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Original Articles.

ON THE IMPORTANCE OF GONORRHŒA AS A CAUSE OF INFLAMMATION OF THE PELVIC ORGANS.*

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When, in 1872, Dr. Noeggerath for the first time published his views on this question under the title *Latent Gonorrhœa in the Female Sex*, so obviously exaggerated were some of his statements that, as he himself subsequently admitted, they "were not received very favorably by the medical press." Four years later, namely, in June, 1876, he returned to the subject by reading a paper before the American Gynæcological Society, entitled "Latent Gonorrhœa, especially with regard to its Influence on Fertility in Women." This was apparently a reproduction of his orignal views, and the same exaggeration of statement char-

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acterized it. It will be convenient to give here his conclusions as the formulated expression of his views. They are as follows:—

1. "Gonorrhœa in the male as well as in the female persists for life in certain sections of the organs of generation, notwithstanding its apparent cure in a great many instances.

2. "There is a form of gonorrhœa which may be called latent gonorrhœa in the male as well as in the female.

3. "Latent gonorrhœa in the male, as well as the female, may infect a healthy person either with acute gonorrhœa or gleet.

3. "Latent gonorrhœa in the female, either the consequence of an acute gonorrhœal invasion or not, if it pass from the latent into the apparent condition, manifests itself as acute, chronic, recurrent perimetritis or ovaritis, or as catarrh of certain sections of the genital organs.

5. "Latent gonorrhœa, in becoming apparent in the male, does so by attacks of gleet or epididymitis.

6. "About ninety per cent. of sterile women are married to husbands who have suffered from gonorrhœa either previous to or during married life."

These conclusions were at the time of the reading of the paper severely criticised by men who represented city and country practice. One gentleman used the naïve but pertinent argument that "he had ascertained in conversation with twenty different physicians who acknowledged having had gonorrhœa in early life, that in no single case had any such symptoms as had been referred to been developed in their wives, and all had had large families of children."

Before the publication of this second paper, the late Dr. Angus Macdonald reported some cases in support of Noeggerath's views. Regarding these I will only say that while Sinclair quotes them with full acceptance, Sänger refers to them as instances of wrong diagnosis.

After the detailed criticism of Noeggerath's second paper by Chadwick, the first to enter the field was the late Dr. Thorburn, of Manchester, who took up the challenge in a paper read before this Association in Manchester in August of the following year, on "Latent Gonorrhœa as an Impediment to Marriage." In contradiction to the conclusions arrived at by Noeggarath, Dr. Thorburn "appealed to the statistics of eighty-one private fami-

lies carefully collected by himself. He showed that there had been thirty-three per cent. of male gonorrhœic infections previous to marriage, twenty-six in all; and, taking all the cases of abortion, sterility, uterine and pelvic inflammations and living births that had occurred in these eighty-one families, he showed conclusively that there had been the merest fractional difference in their proportion between the previously and not previously infected classes. As regards inflammatory pelvic affections, the balance was fractional in favor of the non-gonorrhœic."

Hitherto opinions on the subject of gonorrhœa were based on clinical investigation, but with Neisser's publication of his observations on the gonococcus (in 1879), which had been discovered ten years previously by Hallier, the question entered upon a new phase; the literature of the subject soon assumed large proportions, and the microscope usurped the place of clinical observation. The fortune of war fluctuated between the supporters and the opponents of Neisser's views, yet so much in favor of the opponents that Neisser himself has, in the meantime, been compelled to modify his original views, just as, according to Sinclair, "Noeggerath has lived to greatly modify his first impressions." Confusion entered upon the scene when Bumm, of Würzburg, announced the discovery of more than one diplococcus—five in all—exactly similar to, and indistinguishable by the individual form alone, from Neisser's gonococcus. This state of confusion was not diminished when Kammerer stated that he had found the gonococcus in the fluid obtained from a knee-joint affected with gonorrhœal rheumatism. Nor did it tend to clear up the subject when Kroner, of Breslau (in 1884), formulated his conclusions as the result of his observations on ninety-two cases of ophthalmia neonatorum that there are two forms of blennorrhœa, between which he is unable to make out any clinical difference. On the same occasion Sänger, of Leipzig, stated that the hope aroused by the discovery of Neisser that in the gonococcus we should find a means of diagnosing chronic gonorrhœa had proved to be vain, that it was established as a fact that gonorrhœa could exist without the demonstrable presence of gonococci, and that while the absence of gonococci proved nothing against the gonorrhœal nature of the disease, the presence of diplococci, "seeing there were several varieties indistinguishable from one another,"

did not prove the gonorrhœal nature of the disease. (Here Sänger distinctly throws over the microscope in favor of clinical observation.) Such is the force of Sänger's statements, that Sinclair is constrained to say "that there can be little doubt as to the difficulty of finding the gonococcus in chronic gonorrhœa in women. In cases of only a few months standing, most certainly gonorrhœal in their nature, which have been under treatment, it has been almost invariably impossible to discover the gonococcus, however numerous other bacterial forms might be. If this be established as a fact by general experience, the sooner it is recognized as a fact the better." This has an important bearing on Noeggerath's statements as to the incurability of this affection. Fortunately, at the present day, we are spared the trouble of refuting these and other statements, for Noeggerath has now (that is, since 1887) given up those pessimistic opinions on the question of incurability, which Sänger, in 1884, had declared to be no longer tenable, as to the proportion in which married men who have this disease infect their wives, and the frequency of sterility.

But as if to render confusion worse confounded, in the following year (1885) Fränkel published his observations on non-gonorrhœal colpitis in children, in which he found diplococci which were in every respect identical with the gonococcus. In this he was confirmed by Cséri, of Budapesth, who, in the examination of the discharge in twenty female children from two to ten years of age, found in every case a large diplococcus, bearing a striking resemblance to Neisser's gonococcus. In his opinion both cocci appeared to be biologically identical. The discharge also was very contagious, and he mentions a case in which a nurse lost her eye through accidental infection in the act of syringing a child affected with the disease.

Such, then, is the confusion in which a reliance on the microscope has landed us, and, although we are told that a method has been discovered by which the true gonococcus can be distinguished with certainty, yet the test is so delicate and complicated that it is practically of little use. It is evident, then, that we must fall back on rigid clinical observation in order to arrive at definite and exact results.

In the study of this subject contradictions meet us on every

hand. When we come to the question of the manner in which the disease spreads from without inwards, opinions are equally divergent. Observers are even at variance as to the part played by the gonococcus itself. Thus Bumm affirms that gonorrhœic para- and perimetritis cannot be explained by the action of gonococci; he says there must be another agent, and to account for these conditions he starts the theory of "mixed infection," that is, the addition to the gonococcus of one or more forms of bacteria. It would be unprofitable to dwell on this question.

Again, to account for the obscurity surrounding so many cases of supposed gonorrhœal infection, and to explain away the absence of direct clinical evidence, the theory of "latent gonorrhœa" was advanced by Noeggerath, and it may be assumed that it was this idea which led him to adopt those pessimistic views and make those exaggerated statements from which he has since been obliged to recede. Sinclair has warmly espoused this theory, and, in my opinion, to such an extent as not merely to weaken, but in many respects to effectually destroy, his argument. After describing the chief points in a fairly typical case, he tells us that the "symptoms and signs of the disease vary greatly." These are reached either through a more or less acute attack subsiding into the chronic form, or by gradual development of the creeping form of gonorrhœal invasion, in which, be it observed, "an acute stage either does not exist, or altogether evades observation." The explanation, he says, "is sought in Noeggerath's idea of latent gonorrhœa in the male." The virulence of the gonorrhœal infection appears to depend upon the number and vitality of the gonococci contained in the infecting matter. In an acute attack the number of the gonococci in the secretion is at its highest "with corresponding vigor and vitality of the organism." Hence the infection is conveyed quickly and with certainty. "At the other end of the scale you have the sort of attack produced by the infecting matter, from a man who had been the subject of an acute attack many months, perhaps years, before. Yet we have heard that in cases of only a few months' standing, and after treatment, it has been almost invariably impossible to discover the gonococcus." To explain away this absence of the gonococcus in old-standing cases we find the most extraordinary theory we have yet come across advanced, namely, that "the

gonococci are few and decrepit, probably altogether absent from the periodic emissions of a continent man. It is only the post-nuptial sexual excess that rouses them into sufficient vigor to be harmful." To say that this is a mere speculation, and totally opposed to all analogy, is a mild way of expressing dissent, and one is inclined to re-echo the words of Angus Macdonald, when, in speaking in general terms of Noeggerath's views, he said: "I must confess, however, that I cannot help feeling convinced that he proves too much." I have already called attention to the difficulties surrounding the task of demonstrating the presence of the gonococcus, and although Neisser says he has discovered it as long as three years after the acute attack, yet most investigators have failed to find it a comparatively short time after the disappearance of the acute symptoms.

Once more, to account for the signs and symptoms of the disease in the absence of the gonococcus, Sänger has advanced another speculative idea, namely, that of "a spore as the permanent form of the contagium." Even Sinclair characterizes this as "a mere hypothesis."

Coming now to the more practical aspect of the question, the same divergence of views is observed. Taking Dr. Sinclair's monograph as an excellent summary of this subject, we find the following. Speaking of the acute form, he says: "In the female the urethral form never occurs without other portions of the genital tract becoming involved;" a very strong statement, on a par with what follows, namely: "but the converse proposition is not true; the uterus may be affected, and the most serious complications may develop in the pelvis without the patient ever having noticed any discomfort in micturition." That is a proposition to which I am unable to accord my assent; for it seems to me a strange doctrine that, while the disease extends with the greatest facility from the urethra upwards to the uterus, it meets with obstruction in the opposite direction, in both cases the vagina merely serving as the impartial channel of communication. For it is acknowledged to be "still" a question whether there is any such thing as a "gonorrhœal vaginitis." The very opposite conditions commend themselves to my mind as at least more reasonable. But inconsistency follows upon inconsistency, for he maintains that in the acute form of gonorrhœa in women the

ordinary typical attack extends to the cavity of the uterus, but there is still room for doubt whether, in the ordinary typical case, the process also involves to some extent the tubes, "ovaries and pelvic peritoneum," but he firmly believes that such an extension of the disease is by no means unusual, though it ordinarily disappears without recognition. "We sometimes find that the peritoneum has been reached in a remarkably short period from the time of patient's first contact with the infecting discharge, but as a rule the process takes from two to three months, while it still may be considered in the acute stage." Yet he admits that a general peritonitis, as a result of gonorrhœal infection, must be a very rare occurence.

How very forcibly this contrasts with his opening sentences, in which he says: "Gonorrhœa, as it occurs in the female sex, is still in this country strangely neglected by general practitioner and specialist alike. Its symptoms, the ravages which are its immediate or remote results, are hardly recognized or understood. Yet the virus of this disorder gives rise to a group of diseases, a series of pathological conditions, which, by reason of their social and moral consequences, surpass in importance any other class of affections with which the gynæcologist is called upon to deal." Surely this is the language of exaggeration, which seems to be fatally connected with this subject. Would that Dr. Sinclair had been more mindful of the sentiment thus expressed in his own words. "It is necessary to guard against exaggeration, for there is a danger that in rousing from long ignorance and neglect of the subject, the professional mind may sway to the other extreme, and, amidst the phenomena of disease, obscure in their nature, and as yet inexplicable, be tempted to accept gonorrhœal infection as an easy and sufficient explanation of morbid processes with which it has no kind of causal relation."

Now I would not for a moment have it even suspected that I am desirous of minimizing the importance of this disease, much less of denying its power of evil, or that it is capable of producing salpingitis, with its various results, ovaritis and pelvic peritonitis, even to a fatal termination, yet I must insist on a more rigid adherence to the teaching of facts actually observed.

It is at least a strange fact that I have never seen a case in which I could obtain incontestable confirmatory evidence that a

case of salpingitis, pyosalpinx, much less hydrosalpinx, ovaritis, or ovarian abscess, was of gonorrhœal origin, although I take every precaution so that the history of each case of disease of the appendages where I operate should be as complete as possible. But there is no reason why I should refuse to accept the well-authenticated evidence of other observers. Even Sänger, who admits the frequency of gonorrhœal salpingitis, goes so far as to say that "gonorrhœal salpingitis" is never followed by a "destructive suppuration" of the uterine appendages ; it remains invariably a disease of the surfaces of the mucous and serous membranes. While engaged in the special study of this subject for the purpose of this discussion, in which I acknowledge my special obligation to the labors of my friend Dr. Sinclair, already referred to, I have met with several cases of interest.

About the same time I was consulted by two patients, whose conditions and symptoms were such as to lead me to make special inquiries. In both there was a copious greenish yellow discharge, with redness of the orifices of the vulvo-vaginal glands, and well marked evidence of tubo-ovarian mischief, in the tenderness, enlargement and apparent fixation of the appendages ; in A, on both sides ; in B, on the left side only. Sterility existed in both cases, four years in one and nine years in the other. In the case of A. the following facts were obtained : A great deal of "whites" before marriage; after three or more months discharge became more abundant, and instead of being white became yellow; about the seventh or eighth month first experienced pain on sexual connection ; on one occasion the act performed in the early morning was more than usually painful; in the course of the day, while out walking, was seized with some pain in the left ovarian region, felt sick and fainted; was laid up for a week, and has never been well since. She was the second wife of her husband. He had had gonorrhœa three times before his first marriage, his wife bore him two children without any untoward result, but died from heart disease in her third pregnancy, undelivered at eight and one-half months. He had not even exposed himself to disease after his first marriage.

In the case of B., the physical conditions were very similar, with the exception that the pelvic disease was limited to the left side. On the question of gonorrhœa I requested her usual medi-

cal attendant to obtain information for me. His answer is emphatic. He (the husband) completely denies ever having had any ailment whatever of this nature, and, what is more strange still, tells me that "he was virtuous, never having known woman until his marriage."

In both these cases we have a train of signs and symptoms which tallies very closely with those which are to be found in cases quoted as undoubted examples of gonorrhœal infection, even to the redness of the orifices of the vulgo-vaginal glands, the copious, greenish-yellow leucorrhœa, dysmenorrhœa and pelvic mischief. But he would be a bold man who would assert that gonorrhœa played any part even in the first case.

On the other hand, I have recently seen, with Dr. Campbell Pope, a patient who had contracted gonorrhœa from her husband, and at the time of her first confinement some years ago had an abundant crop of syphilitic warts, not only on the external parts, but also in the vagina. She was very ill with pelvic mischief for many weeks, but ultimately got well. At the time of our consultation she was ill again with pelvic symptoms (threatened puerperal fever) after a premature confinement, the fourth pregnancy since her first illness. Here the symptoms following her first confinement may fairly be set down as the result of gonorrhœal infection, but this did not produce sterility ; and I am not prepared to accept Sinclair's dictum that "the woman who has suffered from gonorrhœal perimetritis is barren."

It is to my mind very strange how Noeggerath and those who think with him ever could have come to the conclusion that gonorrhœa plays such an important part in the production of sterility in the face of the large number of cases of ophthalmia neonatorum that are due to gonorrhœal infection. If this view were well founded, then the infection must have taken place after the pregnancy had begun. Otherwise we must assume that it is only a coincidence or an accident that gonorrhœa and sterility ever stand in the relation of cause and effect.

My own observations fail to supply me with a single instance in which gonorrhœa has produced sterility in the male. One striking example comes before my mind of a gentleman whose wife was barren. This fact might be seized on as a case in proof

were it not for the fact that he was anything but impotent or sterile in the case of another woman, to my certain knowledge.

As the result of my inquiries at the Lock Hospital and amongst numerous general practitioners, I have failed to find any evidence to support the statement that this disease "gives rise to a group of diseases, etc., which surpass in importance any other class of affections with which the gynæcologist has to deal." In the Lock Hospital I was informed by the house-surgeon that he was unaware of a single instance of pelvic disease following gonorrhœal infection. General practitioners tell pretty nearly the same tale of numerous examples of gonorrhœa in young men—marriage, no evil consequences. Listen to what the late Dr. Bumstead said in the fifth edition of his work on *Venereal Diseases*, in a very short notice of Noeggerath's extreme views, and in which he employed a weapon which, according to the French proverb, is so deadly—namely, ridicule. He said that at one of the annual meetings of the British Medical Association, one of the speakers announced that Dr. Noeggerath's views were so generally known and accepted in America that one of the first questions asked by the parents of every young lady to whom marriage was proposed by a gentleman was whether he had ever had the clap: "In short," he adds, "even if eight hundred out of one thousand men have had the clap, the human race did not die out long ago, but still exists, and shows no tendency, so far as I know, to diminution." But a more sober testimony—and it is the last quotation I shall trouble you with—on the authority of Sinclair, is that offered by Martineau, who, after a very long experience and an enormous amount of material from which to form conclusions, says: "You will find by a close examination of the material that primary uterine blenorrhagia is extremely rare. In about two thousand cases I have seen it only ten times at most. You will find, further, that ovaritis and salpingitis are so rare that I have not been able to pick out a single case. As to pelvic peritonitis, I have found it only twice." My opinion, then—an opinion founded on my own observation and on a study of the literature of the subject—is that "the importance of gonorrhœa as a cause of pelvic inflammation" consists in the fact that in a few or limited number of cases it seems to be capable of producing most serious symptoms, rarely, however, terminating in death, and that this importance is diminished by the fact that these cases are comparatively very rare.