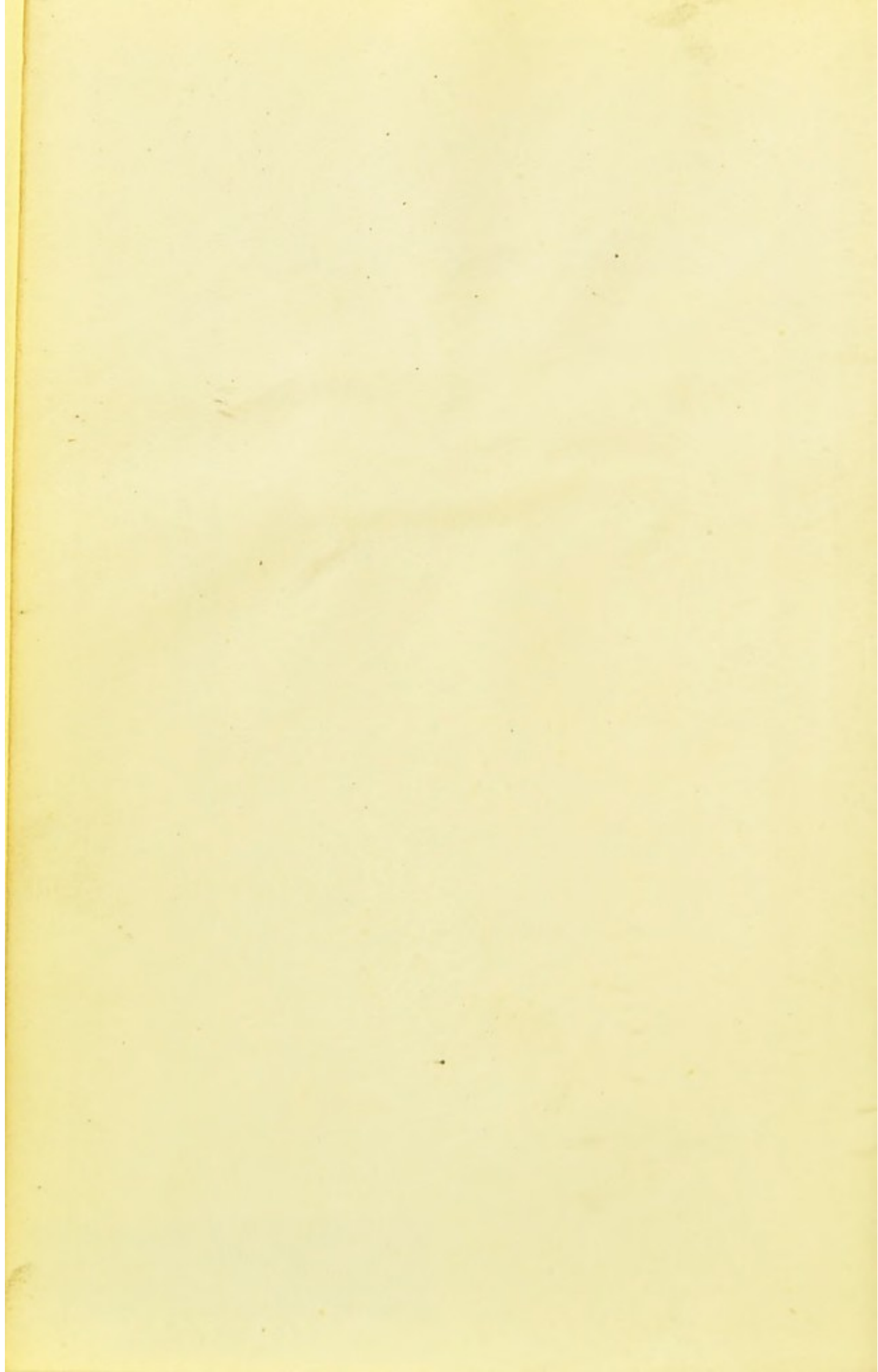
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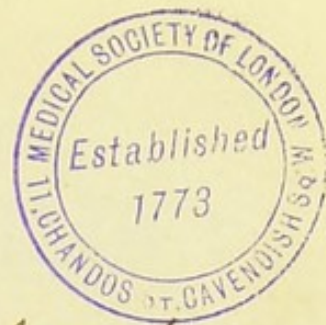
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PREFACE TO THE SECOND EDITION.



THESE Lectures were delivered at the Harveian Society, at the end of 1876, and appeared in the *Lancet* in the following year. At the repeated request of friends I was induced to reprint them in a separate form. The first edition was published in 1878, and has been for some time exhausted.

The object I had in view was to trace as concisely as possible the progress made of late years in the investigation of venereal disease, to point out what may be considered to be the generally accepted views of the Profession at the present time, as well as to indicate the principal points of divergence, and to express, as impartially as possible, my own views and opinions thereupon.

With the exception of those who have specially studied the subject there are few who have had time or patience to wade through the voluminous literature of the last thirty or forty years, and it appears to me there is still much uncertainty in the minds of

practitioners generally on various important points. Amongst them I would mention specially the unity or duality of the venereal poison, the contagion of syphilis in its secondary stage, the possibility of its communication through the medium of vaccine lymph, and the conditions of its hereditary transmission.

I have compressed what I had to say into the smallest possible compass; but I have endeavoured to sketch the leading features of the principal questions which have been the subject of discussion of late years, and to place them in as clear a light as possible, so far as our present knowledge extends.

The speedy disappearance of the first edition has led me to think that the book has been found to supply, in a convenient and concise form, a comprehensive view of those branches of the subject of which it treats, and I venture therefore again to offer it, with some additions and emendations, to the notice of the Profession.

49, NORFOLK SQUARE, W.

January, 1881.

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LECTURES ON SYPHILIS.

LECTURE I.

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MR. PRESIDENT AND GENTLEMEN,—The subject I have chosen for these lectures is, I am well aware, a very difficult one. There may be many who think that too much has been written about it already, and no doubt a great deal has been written which is either obscure, or misleading, or erroneous; but I fear that at present this is inherent in the nature of the subject, and arises out of the uncertainty and confusion in which, on various important points, this mysterious disease is still involved. Nevertheless, it has always been a favourite subject of speculation to many of the ablest and deepest thinkers in the ranks of medicine, and many important advances have been made in recent years, although unfortunately there is still much which remains unsettled and obscure.

In illustration of the conflicting utterances of the leaders of professional opinion on the question of syphilis, I would refer to the discussion which was inaugurated at the Pathological Society in the early part of 1876 by Mr. Jonathan Hutchinson. Mr.

Hutchinson has laboured at this subject most assiduously, and in his paper he put forward many of the conclusions to which his unwearying industry and acute intelligence has led him on a variety of important points in the pathology of this disease. The long debate which followed showed how profoundly the whole subject had been studied by the leading speakers, and was characterised by a display of eloquence and ability of which the profession in this country has good reason to be proud; but those who, like myself, were either present, or carefully followed the published reports in the hope of finding themselves standing on somewhat firmer ground than before, must, I fear, have been very much disappointed. Are there two poisons or only one? Is phagedæna so intimately related to syphilis that the latter may be looked upon, to use Mr. Hutchinson's words, as "the parent of all phagedæna;" or is phagedæna, as commonly supposed, merely an accident of syphilis, as it may, under certain conditions, be an accident of any other wound or ulcer? Is syphilis a specific contagious fever like scarlatina or small-pox, running an equally definite and regular, though much more protracted, course; "a fever diluted by time;" or, as urged during the debate, are not the divergences as striking as the analogies? Are secondary manifestations symmetrical, and tertiary ones non-symmetrical; and can this distinction be made practically available in diagnosis? Is syphilis a blood disease only in the secondary period, and are the tertiary symptoms merely the sequelæ of a condition which has passed away? Does hereditary transmissibility cease when the tertiary period is reached, or does it not? *Tot doctores, tot doctrinæ.* On these and on various other points there was the greatest possible difference of

opinion, and each speaker in succession could adduce arguments, of variable plausibility, in favour of his own particular belief.

Many of the above questions will be well worth our consideration, others are comparatively unimportant. For instance, the analogy between syphilis and the contagious fevers appears to me to have been greatly overstrained and, in any case, to be of little practical interest; while my experience would certainly not lead me to regard symmetry or non-symmetry as a reliable distinction between the secondary and tertiary forms of the disease.

In what I am about to say I shall refrain as far as possible from theoretical speculation, and I hope to avoid adding any contribution of my own to the uncertainties which prevail. It appears to me that I can best utilise the time at my disposal by endeavouring to trace as concisely as possible the principal steps by which progress has been made from the days of Hunter to the present time, and considering what are the points on which we have good reason to think we stand on firm and positive ground, and also what are those other points on which more light is urgently needed.

The study of venereal diseases was for a long time largely influenced, and is influenced to this day to no inconsiderable extent, by the writings of John Hunter. His treatise on this subject was among his later publications, and at the time of its appearance in 1786, seven years before his death, he was at the height of his great reputation. It is not surprising, therefore, that his doctrines were widely accepted, and that the general belief in them was not to be easily shaken. I have the greatest admiration for John Hunter, whose insatiable appetite for work enabled him to do more

than perhaps any other one man ever did towards the advancement of anatomy, physiology, and scientific surgery, but I think his treatise on venereal disease, valuable as it is in many respects, is one of the least happy of his efforts. It has been, in my opinion, very correctly estimated by his commentator, Mr. Babington, in Palmer's edition, published in 1837, whose words I will quote rather than presume to offer criticism of my own. Mr. Babington says of this treatise: "It is not without its defects, and the reputation of John Hunter should rest more on his other works than on this. Many of his remarks are rather theoretical than practical, and some of his doctrines have not obtained that general assent which has crowned most of his other labours." With reference to his other researches, Mr. Babington remarks, that "it is more to the activity of his inquiries than to the strength of his reasoning powers that we owe his discoveries. Indeed, his powers of reasoning were scarcely on a level with his other faculties, but the errors of his logic were perpetually corrected by the variety and accuracy of his experiments. In the venereal disease he had little opportunity for the exercise of his usual means of investigation, for experiments on animals are impossible in this case, and dissections give little information."

Hunter described that form of venereal sore which is characterised by indolent induration, and which bears his name to this day, with a lucidity and accuracy which leave little to be desired; but one cannot help wondering that, with his marvellous powers of observation, he should have said so little about that other and more frequent kind of sore, which must have constantly come under his notice, which runs an equally definite

and regular course, but in which induration is conspicuously absent.

But if Hunter failed to appreciate this distinction, it was equally missed by his contemporaries, and for a long time by his successors, with the exception perhaps of Mr. Evans, who, in 1819, described very accurately what we now call the soft sore by the name of *venerola vulgaris*, and who pointed out that mercury was not necessary for its cure. In fact it was not till quite a recent period that the differences in the characteristics of the two forms of venereal sore, so important in their bearings on pathology and practice, have been clearly and intelligibly laid down.

Hunter's experiments in inoculation, which he was the first to practise, led him, in opposition to his predecessors, to disbelieve in the contagion of secondary syphilis, a view which was afterwards adopted by Ricord, also misled by his inoculations, and which has only within my recollection been finally refuted. Hunter also denied the hereditary transmission of the disease, which had been previously believed in, although, under the head, "Diseases resembling Syphilis," he records several typical examples of it.

But another and perhaps more important point was Hunter's adherence to the doctrine, then pretty generally accepted, of the identity of the poisons of gonorrhœa and syphilis; although some of his contemporaries, and notably Benjamin Bell, had seen through the fallacy and were doing their best to confute it. Hunter firmly upheld this identity, and taught that the only difference between the two diseases depended upon the nature of the surface to which the poison was applied; that it caused ulceration when it acted on a cutaneous surface, but only a purulent discharge, without breach of

surface, when applied to a mucous membrane; and that the morbid secretions in either case might give rise to either set of symptoms, according to the structure with which it came in contact. Holding this belief, he sought to verify it by the experiment, now known to have been performed on himself, the result of which was so unfortunate—in the first place, from its effects on his own person, and, secondly, from the influence which it had in retarding the development of a more accurate knowledge of the nature of the disease.

Hunter inoculated himself on the glans and prepuce with the matter from a gonorrhœa; the result was the development of primary sores, followed after some months by secondary manifestations, from which he was not completely cured for three years. This naturally made a great impression on his mind; he appears to have considered the experiment as conclusive, and not to have repeated it upon others.

Ricord, more than fifty years later, when labouring to disprove the identity of gonorrhœa and syphilis, explained the unusual result of the inoculation of gonorrhœal matter in Hunter's case by the hypothesis of a concealed urethral chancre. But there is now another and perhaps better explanation in the well-proved communicability of syphilis by the morbid secretions and ven by the blood of persons suffering from it in its secondary form. Neither Hunter nor Ricord believed in this mode of communication, and it probably, therefore, never occurred to Hunter to inquire whether the patient from whom he procured the gonorrhœal matter was, or had recently been, the subject of constitutional syphilis. If that were the case, the occurrence of syphilitic infection in the person of Hunter would not now be a matter of surprise.

Now that we are beginning to know something of the remote effects of syphilis on internal organs, may I suggest, parenthetically, in connection with Hunter's own case, that the extensive arterial and valvular disease from which he subsequently suffered, and which was the cause of his sudden death in St. George's Hospital, in 1793, may possibly have been a remote tertiary result of this unfortunate inoculation made in 1767, twenty-five years previously?

The consequences of the support afforded to the doctrine of the identity of gonorrhœa and syphilis by an authority so eminent were most unfortunate. Mercury was then looked upon as the only antidote for syphilis; those who believed that gonorrhœa was a form of syphilis, therefore, gave mercury in every case of gonorrhœa, and a course of mercury in those days meant salivation to the extent of the discharge of two or three pints of saliva in the twenty-four hours, continued over a period of two or three weeks. Hunter, it is true, says, speaking of gonorrhœa: "I doubt very much of mercury having any specific virtue in the *cure* of this disease, for I find that it is as often cured without mercury as with it." But he also says: "It is always necessary to have in view the possibility of some of the matter being absorbed and afterwards appearing in the form of a lues venerea, to prevent which I should be disposed to give small doses of mercury internally."

Benjamin Bell, I have already said, would not admit the identity of syphilis and gonorrhœa, and in his work on "Venereal Diseases," published in 1792, he put forward arguments which very conclusively disproved it. He at the same time strongly condemned the use of mercury in gonorrhœa. Bell also related some valuable

experiments by inoculation performed by three medical students on their own persons, the result of which was in direct contradiction to that obtained by Hunter on himself. All three introduced the matter of gonorrhœa between the prepuce and the glans, and allowed it to remain; the result was inflammation—balanitis, in fact—but no chancre. Two of them introduced chancrous matter into the urethra; the result in both was urethral chancre, followed by bubo, and in one by secondary symptoms, but no gonorrhœa.

Hunter's views on the identity of these two diseases do not seem to have found much favour with the writers who succeeded him. Nevertheless, the practice of giving mercury for gonorrhœa appears to have been the general rule in the profession, and was not till long afterwards finally abandoned. In fact, in spite of authoritative teaching, we find Sir A. Cooper, in his lectures published in 1829, expressing himself in the following uncompromising fashion. He says: "No greater folly, or indeed cruelty, can be committed than that of giving mercury for the cure of this disease; a man must be grossly ignorant or shamefully negligent of the duties which he owes to the character of his profession and to the common dictates of humanity if he persists in doing so. I scarcely ever enter the foul wards, because patients are compelled to undergo so infamous a system of treatment that I cannot bear to witness it. To compel an unfortunate patient to undergo a course of mercury for a disease which does not require it is a proceeding which reflects disgrace and dishonour on the character of a medical institution. If you go to a patient in the foul wards at the end of his course and ask him how many times he has rubbed in, he will generally answer 'Twenty-eight times.' If you

ask him whether he is salivated, he will tell you that he spits three pints a day ; but ask him whether his gonorrhœa is cured, he will reply, ‘No, I have a clap still upon me.’” The practice must still have possessed considerable vitality—in some quarters, at all events—to induce Sir A. Cooper to speak to a class of students in such terms as these on the practice of his colleagues in his own hospital.

But even later than this, in 1833, Mr. Wallace, of Dublin, an able and acute observer, published a work, in many respects of very high merit, in which this unfortunate error was again put prominently forward. He described gonorrhœa as a variety of syphilis, calling it “catarrhal primary syphilis,” and believed it to be capable of conveying by contagion the ulcerative form of primary disease, and also of giving rise to subsequent constitutional infection, to prevent which latter result he advised the use of mercury for its cure.

It is to Ricord that the credit of finally and thoroughly disproving the identity of gonorrhœa and syphilis is undoubtedly due. In his work published in 1838 he gives the result of a very large number of inoculations with gonorrhœal matter, in none of which was any result produced. His experiments and his arguments on this subject have been almost universally accepted as conclusive ; but it is worthy of remark that, reckoning from Hunter’s time, it was more than forty years before this happy and, as we should now think, obvious result was arrived at. *Magna est veritas, et prevalebit* ; but the proverb, it will be observed, wisely refers its fulfilment to the future.

I may have dwelt upon this matter at perhaps too great length, but I have done so because it shows how much evil may arise when theories having an important

bearing on practice are put forward by eminent authorities on inadequate grounds, and how much time and labour may have to be expended in their refutation.

In Ricord's teaching may be said to have commenced a new era in the study of venereal diseases. He has, in my opinion, done more than any living authority to elucidate the subject of syphilis, and by his numerous and carefully-conducted experiments to pave the way towards clearness and precision where there had hitherto been so much conjecture and confusion. Many of his earlier conclusions have no doubt required modification and correction, and have, indeed, been since greatly modified by himself; but though he may have been too hasty in his deductions, the experiments which he performed, and the facts which he observed and recorded, will always remain amongst our most valuable possessions.

Ricord performed his experiments in the venereal hospitals in Paris in the years 1831 to 1838, at which latter date he published the results in a collected form. He made an enormous number of inoculations, and the following are, briefly stated, the principal conclusions at which he arrived. That the pus of a chancre properly inoculated always produces a chancre, provided the pus be taken during the period of progress, and not during the period of repair: consequently, the production of a characteristic pustule by inoculation with the matter of a sore was certain proof of the syphilitic nature of that sore; and conversely, if the result of the inoculation were negative, the sore could not have been truly syphilitic. That the pus from the inoculated pustule again produces a chancre, and the series may thus be

continued *ad infinitum*. That there is no period of incubation, but a continuous evolution from the moment of contact to the complete formation of the ulcer. That the matter of a suppurating syphilitic bubo—*i.e.*, a bubo caused by direct absorption from a syphilitic sore—is as inoculable as the matter of the original sore itself. That it is rarely, if ever, before the fifth day that the induration of a chancre commences; that it is usually the indurated chancre which is followed by secondary infection, the induration appearing to indicate that the poison has penetrated further into the system. That the secretion from mucous tubercles or any other form of secondary or tertiary affections will produce no result when inoculated, and the secondary affections are never communicable in any other way than by hereditary transmission. That in tertiary affections the virulent principle seems to be completely transformed, and they are neither inoculable nor hereditarily transmissible, but may be the cause of the transmission of scrofula, which is often only a degenerated syphilis.

This was Ricord's earlier teaching; it was clear, precise, and practical, and it has the merit, at all events, that there is never any difficulty in understanding what he means. In estimating its value, it must be remembered that his inoculations were always performed on the individuals diseased. He very properly condemned, and always refrained from practising, the inoculation of syphilitic matter on healthy subjects. This led him into error in several important particulars. It induced him to deny the communicability of syphilis by the secretions of its secondary lesions, and to place too absolute a reliance on the result of inoculation as a means of diagnosis; while he failed to appreciate the period of incubation which precedes the development of

the infecting form of sore in many cases. His failing, and that of his school, was in being too hasty and positive in his conclusions, and in aiming at more precision than the subject itself admits of, unmindful that there is no finality in medicine. He and his followers might have reflected with advantage, as indeed may we all, on the following words of wisdom by the late Dr. Latham. In the preface to his clinical lectures, Dr. Latham deprecates summary conclusions in practical medicine, and he says: "Let a man use his own experience as best he can for the present, but let him not, upon the strength of it, rebuke the experience of all past times, and dictate to the experience of all future; for if he live long enough, nothing is more likely than that he may find himself fallen under his own reproof, and inconveniently confronted by his own maxims."

Ricord, at the time of which I have been speaking, and for many years afterwards, was the most popular teacher in Paris. His doctrines were accepted and promulgated by a large number of disciples, and met with almost universal acceptance. It was difficult at that time to believe that they could ever be seriously shaken. *Vanitas vanitatum!* Let us see what were the prevailing opinions twenty years later among the modern French school of writers on syphilis, which was composed mainly of the pupils of Ricord. So far from holding to the doctrine which he originally taught, they contended that the true syphilitic sore—that which infects the constitution—is never inoculable on the individual bearing it, or on any one who is or has been the subject of constitutional syphilis. A successful inoculation, therefore, instead of being a proof of syphilis, should be held to be conclusive that the sore from which the matter was taken was not truly syphi-

litic—not capable, that is, of infecting the constitution of either the bearer or of a recipient. It was affirmed that there are two kinds of venereal ulcers, representing two perfectly separate and distinct diseases, and each only capable of producing its like. One, the true chancre, is never inoculable on the same individual, is usually accompanied by induration, *always* infects the system, but never occurs more than once in the same person. This is accompanied by an indolent painless enlargement of the nearest lymphatic glands, which never suppurate unless from some accidental irritation. The other, the unindurated or soft sore, is the more common of the two; is an entirely local disease, and *never* infects the system; is always inoculable on any person, whether at the time or previously the subject of syphilis or not; and may be contracted any number of times. This is not necessarily attended by any affection of the lymphatic glands; but when it is, the action induced is very likely to end in suppuration, and the matter from the suppurating gland is as inoculable as that of the original sore. The difference in the action of the two kinds of sore on the lymphatic system had not till then been clearly made out, and was, without doubt, a very valuable addition to our knowledge.

This duality view, it is as well to observe, has no analogy with Mr. Carmichael's theory, promulgated in 1818, of a *plurality* of poisons. Mr. Carmichael described five varieties of the primary affection, each characterised by its own peculiar secondary consequences. His views excited a good deal of attention at the time, but they did not stand the test of investigation, and they never obtained a permanent hold on professional opinion.

The modern theory of the duality of the venereal

virus, which differed so materially from Ricord's earlier views, has been very widely, though by no means universally, accepted. It will be interesting to inquire how this striking change was brought about.

It was led up to, to a considerable extent, by Ricord himself. In his "Letters," published in 1850, he laid down, amongst other things, that the specific induration is certain proof of constitutional infection; it is the transition from the primary to the secondary condition; and that a sore, when it becomes indurated, soon loses its principal characteristic, that of the production of inoculable pus. That a patient who has had an indurated chancre will never have another. That the indurated sore with chronic ganglionic enlargement is invariably followed by secondary disease. That if the non-indurated sore sometimes apparently produced a like result, there must have been some error in diagnosis.

It was Bassereau, a pupil of Ricord, who, in 1852, first boldly put forward the theory that the two kinds of sore indicated two entirely distinct contagious diseases, having no relation to each other, and each invariably only transmitting its like. He arrived at this conclusion by the confrontation, as he termed it, of a large number of patients with the individuals from whom they had contracted, or to whom they had communicated the disease. That is to say, he availed himself of every opportunity he could find of examining both parties, and he found that in every case those affected with chancres followed by secondary infection had derived their disease from persons similarly affected with chancre and secondary infection. On the other hand, those affected with chancres not causing symptoms of general syphilis, had, without exception, derived

their disease from persons who, like themselves, were the subjects of sores whose action was limited to the point first infected.

At the time when Bassereau put forward these views on the duality of the virus, the doctrine that the syphilitic diathesis, like scarlatina or small-pox, did not repeat itself in the same individual, was held pretty generally in France, and was gaining ground gradually elsewhere; but the inoculability of the indurated sore on the individual bearing it had not as yet been seriously called in question. It had been noticed, however, by Ricord and others, that it was only inoculated with difficulty, and that when inoculated it did not produce its like, but gave rise to a sore precisely similar to that caused by inoculation with matter from a soft chancre. Therefore, it was argued by M. Clerc, it was not necessary to imagine with Bassereau the existence of two poisons, but that the soft sore was the product of the hard sore when conveyed to a person already syphilitic; that it had permanently lost its infecting property, so that if transmitted further to a person who had never had syphilis it would still be transmitted as a soft sore, without any power of conveying general infection. To this hybrid or degenerated sore Clerc gave the name of chancroid.

Afterwards, in 1857, Fournier, who adopted the duality theory, argued that, although the hard sore when inoculated on a patient already syphilitic, might, by virtue of the incapacity of any one to contract true syphilis twice, produce a sore exactly resembling a soft chancre, yet, if further transmitted to a third person who had never had syphilis, it would resume its original character, and give rise to constitutional infection. It was, in fact, only a hard sore temporarily disguised,

and still possessed of its virulent property when planted in a suitable soil. All this was very ingenious, but after a time, when Fournier and others found great difficulty in inoculating the indurated sore on the bearer of it, the theory was cast aside as unnecessary, and they jumped to the conclusion that the indurated sore is never inoculable on a syphilitic patient, and that the examples of it which they had hitherto seen and believed in were due to a misapprehension of the real nature of the sore inoculated from, or to some other accidental cause.

In 1850 Ricord first noticed the difficulty of inoculating from the indurated sore as its progress advanced. Mr. Lee, writing in 1856, attributed this difference to the nature of the inflammatory action set up, that belonging to the indurated sore being of the adhesive, that belonging to the soft sore of the suppurating character. In the first case there is only a thin serous discharge, destitute of pus-globules; in the latter there is a well-developed purulent secretion, and it is on this circumstance, he thought, that the difference in the inoculability depends. Mr. Lee afterwards, in 1859, pointed out that if the indurated sore were artificially irritated and made to secrete pus, it became inoculable without much difficulty, but he described the results obtained as different from those following inoculation from soft suppurating sores, inasmuch as they more rapidly dried up, and the inflammation set up partook of the adhesive character, like the sore from which it was obtained.

Mr. Lee's view, that the secretion from an indurated sore artificially irritated would produce a pustule when inoculated, has been verified amply by subsequent experimenters; but it has not been found that the sores

so produced have had any of the characters of the indurated sore as described by Mr. Lee, but, as stated by Ricord, have presented appearances precisely similar to those produced by inoculation from a soft suppurating sore.

Thus was developed the doctrine that primary venereal ulcers were of two kinds, representing two distinct forms of disease, which has been accepted and followed by so many excellent authorities. The theory is plausible and tempting, and the separation is practically of great value as a general rule, but there are objections to its acceptance as an absolute law which have always appeared to me to be insuperable.

It will be necessary to refer at some length to these objections, but before doing so I wish to allude to another important point in the pathology of syphilis, which began to attract attention about this time. I mean the period of incubation which is found to precede the development of the indurated primary sore, at all events in a considerable proportion of cases. Every one who has had much to do with the treatment of venereal disease must have noticed that many patients presenting themselves with an indurated sore state that they have not been exposed to any chance of contamination for a considerable time before the appearance of the sore, the time varying from about ten days to five or six weeks, or occasionally even longer. Often a slight abrasion has been noticed at the time, but has disappeared almost immediately and been forgotten; while after an interval a thickening has appeared at the spot where the abrasion took place. The thickening is of an indolent, painless character, and distinctly circumscribed; strikingly different in these respects from the more diffused and

painful inflammatory swelling, which may attend any form of sore when subjected to irritation. It is usually attended with a superficial ulceration, but not necessarily so; sometimes it remains as a mere thickening without breach of surface. At the time of its appearance, or soon afterwards, there will be noticed in most cases an indolent enlargement of the neighbouring lymphatic glands. This form of sore, unless treated at an early period, is nearly certain to be followed by constitutional infection.

The lapse of time, to the extent even of several weeks, between the exposure to contagion and the appearance of the sore was noticed by Hunter, as well as by Swediaur and others before his time; but it is only recently that its special relation to the indurated infecting form of sore has been clearly demonstrated. The existence of a period of incubation was long denied by Ricord, who contended that it was simply a period of *inobservation*, arising from the carelessness of patients in noticing the first appearance of the disease.

But there is now an accumulation of facts which are conclusive as to this question of incubation. Some surgeons have not hesitated to inoculate sound persons with syphilitic matter; other inoculations have been made by surgeons in their own persons to test the question; and a variable time has almost always been found to elapse before the appearance of the characteristic symptoms. The material inoculated has sometimes been derived from indurated primary sores, sometimes from the secretion of secondary affections (such as mucous tubercles), sometimes even from the blood of syphilitic patients, and the result has been almost uniformly the same. The period varies, as I have stated, but it has not, as was once supposed, been

found, as a rule, to be longer when the inoculation has been with the matter of secondary sores than when taken from what is usually called the primary ulcer.

The existence of a time of incubation in many cases may be accepted as an undoubted fact, but it is quite another thing to admit that it is constant, and that it is a necessary condition of every infecting sore, as has been asserted. In many cases no such interval in the initiation of the infecting sore can be observed. It appears in such cases, within a very short period of contagion, as a pustule, in precisely the same way as the soft sore, from which it is impossible to distinguish it. But after a variable time—Ricord says never before the fifth day, but, I think, very rarely so soon as this—a thickening of the base of the sore may be observed. The surface may continue to secrete well-formed pus as before, thus giving the sore the characteristics of both kinds; or the purulent discharge may be changed for a thin, serous secretion, and the appearance becomes that of a typical indurated sore. This mode of origin I have repeatedly seen in practice, and I think many of those whom I am addressing must have witnessed the same thing. In either case, however, there may be said to be a period of incubation before the development of that very significant symptom, the induration of the base of the sore. The result, as regards secondary infection, is the same in both cases; that is to say, it is almost certain to occur unless arrested by early mercurial treatment.

It will hardly now be questioned that a sore attended with indolent painless induration, and similar multiple enlargement of the neighbouring lymphatic glands, will be almost invariably of the infecting character; but is

it equally certain that the soft suppurating sore is *never* followed by a like result? This, as I have already pointed out, has been very positively asserted, and it is the opinion now held by many good authorities, though not, I venture to think, by the majority of surgeons of experience—in this country, at all events.

Of the erroneous nature of this doctrine I have always entertained a strong conviction, for I have repeatedly seen suppurating sores which I have had the opportunity of watching throughout their course, and which have never shown any induration that I could discover, but which have nevertheless been followed in due course by constitutional disease. This has been especially the case with sores situated on the body or root of the penis, a fact which has been noticed also by Dr. Boeck and others. My individual experience on the infecting property of the soft sore may not be worth much when opposed to that of the many distinguished observers who have arrived at the opposite conclusion; but if I am wrong, I can claim, at all events, to err in good company, as may be shown by reference to the Blue-book containing the evidence taken by the Medical Commission appointed by the Admiralty in 1864, to investigate the whole question of venereal diseases with a view to their prevention. That Commission, presided over by Mr. Skey, examined a great number of witnesses, hospital surgeons and physicians, naval and military medical officers and others, including nearly all those who had paid special attention to the subject of venereal disease. The evidence is a most valuable record of professional opinion in this country on most of the controverted points relating to venereal disease, and is well worth an attentive study. On the question now under con-

sideration—viz., the possibility of secondary infection from the soft suppurating sore—nearly every witness was asked his opinion, and a considerable majority gave it in favour of its occasional occurrence, many of them stating it to be a fact within their own experience from cases which they had had under observation throughout. To use the words of the Committee in their report, “twenty-nine experienced witnesses gave evidence that sores, both soft and hard, may be followed by every variety of syphilitic eruption.” Those who deny this possibility explain these cases by saying that the sores have not been observed with sufficient care; that there has been an induration, imperfectly developed or evanescent, at some period of their progress; that there may have been another sore which escaped observation, or that the secondary affection may have been only a relapse of former disease. There must either have been an error in diagnosis, or there must have been some other sore, perhaps at some other place, perhaps at some other time. With a view to meet these cases, it has also been urged that there are two kinds of infecting sore—an infecting sore *with* induration, and an infecting sore *without* induration, the latter being not of frequent occurrence, but still essentially distinct from the soft, suppurating sore. It is admitted, however, that the diagnosis between this non-indurated infecting sore and the ordinary soft sore is very difficult, if not impossible, and can only be really established by the fact of infection when it occurs.

The difficulty of making this distinction being thus admitted, it is surely easier to believe that no such distinction exists, but that the soft and hard sores have an intimate relation to each other, and are in fact the

product of one and the same virus. The action of the virus may be modified in the case of the soft sore, so that its infecting property is materially lessened, but it still retains that property to some extent, and under favourable circumstances, and on a suitable soil, may produce constitutional disease. The modification may arise from the nature of the local process which is induced, the disintegration at the surface caused by ulceration and suppuration being more favourable to the elimination of the poisonous material; while the deposit of organised, but contaminated, lymph round the base of the sore, with blood freely circulating through it, is more favourable to its absorption into the constitution; or it may be, as urged by Clerc, that the soft sore is the result of the action of true syphilitic matter on an individual already constitutionally infected, in whom, the diathesis having been fully established, the virus can only reproduce itself in the modified or hybrid form of the soft sore, which, when transferred to a third person, will, as a general rule, be entirely local in its effects, but may, as an exception, retain the infecting property of the sore from which it was originally derived.

Mr. Hutchinson, who has recently stated his opinion that "duality is dead," believes that the soft sore results simply from pus contagion, this pus being more or less peculiar, its peculiarities being due to its having originated in syphilitic inflammation. He insists that this pus may or may not contain the germ matter of syphilis, and that the soft sore therefore will sometimes prove infecting.

The question is not merely a theoretical one, but is of great practical importance with reference to prognosis, for an implicit belief in the dualistic theory will

not unfrequently lead to error and disappointment. It is unsafe to predict confidently that any venereal ulcer, even a soft sore attended with suppurating bubo, will entail no further consequences. There is a strong probability that an indurated sore will prove infecting; and there is a probability, though not nearly so strong, that a soft suppurating sore will not; but exceptions to both these general rules will be met with, and there is really no absolute proof of the infecting nature of any given sore, but the fact of infection itself.

At the same time, though we may decline to accept the duality theory, it must be admitted that its advocates have done good service by bringing into striking relief the important differences between the two varieties of venereal sore, which thirty years ago, and within my recollection, were confusedly jumbled together. They have been too positive in their conclusions, and too prone to formulate absolute laws without sufficiently considering all the complications of this very difficult question—but they have established a rule as regards secondary infection which is sufficiently general to be a valuable indication for treatment, and to save multitudes of patients from being subjected to an unnecessary mercurial course.

There is one point, to which I have already alluded, about which I wish to say something more, because it has a very important bearing on the pathology of syphilis and on the question of a duality of poisons: I mean the inoculability of the secretion from an indurated sore, which has been so positively denied of late years, since the introduction of the double virus theory. It is said that although this sore is intensely poisonous and will produce its like when inoculated on

a healthy person, it will produce no result when inoculated on the bearer of it, or on any one who is or has been the subject of constitutional syphilis. The syphilitic diathesis being already established in such persons, their receptivity of the poison which caused it is exhausted, as is the case with the vaccine or variolous poisons.

There is some foundation for this view, inasmuch as the indurated sore can only be inoculated on a syphilitic patient with difficulty, and in some cases not at all, whereas the soft suppurating sore is easily inoculable on almost all subjects, whether syphilitic or not. The difficulty, however, has, I think, been exaggerated. Sperino, of Turin, and Boeck, of Christiania, who practised syphilisation, and, therefore, inoculated on a very extensive scale, preferred for their purpose the matter of indurated sores. Sperino says nothing of the difficulty. Dr. Boeck, a most trustworthy and painstaking authority, says that if certain precautions are taken, it is only as an exception that the hard sore is non-inoculable on a syphilitic person. He prefers this matter and constantly uses it, but admits that in proportion as the secretion is thin and serous will be the probability of failure; but if the surface is irritated and made to suppurate, a positive result may usually be expected. In 1865, during the time that experiments in syphilisation were being conducted at the Lock Hospital by Dr. Boeck, under the observation of Mr. Gascoyen and myself, I was cognisant of five cases in which this kind of inoculation was successful, and about the genuineness of which neither he nor I could entertain any doubt. The indurated sore, when successfully inoculated on a syphilitic patient, does not produce its like, but gives rise to an ulcer exactly resembling that produced by an inoculation from a soft

chancre, and is reinoculable through a lengthened series in precisely the same way. We were thoroughly convinced of the identity of the result obtained, whether the matter had been originally taken from the one form of sore or the other. There was no difference that we could appreciate in the mode of development of the ulcers, in their duration, in their progress, or in the appearance of the resulting cicatrices. If, then, it is true that inoculation from one form of the disease will produce a sore in no way distinguishable from that which is characteristic of the other form, and if, as I have before pointed out, both forms of sore may induce the same constitutional results, surely these are very striking proofs that we are not dealing with two perfectly separate and distinct diseases.

Those who support the theory of two poisons have put forward an explanation of all these anomalies and contradictions, which, if it could be accepted as true, would settle the question finally and at once. In addition to suggestions of errors in diagnosis, or that the instruments used for inoculation may have been contaminated with matter from soft sores, they urge that both these supposed separate diseases may find expression in one and the same sore, to which the name mixed chancre has been assigned by its inventor, M. Rollet, of Lyons. This mixed sore is said to be caused by the accidental implantation of both kinds of virus on the same spot. It possesses, we are told, the induration and infecting property of the one kind, with the suppurating surface and ready inoculability of the other. This double contamination may be communicated at once from a person labouring under a similar mixed sore, or from one having both forms of sore at the same time ; or a hard sore may be inoculated with

matter from a soft sore at any period of its progress, or *vice versâ*, and the mixed chancre is the result. This, it will be seen, explains all the difficulties in the way of the acceptance of the duality theory in a very complete manner. Thus, if a sore is of the soft suppurating kind at first, and afterwards becomes indurated and infecting, it must have been a mixed chancre which developed the ulcerative property of the soft sore at first, but became indurated after the proper period of incubation. If secondary symptoms are sometimes found to follow a soft sore, this must have been a mixed chancre, of which the induration may have been masked or destroyed by the ulceration, and have been overlooked. Or if a successful inoculation is made from a hard sore, and a soft sore is the result, as has so often been observed, this must have been a mixed chancre, the real character of which has not been understood. Now, it is hard to conceive that this accidental implantation of one virus upon the other can be anything but a very exceptional occurrence, whereas, if it is to be used as an explanation of all the cases to which I have alluded, it must be exceedingly frequent; and, in the last class of cases mentioned, it implies that such experienced inoculators as Sperino, Boeck, Bidentkap, and others, were incompetent to form a correct judgment of the nature of the sores with which they were dealing. Before admitting the reality of such a sore, the evidence in its favour should be much more conclusive than any which has hitherto been adduced, whereas, in fact, it has been of the slenderest possible character. It has been found that when matter from a soft sore has been inoculated upon a hard one, the latter has taken on an ulcerative action and has become readily inoculable. It has been converted into a mixed

chancre. The mixed sore is also alleged to have been produced in the converse manner by engrafting a hard sore upon a soft one, but the evidence as regards this is less clear and satisfactory. But, granting that both these facts have been observed, there is no need to call in the aid of the mixed chancre to explain them, for there is what I think a much more probable explanation ready at hand. In the first case the indurated sore is excited to suppurate by the irritating properties of the soft matter placed upon it, just as it would suppurate when irritated by a blister or by savine ointment; while the somewhat uncertain examples of the second mode of mixture are explainable by the fact that a soft sore may develop an induration at any period of its course. In point of fact the mixed chancre had its origin in the necessity felt by the advocates of the duality theory of explaining away well-known facts which were incompatible with that view, and but for this necessity, as M. Clerc well observes, it would probably never have been heard of. The hypothesis, for it is really nothing more, appears not so much to have arisen out of the facts, as the facts to have been made to coincide with a previously conceived hypothesis.

SYPHILISATION.

In attempting to give an outline of what has been done of late years in the question of syphilis I can hardly omit to mention syphilisation, the more so that I was myself instrumental, in 1865, in obtaining a trial for that method in this country; in fact, until that time, it had never been attempted here, and had always

been looked upon with disfavour. A further reason for referring to it is that it throws considerable light on several of the vexed questions which I have been discussing.

The term syphilisation was first used by Auzias Turenne, about 1844, to signify a condition which he had found to be produced by the repeated inoculation of syphilitic matter in animals. Contrary to the opinion up to that time entertained, he found it was possible to inoculate the matter of syphilitic sores in various animals; such as monkeys, cats, rabbits, and others; in whom pustules and ulcers were produced similar to those resulting from inoculation in man; and these were found to be transferable back again to the human subject. It is important, however, to notice that in none of these animals were any constitutional symptoms developed; the action was confined to the production of sores resembling soft chancres. One or two instances, however, have since been observed of inoculation from hard chancres, which seem to show that it is not impossible for constitutional syphilis to occur in animals, but without further confirmation they are insufficient to establish the fact.

Turenne found that by inoculating the same animal a number of times, a period at length arrived when no result could be obtained, the animal had become, as he thought, saturated with the poison, or in some way incapable of reproducing it, and was, as he termed it, syphilised. He then conceived the idea that this process of syphilisation might be applied to man, both for the cure of existing symptoms, and also as a prophylactic measure against future disease in healthy persons. The notion that syphilisation should be used, like vaccination, on healthy persons, in order that they

might expose themselves without fear to contagion, was one which French morality could not be persuaded to entertain, and the method was violently opposed. Nevertheless, Turenne succeeded in inducing a large number of patients to submit to it as a mode of treatment, and he came to the conclusion that the system of any individual might by a series of inoculations continued for a period of three or four months, become proof against the syphilitic poison; that existing symptoms, primary or secondary, would disappear during the process, and that he would have obtained an immunity from contracting disease in the future.

Afterwards Sperino, of Turin, took up the practice and carried it out on some hundreds of persons, but at length gave it up in consequence of the opposition which he met with. His results were the same as those obtained by Turenne, but he came to the very proper conclusion that the treatment, if practised at all, should be confined to those suffering from the constitutional forms of the disease.

It remained for Dr. Boeck, of Christiania, to develop the system on a large scale; and the publication of a work by him in 1862, followed by another by his pupil and colleague Dr. Bidentkap, giving the results of between 300 and 400 cases, again directed the attention of the profession to the subject. In fact, at that time patients were resorting to Christiania from different parts of Europe to undergo the treatment. Both Dr. Boeck and Dr. Bidentkap were in England in 1865 and were examined at the Admiralty by Mr. Skey's Committee. The Committee were so much impressed by their evidence that they suggested to Dr. Boeck that he should initiate the system in this country, and he consented to do so. The Governors of the Lock

Hospital, at the suggestion of Mr. Gascoyen and myself, placed some of their beds at the female hospital at his disposal, and he spent three months in conducting the treatment under our observation, the unfinished cases being completed by us after his departure. The experiment was watched with interest by the members of Mr. Skey's Committee, and by many other leading physicians and surgeons.

The therapeutic action of syphilisation is thus explained by Dr. Boeck. He thinks the syphilitic diathesis once established gives rise to a certain train of symptoms, which run a certain definite course, but are usually prolonged over a considerable period of time. He thinks the continued insertion of virulent matter, by stimulating the existing disease, carries it through its regular course more safely and in a far shorter time than would otherwise be required. Relapses and the occurrence of tertiary disease are therefore, he states, far more rarely met with, and on these accounts he thinks syphilisation deserves the preference over every other method.

I shall not dwell upon the details of the cases treated here by Dr. Boeck; they are to be found in the *Medico-Chirurgical Transactions* for 1867. The results were briefly as follows:—Twenty-seven patients were treated. In twenty-two the disease was recent, and no mercurial treatment had been adopted; a point on which Dr. Boeck laid great stress, as he had found mercurial treatment interfere both with the progress of the inoculations and the ultimate result; and this certainly proved to be the case with the other five patients, to whom mercury had been previously given.

The inoculations were made at intervals of three days, three punctures being made each time, till no

further result could be obtained. The average period required was about four months, the longest case taking a little over eight months, the shortest a little less than three. The average stay in hospital was five months twenty-six days. The average number of successful inoculations in each case was 145; the largest number being 296, the smallest 22. As a rule, in the cases which had not taken mercury, the symptoms disappeared completely by the time the insusceptibility to inoculation was reached. Some of them were not seen again after they left the hospital, but there were ten who remained under observation in the Lock Asylum for a lengthened period, and of these only one had any relapse, which was of the mildest character, and speedily disappeared without treatment. As regards the disappearance of symptoms, and the freedom from relapse, the result, as observed in these ten cases, may therefore be considered satisfactory.

The experiment was fairly and impartially carried out, but the number of patients treated was too limited to warrant the expression of a decided opinion. On one point, however, Mr. Gascoyen and I had no difficulty. We saw sufficient to convince us that if the treatment did everything that was claimed for it, the remedy was worse than the disease, and we were therefore indisposed to continue it in any more cases. We felt that it was not justifiable to subject a patient in the early stage of the secondary disease to the infliction of 150 or more syphilitic ulcers, each of three or four weeks' duration, on different parts of the body, thus entailing a life-long marking by the cicatrices, for so very doubtful an advantage over the recognised methods of treatment. The long stay in hospital or of confinement to the house—nearly six months on an average—

is another serious objection, while the treatment is loathsome and painful to the patient, and extremely troublesome to the surgeon and attendants; and it must always be a question whether an equal period of time spent quietly in hospital without any specific treatment at all, might not be attended with equally good results. If syphilisation had proved to be efficacious in those obstinate and protracted cases in which every other means had failed, there might have been much to be said for it, but unfortunately these are the very cases in which it is confessedly least to be depended upon. Syphilisation, it appears to me, has been tried and found wanting, and is not deserving of a place within the domain of practical surgery.

But although syphilisation must, in accordance with the general verdict of the profession, be held inadmissible as a curative measure, it has been of value in illustrating several moot points in the pathology of syphilis. I myself either practised or witnessed as many as 7,000 inoculations in the patients treated at the Lock Hospital in 1865, and they convinced me that several of the statements of late so positively asserted, are of much too absolute a character. It is certainly not a fact that the hard sore is never inoculable on a syphilitic patient. I have already alluded to this point, and will only now repeat that with certain precautions it may undoubtedly be inoculated, and that it will then produce a sore precisely similar in appearance and progress to a soft chancre.

On the other hand, the inoculability of the soft sore has been much exaggerated. It is said that it is communicable to all persons and at all times, whether they have previously been the subjects of syphilis or not. This, again, is not the fact, for, as I repeatedly

witnessed, whenever there is any great depression of the vital powers, in tertiary syphilis for instance, the difficulty of obtaining a positive result with any kind of matter is very great, though if the health can be made to improve, the inoculations will succeed. I saw one patient who was undergoing syphilisation who became refractory to all inoculation during an attack of jaundice, which lasted a month, but in whom the liability returned as soon as the jaundice had passed away. It would appear, therefore, that there are conditions under which a person is exempt from the action of the virus, even when obtained from a soft chancre.

Again, it is not the fact that the soft sore may be inoculated on the same person *ad infinitum*. A time will come, generally in three or four months, when no inoculation will succeed with any kind of syphilitic matter, or only very small pustules will be produced which are not re-inoculable. A condition of immunity is thus obtained, but how long it will last seems uncertain, probably for no very lengthened period. At all events, there is no evidence at present that it can be depended upon, as Turenne originally believed, to act as a prophylactic against future contagion. The Norwegian surgeons, indeed, have never looked upon this as an end which was attainable or even desirable; they practise syphilisation solely because they believe in its superior efficacy as a curative measure.

PHAGEDÆNA.

The relation of phagedæna to syphilis is another point of interest. Either form of venereal sore may,

as is well known, become phagedænic at any period of its course, and phagedæna is perhaps a more frequent accompaniment of venereal sores than of any other kind of ulcer. In fact, venereal sores were at one time described as of three kinds—the indurated, the unindurated, and the phagedænic. Mr. Hutchinson seems to consider that phagedæna has some special relation to syphilis, for he has recently spoken of syphilis as being, with few exceptions, either directly or indirectly, the parent of all phagedæna. I cannot agree with this view, nor can I recognise the near relationship which he suggests. Dirt, drink, neglect, and want, seem to me to be the chief progenitors of phagedæna, and syphilis to have no more than a distant or casual connexion with it. I am confirmed in this opinion by what I have seen of late years of the working of the Contagious Diseases Acts. Before these Acts were passed the most frightful cases of sloughing phagedæna were continually being sent from the Farnham Union to the Female Lock Hospital in London. The patients were the outcasts who frequented the camp at Aldershot, and who, as shown by Mr. Berkeley Hill, who visited their haunts, were driven to sleep where they could—in barns, under hedges, in ditches, and even in open drains. The lower class of women frequented by the soldiers at Woolwich and Greenwich used to be sent in like manner from the Greenwich Union, and these furnished cases only a shade less severe than those from Aldershot. The police supervision, with the frequent periodical medical inspection and consequent *early* hospital treatment, which are enforced by these Acts, have changed all this completely. Since the Acts were passed, the women from Woolwich and Greenwich have all been sent to the London Lock Hospital; the

women from Aldershot were also received there for the first year or two, until the local Government Lock Hospital was built. The result was that in a very short time phagedæna had all but disappeared from amongst them. The same thing was observed in the women sent from Dover, Canterbury, Maidstone, Colchester, and Windsor, who all, when the Acts were passed, were at first received in the London Lock Hospital. Nothing could be more remarkable than the change which was wrought in the character of the cases coming from a district after it had been under inspection for two or three months. At present, if a severe case presents itself among this class of patients, it is almost sure to be in some unfortunate who has migrated from a distance into the inspected district in order to secure being sent at once into hospital for treatment. The sores are now of the mildest character, phagedæna is almost unknown, and, what is equally remarkable, even suppurating buboes are but rarely seen. The present condition of these women is a striking illustration of the extent to which the most painful complications of venereal disease may be mitigated by early treatment, enforced cleanliness, and strictly regulated diet and habits. It is certainly a most satisfactory result that districts in which the lowest prostitutes abound, and in which every one of them is known, should now, when subjected to regular inspection, only quite exceptionally furnish examples of really formidable venereal disease.

Phagedæna, I contend, is only an accident of a syphilitic sore, the offspring in most cases of dissipation, debility, and neglect. Probably any other ulcer, under similar conditions, would be equally liable to phagedænic action. It is rarely seen in persons well to

do, or in those who are careful and cleanly in their habits ; or, if it does occur in such, it will pretty surely be found to depend upon some palpable constitutional defect which predisposes to destructive action. It certainly need not depend upon the character of the sore from which the contagion was derived, for it was abundantly shown in the early part of this century by the military surgeons in the Peninsula that the most destructive ulcerations in the male may arise from connexion with females suffering from disease so mild as to cause them scarcely any inconvenience. The faulty predisposition here evidently existed in the patients themselves.

But although it may be abundantly clear that phagedæna, in the great majority of cases, depends upon conditions proper to the sufferer, it may still be that, when once set up, it is capable of producing its like, and that the secretion from a phagedænic sore may communicate phagedæna to another person. At the Lock Hospital, two patients who had been undergoing syphilisation were, unadvisedly and without instructions, inoculated by the house-surgeon with matter from a spreading phagedænic sore. Four punctures were made in each case. In both patients sloughing sores were produced at each puncture, and in one of them they spread to an alarming extent. Something very similar, though less severe, happened in a patient with tertiary syphilis, whom I was treating by syphilisation in conjunction with the late Mr. Skey, and whom I inoculated with matter from a sore which, as it turned out, was just beginning to become phagedænic. In all these three cases the inoculated sores, even in their early stage, did not present the usual aspect of a syphilitic inoculation, but rapidly gave

evidence of spreading phagedænic action. Here the tendency to phagedæna certainly did not reside in the patients themselves, for all three had been previously inoculated with ordinary syphilitic matter, until it had become almost impossible to obtain a positive result, and the numerous old inoculations which still remained open pursued their ordinary course.

Phagedæna, then, seems capable of reproducing its like, and although it may have supervened upon a syphilitic sore, it may supersede and destroy the syphilitic action by the destruction of the infected part, and communicate to another person not syphilis, but phagedæna; and thus, if it sets in before there has been time for general contamination, it may sometimes prevent constitutional disease in the patient himself.

CONTAGION OF GONORRHŒA.

I have alluded in the early part of this lecture to the connexion which was formerly supposed to exist between gonorrhœa and syphilis, and to the abundant proof which has been furnished of the erroneous nature of this view. I now take the opportunity of expressing my entire concurrence with those who hold that there is no such thing as a *specific* gonorrhœal poison. I know of no proof whatever that a gonorrhœa is anything more than an inflammation of the mucous membrane, varying in intensity partly in accordance with the more or less irritating properties of its exciting cause, and partly in accordance with the individual constitution and susceptibility of the recipient. The most frequent exciting cause is undoubtedly to be found in the morbid secretions of the generative organs when transferred from

one sex to the other. These secretions are communicable, and in that sense, of course, contagious; but they act only as simple irritants, and the result is an inflammation without any specific character. As a general rule, the more active the inflammation which gives rise to such secretions the more acute will be the effect. But an acute urethritis, answering in every respect to an acute gonorrhœa, may be caused in the male by mucous or muco-purulent or menstrual discharges in the female, the origin of which is altogether independent of contagion. It may be caused, in young men especially, by excessive sexual indulgence, and still more by excessive sexual and alcoholic indulgence combined, and this often happens when no disease whatever can be discovered in the female supposed to have been in fault. I have had the opportunity repeatedly of examining both parties involved, and can testify to this as a fact of not unfrequent occurrence, and I quite concur in the opinion which has been expressed by Fournier, that it is as possible for a man, and especially a very young man, to *give himself* a gonorrhœa through his own imprudence and debauchery, as to *receive* it from the woman with whom he has been connected. Gonorrhœa or its equivalent has been produced artificially by the injection of solution of ammonia or of other stimulants into the urethra, and it is well known to occur occasionally after the passage of instruments or from the irritation of urinary deposits.

I believe the great majority of vaginal discharges which may give rise to gonorrhœa in the male do not depend upon specific contagion in the female at all, but arise spontaneously as the result either of some constitutional or local disorder, or, in the case of prostitutes, of the continual irritation to which the generative organs

are subjected. In proportion as these discharges are purulent, will be the tendency to infect others. I have had large opportunities of examining women of the prostitute class, suffering from all forms of venereal disease, and I have found the great majority affected with some kind of vaginal and uterine discharge. In the older or more seasoned women this is usually of a muco-purulent, more or less leucorrhœal character—hyper-secretion, with little or no inflammation—and without, in my opinion, any very actively irritating property. The younger girls, on the contrary, frequently show obvious hyperæmia of all the parts, which are acutely tender when touched, and secrete a bright yellow discharge of a very irritating quality, as is evidenced by its tendency to cause excoriations on the adjacent external parts. This is a phase through which most of them pass when they first enter upon a life of prostitution, and they, much more than the others, are therefore likely to communicate disease, probably of an acute character, to those with whom they associate. But this also is one of the consequences of their mode of life, resulting from the unaccustomed and excessive irritation of their generative organs; and does not necessarily, or as I think most frequently, arise from direct contact with gonorrhœal matter conveyed to them by men suffering from that disease.

There is, in fact, no sign whatever by which discharges from the genital organs, in either sex, arising from causes inherent in the individual, can be distinguished from those communicated by contagion. The latter are usually more acute, but the difference is only in degree, and not in essential character.

LECTURE II.

REINFECTION WITH SYPHILIS.

I HAVE spoken of the doctrine that syphilis does not repeat itself in the same individual ; that is to say, that a man does not have an indurated sore and secondary syphilis more than once in his life. Ricord was the first to enunciate this view, and he did so in very positive terms. At a later period, however, he did not deny the possibility of such a recurrence, and he likened it to the occasional recurrence of small-pox or scarlatina, but said that in all his long experience he had never seen an instance of it. Now, there can be no doubt that a patient does not contract a second indurated sore so long as he is under the influence of the syphilitic diathesis, unless perhaps when the disease has passed into the tertiary stage. If he is exposed to such contagion, the result is either altogether negative, or a soft, suppurating sore is produced, just as has been shown to be the case when an indurated sore is artificially inoculated on a syphilitic patient.

It is a very important practical question whether this immunity is permanent, and will last the patient his life, because upon this depends the question whether syphilis is curable by treatment or may wear itself out by lapse of time ; or whether it is a life-long disease, as many good authorities believe. One distinguished

speaker in the debate at the Pathological Society (Sir W. Gull) expressed his opinion that "syphilis continues to be a constitutional disease throughout the whole life of the man who has it. Syphilis once, syphilis ever; syphilis general, syphilis universal, in the man all the time he lives."

I cannot acquiesce in this doctrine; it probably owes its origin to the impression naturally made on the minds of practitioners by cases of obstinately persistent syphilis, in which, spite of all treatment, the disease seems to follow an independent course from one year's end to another; or in which, in the case of married persons, a long series of abortions or of diseased children is the result. But does not every one whose experience is long enough know of patients who had syphilis in their youth, who recovered in due course from all its symptoms, who had no relapse, and who afterwards married and had perfectly healthy children? I myself, looking back over thirty years, can call to mind numbers, not of patients only, but of friends and acquaintances, of whom this has been the history. I therefore strongly deprecate the hopeless view of the case which is conveyed by the expression, "Syphilis once, syphilis ever."

The best proof that the syphilitic diathesis can wear itself out is the fact, if it can be demonstrated, that the disease may be contracted a second time by the same person. I have seen what I believe to be unmistakable examples of this, and I fancy most of those who have been long in practice must have seen the same. There are not many instances of a second constitutional infection specially recorded, probably because till recently nobody doubted its possibility, and many who now meet with such cases do not appreciate their significance

or think it worth while to publish them. Probably also, they are not very common; for, after a patient has thoroughly got over his first attack, he either marries, or he is very careful about exposing himself to risks of contagion; he is an older and a wiser, if not necessarily a better man.

The Report of the Admiralty Venereal Committee, published in 1865, states as follows:—"Of thirty-three witnesses who were asked for their experience whether one attack of true syphilis gives immunity to the individual from a repetition of the disease, twenty-three not only declared it to be their opinion that such was not the case, but several amongst them stated that they had positively seen repeated attacks in the same person, which certainly were not relapses; whilst ten considered that an individual could be the subject of true syphilis only once."

The most recent authority on this point is my late lamented colleague, Mr. Gascoyen. A paper by him was published in the *Medico-Chirurgical Transactions* for 1875, in which he records eleven cases of re-infection which had come under his own observation. In addition to his own cases he gives in a tabular form sixty others collected from various publications, British and foreign. In thirty-five, constitutional symptoms followed the second contagion; in twenty, indurated chancre alone was the result; in five, an indurated chancre occurred while the patient was still suffering from tertiary disease. The fact last mentioned is of interest as corroborating Ricord's doctrine that in the tertiary stage the virulent principle has undergone a complete transformation, and it accords also with Mr. Hutchinson's opinion that syphilis is no longer a blood disease when the secondary period has been passed through.

I think, therefore, there is sufficient proof that a second infection with syphilis may take place; and the corollary from this is that the disease may be cured or become extinguished, a result which I fully believe does take place in a very large proportion of cases.

CONTAGION OF SECONDARY SYPHILIS.

The next point to which I have to refer is one of great importance—viz., the contagious quality of the secretions of secondary syphilitic lesions, in the elucidation of which great advances have been made of late years. In former times, and, indeed, soon after the great outbreak of syphilis at the end of the fifteenth century, the disease was supposed to be communicable not only by contact with its secretions in all its stages, but even by the breath of those infected, and very stringent measures were taken for the isolation of the sufferers to prevent its spread. Hunter denied the possibility of this mode of contagion, being led to do so through his having failed to inoculate secondary syphilitic secretions on the patients themselves. In his chapter on "Diseases resembling the Lues Venerea," he describes cases which obviously illustrate the transference of the disease from nurse to child, and from child to nurse, but he believed them not to be syphilitic, partly from their history, but mainly because they got well without mercury. At that time, and for long afterwards, mercury was held by many to be the crucial test of syphilis. If it did not cure the disease, the case was not syphilitic; if the disease got well without mercury, it could not have been syphilis. Benjamin Bell did not agree with Hunter's view as to the non-contagious

nature of secondary disease, and Hunter's commentator, Babington, who was surgeon to the Lock Hospital, also disputed its accuracy. The non-contagious theory was, however, strenuously upheld by Ricord, because he failed to inoculate from secondary lesions on the patients themselves, and it became an article of belief in the profession till about twenty or twenty-five years ago.

But some time before that the fact of the contagious character of secondary secretions had been clearly demonstrated by Dr. Wallace, of Dublin. In some admirable lectures published in *The Lancet* in 1835, he records five cases in which he inoculated healthy persons successfully from secondary lesions. All five developed indurated sores after a period of incubation, and all had secondary disease. The scientific value of these experiments was very great, although the mode in which it was obtained was no doubt unjustifiable. Dr. Wallace clearly deserves the credit of first re-establishing the fact of the contagion of secondary syphilis, but he must also bear the discredit of having been the first to communicate syphilis wilfully, and for experimental purposes, to healthy persons, they being left in ignorance of the nature of the proceeding. I am not aware that this attracted any hostile notice at the time; but what, I wonder, would happen now if any one of us were rash enough to do the like? It is not long since that a jury awarded heavy damages against a surgeon who, having contracted a venereal sore on his finger in the practice of his profession, and being unaware of its real nature, had the misfortune to communicate the infection to one of his patients during childbirth. I presume that any one who now inoculated a healthy person with syphilis as an experiment, without first fully explaining the probable result, would run the risk of a prosecution for

wounding with intent to inflict grievous bodily harm, and imprisonment or even penal servitude might be his portion.

I believe the practice has not been repeated in this country since Dr. Wallace's time. Ricord always very properly denounced it, but other Continental practitioners have been less scrupulous, and, at the cost of a number of unhappy victims, a series of valuable facts has been obtained, which confirm those given by Dr. Wallace, and place the question beyond all doubt. Time will not permit me to refer to these in detail; it is sufficient to say that the result, when successful, has uniformly been an induration at the inoculated point after a period of incubation, and secondary affections in due course.

The clinical evidence of the communicability of this form of sore by the secretions of secondary syphilis is abundant. Mucous patches on the vulva or on the os uteri are probably among the most frequent causes of infection, and similarly in the opposite direction as regards the sexes, but less frequently, the same kind of patches on the glans penis, prepuce, or scrotum. Instances of the infection of healthy children by diseased wet-nurses, and of healthy nurses by suckling diseased children, are familiar to all. In these the contagion, unless under very exceptional circumstances, must have been derived from a secondary source—mucous patches on the mouth of the child or on the nipple of the nurse. Too familiar again are the cases—I have seen numbers of them—of members of our own profession who have contracted disease in ministering to syphilitic patients, especially in midwifery practice. In these latter the poison must have been nearly always from secondary lesions, for the

primary affection has usually passed away before the period of parturition.

There are many other channels, less obvious and more circuitous than those just mentioned, through which this noxious contagion may be disseminated. A drinking vessel, a spoon, or a tobacco-pipe passed from one person to another have all been known to act as vehicles. The danger of drinking after a diseased person was recognised in Shakespeare's time, and is alluded to in "Measure for Measure," where Lucio says "By thine own confession I will learn to *begin* thy health, but, while I live, forget to drink *after* thee." Men engaged in blowing globes of glass, and who are in the habit of passing the blow-pipe from one to another, have thus been infected, and this so frequently that at some of the glass-works in France it has been found necessary that each man should have a special mouthpiece of his own to receive the blow-pipe. In the Jewish rite of circumcision, in which it is the custom for the operator to take some astringent fluid into his mouth and to apply it to the wound to arrest bleeding, the contagion has been known to be transmitted to the child, and similarly a diseased child with patches of syphilis on its genital organs might infect the person officiating. Eustachian catheters, which have been used on patients with secondary patches in the throat, and which have not been properly cleansed, have been known to convey the disease to others. I have seen one well-marked example of this. I believe also the use of public waterclosets at railway stations, hotels, and clubs, may be a not very unfrequent cause. I know this latter mode of contagion is alleged so often by patients as an excuse for their condition that it has come to be received almost with derision, but I see nothing

improbable in, we will say, a female with mucous patches on the vulva, or a male with a similar condition of the penis or scrotum, depositing some poisonous secretion, by which the next comer may be contaminated. At all events the knowledge we now possess of the many unexpected and unlikely ways in which syphilis may be conveyed, ought to make us extremely cautious in looking upon the existence of the disease as in itself evidence of immoral conduct. This is a point which, I think, cannot be too strongly insisted upon, and a proper appreciation of it will enable us often to dissipate unfounded suspicions, and to save reputations which, under the old belief, would have been perhaps irreparably damaged. It is better to allow ourselves to be deceived many times by false excuses, than to countenance one unjust imputation.

Contagion by Syphilitic Blood.—The contagious quality of the blood of syphilitic persons must also now be considered an established fact. In 1850 Dr. Waller, of Prague, inoculated a healthy boy, aged sixteen, with the blood of a woman with secondary syphilis. Five weeks afterwards indurated tubercles appeared at the inoculated points, and a secondary eruption followed. Lancereaux has collected twenty-three cases in which the inoculation of healthy persons with syphilitic blood has been practised. In six the result was the same as that just related—viz., the formation of indurated tubercles locally after an interval of incubation, and subsequent secondary disease. In the remaining seventeen cases the result was negative. The failures, however, cannot invalidate the positive results obtained in the other six cases, which seem to demonstrate beyond

doubt that the contagion is present in, and is communicable by, the blood of a person with secondary syphilis. "The life of all his blood is touched corruptibly," although, perhaps, the poison may not exist there in so concentrated a form as in the secondary secretions, as the proportion of failures to inoculate successfully seems to indicate.

Hunter has recorded several cases of disease following the transplantation of teeth, a practice which was common in his time. The transplanted teeth at first became firmly fixed, but at the end of about a month the gums ulcerated, the teeth dropped out, and secondary symptoms followed. Hunter did not look upon these cases as really syphilitic; he describes them while speaking of "Diseases resembling Lues Venerea." Mr. Lee regards them as examples of contagion by diseased blood, but the diseased tissue of the transplanted tooth is, perhaps, even more likely to have been the cause than the blood. In either case they are examples of secondary syphilitic contagion.

VACCINO-SYPHILIS.

In connexion with the question of contagion by secondary secretions and by syphilitic blood we come to the interesting question of vaccino-syphilis. The reality of the communication of syphilis by impure vaccine lymph has been clearly proved, and the opinion generally held is that it has depended upon the accidental admixture of blood with the lymph employed. Cases of this kind are extremely rare, and have fortunately till lately been almost unknown in this country; at all events, if they have occurred, they have not been

made sufficiently public to awaken the profession to a sense of the danger until Mr. Hutchinson, in 1871 and 1873, communicated to the Medico-Chirurgical Society the history of some remarkable groups of cases.

There were some who blamed Mr. Hutchinson at the time for making these cases public, but he wisely deemed it better that the knowledge of such a danger should be diffused as promptly and widely as possible. The profession in England had till then been incredulous on the point, and a full knowledge of the risk was clearly the first step towards preventing the occurrence of such lamentable accidents in the future.

It appears to me to be of the greatest importance at the present time that the true state of the case should be understood as accurately, both by the profession and by the public, as the state of our knowledge admits; even although the publicity has, as might have been expected, afforded a powerful handle to the unscrupulous opponents of vaccination, and has given rise to the most exaggerated and mischievous statements.

At first, I think, some tendency to ignore or make light of the danger was manifested in influential official quarters, but the mind of the profession is certainly now fully alive to the importance of the question. This was made very evident by the remarks which fell from several speakers in the interesting discussion which took place at the end of 1879 on the introduction of calf lymph into general use in this country.

The risk seems to have been suspected long ago, for Lancereaux states that attention was drawn to it in 1805 by an English surgeon, named Moseley. The following are the principal facts which have been recorded:—In 1814 an Italian surgeon, named Marcolini,

records as follows. A little girl, born of syphilitic parents, served for the vaccination of ten children; from these thirty others were vaccinated, making forty in all. The whole number are said to have had syphilis, and several died. Other examples of this kind appear to have cropped up at intervals in different parts of the Continent, including the well-known one at Lupara, in Naples, in 1856, when thirty-four children contracted disease, and were the means of conveying it to a number of other persons. But, in spite of these facts, the possibility of this mode of infection was hardly admitted till 1860, when Dr. Viennois, of Lyons, published a work on the subject; and he, after examining all the cases recorded, came to the conclusion that it was the blood, and not the vaccine lymph, which conveyed the disease. Soon after—in 1861—the celebrated Rivalta outbreak occurred, where the same lymph served in succession for the vaccination of sixty-four children, of whom forty-six were infected, and conveyed the disease to twenty of the mothers and nurses. Several of the children died. It is a curious coincidence, that all these severe outbreaks of vaccino-syphilis should have happened in Italy, and in obscure and unimportant places rather than in large towns.

Lancereaux collected 351 cases in which vaccination was performed with lymph which appears to have been impregnated with syphilis. Of the 351, 258 contracted syphilis, while 93 only escaped.

Since Lancereaux's work was published, we are indebted to Mr. Hutchinson for the series of cases, which are recorded by him in his usual careful and complete manner. In the first series in 1871, twelve persons, mostly young adults, were vaccinated from an apparently healthy child, but which soon afterwards

showed evident signs of constitutional syphilis. In all twelve the vaccination progressed satisfactorily, but in ten, indurated chancres were developed at the seat of the punctures by the eighth week. All were treated with mercury, and in all the primary sores quickly disappeared, but in four of them constitutional symptoms appeared five months after the vaccination. There is reason to believe that blood was mixed with the lymph in these cases. Mr. Hutchinson's second series occurred in May, 1871. The vaccinifer was a child in good health and condition, but with slight local symptoms of inherited syphilis. Twenty-six persons had been vaccinated at the same time from this child. Out of the twenty-six there were unmistakable symptoms of secondary syphilis in nine, all children, suspicious symptoms in six others, and entire escape, as far as was known, of the remainder. Mr. Hutchinson published two other cases, which I need not detail; I will merely observe that in them, as in the second series mentioned, there was no evidence that the lymph was contaminated with blood.

It seems clear, then, that cow-pox and syphilis can be conveyed simultaneously by inoculation. When this takes place, the two diseases are developed independently and without interfering the one with the other, and there is a singular uniformity in the course which they are found to follow. The vaccine vesicles are developed as usual, but at the end of a month or five weeks, and usually not till after they have healed, a chancrous induration appears at one or more of the inoculated points, and a few weeks later the ordinary train of secondary affections follows. Or the result, as regards vaccination, may be negative, as it so often is in re-vaccination; the punctures heal and

are forgotten, but after the usual interval the syphilitic symptoms make their appearance.

I have referred to the prevalent opinion that it is the blood, and not the vaccine lymph, which conveys the contagion—a view which seems to be strengthened by the fact that Rollet and others have vaccinated with lymph from syphilitic children, carefully avoiding any admixture of blood, and the result, as regards syphilis, has hitherto been negative. Such negative results, however, are quite inconclusive; for, even with syphilitic blood, carefully and purposely inoculated, the failures have been in the proportion of about three in every four attempts. The question is—Can we be certain that the admixture of blood is necessary? Mr. Hutchinson thinks that probably it is not, but he evidently thinks that it greatly increases the risk; for he suggests that if the vaccine vesicle be drained too freely, corpuscular elements of the blood and tissues may readily become mixed with the lymph, and that this mixture may account for some persons becoming infected, while others vaccinated from the same source escape. This, however, is hardly more than an hypothesis, and the only safeguard we have is to inquire carefully into the antecedents and present condition of every child before vaccinating from it, and if there is the slightest doubt attaching to its history, to reject it as ineligible. It is clearly proved that syphilitic blood may convey syphilis, but it is not yet proved that vaccine lymph from a syphilitic child, even though unmixed with blood, will not do so, and it would therefore be the height of imprudence to act on any such belief.

Mr. Hutchinson's cases are all examples of direct communication from a diseased child to those vaccinated from it, and this is a mode of transfer which one's mind

admits without much difficulty; but it is not easy to conceive that healthy persons so inoculated, when made use of a week afterwards to vaccinate others, and long before they have themselves developed any syphilitic symptoms, should be equally capable of conveying that disease, as is stated to have happened in most of the Continental groups of cases. Can it be possible that a syphilitic inoculation which does not affect the system sufficiently to produce even a local result, in the form of a primary sore, for five or six weeks, and no general result till some weeks later, should in one week have so contaminated the whole mass of the blood as to render it, or its products, inoculable at the end of that short time? This would imply a general and thorough infection of the system in the early part of the incubation period — a view in the pathology of syphilis which, as far as I know, has never yet been suggested.

It is so extremely unlikely that, for myself, I cannot accept the facts as authentic without much fuller and more rigorous investigation. If it were true the prospect would be indeed alarming. It would be useless to inquire into the health of a child before vaccinating from it, for it might have been infected by vaccination from a syphilitic child a week before, in which case it would appear perfectly healthy, and would not present any outward sign of syphilis till four or five weeks later. It would, in fact, be necessary to search back through a series of five or six consecutive vaccinations before any certainty of safety could be attained. If the disease were really capable of being conveyed in this manner in geometrical progression, vaccino-syphilis would surely have been much more frequent amongst

us by this time, instead of being the very rare exception which we know it so fortunately is.

If proper precautions are taken in the selection of the children from whom the lymph is procured, I believe the chance of the communication of syphilis by vaccination to be so small that, though it should never be lost sight of, it should not be allowed to weigh in the balance for one moment against the thousands of lives which are annually saved, and the many more thousands of persons who escape mutilation and disfigurement, through the beneficent influence of Jenner's great discovery.

Public attention having now been fully directed to this subject, it is very satisfactory to find in the Report of the Local Government Board for 1879, the statement that in no case where the lymph had been supplied by the Government had there been "any suggestion of a resulting case of the disease which is alleged to be specially communicable by vaccination."

But, however small the risk, it seems greatly to be desired that a systematic provision for the supply of vaccine lymph direct from the calf should be introduced by authority in this country. It has been found to answer admirably in Belgium and elsewhere, and has of late been very powerfully advocated here. One great advantage arising from it would be, that by furnishing an alternative supply, it would remove all reasonable grounds of objection from the minds of those who have been unduly alarmed by the exaggerated statements which have of late been widely promulgated, and who would prefer to expose their children even to the risk of small-pox than to any possibility of syphilitic contamination.

CONTAGION OF THE SECRETIONS OF SYPHILITIC
PATIENTS.

The question of the contagious property of the morbid secretions of syphilitic persons, which are not directly derived from the specific syphilitic lesions, such as the pus of an ordinary abscess, the morbid secretions of mucous membranes, the pustules of acne, the serum of eczema, etc., is analogous to that of unmixed vaccine lymph. The matter of acne and of eczema from syphilitic patients has been inoculated by Diday on healthy persons, but with no effect, and the tendency has been to consider all such secretions as harmless, or, at least, that the contrary is not proven. Recently, however, in 1870, the late Mr. Morgan, surgeon to the Westmoreland Lock Hospital, Dublin, related some experiments which favour the opposite conclusion. He performed a number of inoculations with the matter of purulent vaginal discharge. When the discharge proceeded from a patient free from syphilitic taint the result was negative, but when it was taken from a patient with secondary disease, the effect was the production of an ulcer exactly resembling a soft chancre, and, like it, readily re-inoculable. He was careful to ascertain by examination, externally and through the speculum, that no ulceration, either primary or secondary, coexisted with the discharge. His experiments were performed either on the patients themselves or on others who were syphilitic; he did not, of course, feel justified in inoculating healthy persons. The result was exactly what has been found to happen when the matter of hard sores or of secondary affections has been inoculated on syphilitic

patients, and there is good reason to suppose, therefore, that these purulent discharges from syphilitic patients are charged with syphilitic infection, and might convey it to an untainted person. This would explain the result of Hunter's inoculation of himself, and also those exceptional cases in which syphilis is caused by the secretions of patients apparently only suffering from gonorrhœa.

Next, as to the *physiological* secretions of syphilitic persons—the tears, the saliva, the perspiration, the semen, and the milk. The balance of opinion is against their contagious quality, always supposing them to be unmixed with the products of any secondary lesion. Inoculation with the first three—namely, saliva, tears, and perspiration—has been tried, and has hitherto failed. I do not know that the inoculation of semen has been tried, but I believe there is no instance known in which, when the male organs were free from disease, the semen of a syphilitic patient has infected a female in the usual way by the production of an indurated sore. There is, however, another way in which the semen may possibly convey disease. There are some who think that an infected man, cohabiting habitually with the same woman, may develop a secondary infection in her by the gradual absorption of the elements of the contaminated seminal secretion into her circulation, without the intervention of pregnancy, or any kind of primary lesion. The late Mr. Porter, of Dublin, and the late Mr. Langston Parker have recorded cases in favour of this view, which is supported also by Mr. S. Lane. All these are authorities whose opinion deserves attention, and the point is one well worth further consideration. The difficulty in

all these cases is that the woman may at some time during the cohabitation have had an indurated sore, which caused her so little inconvenience that it remained unnoticed.

With respect to the milk, it is generally believed that a healthy child suckled by a diseased nurse will not contract disease unless the nurse has some secondary lesion on the nipple or in its neighbourhood;—that the milk itself, in fact, does not convey the disease. It is possible, however, that the milk may be possessed of infecting properties, but, if the child's mouth and lips are free from abrasion, may yet be passed into the stomach without causing any mischief in its transit, and then be so changed by the action of the gastric fluid as to lose its contagious quality. Some experiments do, in fact, throw doubt on the innocuity of this secretion. M. Voss, of St. Petersburg, has recently inoculated three prostitutes with milk from a syphilitic woman. The milk was injected subcutaneously with a syringe. One of these women was already syphilitic; in her no result was obtained. In another, whose condition as to previous syphilis was doubtful, the result was also negative; but in the third, a girl aged sixteen, who had never had syphilis, papular spots appeared around the inoculated point forty days afterwards, and were soon followed by a general syphilitic eruption.

I do not pretend to offer a decided opinion on these various uncertain points; but I see no reason why secretions, whether natural or morbid, which are formed from diseased blood, should not partake of the contagious quality of the fluid from which they are derived. At all events, the only safe course is to regard every one of them with profound distrust; and to take such

precautionary measures, and to give such advice, as the circumstances seem to call for.

HEREDITARY SYPHILIS.

The question of hereditary syphilis is one of great practical interest, and it is important that a distinct and comprehensive idea of the modes in which, so far as is known, this form of contamination is possible, should be present in the mind of every practitioner. The following is an epitome of the ascertained facts and prevailing opinions on this subject:—

Hereditary transmission is possible (1) From father to child; in connexion with which arises the question whether the mother may remain healthy throughout, or whether she will probably be infected by the diseased foetus during pregnancy; (2) from a previously healthy mother who becomes infected during pregnancy; (3) from a previously diseased mother, the father being healthy; (4) when both parents are diseased.

1. The first question is—Can a syphilitic father beget a diseased child, through the influence of the semen on the ovum? and if so, may the mother remain healthy, or does she, probably or necessarily, become infected secondarily through the diseased foetus? The possibility of the father thus infecting the child has been doubted, but the balance of opinion is now strongly in favour of its occurrence: in fact, in the late debate at the Pathological Society, all the speakers who alluded to the subject seem to have adopted this view as beyond all doubt. The question of the mother remaining healthy under such circumstances requires

more consideration. This mode of infection of the mother has been disputed, partly on account of the difficulty of proving that she has not contracted the disease through some other channel, but also on the ground that there is no direct communication between the maternal and the foetal blood-vessels. The latter objection is not worth much, when we consider the interchange of blood-material constantly taking place between mother and foetus. Still, many good authorities have supported the view that the mother may escape. Recently Dr. Kassowitz, of Vienna, has investigated a large number of cases at the Lying-in and Foundling Hospitals of that city. In seventy-six of these cases his information was sufficiently accurate to exclude, in his opinion, every source of fallacy, and in forty-three of them the mothers were not syphilitic, the fact being established, it is said, by a careful watching of their health, continued over a series of years.

Such evidence, at first sight, seems conclusive; still one cannot help doubting the frequency, to say the least, of so favourable a result, and for the following very cogent reason:—If perfectly healthy mothers frequently gave birth to diseased children, their subsequent infection by suckling such children would surely have been noticed before now. It is well known that a diseased child almost invariably infects a healthy wet-nurse who suckles it, but the infection of a mother by suckling her own diseased child is as yet unknown. This fact was first noticed by Abraham Colles, of Dublin, so long ago as 1837, and quite recently Mr. Hutchinson has again drawn attention to it, speaking of it as “Colles’s law.” Mr. Hutchinson puts the question, “Has any one ever known a case in which a mother suckling her own syphilitic infant contracted

a chancre on her nipple from its mouth?" No such case has, I believe, yet been recorded.

In favour of the possible and probable transmission of syphilis through the foetus to the mother, most authorities, both British and foreign, are agreed. Ricord, who for a long time denied the communicability of secondary syphilis in any other way than by hereditary transmission, has always taught that a man may beget an infected child which shall convey the secondary virus to its mother. Depaul, in 1851, affirmed that the embryo, alone diseased for a time, might, in its turn, during its sojourn in the womb, infect the mother. Mr. Hutchinson, twenty years ago, published fifty cases confirming this view, and he believes that the liability of the mother to contagion of this kind is very great; whereas the cases are rare in which a wife contracts disease from her husband until pregnancy supervenes, always supposing, of course, that he has no symptoms of a directly communicable kind. Mr. Hutchinson also thinks that when the mother is thus infected, the form of disease conveyed to her closely resembles that of the father, so that if the father has passed the secondary stage and possesses only a taint of the blood or tertiary manifestations, the mother is likely to possess symptoms resembling the tertiary class. Dr. Broadbent considers that when a woman becomes contaminated through a syphilitic foetus, she may have no secondary symptoms whatever, but pass straight to the manifestation of tertiaries. My own experience would be confirmatory of these opinions as regards the mothers to this extent: that they *may* from the first show symptoms such as nodes, generally referred to the tertiary class, but they usually also show some symp-

toms belonging to the secondary period; and I very much doubt whether the blood-contamination belonging more especially to the latter period is ever altogether absent in such cases, and whether close inquiry would not generally reveal some evidence of its existence. But although one or both parents may have the disease in a form which may perhaps be transitional between the secondary and tertiary periods, this is quite exceptional in the child, which almost invariably shows symptoms of early secondary disease in a severe form. Why there should sometimes be this marked difference in the character of the disease in the mother and the child there is at present no satisfactory explanation at hand.

Again, there are cases in which the mothers of diseased children present no outward symptoms whatever, or if any such have existed they have been so slight as to have been unnoticed, instances of which have probably come under the observation of many of those present. But something must have happened to them, or how is it, as before explained, that they are never infected by their own children after birth? Mr. Hutchinson suggests that they are probably examples of the secondary stage being passed through without external signs, except, perhaps, some slight indefinite ailments during pregnancy, and that the mothers may have thus obtained their immunity, but that they may develop tertiary disease after long intervals. "We have here," he says, "a form of syphilis which is protective, but which is unattended with any cutaneous outbreak. Thus, syphilis acquired by blood-contagion from the foetus would appear to be for the mother a parallel with vaccination with regard to small-pox; she gains immunity without suffering from any severe form of disease."

There are then ample grounds for the belief that the ovum, though it may possibly escape, is very frequently infected by the semen of the father; in which case it is nearly certain that the mother will be infected through the foetus. At all events, her complete escape, under such circumstances, is of exceedingly rare occurrence.

2. The second case for consideration is that of a mother becoming first infected during pregnancy, and conveying disease to a healthy foetus in utero. This is analogous, inversely, to the case of a foetus diseased by the father infecting in utero a healthy mother. If the one can take place, why not the other? Ricord taught that until the sixth month the mother may transmit constitutional syphilis acquired during gestation, but if the infection of the mother take place during the last three months, it is not certain that transmission is possible. The evidence available seems to be pretty clear that a mother contracting syphilis during the first six or seven months of pregnancy may communicate it to her child before its birth, and that the earlier she is infected the greater will be the risk. Diday gives eleven cases of its occurrence, though to the genuineness of some of them exceptions have been taken, and, indeed, it is extremely difficult to find cases so complete as to exclude all the numerous sources of fallacy. There will often be strong probability, but seldom absolute certainty.

3. Next, supposing the mother to be diseased before conception, the father being healthy. It is generally admitted that in this case the probability of the child being diseased is very great. There is a double chance of this happening—first, through the infection of the ovum by the mother *ab initio*, which is analogous to infection by the semen of the father; and secondly,

through the maternal fluid during intra-uterine existence. At the same time, it is not a certainty that a diseased mother should infect her child. Mr. Berkeley Hill records three cases under his own observation in which the children escaped, although the mothers all showed symptoms during the pregnancy.

4. The only remaining case is when both parents are diseased. This is a question which hardly requires discussion; for, if hereditary transmission is admitted, these are the circumstances most favourable for its occurrence; and, in fact, there are many who have denied that the child can possibly escape. This, however, is probably too sweeping an assertion. As Diday remarks, when both parents are under the influence of a diathesis weakened by time and by the action of remedies, may it not be that this double chance is less serious than the single one in which one of the parents has the disease in a severe form?

My own belief is that syphilis may be transmitted hereditarily in every one of the ways above mentioned, but that in every one of them there is a possibility that the child may escape. The older the disease in the parents, the greater is the chance of immunity. The possibility of escape is shown by the occasional birth of a healthy child intervening between others who are diseased—a fact which has been repeatedly observed, and of which I have seen several well-marked examples. When this happens, the diathesis in the parents is probably temporarily in abeyance under the influence of remedies or from some other cause, and they are thus rendered incapable of conveying the infection.

The ordinary history of persons who produce syphilitic children is pretty much as follows. At first the ovum perishes and abortion takes place, usually at an early period. Subsequent impregnations are also followed by abortion, but probably at a later stage. So common is this that repeated abortions in early married life afford good grounds for suspecting syphilis, and investigation will probably reveal present symptoms or a syphilitic history in one or both parents, more often the father. Later on a child is born at the full term, but with well-developed symptoms at birth; this, however, is not very common; but when it does occur, such children, I believe, usually die. Much more frequently the child is born apparently healthy, but the ordinary train of symptoms sets in a few weeks after its birth. Mr. Hutchinson suggests as an explanation of this that the development of the syphilitic yeast, as he calls it, is restrained during intra-uterine life, and comes into full activity only with the function of respiration. These children, under judicious treatment, very frequently recover. In the end, the taint in the parents appears to wear itself out, and healthy children are born who may show no evidence of disease throughout life.

I do not, of course, mean that the whole series of events just sketched occurs in any given case; I merely wish to indicate the *kind* of sequence which is met with in practice. It will coincide more or less closely with the stage of the disease present in the parents.

The question of the period at which hereditary transmissibility ceases is another point of interest. The general opinion, which I believe to be correct, has long

been in accordance with that originally enunciated by Ricord that when the tertiary stage is reached the disease is neither contagious nor hereditarily transmissible. The secretions of tertiary syphilis have been inoculated, but never successfully, and the evidence is altogether negative as to the occurrence of contagion in this stage. Again, it is certainly the rule that when the parents have fully reached the tertiary stage the children born to them are free from all signs of syphilis. Mr. Hutchinson, who holds that syphilis is no longer a blood-disease after the secondary period, believes, I think somewhat inconsistently, that the power of hereditary transmission persists long after the cessation of blood-contamination; but I venture to ask—how can we ascertain when the blood ceases to be contaminated? and is not the fact of the transmission sufficient evidence that it still is so? It is often noticeable that symptoms of both the secondary and tertiary stages coexist; the two stages, in fact, overlap each other; and perhaps the tertiary stage which is setting in attracts more attention than the secondary stage which is dying out. Are not these the cases which have been supposed to be examples of hereditary transmission in the tertiary period? I would ask, further, whether there are any instances in which persons who have fully passed into the tertiary stage, and are suffering, we will say, from gummata or destructive bone-disease, have produced syphilitic children?

What I have just suggested would be corroborative of Mr. Hutchinson's view, that syphilis is only a blood-disease during the secondary stage, and that tertiary symptoms may be sequelæ arising out of deposits in the tissues which have been left over from the secondary period, and have been long quiescent. Several dis-

tinguished speakers in the late debate argued that syphilis is a blood-disease from beginning to end; but if it is so, it has undergone very remarkable modifications in the tertiary stage, as shown by its mode of action on the individual, by its non-contagious effects on others, and also, I would urge, by its non-transmissibility to offspring. The instances to which I formerly referred of the production of an indurated sore followed by renewed secondary affections during the tertiary stage, afford striking proof of the change which has taken place.

In relation to hereditary transmission the question is often asked of us by men who have had syphilis whether they may safely marry. The answer should always be a very guarded one. It is impossible to fix with any certainty the time when the taint has passed away, but a year at the very least should have elapsed after all the symptoms have disappeared before a man is justified in marrying; and he should be told that even then he does not do so without risk, the responsibility of which he must take upon himself.

The effects of inherited syphilis in after-life are interesting and remarkable. Here, again, it is to Mr. Hutchinson that much of our present knowledge is due. He has pointed out the connexion between infantile syphilis and the affection of the cornea known as chronic interstitial keratitis, which has been generally accepted by the profession as arising from inherited taint. The infantile symptoms may have passed away at the end of a year, and the child appear perfectly healthy; but at the age of ten to fifteen, or later, the corneitis makes its appearance. He considers

that although it is so long deferred, it should, nevertheless, be classed amongst the secondary affections, partly because it is usually symmetrical, but also because in the rare cases in which it has been observed in connexion with *acquired* syphilis, it has been associated with the early secondary stage. Mr. Hutchinson has also described a form of deafness which is not unfrequently met with in the subjects of inherited syphilis, which, like the corneitis, he considers to be a delayed secondary manifestation.

Another point which, as is well known, has been much insisted upon by Mr. Hutchinson as evidence of inherited syphilis is the presence of a peculiar lunated notch in the lower border of the permanent central incisor teeth of the upper jaw. His views on this point, however, have not been quite so generally accepted. At a recent meeting of the Association of Surgeons practising Dentistry the relation between these teeth and hereditary syphilis, though acquiesced in by the majority, was called in question by several speakers of note, and amongst them by Mr. Henry Lee, who believes them to be merely the result of impaired nutrition, whether caused by syphilis, scrofula, or any other condition. I have myself never been convinced that these deformities of the teeth are pathognomonic of congenital syphilis, and I agree with Mr. Lee that much unhappiness may be occasioned in families by the too ready assumption that they are invariably so caused.

Another peculiarity in connexion with inherited syphilis on which Mr. Hutchinson lays stress is that the subjects of it seldom present the conditions which chiefly attract attention in the tertiary stage of acquired disease. Gummata in the cellular tissue, muscles,

viscera, or cerebral meninges are, he says, almost unknown, and the same is the case with the paralysis of single muscles and other affections of the nervous system, which, from recent researches, are now believed to be not unfrequent in the subjects of acquired disease.

I think, however, that on this point Mr. Hutchinson's views are open to question; for in my experience, tertiary affections, and especially gummata, soft nodes, and destructive bone-disease, are at least as frequent in the after-life of the subjects of inherited syphilis as the corneitis and otitis which he has described.

VISCERAL SYPHILIS.

I must now allude, though but very briefly, to the subject of visceral syphilis, which has only attracted the attention of the profession within quite a recent period. In this direction most of our present knowledge is owing to the labours of physicians. In former times physicians used to look upon venereal disease as an unclean thing, with which they would rather not meddle; and the subject, therefore, fell almost exclusively into the hands of the surgeons. When the physicians began to direct their attention to it more closely, there were some who thought that in doing so they were intruding upon the province of the surgeons. It was an intrusion, however, from which nothing but good was likely to arise to both sides; much good has already arisen from it, and much more, I doubt not, will be forthcoming in the future. I do not think it is sufficiently remembered that we owe to a physician—the late Dr. Robert Williams, of St. Thomas's Hospital—the introduction of the iodide of

potassium in the treatment of syphilis, without doubt the greatest improvement which has been effected in this century. Specialism in a limited sense may be admissible, and even advantageous; but specialism, in the spirit in which it has been of late years too often pursued, not only in this disease, but in many others, is one of the worst features of the profession in our time. Anything like a narrow specialism in a disease such as we are discussing, with its complicated and wide-spreading ramifications, cannot be too strongly condemned.

In this country it was Dr. Wilks who, some fifteen years since, took the lead in pointing out the injurious effects of syphilis on internal organs. Modern research, he then said, has been mainly in the direction of discovering a wider influence for the venereal virus, and tends to show that the internal organs may be affected equally with the external, that "not only the cranium, but the brain within it, or the nerves; not only the muscles of the limbs and tongue, but the heart; not only the pharynx, but the œsophagus; not only the larynx, but the trachea, bronchi, and lungs; also the liver, spleen, and other viscera." He thinks that the fibro-plastic material deposited in various organs in infiltrated though circumscribed masses, usually called gummata, belong to the stage of true syphilis, but that this may leave behind a morbid state of system tending to fatty degeneration, and especially to the changes known as lardaceous or waxy, which latter should alone be looked upon as sequelæ, and to them alone should the term tertiary be applied.

Dr. Wilks does not approve the usual division of the disease into stages. He has recently said: "Either a man has syphilis or he has not; he either has poison

in him producing all these peculiar morbid products, or he has not." And he believes "that all these visceral changes which are observed are due to the true syphilitic process, and take place at the same time." Most of us are in the habit of looking upon these deposits as belonging to a later or tertiary stage; but the opinion expressed by Dr. Wilks is well worthy of consideration; and, further, the period at which these deposits may originally be laid down, to wake up, possibly, into activity long afterwards, is a point of great interest and deserving further investigation. Mr. Hutchinson has well said that "the visceral pathology of the secondary stage forms a chapter in the history of syphilis which has not yet been written, and for which we have but few data." We have been apt, no doubt, as surgeons, to occupy ourselves too exclusively with those outward symptoms which force themselves upon our observation, thinking little of possibly deeper and more vital influences; but it is to be hoped that, if they frequently coexist, they are equally amenable to the beneficial influence of treatment or to the still more beneficial influence of time.

In the investigation of syphilitic diseases of the nervous system great advances have recently been made. It has been shown how, with good reason, many cases of epilepsy, of hemiplegia, of limited paralysis of particular nerves, may be ascribed to this cause, and may, in a large proportion of cases, be remedied or entirely cured by treatment directed on this assumption. In the elucidation of this difficult branch of the subject the labours of Dr. Reynolds, Dr. Hughlings Jackson, Dr. Buzzard, and of my colleague Dr. Broadbent, are deserving of special mention and acknowledgment.

In the study of internal syphilis, however, which is now being so actively pursued, I may perhaps be permitted to suggest a caution against too great a readiness to form *post hoc, propter hoc* conclusions, and to rely too implicitly on a previous syphilitic history as a mode of accounting for remote pathological phenomena.

LECTURE III.

TREATMENT.

WHETHER syphilis has existed from remote ages, or made its first appearance towards the end of the fifteenth century, soon after the return of Columbus from America, is a question which cannot be said to be finally decided. There can be no doubt, however, that a very serious epidemic of syphilis broke out in 1495, in the French army besieging Naples, and that the disease then assumed a form which attracted an amount of attention never before accorded to it. If it had existed previously, it was probably not till then that the relation of the primary and secondary manifestations to each other, as cause and effect, became fully recognised.

With reference to treatment, it is remarkable that within a very short period of this great outbreak of syphilis in 1495, mercury began to be employed for its cure; and, through evil report and good report, it has held its own as the first among anti-syphilitic remedies. At length, in spite of much persistent and influential opposition, its reputation seems to be firmly and permanently established.

Mercury was not at first administered internally, but was used in the way of inunction or fumigation, but so injudiciously that patients were almost killed by the

violent salivation, and the remedy was soon felt to be worse than the disease. In consequence of this, it was succeeded for a time by sudorific decoctions, especially of guaiacum, which was introduced about 1518, and some years after by sarsaparilla. These remedies had a great reputation for a time, but it was not long before their inefficiency was appreciated, and practitioners began to fall back upon mercurial inunction. The mercurial and the sudorific plans had each its partisans for a long time, but by the middle of the eighteenth century the latter was pretty generally abandoned, and mercury again had the field to itself. In Hunter's time, and long afterwards, it was believed that mercury was the only remedy for syphilis; that syphilis could not get well without it; and that anything which did get well without it could not have been syphilis. Speaking of his treatment of his own case following inoculation, Hunter is reported to have said in his lectures, "I knocked down the disease with mercury and I killed it." Similar views were held by Bell and by Abernethy. Many of Abernethy's cases of "pseudo-syphilis" were evidently examples of the true disease, but he was misled as to their real nature, because they got well without mercury.

The fact, now so well established, that syphilis has a strong tendency to get well of itself, accounts for the supposed success of the various non-mercurial methods which have been in vogue at different times; while the gross abuse of mercury formerly equally accounts for the disrepute into which that remedy has repeatedly fallen. In the first two and a half centuries after its introduction I think there can be no doubt that it did infinitely more harm than good; and, in fact, it is only comparatively recently that the proper limits to its use

have come to be understood, and we have learned to avoid its injurious while obtaining all its strikingly beneficial effects. Hunter and Abernethy, though believing in nothing but mercury, do not appear to have habitually employed it to any injurious extent; but this cannot be said of many of their contemporaries and followers, for we have Sir Astley Cooper stating in his lectures, in 1829, that in the Borough hospitals it was the custom then to salivate patients to the extent of three pints in the twenty-four hours; and even in the advanced stages of the disease the principle followed was—more syphilis more mercury. This was long the rule in civil practice, although it had been clearly shown by the army surgeons that syphilis, both in its primary and secondary stages, can be safely conducted through its course without any mercurial treatment. The names of Dr. Ferguson, Mr. Guthrie, Mr. Rose, and Dr. Hennen deserve mention in connexion with this experiment, but it was Mr. Rose who especially called attention to it in a paper published in the “*Medico-Chirurgical Transactions*” in 1817. He was, however, contented with having demonstrated the fact, and at a later period again resorted to mercury, because he found that by its use in primary sores the proportion of secondary affections was lessened, and that when the latter supervened their course was materially shortened.

No distinction was then made between the two kinds of primary sore; indeed, when I first became connected with the Lock Hospital, thirty years ago, it was the custom to give a course of mercury in every case of primary sore, whether indurated or not. This was done with a view to diminish the liability to secondary affections, although it was generally understood that the healing of the non-indurated sore was in no way

accelerated by its use. Since then, as I have explained, it has been demonstrated that the soft sore is only exceptionally followed by secondary consequences, and it has, therefore, become the rule to abstain from giving mercury in such cases. This is, in my opinion, a great improvement in the treatment of the primary form of the disease. It saves a large majority of sufferers from an unnecessary course of mercury, and entails no serious damage to the small number in whom secondary affections occur.

The local treatment of the unindurated sore should consist of destructive cauterisation in all cases seen within the first seven days. Nitrate of silver is inefficient for this purpose; it irritates the sore and causes inflammation of the adjacent parts, but does not sufficiently destroy. The best caustic is, I think, the strong fuming nitric acid, which should be applied pretty freely to the surface and edges of the sores. Another, perhaps equally efficient, is a solution of potassa fusa in an equal weight of distilled water. If the destruction be complete, the sore rapidly heals when the slough has separated, and all further consequences are averted. After the period mentioned, however, cauterisation is so often ineffectual that the better plan is to let the sore take its course. The treatment should then be scrupulous cleanliness, the application of gently stimulating and cleansing applications to the ulcerating surface, and the avoidance of all extraneous irritation by over-exertion, friction by the clothing, or otherwise; with, of course, due attention to the state of the general health.

By means of this kind, and with patients under effectual control, it is surprising within what narrow limits this form of disease may be confined. This is

well seen at the Female Lock Hospital in the women sent from districts under the Contagious Diseases Act, where they are subjected to a fortnightly medical inspection. In consequence of the early treatment thus enforced, with rest, strictly regulated diet, and freedom from all excitement, the sores are for the most part mild and superficial, they rarely attain a large size, and speedily heal, usually in from a fortnight to three weeks. Suppurating buboes are extremely rare, and anything like phagedæna or sloughing still more so.

If the progress be slower, and severe complications more frequent, in the cases met with in ordinary hospital and private practice, in both sexes, it is doubtless because the patients are generally compelled to follow their usual avocations; they cannot attend carefully to the dressing of their sores, which are subjected to various kinds of local irritation, and they are often irregular and imprudent in their diet and habits.

With respect to the treatment of the indurated primary sore, modern opinion is divided. There are many good authorities who think that it is better to abstain from mercury in this stage, and to wait until the secondary affections present themselves. They argue that secondary disease is certain to occur, and that it is not prevented but only retarded by mercurial treatment. I cannot agree with this view, for, according to my experience, after-consequences may often be prevented altogether by resorting to mercury as soon as the primary induration becomes pronounced, and by persisting in it till it has completely passed away.

Whether we look upon the induration as the peculiar local action through the agency of which contamination takes place, or as an indication that the system is already contaminated, it must be right to attack it at

the outset with the most efficient remedy we possess; on the first supposition to favour the elimination of the noxious influence as fast as it is being introduced; on the second, to get rid of an already existing infection; and it is reasonable to hope that the disease may often thus be prevented from passing beyond its initial stage. Ricord, when recently in this country, expressed himself strongly on this point. He said that as soon as there was an indurated chancre with corresponding swelling of the glands, he immediately commenced the mercurial treatment. If this were soon begun and well carried through, the breaking out of secondary symptoms might be prevented. If this were not frequently the case, it was because the treatment was resorted to too late, and when the secondary symptoms were about to show themselves; but if it were begun early, the observation of forty years gave him the assurance that secondary symptoms would not appear. I fully agree with this principle of treatment, though I do not feel quite so sanguine as to the certainty of the favourable result.

All attempts to destroy the indurated sore by cauterisation are useless. To have any chance of success, it would be essential to destroy thoroughly the whole of the thickened base, and it is probable that even this would be insufficient. Even free excision is not more promising. Many years ago I repeatedly excised this form of sore, taking care to cut well beyond the apparent limits of the affected tissue; but in every case the induration returned before the wound healed, and the progress of the disease was in no way interrupted. Locally, mercurial applications would seem to be especially indicated, and probably there are none better than the old-fashioned black wash.

Phagedæna, or sloughing, may occur as a complication of either the soft or the hard sore; but is met with much more frequently in connexion with the former than the latter, no doubt on account of the greater intensity of the inflammatory action accompanying it. I have already explained that in my opinion phagedæna is only an accident of syphilis, and bears no direct relation whatever to the specific venereal poison. Its treatment must therefore be conducted on the same principles as that of phagedæna generally. I will merely say here that it is a condition which is almost always associated with diminished vital power, and requires, therefore, to be combated by tonic and sedative medicines, with generous diet and stimulants. Should it supervene upon an indurated sore it is an imperative reason for discontinuing any mercurial treatment that may have been commenced. Sometimes, if it sets in early in this form of sore, it may perhaps prevent constitutional infection by its destructive influence. Locally, perfect rest, cleanliness, and antiseptic applications, such as the carbolic or sulphurous acids; or balsamic stimulants, of which the compound tincture of benzoin is as good as any; or the new remedy, iodoform, sprinkled in powder, will in many cases speedily bring about a cessation of the destructive process. Should this not be the case, or should the sore be spreading rapidly, stronger measures are required, and the surface and edges must be freely cauterised with strong nitric or sulphuric acid, or the potassa fusa. The caustic must be used so as to thoroughly destroy the edges and to penetrate through the diseased surface into the healthy tissue beneath. One such application will often suffice; and when the slough comes away healthy reparative action sets in.

Should this not be the case, the caustic must be applied again and again, either to the whole or parts of the surface, as may be found to be required.

The treatment of *constitutional syphilis* next comes under consideration. The division into a secondary and tertiary stage, although the line of separation may not always be distinctly marked, is very convenient with reference to the question of treatment. The secondary stage comprises cutaneous eruptions, of which the mildest form is the discoloration or mottling, termed roseola. Next come the scaly varieties, psoriasis and lepra; the papular eruption, lichen; and cutaneous tubercle. Associated with these are chronic enlargements of the lymphatic glands in various parts of the body. In the same group come the mucous patches, so common in the external female genitals, and sometimes on the scrotum, penis, and around the anus in the male; also mucous patches and superficial ulcerations on the tonsils, tongue, lips, cheeks, and palate, and on the os uteri. With the secondary group may be also mentioned some, but not all, cases of iritis, most cases of chronic orchitis, and osteocopic pains in the bones and joints.

In the treatment of secondary syphilis the balance of opinion is now undoubtedly in favour of mercury as the remedy which exercises the most decided therapeutic influence over the disease. Its efficacy is almost universally admitted, in the removal of existing symptoms, in hastening the progress of the disease towards a favourable termination, and where important structures are attacked, as in iritis, in the prompt arrest of morbid processes, which, without its counteracting influence, would often be rapidly destructive. There are few probably who would now contend that mercury is a

specific or antidote for syphilis, in the sense in which those terms were formerly employed; for it has been abundantly proved that the disease may run its course, and terminate favourably, without any mercurial treatment at all. Still there can be no doubt that, especially in the severer cases, it has a most powerful influence, not only in removing the outward evidences, but in remedying the syphilitic cachexia; and this without any equivalent disadvantage, if used at appropriate periods, within certain limits, and with those precautions which are now very generally understood. In infantile syphilis especially, the advantage of a properly conducted mercurial treatment is very strikingly manifested.

It may be a question, however, whether mercury should always be resorted to indiscriminately in every case of secondary syphilis, or whether the milder forms may not safely be left to the unaided operation of nature. The latter plan has been recommended by experienced authorities, especially by Diday; and I have of late years adopted it in many cases with satisfactory results. There are, I think, two classes of patients in which the abstention from mercury may be advisable; first, those who are in sound health, and capable of throwing off the morbid influence without assistance; and, secondly, those in broken-down health, or in whom there is an evident strumous or consumptive tendency, which may render it desirable, if possible, to dispense with a mercurial course. In both these classes, however, should the symptoms become more severe, and the disease be evidently getting the better of the natural powers, mercury should be unhesitatingly given, and it will often be of the most signal service, not only in the removal of symptoms, but, even

in debilitated patients, in the improvement of the general health and condition.

The three recognised modes of administering mercury are by the mouth, by inunction, and by fumigation. Inunction is perhaps, on the whole, to be preferred, especially for hospital patients, but it is troublesome and dirty, and therefore not so well adapted for private practice. The unguentum hydrargyri is the preparation usually employed, and from half a drachm to a drachm daily is about the quantity required. The oleate of mercury, introduced recently by Mr. Marshall, of University College Hospital, is a much cleaner and more convenient application. Being a solution, instead of a mechanical mixture like the blue ointment, it is more readily absorbed, and a smaller quantity suffices. The oxide of mercury, in the proportion of 20 per cent., dissolved in oleic acid forms an ointment of which from ten to twenty grains may be placed in the axillæ, or gently rubbed into the skin of the abdomen or thighs. Inunction should not be repeated daily on the same cutaneous surface, or an eczematous eruption is likely to be induced.

Fumigation, like inunction, is a troublesome process, and objectionable in private practice when patients are desirous of concealing their disease. In addition to this it is, I think, more debilitating, for the nightly vapour bath seems to exercise a decidedly depressing influence. It is, however, very efficient, especially in cases where there is an obstinate chronic eruption, and it gives the patient the advantage of the local, as well as constitutional, operation of the remedy. Fumigation has again been brought into prominent notice of late years, by the advocacy chiefly of Mr. Langston Parker and Mr. Henry Lee. It was Mr. Parker who

introduced the improvement of moist mercurial fumigation by combining watery vapour with the mercurial fumes. Mr. Parker used the bisulphuret of mercury, Mr. Lee prefers the chloride, the quantity used being about thirty grains every night, and this is the preparation now commonly in use.

Most surgeons, however, still continue, in ordinary cases, to give the remedy by the mouth, and only resort to inunction or fumigation when its internal administration causes gastric irritation or disturbance. There are, I believe, no better preparations than the blue pill or the grey powder. They are equally effectual and less irritating than the iodide of mercury, which is generally preferred in France. About three grains of either of the two former, night and morning, with a sixth of a grain of opium, will usually be sufficient to produce some slight effect on the gums at the end of about a fortnight. Should it not do so, the dose may be increased; but, as soon as tenderness is observed, the quantity should be diminished again; for though it is desirable to have this evidence of mercurial action, it is important not to pass beyond the remedial into the poisonous dose, as was so constantly the practice in former times. In this way we may get all the good that can be obtained from a mercurial course without any of the evils—real and supposed—which have been alleged to result from it, and it may be continued without injury for a lengthened period. The disease is essentially chronic, and the employment of the remedy should be equally so. It should be continued, but in diminished doses, for some time after all outward symptoms have disappeared; though probably the six months' course laid down by Ricord is more than is necessary in most cases. Ricord says that unless a

patient will consent to submit to treatment for twelve months he will have nothing to do with him. The first six months he is to take mercury, and then, if no symptoms reappear, treatment with iodine is to be begun and to be continued for six months longer. The six months' mercurial treatment may often be necessary, but I do not understand why it should be followed by a six months' course of iodide of potassium. Ricord admits that iodine is useless in secondary affections, and is only appropriate when the tertiary stage has set in. Why then use it at all as an adjunct to the mercurial treatment in the secondary stage? or why combine the two remedies in this period, which is the practice I find so frequently adopted? Again, if from any cause it should be considered undesirable to give mercury, or become necessary to discontinue it, it appears to me a mistake to suppose that the next best thing is to give the iodide of potassium, which seems to have no therapeutic value whatever, either in the primary or early secondary stage.

Another mode of administering mercury which has been sometimes adopted of late years, is by the subcutaneous injection of solutions of the perchloride, bicyanide, or biniodide. I refer to the practice only to inculcate its avoidance. It is admitted that it constantly produces local irritation and not unfrequently abscess, and it is difficult to conceive what object can be gained by introducing such powerful irritants into the areolar tissue when there are so many other and far preferable modes of arriving at the same result.

I have indicated a preference for the blue pill or grey powder; but the perchloride and biniodide of mercury have their appropriate uses, and are often

of great value. They are especially indicated in cases of relapse, in which mercury in other forms has already been used, and in which the health is beginning to suffer, although the manifestations still appertain distinctly to the secondary group. I allude especially to obstinate palmar and plantar psoriasis, and to those affections of the mouth and throat which are often so troublesome and persistent. The perchloride or biniodide may be combined with tonics, such as bark and sarsaparilla, and should be used in doses of from one-twelfth to one-eighth of a grain thrice daily.

The next point for consideration is the treatment of the tertiary stage of the disease, which comprises destructive skin affections, such as ulcerating tubercle and rupia, deep ulcerations of the throat, palate, and fauces; some forms of iritis and orchitis, affections of the periosteum and bones, and of the fibrous tissues of joints; gummatous tumours of the cellular and muscular tissues, and the more deep-seated deposits of similar character in internal organs.

The tertiary manifestations have often been attributed to the excessive use of mercury for the cure of the earlier stages, and the occurrence of bone-disease, and of destructive ulcerations especially, have been ascribed to its malignant influence. No doubt in former times the abuse of the remedy greatly aggravated the later train of symptoms, and it was through its persistent use in this stage that it became, from time to time, further discredited. But there can also now be no doubt that these symptoms are veritable sequelæ of syphilis; they all may, and not unfrequently do, occur in persons who have not taken a single grain of

mercury, and they never happen in those who have taken mercury, in however large doses, for any other disease.

Nothing, I think, is more certain than that, as a general rule, mercury is injurious in this class of affections. They are essentially asthenic in their character, and the measures required are such as will repair the failure of nutrition and restore the debilitated powers. Good diet and stimulants, with pure air and other favourable hygienic influences, are therefore of great importance; but they are not in themselves sufficient, and fortunately in the compounds of iodine a remedy has now been found which is as effective in the tertiary as mercury is in the secondary stage.

The use of the salts of iodine is of comparatively modern date; iodine in its pure state, in tincture, or in the form of burnt sponge, was used in the earlier part of the century, in the treatment of syphilis, but without any definite result. It was in 1831 that Dr. Robert Williams first used the iodide of potassium, in St. Thomas's Hospital, in cases of periosteal nodes, in which, as in other forms of bone-disease, the unsatisfactory results of mercurial treatment had begun to be generally acknowledged. The result in Dr. Williams's cases was that with which we are now so familiar: complete relief from pain in a few days, and gradual disappearance of the nodes. He soon after used it in rupia and in tertiary ulceration of the throat, which, as we now well know, heal so marvellously and rapidly under its influence. Iodide of potassium was soon afterwards taken up by Dr. Wallace, of Dublin, who used it in a large number of cases, and published the results in the *Lancet* for 1836. It was adopted about the same time

by Ricord, and its value was soon fully appreciated by the profession.

The beneficial effect of the compounds of iodine (iodide of potassium, sodium, or ammonium) seems to be confined almost entirely to the tertiary stage. I believe that in the earlier secondary period, at all events, it has no influence whatever. The marked difference in the effect of the two remedies in the two stages affords further proof of the striking change which has been brought about in the constitution of the patient and in the character of the symptoms, as the disease progresses.

It is held by many good authorities that the beneficial action of the iodides in tertiary syphilis is confined to the removal of existing symptoms and to the restoration of the health of the patient from the cachectic condition into which it has fallen, but that it does not cure the disease or prevent relapse, mercury being afterwards required for that purpose. What I have seen would not lead me to favour this view; there may be occasionally cases of tertiary disease in its earlier stage in which mercury is beneficial, but in the great majority it is positively injurious. It is in the nature of tertiary syphilis to relapse, and I doubt much whether the relapses are rendered less frequent or less severe by mercury, while I can testify to the mischief which may be occasioned by its imprudent employment at this stage. It tends to aggravate spreading ulcerations, to cause rupial sores, already healing or healed, again to ulcerate or re-open, and soft nodes to suppurate and give rise to caries or necrosis.

I have long acted, in this stage, on the principle of more syphilis more iodide, and in the end, even in the

severest cases, perfect and permanent recovery has very frequently been the result.

It is not a sufficient reason for discontinuance that the symptoms remain stationary: they will often remain so even for months, while the iodide is being taken, whereas they will immediately get worse if it is left off. This is no evidence, therefore, of its failure, for we are indebted to it for the maintenance of the *status quo*, which is no small matter in so serious and protracted a cachexia as that with which we have to deal. Should a fresh accession of symptoms supervene, this may even be an additional reason for persisting, and for an increase of the dose.

One important point in the administration of iodide of potassium, in my opinion, is to abstain from large doses at the commencement. It is better at first not to give more than three or four grains, thrice daily, increasing each dose by a grain about every four days, till fifteen, twenty, or thirty grains are reached. I have not usually found any further advantage gained by resorting to the heroic doses sometimes prescribed, though I do not say they may not sometimes be beneficial. Gradual increase of dose is the important point to be attended to, and disappointment often occurs, and the remedy is discredited, when continued at the same dose, whether small or large. Its efficacy seems to be increased by combining it with carbonate of ammonia, and it is less likely to disagree when taken in a large quantity of fluid. There is no better vehicle and adjuvant than sarsaparilla in the old form of decoction, which has so undeservedly fallen into disuse. Sometimes there is great intolerance of the iodide of potassium, even small doses producing dryness of the throat, headache, and coryza. Here the iodides of

sodium and ammonium may be substituted with advantage; they are not, perhaps, quite so efficient, but they are certainly less irritating. The iodide of sodium is, I think, to be preferred. I have found it necessary, in very susceptible patients, to commence even with half-grain doses, and have in this way been able to get gradually up to ten or twelve grains without causing any of the above unpleasant symptoms.

I am not disposed to favour the very common practice of giving mercury and the iodides in combination. I think it is better to act on the principle that when one is required the other is likely to be prejudicial, and that when they are used in combination the tendency of each is to counteract the other. The only cases in which this principle may perhaps be departed from with advantage are those which are in the transition period, and in which secondary and tertiary symptoms coexist. Here it may be admissible to combine the two remedies, or to alternate them according to the rise or fall of either set of symptoms, and this, I think, may often be done with very satisfactory results.

The best mode of combination is to add an eighth of a grain of the perchloride to the mixture containing the iodide, the result of which will be the formation of biniodide of mercury with an excess of iodide of potassium.

The beneficial influence of mercury is in proportion to the sthenic character of the morbid process which is to be counteracted. It is useful when the associated general condition tends in the direction of hyperæmia and of vascular and nervous excitement. It is injurious in the opposite condition of vascular depression, anæmia, and general failure of power, and these latter are the

almost constant characteristics of tertiary syphilitic disease.

LEGISLATIVE PREVENTION.

In conclusion, I will allude briefly to the question of the prevention or diminution of venereal disease by legislative measures. If we consider the prevalence of the disease amongst all classes, the many ways in which the innocent, as well as the guilty, may become its victims, its disastrous results in many cases, and its general deteriorating influence on the public health, it is not going too far to speak of it as one of the most important of the sanitary questions of the day. Unfortunately, the subject is unsuited for general discussion, and its urgency has therefore never yet been properly appreciated by the public.

Most Continental states have long had regulations for the control of prostitution and the prevention of venereal disease; but it is only recently that an attempt in the same direction, and that as yet a very partial one, has been made in this country. In 1864 an Act of a tentative character was passed by Parliament, but it failed on account of its voluntary character, and the consequent impossibility of exercising sufficient control over the women brought under its operation. It was superseded, therefore, in 1866, by a more efficient measure, founded on the recommendations of the Medical Committee appointed by the Admiralty. Another supplementary Act was passed in 1869. The operation of these Acts was confined to certain military and naval stations, and their main object was to check the prevalence of venereal disease in the army and navy. They were founded on the principle of police

registration and supervision of all known public prostitutes in a district, their periodical medical examination, their immediate transfer to hospital when found diseased, and their compulsory detention till cured.

In 1867 the Harveian Society appointed a committee, under the presidency of Dr. Pollock, to inquire into the question, and a large mass of evidence was collected from all parts of the country, showing the great prevalence of disease and the necessity for general restrictive measures for its prevention. The report was published, and has been frequently quoted and referred to. Out of that committee arose an association for the purpose of extending the benefits of the Acts to the general population, which was soon joined by a large number of influential persons. That association has not succeeded in effecting its main object of extension, in consequence of the fanatical and unscrupulous opposition which soon afterwards rose up and has been strenuously maintained; but it has worked hard to combat that opposition, and to place the matter in its true light before the public and before Parliament; and it may fairly claim to have had a large share in saving the Acts from repeal; in fact, repeated motions for their repeal have been rejected by large and increasing majorities.

A Select Committee of the House of Commons has, during the last two years, 1879-80, been pursuing an exhaustive inquiry into the whole question, and it is to be hoped that the voluminous evidence which they are collecting will be such as to satisfy the public at large of the great benefits which the Acts have already brought about, and to show the unfounded and untruthful nature of the statements which have been so widely spread abroad as to their oppressiveness and cruelty.

It is worthy of remark that the opposition, promoted mainly through the agency of platform agitators, has been most active in places remote from the operations of the Acts, where there has been no opportunity of obtaining practical knowledge of their working; whereas their advantages are fully understood and appreciated in the garrison and seaport towns where they have been in force, a great majority of the respectable inhabitants of which are most anxious that they should be maintained.

A movement in favour of the extension of the Acts to some of the large mercantile seaport towns has lately been set on foot, and notably in Liverpool, where it is being carried on under the able and energetic advocacy of Mr. F. W. Lowndes, of the Liverpool Lock Hospital. In Liverpool the need of some preventive legislation against venereal disease is very urgently felt.

Our principal seaport towns are hot-beds of syphilitic contagion; the amount of disease amongst merchant seamen—who are under no inspection or control while on shore—is consequently very great, and the inconvenience and danger often arising from their becoming disabled from this cause while at sea is notorious. It is a disgrace to this country also that the sailors belonging to our mercantile marine should with any justice be regarded by the authorities of foreign ports as a fertile source of disease, and one of the chief obstacles to the efficiency of their preventive measures. Much of this mischief might be prevented by insisting on these men being submitted to a medical examination before allowing them to proceed on their voyage.

As regards the women there would be no serious difficulty, beyond that of expense, in carrying out the

provisions of the Contagious Diseases Acts in seaport towns, in the east-end of London, in the lower purlieus of London and of all large towns, and thus subjecting the lower class of prostitutes to police supervision, to medical inspection, and, when diseased, to compulsory detention in hospital till cured. The lower class of prostitutes should be dealt with first, because they are most frequently and seriously diseased, are most easily reached, and are least able to help themselves. More comprehensive measures might follow in due course.

These Acts have been strikingly successful at the military and naval stations to which they have been applied, notwithstanding the disadvantages arising from the limited area in which they work, and the constant influx of new-comers who are diseased. Primary syphilitic sores have been diminished in the army and navy at the protected stations by more than one-half, although at those which are unprotected there has been increase instead of diminution. Secondary affections have also been found less frequent and less severe. Military and naval medical officers bear unanimous testimony to the great diminution of syphilitic disease in the men under their charge, and also to its milder character. In the less important complaint, gonorrhœa, the result has not been so strongly marked, although, in the last few years especially, there has been a very decided diminution. I do not think this is to be wondered at if we consider the various exciting causes of this complaint, and its not unfrequent origin in both sexes independently of direct contagion. I fear that Acts of Parliament can never be expected to succeed to any very great extent in the suppression of gonorrhœa. It is a complaint which we must be content to have

always with us more or less, until we can contrive to attain a much higher moral elevation than there seems to be any prospect of at present. But it matters comparatively little about gonorrhœa; it is the arrest, or, at least, the diminution, of syphilis which is all important from the sanitary point of view, and the first and most essential step towards this is the prompt seclusion and treatment of all public women who become diseased.

If any real sanitary good is to be done in this direction, it can only be by legislative measures. It is useless to talk of voluntary Lock Hospitals. I have myself assisted in carrying out the voluntary system at the London Lock Hospital for five-and-twenty years, and for ten years I have seen the voluntary and compulsory systems at work side by side in the same institution. From these exceptional opportunities for comparing the two, I am convinced that nothing but the compulsory periodical examination of prostitutes, and their compulsory detention in hospital until cured, will have any material effect in diminishing the prevalence of venereal disease among the population; while to the women themselves the advantage of early surgical treatment is incalculable. This latter fact is well exemplified by the marked contrast, as regards severity, between the two classes of cases. The women under the Acts only exceptionally present symptoms of an aggravated character; while the voluntary patients, in consequence of neglect and delay in applying for treatment, constantly furnish examples of all the most severe forms of disease.

Voluntary Lock Hospitals on an adequate scale will never be supported by the public, and are not likely to be established by the State, and if they were they

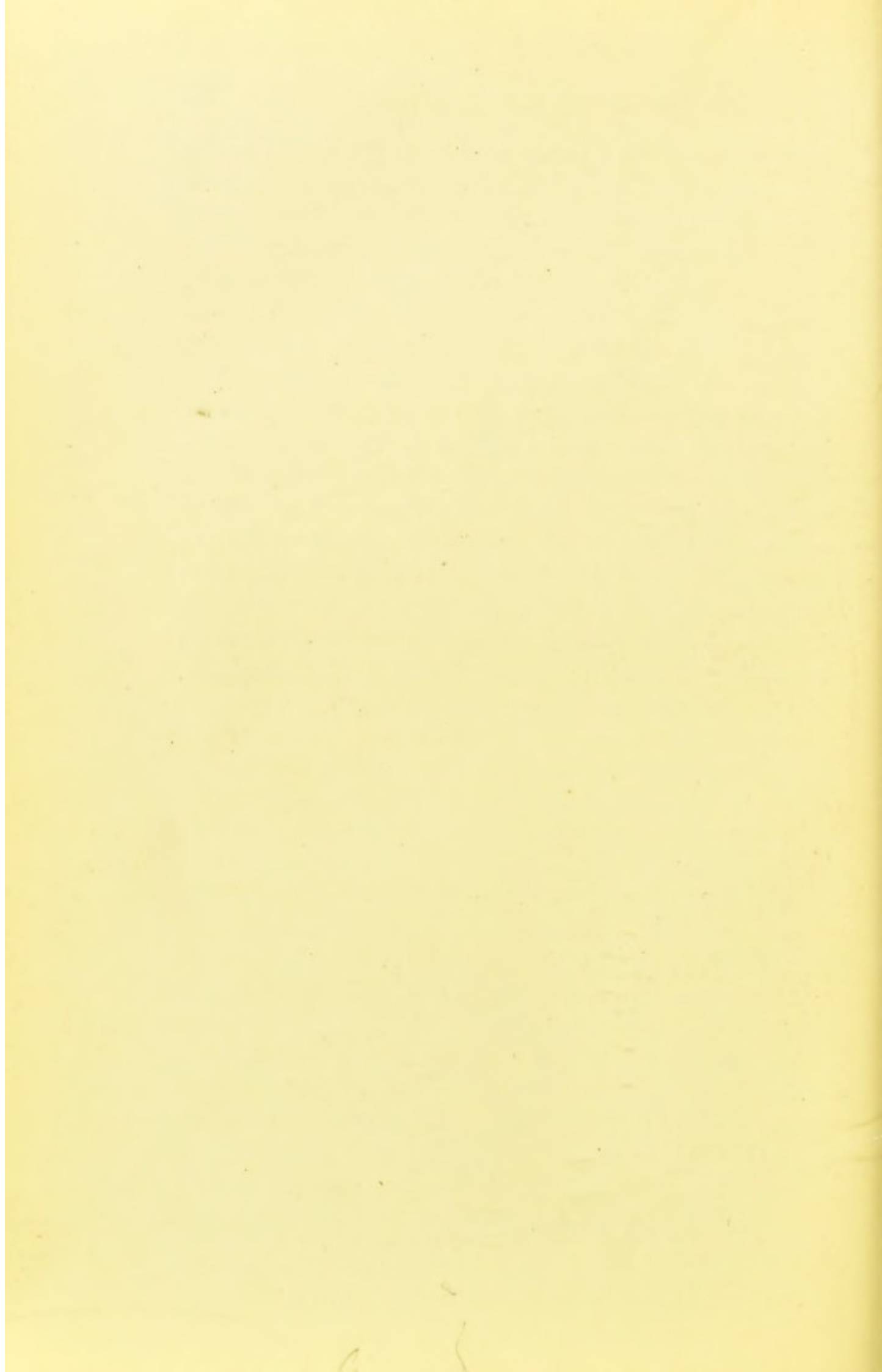
would be of little use. These women are far too reckless of consequences to apply for admission, and to seclude themselves for treatment, until their disease has reached a stage which renders it impossible for them to pursue their calling any longer, and until they have already done all the mischief of which they are capable; while a large proportion will insist on leaving hospital before they are cured, and while still in a condition to communicate infection. The treatment of them as out-patients, which is done on so large a scale at present, is a positive injury to the public health, by enabling them to practise prostitution with less pain to themselves, and for a longer period than it would otherwise be possible for them to do.

The collateral advantages arising out of legislation of this kind are many and striking. In the towns in which the Contagious Diseases Acts have been in force there is ample testimony, in the evidence taken by the Royal Commission in 1872, from influential inhabitants and from the clergy of all denominations, of the greatly improved outward demeanour of the prostitute class, of the almost complete cessation of riotous disorder, and especially of street solicitation. Testimony of a similar character has been accumulating year by year.

But it is the women themselves who have been most strikingly benefited, notwithstanding the gross misrepresentations as to their oppression which have been so persistently put forward. Twelve years ago the "wrens of the Curragh" and the outcasts of Aldershot were left to rot upon the ground without a roof to cover them, and they would have been so left to this day, had they not been rescued from their state of almost bestial degradation by the operation of the Contagious

Diseases Acts. The public women in the towns under the Acts have diminished in numbers by more than one-half; those who remain are cleanly in their persons and comparatively subdued in manner, and they present at least the outward seeming of civilized beings. Great numbers of them, who would never have been approached in the way of kindness but for these Acts, have been induced to resort to reputable modes of life, to enter reformatories, or to return to their friends. Numbers of young girls hovering on the brink of prostitution have been rescued through the agency of the police, and persuaded to return home, without coming under the operation of the Acts at all. Private benevolence never has done, and never will do, anything approaching to the good which has been effected in this direction, quietly and unostentatiously, through the agency of these much-maligned Acts of Parliament.





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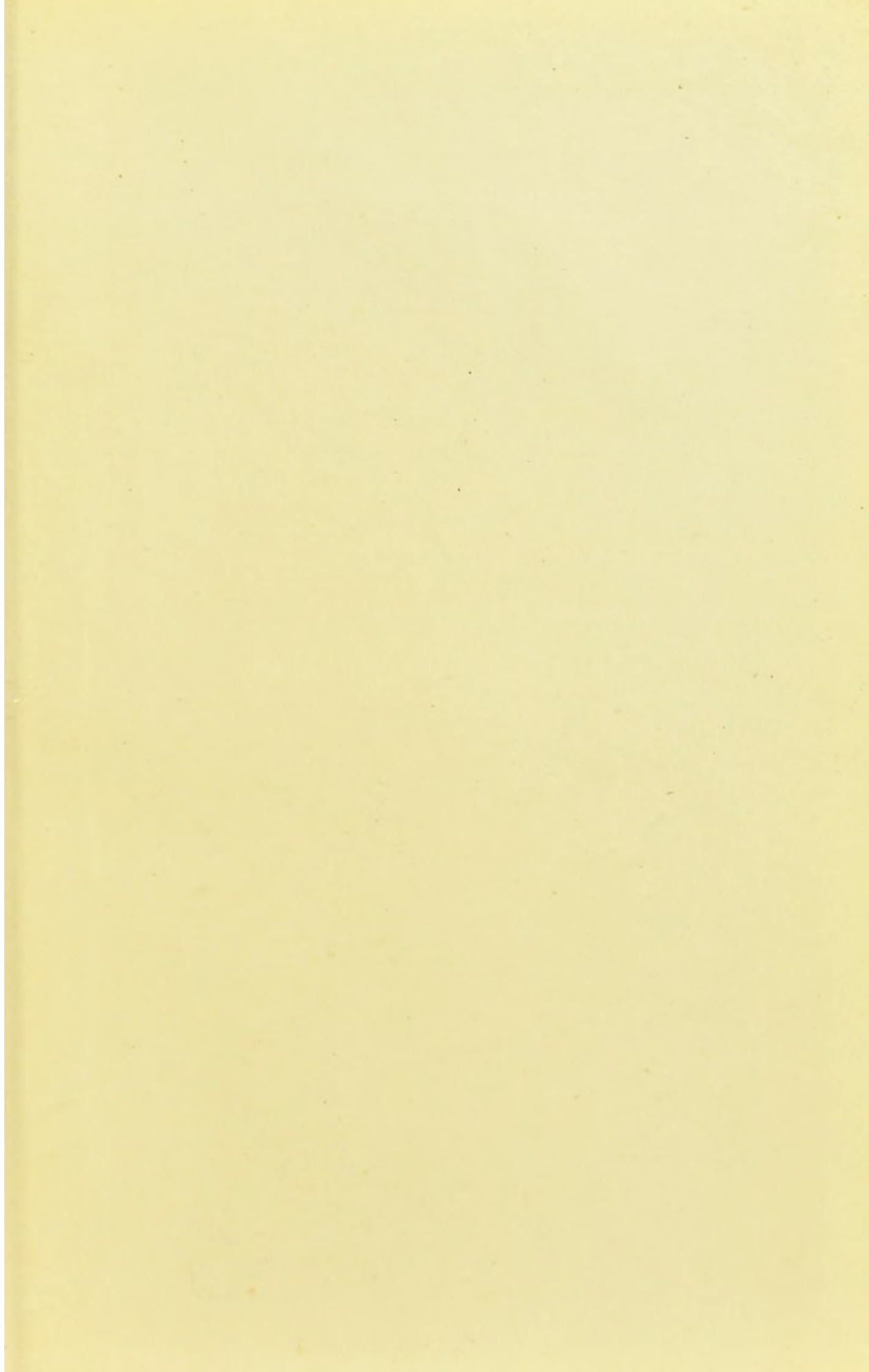
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