impairment, as in epilepsy, it is difficult to prove that the long continued use of the bromides tends towards dementia of even a slight degree, but I believe that they cannot be used for a long time without some disadvantage, and that in children or youth, they cannot be taken for years together, to a sufficient extent to control epileptic convulsions without not only direct injury to the brain, but also, without producing an arrest of development due to lessened energy and diminished use. I have no doubt, however, that there are cases where it is well to give them, nevertheless, and I cannot say how often, or in what cases, borax will prove successful. But we should not abate our zeal in trying to find some other means for the same end, with which there may not be so manifest injury with the benefit.

ON THE LIMITATION OF THE CONTAGIOUS STAGE OF SYPHILIS ESPECIALLY IN ITS RELATIONS TO MARRIAGE.¹

BY FESSENDEN N. OTIS, M.D., OF NEW YORK.

In whatever consists the true contagium of syphilis, one important fact in relation to it is well proven, namely: that it is contained in the substance of the initial lesion of chancre and the lymph channels and glands in immediate relation thereto. And that, as a rule, for a period of at least eight weeks, it is confined between this point and the general blood current.

It will thus be seen that the doctrine of a mysterious, instantaneous, syphilitic infection, as still claimed by some authorities, is denied, and that a gradual infection, through physiological channels alone can be considered possible.

A few words in proof of this position. Inoculations of healthy persons with the blood of a person affected with an initial lesion of syphilis failed to communicate syphilis, until after the eighth week from the date of infection. Several well authenticated cases of this sort have come to my knowledge. Twice the inoculation was in my own person. A great amount of evidence on this point is furnished by Diday. In his work on Infantile Syphilis he says, that syphilis contracted by the mother after the completion of the seventh month of gestation *has never produced the disease in the fœtus*. Abernethy had previously claimed the same immunity for the fœtus, after the seventh month, but Diday asserted it as a *law* to which there were practically no exceptions. On page 32 of his work on Infantile Syphilis (Sydenham edition, 1859), he says: "Under similar circumstances a child born, apparently healthy, of a woman who had contracted syphilis in the eighth or ninth month only, might be entrusted to a healthy nurse without fear of communicating the disease to her."

Practically, as it is thus shown, the disease is limited to the initial lesion and its immediate vicinity during the first two months after infection. Syphilis may then, up to such period, be claimed as a local and not a constitutional disease.

It has been proven and accepted by such authorities as Fournier and Cornil of Paris, Mireur, of Marseilles, Van Buren and Keyes and Bumstead and Taylor, of New York, Hill and Cooper, of London, and others, that the physiological secretions, mucus, milk, tears, sweat, sebum, urine, semen, etc., do not contain the

contagium of syphilis. Fournier says: "It has long since been established that the semen of a syphilitic subject is not susceptible of transmitting syphilis."²

Bumstead and Taylor say, (Fifth Edition, page 463): "The non-inoculability of the semen of syphilis was fully proven in a number of experiments made by Mireur, of Marseilles"; and further at page 807, "It has been proved that the semen is not an infecting fluid, as are syphilitic blood and the secretion of specific lesions. . . . We therefore cannot admit the infectious properties of semen as regards the female or the fœtus subsequent to conception." Hill and Cooper (Second Edition, page 73) say: "The semen cannot be asserted to be contagious with the knowledge we at present possess." Cornil, 1882, says: (Am. Edition, page 351) "There are no reliable cases evincing the truth of syphilitic contagion by the spermatic fluid." Acceptance of this important fact enables us to eliminate from the mysteries of syphilitic contagion such cases as those where the father is claimed to have infected the embryo or fœtus, while the mother has escaped the disease, and also makes its clear beyond dispute that syphilis is never, under any conceivable circumstances, communicated from the father directly to the fœtus, but that in order to infect the product of conception before birth the mother must first be infected.³ The semen having been proven not to contain the contagium of syphilis, settles this much discussed question, inasmuch as through the semen alone has the father any possible access to the ovum of the fœtus.

It will also be seen that if the physiological secretions which do not contain germinal cells, are free from the contagium of syphilis, this constitutes a strong argument in favor of the nature of the contagium of syphilis as claimed. And further, if contact of a degraded germinal cell is essential to the production of syphilis and the disease is conveyed to the embryo or fœtus only through the blood of the mother, there is then no longer, strictly speaking, any *hereditary* transmission of syphilis. In order to the acquirement of syphilis an organism free from that disease is essential. The infecting cell of syphilis must first be brought in contact with otherwise healthy cell material. "No mysterious hereditary influence is necessary nor can be admitted. If the disease germ of syphilis, by contact with external parts or through its amœboid power, traversing tissue, reaches healthy cell material, whether in the adult, the infant or the embryo, then the syphilitic influence is directly transmitted and its development must be governed by the same laws that characterize its progress in the known behavior of the disease in the adult; modified to a greater or less extent by the age and degree of stability of the tissues in which the disease is developed. . . . It is undoubtedly the fact that various adynamic diseases and conditions in fœtal and infantile life result from pre-existing disease in the generative organs of parents who have been subjects of syphilis, but that any syphilitic disease, proven to be such by its power to transmit syphilis, has been communicated to healthy persons after the active or so-called *secondary* stage of the disease has passed, there is no well authenticated evidence to prove."

¹ Alfred Fournier, *Syphilis et Mariage*, Paris, 1880, page 26.

² It is important to note that the very authorities which claim that the embryo or the fœtus may be affected while the mother escapes still advocate the administration of mercury to the mother, and they also claim distinctly that such treatment prevents subsequent abortions and disease in the fœtus.

³ Read before the New York State Medical Society. Some introductory paragraphs on the nature of the syphilitic contagium have been necessarily omitted.

I am quite aware that this position will at first be received with surprise, but it is the logical sequence of the acceptance of the degraded human germinal cell as the true contagium of syphilis, and it can, I believe, be shown to be in the fullest accord with all well authenticated facts in regard to hereditary syphilis.

These views were originally published by me in a series of class-room lessons in 1879, and again in my volume on the "Physiological Pathology of Syphilis," in 1881, and elaborated more fully in an article on the "Syphilis of Infants and Hereditary Syphilis," in 1884. It is worthy of remark that Mr. Jonathan Hutchinson, of London, the leading authority on syphilis in Great Britain, has arrived at conclusions identical with my own in regard to the nature of the so-called hereditary syphilis. Thus in his "Pedigree of Disease," published in London, 1884, page 90, he says: "A child, then, I assert, inherits syphilis in precisely the same sense and in precisely the same manner as it may inherit small-pox. It inherits not the diathesis but the disease. The reason why the inheritance of small-pox is rare, while that of syphilis is unfortunately common, is simply that the period during which the virus is extant in the blood is very different in the two cases. The conspicuous facts then, in reference to the syphilis of infants, afford no proof that the diathesis of syphilis, any more than those of the other exanthemata is capable of transmission." Mr. Hutchinson further says: "My argument, if I have made it plain, has pointed to the conclusion that no transmission of syphilis is possible, and that the child gets either nothing at all or the germs of the disease, and that in the latter case they will, subject to the laws of idiosyncrasy, develop equally in all cases."

Mr. Hutchinson does not attempt to explain the grounds on which these conclusions—so radically opposed to the usual teachings of authorities—are based. It is not, however, possible that in the face of generally accepted traditions to the contrary, these questions could be settled in such a positive way through personal, clinical observation alone. Mr. Hutchinson's teaching is the legitimate and logical sequence of the acceptance of a gradual syphilitic infection through cell contact, as heretofore claimed in this paper.

Having thus indicated the limitations of the contagious element of syphilis in the matter and mode of infection, we have now reached a point where we may, I think, proceed to the consideration of the probable or possible limit of the contagious stage of syphilis in point of time. A very general impression prevails, both in and out of the profession, that syphilis is capable of being communicated to healthy persons by a person once the subject of the disease, either by contact or by heredity, throughout the lifetime of the individual. It is a common belief that when after years of freedom from any evidence of the disease any lesion is recognized as due to a former syphilis, it is the indication of a poison still remaining in the blood of the individual, and that it may be transmitted to healthy persons by contact or by heredity. It has, however, now come to be understood practically that when the accumulations of cell material which have caused the lesions of the secondary stage of syphilis, and the enlargement of the lymphatic glands at various points have been eliminated, the disease is, as a rule, no longer contagious, and persons the subjects of syphilis who have by systematic and thorough treat-

ment been brought to this condition, are said to be cured of syphilis. The time during which the treatment is recommended by authors to be continued, is pretty uniformly fixed at about three years. If then, the patient has been free from all signs of syphilis for one year he is pronounced cured, and he is permitted to marry.

Fournier says: "The truth is that with some very rare exceptions syphilis constitutes only a temporary bar to marriage." Bumstead and Taylor say: (Fifth Edition, page 91) "It may be stated in broad terms that no syphilitic father should procreate children until two years after infection; during which he should sedulously follow a systematic course of treatment." Keyes ("Venereal Diseases," Wood & Co., 1880) says: "After the virulence of the disease has been exhausted a man may marry and should marry." Again (page 78), "In a general way it may be safely said that a man should not marry until at least three good years lie between him and his chancre, and at least one year has elapsed since the last symptom which can be ascribed to syphilis." Hill and Cooper (London, 1881) state that "Under any circumstances the shortest period between infection and marriage should be three years. . . . Under no circumstances should a person with obvious signs of syphilitic disease marry, however long a time has elapsed since his infection, for though communication is rare when several years (four or five) have elapsed, *it may still take place after as many as ten or even more years, even when the form of disease is of the character commonly called tertiary.*"

It will thus be seen that authors are not quite harmonious in regard to the safety of marriage after syphilis, although all are agreed that no marriage should be permitted, until a year after the disappearance of any lesion which could be attributed to syphilis. All, however, are also agreed that the communication of syphilis after the third year of its existence is rare. It has been determined by careful clinical observation that the contagious element of syphilis is, as a rule, eliminated within a period varying from perhaps two to four years, and that all the manifestations of disease beyond this time, apparently due to syphilis and called tertiary lesions are not entitled to be considered strictly speaking as syphilitic. Mr. Hutchinson says of them: "What are called tertiary symptoms do not constitute a necessary stage of syphilis, and are to be regarded in the light of sequelæ." This statement is corroborated by the fact now well determined that the secretions of all such lesions are innocuous; and that neither they nor the tissues, nor the blood of patients bearing such lesions contain a contagious element.

Without the *contagium* there can be no syphilis. Ricord claims that tertiary lesions are not inocuable and cannot be transmitted by hereditary descent. Bumstead and Taylor (Fourth Edition) after reviewing this matter say: "Hence we consider the blood and the secretions in tertiary syphilis innocuous." Diday performed inoculations with the blood of persons in the tertiary stage of syphilis with a negative result.

If time permitted I think it could be satisfactorily proven to you that the tertiary lesions of syphilis are invariably due to obstructions of lymphatic spaces and channels, the result of simple pathological changes occurring during the active or contagious stage of syphilis. I will simply say that it has been determined through microscopic examination that *all* the lesions of tertiary syphilis are characterized by an accumula-

tion of so-called gummy material and that this gummy material has been found not to differ in any essential respect from normal germinal material, and that the damage associated with it may, in every case, be fully accounted for by the mechanical influence of its presence. It is to the occasional occurrence of tertiary lesions in persons who have passed through the active stage of syphilis, that a persistence of the syphilitic diathesis, with all its power of communicating syphilis for an indefinite period, even possibly through the lifetime of a patient is claimed. If, therefore, it can be absolutely proven that the active or contagious stage of syphilis does *not* last indefinitely, and that the lesions of the tertiary stage and the blood during this period are also free from the contagium, we may then hope to reach some definite limitation of the contagious stage of syphilis in all cases.

The acceptance of the degraded human germinal cell as the true contagium of syphilis and its logical sequences as determined through our present knowledge of minute physiology and pathology will lead towards, if not definitely to, such a result. But it is to competent and well authenticated clinical records that we must now look for the solution of the important question at issue.

In the recent work on "Syphilis et Mariage" M. Alfred Fournier (for the purpose of justifying his statements that syphilis is but a temporary bar to marriage previously quoted), has presented a series of eighty-seven examples occurring in his own experience. In presenting the carefully tabulated report of these cases, he says: "For my part alone I have in my hands, to speak only of written facts, eighty-seven observations relative to syphilitic subjects, undoubtedly syphilitic who, having married, have never communicated to their wives the least suspicious phenomenon; and moreover, these eighty-seven have produced among them a total of 156 absolutely healthy children."

In examining the tabulated records of these eighty-seven cases (Fournier, "Syphilis et Mariage," page 231, et seq.) I found that thirty-six out of this number of men who were thus proven to be free from any power to transmit syphilis, either by direct contact or by heredity, were subjects of late or tertiary lesions, *after marriage*, some *before* and some *after* the birth of children.

These lesions comprise almost all the varieties of the sequelæ of syphilis, thus: gumma of the penis, palmar psoriasis, dry tubercular syphilide, gumma of velum palati, cerebral syphilis, papulo tubercular syphilide, costal periostitis, cerebro spinal symptoms, evidently of syphilitic origin, diplopia, passing attacks of hemiplegia, nasal syphilides, ecthyma of legs, syphilitic sarcocoele, nasal ulcers, ulcerative laryngitis, papulo-squamous, palmar and plantar syphilides, sclerous and ulcerative glossitis, papulo-scaly syphilides of circinate form, tubercular ulceration, syphilide of the nose, etc.

In examining the tables of M. Fournier still farther it was found that the average time of marriage after infection in the eighty-seven cases was 5 8-10 years. That twenty-five per cent. were married within *three* years after infection, and over ten per cent. within *two* years.

In regard to the length and quality of treatment over twelve per cent. of the eighty-seven cases had treatment of less than a year's duration. Several with only a few months, one with the iodide of potash, and another with no treatment at all.

In this enormous mass of evidence, consisting of eighty-seven authentic examples, involving prolonged observation of 230 individuals adduced by an acute, thorough and competent observer, we have, what I shall claim, as absolute proof, first, of the non-contagiousness of all lesions of the tertiary stage of syphilis; second, of the possibility of the spontaneous cure of the contagious stage of syphilis (that is, without treatment) as shown in one case where there was no treatment, and in another case where only the iodide of potash was used. Third, that the limit of the contagious stage in syphilis may then, it seems to me, be certainly fixed at a point within the period of five years.

For my own part I have never seen a case of syphilis presenting an undoubted lesion of the secondary or active stage after the termination of the second year. I have seen a goodly number of cases of recurring papular syphilide, especially upon the hands and feet, occasionally upon the body, two and three years after an infection which had been thoroughly and systematically treated from the beginning, and I have always considered them as due to damage done to the lymph channels during the previous active stage of syphilis. After the third year in three such cases, marriage has been entered into with my consent, and in two, recurrences in the *identical places formerly occupied* took place nearly a year after marriage, and in both these cases the wife remained uninfected, and healthy children were reported, one now three, and the other two years old. I have seen frequent lesions of the tongue from two to a dozen years after infection, the sequel of mucous patches occurring during the active stage, which had been asserted by physicians to be mucous patches and capable of transmitting syphilis. Sometimes these would present simply as pale pearly stains or whitish patches, sometimes erosions and ulcerations and cracks again appearing as irregular spots covered with a white or grayish pellicle or scales with more or less induration. Such lesions I have never known to prove a source of syphilis to others, although coming into most intimate contact, such as between man and wife, for a series of years during which also occasional recurrences had taken place. Hence I do not hesitate to state that such forms of trouble occurring as a sequel of syphilis three years or more after infection do not contain the contagious property of syphilis and hence cannot communicate it. I have also seen numerous cases and observed them during long periods in which marriage has taken place from three to twelve or more years after a syphilitic infection of the father, and I have never yet seen a single symptom of syphilis in the wife or in the children born of such marriage. In one case reported in my class-room lessons and in my work on "Syphilis and the Genito-Uninary Diseases" five years after such a marriage, mucous tubercles appeared in the wife who, moving in good society was also apparently above reproach. The husband had been three years treated by me for undoubted syphilis previous to marriage, but he had never been free from the fear that he should sometime communicate syphilis to his wife. He accepted her syphilis as a result of an infection from him, and I was almost compelled to the same conclusion although it stultified my belief and public teaching for the previous five years. By a fortuitous train of circumstances, I was, however, enabled to obtain a confession from her, of an accidental carnal contact with an old lover, who

subsequently wrote to her inquiring after her condition and stating that while apparently well, except a little erosion on his penis, he found afterwards that the erosion was a chancre and he feared he had given syphilis to her. In the reported cases where syphilis has been claimed to have been contracted from persons whose syphilis had its initiation three or more years previously, I believe that if the truth could be ascertained it would be shown that the disease was not contracted from such erosions, but always from a source less than three years from date of the infection. The overwhelming evidence as to the improbability of the communication of syphilis after the first three or four years from the date of infection as shown by the statistics of Mr. Fournier, should lead to the greatest opposition in accepting cases alleged where syphilis has been apparently acquired in contradiction of this position.

The traditions of syphilis claim that, once a man has had syphilis, he is a possible focus of contagion forever, and any evidence that a man has once had syphilis, is held as competent to prove that any syphilitic accident in his family, to the latest generation, may be reasonably credited to him. The important fact that syphilis may be and often is communicated through other sources is practically ignored. Syphilis from mediate contagion is common. It may be through the medium of a spoon, a pencil, a cane, a cigar, a kiss, the dentist's instruments. The accoucheur may acquire it through his finger. Nine cases of this sort I published some years since as occurring under my own observation, and I have seen several others since that time. Besides this, I have seen at least double that number of cases of syphilis, where no possible trace of the source of contagion could be ascertained.

A cup or any other article in common use, defiled with the secretions of a mouth, harboring a mucous patch, coming in contact with a crack, or abrasion of the lips of a healthy persons, may communicate syphilis through a resulting lesion, which shall pass away unnoticed. Any similar contact with the blood of a person in the active stage of syphilis, will communicate it. And yet, if the resulting syphilis is not distinctly traced to some one of these sources, the disease, where possible, is referred to some person who has been found to have had syphilis perhaps a quarter of a century before. It is the tradition based upon the everlasting contagious nature of the disease that the profession are wont to fall back upon to explain the mystery. Even M. Fournier is not free from the tyranny of this unproven tradition. Notwithstanding the convictions that his intelligent experience has imbued him with, that syphilis is not liable to be communicated by any person after the first three or four years from infection, and which impel him to state that *syphilis is but a temporary bar to marriage*, and which also make him say, "Yes, a hundred times yes, a man may marry after having had the pox, and the result of such a marriage, under these conditions, may be absolutely safe, medically speaking." Yet Fournier subsequently says: "We must still recognize some rare exceptions where the disease retains its contagious properties indefinitely. Such, for example," he says, "is the case of a patient whom I treated some time ago. This young man had been infected with a syphilis five years before, which one could fairly call mild, since the initial chancre was only followed by a roseola, a palmar syphilide of slight intensity, and a

sore throat. He treated it, almost from the beginning, sufficiently well: several times he submitted, under my advice, to a strong mercurialization (fifteen to twenty centigrammes of proto-iodide daily). Well, in spite of this treatment, and in spite of all my efforts, the patient (who, by the way, is a smoker, a circumstance essential to note) had not ceased to be affected, during a period of five years, with lingual syphilides almost continuously. I cured him of one breaking out: one or two months later a new one attacked the tongue. Then came a new treatment, followed by a new cure: then reappearance of the malady, and so on. To be brief, I always cured him, and it always began again, to use his own expression. Now that he has completely given up tobacco (at my earnest solicitation), the eruptions become less frequent, but have not altogether ceased; and quite lately I have again seen him with syphilis coming on the back part of his tongue. Now, what would have happened if, relying on the mild nature of his disease, and satisfied as to the treatment followed, I had allowed the patient to marry, between the two outbreaks of such symptoms? What would have happened, I need not predict theoretically, because I have had a practical demonstration. This young man took as a mistress last year, a woman who till then was perfectly healthy; exempt from every venereal symptom. Some weeks later he brought her to me, affected by an indurated labial chancre, manifestly received from the lingual syphilides of the patient."

This case, and as he does not allude to any other, presumably the only one in his own experience, is presented as a typical one, to illustrate the possible persistence of contagious lesions after many years, notwithstanding the disease is of mild form, and has been systematically, persistently and efficiently treated, "almost from the beginning."

Now, is such a conclusion sufficiently warranted by this evidence, on a matter of such moment? Let us look at other causes equally possible, equally probable. The young man did not take for a mistress a woman whose virtue was above suspicion. Such a coincidence, as the contact of such a woman's lip, with some other lip, with fresher syphilitic lesions, would not be so extraordinary, as the acquirement of syphilis, from a lingual lesion, five years after infection. Such a woman would be quite in line of coming in contact with persons having active syphilis, and, either directly or by mediate contagion, might have acquired her labial chancre, even if she had not become this man's mistress, without exciting special comment.

Let me place here in contrast to this, a case taken from my own carefully recorded experience. A young man had undoubted syphilis; from the first under my own observation, characteristic initial lesion, general gland enlargement, roseola; no pronounced papular eruption; mucous patches on tongue and inner surface of cheek. After a somewhat desultory treatment, of two years, he was apparently well. Remained free from all trouble for two years; then began to have ulceration at the side of tongue, pearl color at edges; characteristic appearance of the so-called chronic mucous patch; was greatly addicted to tobacco, tongue resisted local treatment, unless accompanied by exclusion of tobacco! repeated recurrences for nearly five years; not markedly affected by specific treatment, which was tried from time to time, when at last, he married a *virtuous* girl. Since that time, already four

years have elapsed, and the wife has not had a symptom of syphilis. As a result of this marriage, there is to-day, a perfectly healthy child three years old, and yet within the last month her husband has consulted me in regard to a recurrence of the ulceration at the border of the tongue.

M. Fournier ignores entirely the possibility of accounting for the initial lesion of the lip, in the rare case he quotes, in any way, except through the chronic lesion, which his history has shown, chiefly to be dependent, for the difficulty of cure, on the use of tobacco. This one case is placed squarely, as an offset against his eighty-seven cases, some of which were also cases of ulceration of the tongue, incontestably proved to be free from the contagious element of syphilis by the immunity from infection of the eighty-seven wives and one hundred and sixty-seven children involved. It is true he brings forward a few cases, in the practice of other physicians, to prove the possible inoculability of late lesions of syphilis. But these, in the face of such statistics as he has given us, are not to be accepted, while they all open to explanation, in various reasonable ways, independently of the claim of a contagious element, persisting for years from the date of infection. When we consider the important interests at stake in a just conclusion of this matter; when we realize that thousands of individuals acquire syphilis, every year, often innocently, and that very many of these are now doomed to suffer a life-long apprehension of communicating the disease, and this too, it is claimed without reason. We cannot admit in the adjudication of the point at issue any evidence unfavorable to such claims, without the strictest scrutiny, nor accept any but the most unequivocal facts. We cannot allow even such an authority as M. Fournier to force the acceptance, without question, of a conclusion, in such direct antagonism with the mass of positive and conclusive evidence that he has given us of the non-inoculability of the late lesions of syphilis. We are, I assume, fully warranted in claiming that the explanation of the only case, which he brings up directly, to prove that the late lesions of syphilis may, in exceptional cases, be contagious, undoubtedly lies in the acquirement of syphilis from a source quite independent of the seven years old lingual lesions to which he hastily attributed it. Eliminating this case, we have then a solid mass of evidence, which on any other point, would be absolutely conclusive.

In support of this, we have also, the failure of all experimental inoculations, with the secretions of the late lesions of syphilis, which have been failures *in every instance*. We have also the knowledge of some of the various occult ways in which syphilis may be acquired, defying absolute demonstration. In the absence of an old syphilis in the husband, we have various ways of accounting for repeated abortions, and pemphigoid infants, which rest upon the diagnosis of syphilis as a cause when such syphilitic history in the male parent might have been accepted, before we knew that the semen does not contain the contagium of syphilis. The great discussion in the Academy of Medicine in Paris, a résumé of which is given by Diday, (page 72 of his work on "Infantile Syphilis") shows that the conclusion was arrived at that "Pemphigus of the foetus, was not an immediate result of syphilis, but an indirect sequel of the exhaustion which this disease produces." Disease of the placenta caused by syphilis has been shown to result in the con-

tinuation of abortions and the production of diseased infants, independently of the continuance of any contagious element in the mother, and thus we may reasonably eliminate the contagium of syphilis, as a necessary factor in the production of such accidents.

With this presentation of some of the facts and arguments which may be brought to bear in favor of a positive limitation of the contagious stage of syphilis to three, and at farthest to four years, with or without treatment, I rest my case.

RECTAL DISEASE OF UTERINE ORIGIN.¹

BY C. P. STRONG, M.D.

NEARLY every subject in gynecology has been so thoroughly written about and illustrative cases so multiplied that it was with surprise I noted the lack of definite and detailed information regarding the causative influence of uterine disorders in cases of rectal disease. The literature of the subject, as found in the larger text-books is but the most general statements, while in monographs it is treated more from the point of view of the rectal specialist than the gynecologist. It being my fortune to have several patients whose symptoms were mainly or entirely rectal, I sought to find some classification of symptoms or of lesions recorded, that would aid me in determining just how far I was justified in attributing their condition to uterine disease. My search was so unproductive that I have been encouraged to contribute the results of my personal observations and a few illustrative cases with the hope of adding, although it be but little, to the general knowledge.

There are certain symptoms that suggest at once rectal examination; constipation alternating with diarrhoea, frequent and ineffectual attempts at defecation, painful defecation, hæmorrhage, muco-purulent discharges, sensations of fullness in the rectum with general reflex disturbances of the digestive tract; and when one finds, associated with these symptoms, the lesions, internal hæmorrhoids, subacute and chronic proctitis accompanied usually with fissure of anus and often with ulceration of the rectal mucous-membrane, certainly an investigation of the uterus and its adnuxa is demanded. I do not intend to imply that these conditions necessarily mean uterine disease, for there is a vast number of other sources from which they may arise, but rather, that when uterine disease does cause rectal trouble, these are the symptoms, and these lesions present, and that cure will be more certain and speedy, and relapses less frequent, proportionally to the attention paid to the uterine complication. Certain conclusions, too, I formed as to the nature of the uterine disorders that would produce these lesions: first, uterine malpositions, especially retroflexion when adherent, and retroversion, prolapse associated with laxity of the vaginal walls; second, periuterine cellulitis, most markedly when retrouterine and hardening and fixing the anterior rectal wall.

There are several reasons for the disturbing effect of uterine malposition upon the rectum, but the important one is the disturbance of the venous circulation in the lower part of the bowel, which has both a mechanical and physiological explanation. The internal hæmorrhoidal veins, valveless and thin-walled, as

¹ Read before the Obstetrical Society of Boston, December 12, 1885.