fer from pain at all, and that except for the discharge, he was not able to say there was anything the matter with him. At the time, I called your attention to the fact, that it was one of the milder varieties of gonorrhœa. There was, perhaps, no question regarding the origin of the disease, and yet I doubt very much whether that discharge would, if placed in contact with healthy mucous membrane, have communicated gonorrhœa. The grade of inflammation was low, and so low, that it is a question whether its products were capable of establishing an inflammation of like character. But let such a man go on a drunken spree, or have excessive sexual intercourse, you would find that his discharge would become painful. If, then, it was brought in contact with healthy mucous membrane, you would see developed a corresponding discharge, and an active contagious property would be present. There are, therefore, two varieties of the affection: 1. That in which there is a slight, painless, purulent secretion. 2. That in which a higher degree of inflammatory action is present, which gives rise to pain and a greenish purulent discharge.

A man may come to you, complaining of an almost white purulent discharge, and at the same time saying that the connection had been with a woman whom he knew to be free from gonorrhœa. Perhaps the additional statement will be made, that she was suffering from the " whites," or that she had just completed her menstrual period. If you do not interfere with the discharge, it will disappear within four or five days, as a rule, and the man will be well. But the trouble is that, as soon as a man sees a drop of pus at the meatus urinarius, he goes to a doctor, who attempts, in very many cases at least, to abort the gonorrhœa. A solution of nitrate of silver is employed as an injection, and if that is kept up for four or five days, a sharp urethritis is usually established. I do not believe there is any such thing as aborting true gonorrhœa. Gonorrhœa runs its course, and lasts about four weeks, and any urethral discharge that exists for a less period, I should be willing to say was not gonorrhœa.



## LESSON XXXII.

Specific origin of gonorrhœa still claimed—This claim opposed by both early and recent accepted authorities—Arguments advanced in favor of its non-specific origin—Necessity of the understanding of physiological processes to settle the question—Claims of a specific origin of gonorrhœa through a micrococcus—Arguments to prove this a fallacy—Differential diagnosis—Urethral stricture a cause of discharge which may be mistaken for a gonorrhœa—Abortive treatment of gonorrhœa not warranted by experience—No well-authenticated case of the success of such treatment yet produced—Gonorrhœa acquired from a well-authenticated gonorrhœa not susceptible of speedy cure.

The importance of a clear appreciation of the nature of gonorrhœa, warrants me in again calling your attention to this subject. Is it, as I have positively claimed, a non-specific disease—that is to say, one capable of being originated, in typical form, through a variety of causes, or must it always and invariably be referred to contact with the secretion of a pre-existing gonorrhœa? I need not assure you that I have no doubt, personally, *that this is a fact.* The reasons for believing the disease a non-specific one, and some of the cases proving it, have been laid before you in a previous lesson. But, I gave you simply the results of my own thought and experience. I did not say to you that a contrary view is still held and taught. This is so prominent a fact, that it appears to me desirable, to present to you some of the reasons given, for belief in the specific origin of gonorrhœa, and also to support my own views by citations from eminent contemporary authorities.

In the year 1877, a prominent English surgeon issued a book, confined to the subject of gonorrhœa alone, and containing about 400 pages. Its author is a gentleman who has been identified with the study and treatment of genito-urinary diseases in England for many years, and I can commend this book to you as the most valuable and comprehensive work on this subject in our language. But, in it, Mr. Milton has raised some issues which I

thought were settled; especially that most important one with reference to the *specific character* of gonorrhœa. After considering the question, as it would seem, quite thoroughly, the author arrives at a conclusion which, to me at least, is extraordinary, which is that, so far as he is able to judge, *gonorrhœa is the result of gonorrhœa only.* Now, the French authorities, of which Ricord is the leading representative, almost without exception make the definite and unequivocal statement, that true gonorrhœa may be acquired from a simple leucorrhœa. Ricord gives repeated instances of this kind, and also cites examples in which a gonorrhœa, not distinguishable from that acquired by contagion, has been produced solely by a persistent and continued ungratified venereal excitement.

In this country we have Drs. Bumstead and Taylor (favorably known as authorities in this department) confirming this view on page 40 of their work (ed. 1879), as follows: Dr. B. says, "I have been convinced, by somewhat extended observation, that gonorrhœa originating in this mode, is of very frequent occurrence. Of one thing I am *absolutely certain*, that gonorrhœa in the male may proceed from intercourse with a woman, with whom coitus has for months, or even years, been practised with safety, and this, too, without any change in the condition of her genital organs, perceptible upon the most minute examination with the speculum. I am constantly meeting with cases, in which one or more men have cohabited with impunity with a woman, both before and after the time when she has occasioned gonorrhœa in another person; or, less frequently, in which the same man, after visiting a woman for a long period with safety, is attacked with gonorrhœa, without any disease appearing in her, and after recovery, resuming his intercourse with her, and experiences no further trouble. The frequency of such cases, leaves no doubt in my mind, that *gonorrhœa is often due to accidental causes and not to direct contagion.*" And page 41, "Most cases of gonorrhœa from leucorrhœa, or the menstrual fluid, present no characteristic symptoms by which they can be distinguished from those originating in contagion." Drs.



Van Buren and Keyes (who have written an excellent work upon genito-urinary diseases), express themselves with the greatest positiveness on this point, on page 53: * "Experience proves beyond a doubt that a high condition of urethral inflammation, attended by an abundant discharge, and presenting absolutely no features to differentiate it from a gonorrhœa derived from a prostitute with a virulent discharge, may be acquired by a young lover, from his equally healthy mistress, by a young husband from his wife, or may be produced by applying a chemical irritant to the urethra."

Dr. Fordyce Barker, of this city, quoted by Dr. Bumstead (Bumstead and Taylor, page 41), speaks of a peculiar form of metritis, which has associated with it, a leucorrhœal discharge, that, in repeated instances, has produced a purulent urethritis in the male. Not long since, a gentleman came into my office, with the statement that he had been married nearly one year; that shortly after marriage a purulent discharge came from his urethra. On consultation with his physician he was told it was of slight importance, and had probably been acquired from a leucorrhœal trouble from which his wife was then suffering. From that time, within three or four days after each sexual congress with his wife, a purulent discharge appeared in his urethra and continued until assisted by an astringent injection. When examined, the man was found to be free from any trouble whatever. Upon examining the wife it was ascertained that she was suffering from retroversion of the uterus; that the surface of the external os was eroded and covered with a thin layer of muco-purulent material. The uterus was restored to its normal position and retained by a pessary. I was of the opinion at the time that the erosion and discharge were due to displacement of the uterus, and so stated. Under suitable treatment the lady made a prompt recovery, and from that time the gentleman suffered no more from his urethral trouble.

* "Genito-Urinary Diseases with Syphilis." Van Buren and Keyes. Appleton & Co., N., Y. 1874.

To me, the case seemed to be one that pointed clearly to the leucorrhœal discharge, dependent upon the condition of the uterus, as the cause of the urethral discharge in the husband. This, and the case which I related at the last lecture, in which the patient produced a urethritis by the violent introduction of a sound, I would put forward as among those, where there is every reason to believe the urethritis to have been produced, by other causes than contact with a gonorrhœal discharge.

Mr. Milton admits that "urethral discharges do appear in men as the result of connection with women laboring under leucorrhœa, *in whom there is no reason to suspect a present or a previous blenorrhagia.*" This admission, however, is wholly negatived by the sentence which follows it, thus: "It must, I think, be equally admitted that the facts supposed to establish this, are, when we come to sift the matter closely, generally vague and few." The property of contagion, with Mr. Milton, points directly to specific cause; to already existing gonorrhœa as the origin of it, in all cases, and where there is absolutely no evidence of such origin, and no reason to suspect it, belief to the contrary, Mr. Milton says, "must remain a mere conviction, and cannot be raised to the stability of truth."

Mr. M. states, that in no case that he has read of, "is there anything to show that the surgeon had satisfied himself as to the *previous state of the organs in both persons.*" On the strength of this, he would seem to claim that all the evidence in favor of the non-specific character of gonorrhœa, the carefully observed and reported cases of accepted authorities, are without value. Notwithstanding the positive belief acquired through the long and studious experience of such men as Ricord, Diday, Bumstead, Van Buren; and, unequivocally stated, we must believe all urethral discharges associated with the contagious element to have a gonorrhœal origin, "even when there is no evidence of such origin, and no reason to suspect it." Personal statements, from no matter whom; circumstantial evidence; character of individuals; repeated coincidences; all go for nothing.

Even the case previously cited, when the disease was



*proven* to be of traumatic origin, and which was proven to be contagious (and hence gonorrhœal) by the supposed *proven* communication to a presumably healthy wife, even this case may be waived out of the contest as among the not *absolutely* proven cases.

We, however, must admit that all clinical observations, and investigations, are of necessity more or less empirical, until carried to a point coincident with, or to the establishment of some fixed physiological or pathological law. Until such is established, the most important, the most conscientious, and extensive, clinical labors must be open to the denial and depreciation of every passing observer, of little or much experience. Thus it happens, that the most important question, upon the correct or faulty solution of which, the honor and happiness of individuals—even of whole families—may depend, the accumulated experience and the deliberate conclusions of our wisest men, are disregarded, and the specific nature of gonorrhœa asserted, upon the basis of a purely negative personal experience. If anything could give an incentive an dimpetus to a movement towards a more scientific solution of the matter, we certainly have it here. Naturally, we look to the great authorities in histological and pathological science, for some clue which shall lead to a more definite understanding of the true nature of the contagious element of gonorrhœa, as well as that equally contagious property which is characteristic of conjunctivitis and infantile leucorrhœa.

Dr. Salisbury and others have, for many years, claimed a vegetable spore as the cause of gonorrhœa, but the persistent denial of this claim, by distinguished microscopists, at home and abroad, appears to have settled this question in the negative. But in a recent series of lectures on the relations of micro-organisms to disease, delivered before the Alumni Association of the College of Physicians and Surgeons, N. Y., by Dr. Belfield of Chicago, attention is again called to the alleged parisitic origin of gonorrhœa. In the opening of his third lecture Dr. B., alluding to the possible origin of gonorrhœa, says: "In 1879 Neisser made the assertion, based upon

numerous examinations, that there is present in the purulent discharge of gonorrhœa, whether from urethra, vagina, or conjunctiva, a *micrococcus* not found in other pus, distinguished by its size, shape, and mode of reproduction. Neisser's previous work entitled this assertion to respectful consideration, and it was at once subjected to extensive tests. The reports have been, with one exception, unanimous in corroborating Neisser's assertion in all its details. I may mention especially Ehrlich, a most expert and experienced, yet conservative and trustworthy observer; Gaffky, a pupil and present assistant of Koch; Aufrecht, of Madgeburg; Löffler, Leistikow, Bockhart, Krause; and among the ophthalmologists, Leber, Sattler, and Hirschberg. The only dissenter, so far as I know, is Dr. Sternberg, who asserts that this micrococcus form is widely distributed, and is, in fact, the same as that which Pasteur has shown to cause fermentation of urea.

Several attempts have been made to inoculate human subjects—since animals are not susceptible to the contagion—with the isolated micrococci. Bokai, in Pesth, asserts the induction of urethral gonorrhœa, in three out of six students so inoculated; but as he neglected to keep them in solitary confinement during the trial, the experiment is not so convincing as it might be. Bockhart, having cultivated the organisms on gelatine, inoculated with the fourth culture a paralytic hospital patient, and observed a typical gonorrhœa on the sixth day. Sternberg cultivated micrococci from gonorrhœal pus in flasks, and observed only negative results in each of five patients inoculated therewith. Thus far, therefore, it is not decisively established that the bacterium associated with gonorrhœa is the cause of the disease.

Sattler has recently found micrococci, apparently identical with those of gonorrhœa, in the conjunctival granulations, and affirms that inoculation with the organisms, isolated by cultivation, induced the disease in a human subject.

Micrococci, then, exist in the human body, locally and generally; yet excepting gonorrhœa there is no



decisive evidence that a specific micrococcus is associated, exclusively, with any one specific morbid process in the human subject."

It will be remembered that in 1872, Lostorfer, of Vienna, claimed to have discovered a corpuscle or baccillus, which was peculiar to syphilitic disease, and this discovery was announced and proven to the entire satisfaction of Skoda, Hebra, and Stricker, and was disputed by Wedl, Gruber, and Neuman. Professor Stricker, however, at once commenced a series of independent operations for the purpose of ascertaining the validity and real significance of Lostorfer's discovery. The alleged corpuscle was found in syphilitic blood, but it was also found in the blood of persons not syphilitic, but who were subjects of cachexia from other causes.

In the more recent discovery of the gonorrhœal bacillus not only has it been asserted present in every specimen of gonorrhœal pus, but typical gonorrhœa is claimed to have been set up, through its inoculation upon persons previously free from the disease. Through the kindness of Dr. Belfield I have been enabled to observe the microscopic appearances of the so-called gonorrhœal bacillus or micrococcus. About the diameter of a white blood-corpuscle, it is made up of fine, apparently round points, loosely arranged in a kidney-like form about the diameter of a white blood-corpuscle, and apparently occurring in about the same frequency, in comparison with the red. Other and more critical experiments will be required before the claim of its specific power can be admitted. In the cultivation of these bacilli for experimental purposes the difficulty of securing absolute freedom from the fluid originally associated with them, and commonly accepted as capable of setting up a gonorrhœa, without the intervention of a bacillus of any sort, must be very considerable.

The subjects of experiment must be secured against even the possibility of sexual contact. In the six cases cited, the chance of acquiring a gonorrhœa in the interest of medical science, *might* be accepted as an opportunity to take the risks of ordinary contagion under cover of

the scientific experiment. The fact that out of six inoculations, under apparently similar circumstances, three only were successful, is worthy of note. Sternberg's claim that a micrococcus, identical in appearance with the so-called gonorrhœal micrococcus, is often found in fluids entirely independent of gonorrhœal influence, would, if proven, at once relegate this discovery to the records of Lostorfer's corpuscle. The statement (oral) of Dr. Belfield that, in his experiments, such a micrococcus, has been repeatedly found in the pus of sores on the hands of students working much in the dissecting-room, is significant. If it does not wholly disprove that the origin of the alleged gonorrhœal micrococcus, it nevertheless shows that it is only one among the causes hitherto claimed as capable of initiating the disease and hence loses much if not all of its practical value as a scientific discovery.

It has occurred to me that the difficulty of settling the question with reference to the specific character of gonorrhœa, may be simplified, if we can go behind all clinical records, and consider the changes which occur in the transition between the normal formation of epithelial elements and the characteristic pathological products. Rindfleisch—and his opinion is entitled to consideration—in discussing the question of the formation of epithelium, uses a significant phrase, namely, "epithelial infection," and says: "The opinion is inevitable that the embryonal formative cell, can only become an epithelial cell, when it comes in contact with such; *we must believe in a kind of epithelial infection.*" That is to say, when a formative embryonal cell comes up from the cellular tissue to take the place of a cell that has preceded it, its form and character is determined by coming in contact with epithelial cells already formed.

With reference to other tissues, it is claimed that the peculiar form which the germinal or formative cell is to take is determined by contact with the tissue of which it is destined to become a portion.

If this view is accepted, we have infection as a physiological process, and then the interesting question



arises, at what point does the epithelial element cease to have the infective influence? In other words, at what point does it cease to have the contagious property? For, it is here distinctly shown, that normal epithelial elements, possess an essential and characteristic property of contagion. If this be the case, shall we accept the contagious element as evidence of gonorrhœa, when we find it in the physiological process? As soon as the fact, that a contagious principle is contained in the physiological constitution of the parts, is recognized the doctrine of contagion receives a fatal blow. One thing is very certain, viz., that as long as the physiological infection, this influence of epithelial cells by their contact with each other, is continued in a salutary way, it gives rise to no disturbance whatever, and the thought of contagion does not arise. But when a cell, which has fallen from its integrity, exerts its vicious influence upon other cells, disturbance is manifest, and compels attention to its character and powers.

Because gonorrhœal disease is contagious, it has been claimed that this property must be regarded as evidence of the presence of a special virus. If, however, it is the contagious property alone that is characteristic, and this property is the proven attribute of all epithelial cells, it can no longer be regarded as evidence of the specific character of gonorrhœa; hence the only distinction to be recognized, between gonorrhœa and urethritis, is a moral one. The term *gonorrhœa* implies gross immorality. A good man, then, with simple urethritis, perhaps occurring independent of sexual contact, may, by injudicious living or venereal excess, elevate his simple urethritis up to a contagious point. Then he has *gonorrhœa.* He has done nothing morally wrong; he has simply been placed without the pale, because of his urethral discharge assuming a contagious form. In that condition, he has, perhaps, communicated the disease to his wife, who would, under Mr. Milton's teachings, be qualified to sue for a divorce, and to obtain it, notwithstanding there was no evidence of gonorrhœal origin, nor any reason to suspect it.

DIFFERENTIAL DIAGNOSIS.

Having endeavored to ascertain what gonorrhœa is, we will briefly allude to the question of differential diagnosis.

Inflammation of the mucous membrane covering the glans penis and reflected upon the prepuce, termed *balanitis*, may be mistaken for gonorrhœa. This condition may arise from various causes, such as irritation produced by accumulation of sebaceous material beneath the prepuce, or from hypertrophy of the papillæ of the mucous membrane covering of the glans, as in the case exhibited this afternoon.

In that case, the irritation produced by the enlarged papillæ, had given rise to pus, which had accumulated beneath a long prepuce, in such quantity, that, when the prepuce was pressed, a discharge from the preputial opening took place, that could very readily have been mistaken for one issuing from the urethra.

I have encountered cases in which such a mistake has been made, and injections containing various astringent and irritating materials used for months under the supposition that the patient was suffering from gonorrhœa, when in truth the urethra was free from all disease except that produced by the injections. In some of these cases, because of the degree of phimosis present, it is difficult to ascertain whether the discharge issues from the urethra or not; but your treatment should not be commenced until that question has been settled.

The next point is to ascertain, whether the disease you are called upon to treat, is a simple urethritis or a gonorrhœa acquired by contagion.

In most cases, the character of the exposure is not well understood, either by the person exposed or by the physician. When a discharge appears in the urethra of the male, after illicit intercourse, the presumption is in favor of a gonorrhœa, existing in the woman with whom such intercourse is held. At the same time, this is not certain, for we know that other causes are capable of originating a discharge, to all appearance identical with it.



Quite commonly, (not so much now as formerly, however) an attempt is made to cut the disease short by the use of some active injection. This plan is fast going into disrepute, and justly so, for by adopting it, the advantage is at once lost, of being able to determine whether the disease would not have readily passed away, within a few days by the use of simple means. By such treatment you necessarily add an inflammatory trouble, and if the case is one of real gonorrhœa, you will increase the activity and the inflammatory character of the disease.

I have met with a large number of urethral strictures, several thousands, and I have made it a point to look up their origin, and to learn concerning the disease which gave rise to them, how the previous gonorrhœas were acquired, and how treated. I have not yet found a single case, which recovered within four weeks, where the symptoms were such as to convince me that the patient suffered from a gonorrhœa *that had been acquired from a gonorrhœa*. In every instance in which the evidence was positive upon this point, the gonorrhœa had lasted at least *four weeks*. There are many cases of gonorrhœa in which the patient, on account of his youth or extreme modesty, neglects to seek professional advice, and yet these very ones pass through their trouble in about the same time as those who avail themselves of eminent professional service. The same is also true of many cases reported to me treated by homeopathic remedies, and through the German method, by baths alone. I do not give much credit to the plans which have been brought forward for shortening the duration of the disease, be the treatment what it may. Treatment may aggravate the case, and it may also make the patient much more comfortable than if the disease is left to pursue an unmodified course. The urine can be made less acrid by a variety of means; the distressing symptoms, such as chordee, etc., may be relieved; but as to cutting the disease short, I do not believe that it can be done.