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COURSE OF LECTURES ON VENEREAL AFFECTIONS

DELIVERED

AT THE HOPITAL DU MIDI, PARIS.

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LECTURE XIV.

June 23, 1843.

General diagnosis of chancre—Evidence derived from its origin; from its seat; from its aspect; from its progress and duration; from the effects of mercurial treatment—Fallacy of the above evidence—Effects of inoculation—This test previously experimented on by Hunter, Hernandez, &c.—Deductions from the results of inoculation—Objections to the practice of inoculation—Estimate of its real value.

GENTLEMEN,—I have never refused to admit that, in most cases of chancre, the evidence collected from the origin of a sore, its seat, aspect, duration, &c., is sufficient to satisfy the practitioner of the specific nature of the ulcer, and, in particular, to guide him in the treatment. But, in exceptional cases, the various circumstances alluded to yield no positive information, and the surgeon must consequently remain in doubt as to the nature of the sore, its possible consequences on the system, and the most proper mode of treatment, unless he resorts to inoculation. This I will endeavour to demonstrate, by showing that the diagnosis of the nature of a sore, purely founded upon its suspected origin, its seat, its aspect, &c., would be essentially imperfect, and could not empower a medical jurist to give a conscientious and well-grounded opinion.

Origin.—The belief possessed by the patient in the virtue of the person who has communicated a sore, can in no instance be taken into consideration. Nothing should satisfy the physician but an investigation of the infecting party with the speculum, and not always even that examination. Every one knows how difficult is the complete inspection of the genital organs of women; and if the medical jurist finds a chancre, may it not have been communicated to the woman since the time when she is accused of having poisoned another person? If no chancre be found, on the other hand, who can confidently assert that a chancre has not recently been treated without leaving any cicatrix? In either case, what becomes of the accuracy of the diagnosis?

Seat.—Primary syphilis is formed oftener, it is true, on the genitals than elsewhere; but it may occupy the hands, feet, or any other part of the skin. On the other hand, the organs of reproduction are, undoubtedly, subject to ulcers and sores which have nothing to do with syphilis. The seat, therefore, can

never be brought forward as a certain proof of the nature of an ulcer. Thus I have observed true chancre on the finger. I accidentally inoculated myself in that manner at the hospital some years ago; axillary bubo and secondary eruption followed in due course. The unfortunate Dr. Housman's was a parallel case. Last month a patient left us cured of a chancre of the left ear; at the present instant we have in the wards several examples of chancre occurring in strange situations.

I can show you, in contrast, an excoriation and sore of the prepuce, which has been inoculated without success, and is, consequently, not a chancre. In many cases of accidental infection of the hands, for instance, the patient remains ignorant of the nature of the sore, and at a future day, interrogated on the previous occurrence of primary syphilis, unhesitatingly replies he has never had chancre. In the other instances alluded to, it is only with the very greatest difficulty that an avowal is wrung from the patient.

I conclude that neither the seat of chancre nor its origin can afford an unerring clue to its nature.

Physical Aspect.—The physical appearances, called Hunterian, are not absolute signs of the virulence of a sore. One or all of them may be absent in true chancre, or be present in simple ulcerations. Such is the difficulty of the diagnosis, founded only on inspection, that, in 1834, my excellent and learned friend, Mr. Carmichael, a highly respectable authority in syphilitic affections, examined with me the patients of my wards, wherein, you will acknowledge, there is no dearth of primary symptoms, without once finding an instance of what he calls true chancre.

Moreover, Fricke's experiments have shown that all the physical characters of chancre may be produced at will by the application of corrosive sublimate to the mucous membrane of the prepuce; and the identity between the artificial sore thus produced and genuine chancre is such that the physiological school made it one of their chief arguments to prove that no virus exists in syphilis, and that the special form of the ulcer is merely due to its seat, and to the degree of irritation of the part. I have demonstrated that the ulcers produced by the bichloride of mercury are never at any time inoculable—a negative sign, which is sufficient to separate for ever the artificial sore from primary syphilitic ulcerations.

Incubation, Progress, Duration.—Incubation I have shown not to exist in chancre, consequently it is impossible to conclude, from its supposed presence, the nature of the sore inspected. The progress or duration are not of more value.

Treatment.—Can the influence of mercurial treatment on a sore be brought forward in proof of its syphilitic nature? At one time the query would have received immediately an affirmative answer; and still, at the present day, with many persons, mercury is the

touchstone of syphilis, the unerring test from which they draw conclusions which no one must presume to discuss.

I will mention to you a case in which a sore, submitted to the test, was proved, in the physician's opinion, to be syphilitic.

A gentleman, affected with an inveterate sore of the leg, consulted the late excellent and honored Professor Boyer. The professor stated his belief that the ulcer was syphilitic, which the patient stoutly denied. M. Boyer proposed to test the ulcer by the application of mercurial ointment to *one half* of it, whilst the other half would be covered by simple cerate dressings. The patient acquiesced, and the wary professor took care to choose the upper half of the sore for his experiment, which was fully successful, to the great glory of mercury. Such facts require no comment. It is more than evident that mercury, one of the most powerful drugs of the *materia medica*, cures a great many disorders besides syphilis, and it is not less clear that it is altogether inert in several forms of this disease. What, then, becomes of the test? You perceive, Gentlemen, that no one physical character hitherto mentioned can be depended upon for the accurate diagnosis of chancre. Inoculation of its pus is the only unerring test; and we may, therefore, conclude the foregoing observations with the following proposition:—

Aphorism.—The essence of chancre is not in its aspect, fundus, areola, base, cause, or duration; it is entirely in its secretion.

Inoculation.—Much has been said, Gentlemen, on this subject; much abuse has been lavished on my devoted head for having dared to follow the example set by Hunter, in inoculating the secretion of chancre; but, notwithstanding the virtuous indignation of my adversaries, I still have the perversity to say that I have only one regret—it is not to have been the *inventor* of inoculation.

When I was first attached to this hospital, the most deplorable confusion reigned in all subjects connected with syphilis. Contradictory statements, opposite assertions, and diversity of theories, had produced an inextricable medley, which I conceived could only be cleared up by recurring to the experimental method, and submitting anew all litigious questions to the test of honest, unprejudiced, experimentation. Nor was I, in so doing, altogether unsupported by respectable authority.

Hunter had recorded his inoculations, which Bell found fault with, only on the ground of their not being sufficiently numerous.

Hernandez made a great number of inoculations, the object of which was to prove that gonorrhœa and chancre are not identical. His purpose was to demonstrate a favorite theory—a bad starting point for experiments; and the far-fetched and labored explanations which he enters into, to account for the production of chancre in a few instances of the inoculation of blenorrhoid discharges, show how embarrassed he was to bend facts to his preconceived notions.

Bru published sixty cases of inoculation of all sorts of venereal symptoms; in none did the experiments succeed.

Cullerier (junior) had stated that the pus of bubo

can in no case be inoculated with success. I inquired confidentially from him on what grounds he had made such an extraordinary statement. His answer was, that he had not instituted any experiments himself, but that he had heard his uncle make the assertion, and had written on that simple authority.

Richond and Devergie both deny the existence of a virus, and assert they have been able to produce, by inoculation only, trifling ulcerations.

It is clear that those persons who studied this matter previously to me had been more or less governed by the desire of illustrating their own preconceived theories, and endeavoured to render experiments subservient to their purpose. When I undertook the subject, forewarned by the errors of my predecessors, I determined to proceed in a different manner, and to substitute a science *a posteriori* to a science *a priori*, by avoiding to look into facts for the demonstration of any particular notion, and drawing my conclusions only after the experiments had been left to speak for themselves.

I inoculated all the venereal symptoms I met with on the patients themselves; I examined the progress of the experiments hour by hour, and patiently awaited the result; and when a large number of inoculations combined to give my assertions the sanction of figures, I drew the following conclusions, which, to the present day, wait for a contradiction in point of fact:—

1. The pus furnished by ulcerated surfaces in primary syphilis is inoculable when the sores are in their stage of progress, or of *statu quo*.
2. The ulcers, inoculable at first, cease to be so from the time of the appearance of granulation.
3. Constitutional symptoms, in whatever shape they may be, cannot be inoculated.

Hence, the true nature of a sore can only be tested by inoculation.

Although my experiments have yielded in other hands the same results as they did in mine, as their truth and accuracy could not be contradicted, the opponents of inoculation have chosen another ground for discussion, and have accused the method of being immoral, dangerous, and unprofitable. I will examine one by one the various arguments adduced, without in the least attenuating their strength, and hope to convince you that inoculation is an advantageous, though exceptional, instrument of diagnosis, which deserves not the obloquy which has been heaped upon it.

When scientific bodies rid themselves of prejudice, that prejudice almost invariably descends to the lower orders, where it continues for a long time to blast and neutralise the most useful discoveries. The most striking illustration I can think of is, the notorious and absurd objection entertained by the working classes of society against vaccination. Every new doctrine must pay its tribute to public prejudice, and that tribute, the inoculation of chancre may be said to have amply paid.

It has been said that, in multiplying the surfaces of infection, the chances of secondary disease are increased by the operation.

In the first place, I cannot well understand how those persons who bring such an accusation against inoculation, can possibly conciliate it with their

theory that chancres are the gates opened by nature for the excretion of the poison, which will invariably produce its constitutional effects, if the chancre be rapidly healed. The persons who profess such a theory, far from opposing inoculation, ought to consider it the most favourable prophylaxis of secondary syphilis, opening, as it seems to do, as many gates as possible to the free exit of the virus. But, speaking seriously, we assert that inoculation *does not* expose patients to secondary infection, although it increases the number of primary ulcers—1st, because if cases are carefully examined, it will be found that general infection bears *no ratio whatever to the number of previous ulcers*, and that, in the majority of cases, it is to *one chancre alone* that syphilitic infection is to be attributed; 2nd, furthermore, Hunter, and others, have shown, that, during the first days of its existence, chancre is entirely in dependance on the surgeon, who can, at will, arrest its progress, and oppose his successful endeavours both to the local injury the inoculated sore might be the cause of, and to the infection which might be its consequence.

The opponents of inoculation assert, that syphilis inoculated with the lancet is far more severe than the disease resulting from sexual intercourse.

The celebrated case of the unfortunate Beaudier, who, in 1824, having inoculated himself with the secretion of a chancre, was so terrified at the sight of the primary sore he had produced as to put an end to his life, is brought forward whenever the question is mooted. Although this case has no bearing whatever on the point in question, yet it is always coupled with it, and demonstrates only one thing, the want of moral courage in the poor victim.

To support the position, cases are brought forward of surgeons who, in the discharge of the duties of their profession, inoculated their fingers with chancres, and were afterwards affected with unusually severe symptoms. It must be recollected that when, from negligence, inoculation of a finger, for instance, does occur, the sore finger is not in general attended to, the patient not being aware of the injury he has suffered, so that time is allowed for the disorder to make considerable progress before anything is done to oppose it. On the other hand, syphilis resulting from sexual intercourse is not always so benign a disease, and death is occasionally its consequence. Besides, the manner in which physicians are liable to be affected, explains why the cases of syphilis communicated during professional avocations, are made to stand out in relief, the subject of general attention. If the same persons had been so unfortunate as to be infected in the more usual manner of common mortals, much less would be said about it, even though a limb or a feature of the countenance were sacrificed to the malady.

M. Desruelles objects to inoculation, that a *form* not a *principle* is inoculated. This argument I acknowledge my incapacity to answer. *Piat lux.*

It is said that the results of the operation are negative or uncertain, the ulcer which succeeds to inoculation not presenting more characteristic appearances, to those who have been unable to recognise the nature of the sore submitted at first to their inspection.

To this I answer, that the results of inoculation are

negative or uncertain only when the operation has been performed imperfectly, or after the process of granulation has begun; that the primary sore presented at first to inspection may have deviated from the type from various circumstances which we have enumerated, but still preserves unaltered the property of producing, by the inoculation of its secretion, the characteristic pustule and ulcer.

Dr. Cazenave asserts that inoculation succeeds because it is performed on the already infected patient. Why then, we will ask, do not all the wounds of a patient affected with chancre become also chancrous? Besides, Luna Calderon and Hernandez have inoculated chancre to healthy persons, a practice which I most positively condemn.

Dr. Cazenave, comparing the inoculation of chancrous with vaccinal matter, says, that both proceed in the same manner: for instance, the first vaccination is followed with three days incubation, after which the pustule forms; and if a second inoculation of vaccine is made the third day after the first, this second pustule is not preceded with any incubation, but both pustules arrive to their full period at the same time. The assertion, to say the least, appears to me to deserve confirmation; but supposing it to be correct, it would prove the very contrary proposition to that which Dr. C. wishes to establish—viz., that it is the action of the vaccine virus on the whole system which occasions the more rapid evolution of the second pustule. What is the proof usually brought forward of the efficacy of vaccination?—is it not that the operation is afterwards unsuccessful on the same subject? How, then, can it be stated that the action of the virus on the system has hastened the evolution of the second inoculation, when the only proof we have of that action is the impossibility of reproducing the vaccination on the same subject?

It is further asserted that no conclusions can be drawn from inoculation of chancrous pus with the lancet, because artificial syphilis differs from the natural form of the disease as much as inoculated variola from the spontaneous eruption. We will ask what is the alleged difference between the two forms of small-pox; we know of none. Besides, our adversaries commit a capital error in comparing the *constitutional* effects of variola to the *local* symptoms of syphilis. On the other hand, the differences alleged are immaterial as regards the nature of the disease (since the artificial differs from the spontaneous variola only in its intensity, and may be explained from the physician choosing his own time for the operation, and causing it to be preceded by such form of internal treatment as he may deem capable of producing a milder form of eruption).

When the inoculation of chancre gives negative results, it is of no use; at any rate, we will add, it is productive of no injury. But the negation of syphilis acquires a considerable degree of importance, when it is recollected that all primary syphilitic symptoms are inoculable during the first week of their existence, precisely at the period when the diagnosis is most difficult.

Now, is inoculation *always* to be attempted? Certainly not. Whatever my opponents may have been pleased to state, I am the greatest adversary of the universal application of the method to *all* cases.

But when I performed my first experiments, it was a generally received doctrine that mercury should be applied to *all* cases of syphilis, an opinion which I do not personally partake of. For those physicians who still conceive that mercury is the only treatment applicable to syphilis, it is evident that they must first have a positive and incontrovertible test of the nature of all primary ulcers, and the only test of that character is, I do not hesitate to say, inoculation.

OBSERVATIONS ON SPERMATORRHOEA;

OR

THE INVOLUNTARY DISCHARGE OF THE SEMINAL FLUID.

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(For the *Provincial Medical Journal*.)

That important and most afflicting class of affections of the sexual organs, which is characterised by the frequent involuntary discharge of the seminal fluid, although sufficiently familiar to the majority of practitioners engaged in the large towns of this and other countries, has not met with such public notice on their parts as it should seem to demand. For this reason, and to the great detriment of society, the treatment of the effects of sexual abuse or excess, exclusive of syphilitic and blenorrhagic affections, is too generally consigned to a section of that horde of unprincipled pretenders, which the government of this country, to its shame, by tolerating continues to patronise, and from whose obscene advertisements it condescends to derive a portion of its revenue. It thus happens that a subject, fraught with the deepest interest both as regards the individual and society at large, is rarely in all its extensive details submitted to honest and scientific investigation. It is to be hoped, however, that as the eye, the ear, the teeth, have one by one been rescued from the unclean grasp of quackery, so in its turn this most wretched of all the curses which man's imprudence entails upon him, may be thought worthy the attention of the educated practitioner. He only who has been able to restore the despairing and unmanned being, whose very life is a burden to him, to the full possession of physical and mental vigor, can form an estimate of the lasting gratitude with which a medical man may sometimes be regarded by his patient; and truly, whether gratitude alone content him, or whether his soul long for more substantial recompense, he will never regret the having paid attention to this neglected subject.

If a person after the age of puberty, and more especially if he have previously indulged in regular sexual intercourse, be from any cause induced to lead a life of perfect continence, he will experience involuntary emissions during sleep in greater or less frequency. The secretion of semen being continuous, and not, as is by some believed, incidental upon erotic excitement, nature adopts this mode of disembarassing the system of a product which is not got rid of in sexual intercourse. Emissions, therefore, occurring under such circumstances in robust individuals, so far from being injurious, must be regarded as a salutary provision.

These nocturnal emissions, however, though beneficial, or at least harmless, at first, may, if the conti-

nence be prolonged, be, by an acquired habitude of the parts concerned, repeated to an extent which becomes positively injurious, and, as in the case of other habitual discharges, then produce inconveniences proportionate to their frequency, and the original constitution of the individual.

But the cases in which spermatorrhœa is consequent upon prolonged continence are comparatively rare. The involuntary emissions which occur in such abundance as to constitute a really morbid phenomenon, are usually to be traced to one or other of the causes hereafter to be mentioned.

Symptoms.—From the almost insurmountable disinclination to speak of their ailments, which is generally observed in the unhappy subjects of this complaint, it is difficult to procure a complete account of its origin. The history of a case of seminal pollution will, however, be found to be somewhat as follows:—After a greater or less amount of abuse of the genital organs, either natural or unnatural, the individual makes the discovery that he has become infested with seminal emissions during sleep. The emissions at first are accompanied by erection, but soon occur with diminished rigidity of the penis. If he at this time indulge in sexual intercourse, he experiences more than usual difficulty in consummating the act, he is frequently disappointed altogether, or, if not, erection is incomplete, and ejaculation more than ordinarily precipitate, and, as some patients have informed me, even painful, rather than accompanied by the usual sensations.

As the disease advances, the nocturnal emissions increase in frequency and abundance, at length occurring without either erection or pleasurable sensation; in fact, the patient is often only made conscious of them by the sense of feebleness upon waking, and by the marks upon his linen. In sexual intercourse ejaculation becomes more and more hurried, till at length mere contact with, or even sight of, the female will induce it, and complete impotence is thus established. The pernicious effects of these discharges upon the genital economy is soon evidenced. The mind becomes enfeebled and incapable of protracted attention, the memory fallacious and uncertain, and the patient feels that he is no longer fitted for his usual avocations. His disposition undergoes an equal change; he becomes morose and suspicious, fond of solitude, lachrymose upon trivial occasions, and exhibits those apparently causeless contrarieties of temper which are commonly received as evidences of hypochondriasis or eccentricity. Cerebral and thoracic symptoms—as giddiness, noises in the ears, palpitation, and cough—present themselves in greater or less intensity; the body gradually emaciates, especially about the lower extremities; the aspect becomes dejected; the patient seldom raises his eyes to the person he addresses, as if conscious that the expression of his countenance would reveal his wretched condition; digestion is impaired, and accompanied by pain and flatulence; the bowels are almost invariably costive—indeed, I know of no other disease, short of mechanical obstruction, in which they so obstinately resist the most powerful cathartics. That this state of the bowels is intimately connected with the emissions is shown by the fact that, upon the suspension of the latter, the bowels at once resume