Clinical Lecture

ON

CHOLERA AND ITS TREATMENT.

Delivered October 8th, 1866.

BY

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I AVAIL myself of the very earliest opportunity to give you the results of my experience of cholera during the present epidemic. Our experience here within the walls of the hospital has been very limited. We have had only twenty cases of actual cholera, and twelve of these have been fatal cases.* A mortality of sixty per cent. appears sufficiently unsatisfactory. I scarcely need tell you, that our cases have been too few to afford a trustworthy average of the results of treatment. When the numbers are small, the accidental concurrence of a few intractable cases gives an entirely fallacious result.

We had two large wards ready for the reception of cholera patients; and we were prepared to put our plan of treatment fairly to the test in a large number of cases, but the opportunity has not now been afforded us. We have had very few cases, and most of them have been beyond the reach of any remedial treatment.

With regard to the question of the treatment of cholera, we have advanced beyond the point at which mere statistics can render us much assistance. Our knowledge of the natural history of cholera is now so far complete that we have the key which will unlock most of the pathological intricacies of the disease. We have freed ourselves from that delusive and mischievous theory which attributes collapse to the mere drain of fluid from the blood. That death may occur in spite of the discharges, nay that it may directly result from the profuse discharges excited by the amount or the virulence of the morbid poison, we have never doubted or denied; but, nevertheless, we know that a continuance of the discharges from the alimentary canal is an essential condition for recovery from collapse; while an entire cessation of these discharges during collapse is a fatal sign. Death may occur in collapse without discharges; recovery never. It is a mere mockery to say that in cases of *cholera sicca* the morbid secretions are found in the digestive canal. The amount thus collected is far too small to cause death by exhaustion; and we know that the symptoms of collapse are not such as a drain of liquid from the blood in any quantity would occasion.

I maintain, then, that our knowledge of the natural history of cholera and of the natural process of cure, enables us to judge of the influence of treatment in each case; that a few cases carefully observed are far more instructive than a large num-

ber imperfectly noted, and that our failures may be as instructive as our successes.

The careful study of our twenty cases, with twelve deaths, has not in the slightest degree diminished, but rather tended to confirm, my conviction that, as the natural cure of cholera in all its stages occurs by a process of elimination, so the business of the physician is to avoid all such means as tend to impede that process, and to assist it, when necessary, by all available means. I shall presently show you in what way death occurred in the twelve fatal cases. But, first, I have to say of the cases of recovery, that they were all in collapse of varying degrees of severity at the time of admission. All were treated by castor oil. In each case, the vomiting and purging continued until reaction occurred; then the discharges gradually ceased; and convalescence was quickly established. There were besides some cases of choleraic diarrhoea in the wards, which quickly recovered.

To return now to our twelve fatal cases. Of these twelve cases, six died during the stage of collapse, six during the period of reaction. All were treated by repeated doses of castor oil. One had two doses of croton oil, and one took two doses of calomel; and we have to inquire whether there is any reason to suppose that this evacuant treatment was injurious. Now, in the first place, it is remarkable that, in four out of the six cases fatal during collapse, there was neither vomiting nor purging after the patients were admitted; and, in two of these four cases, death occurred within an hour after admission, and after a single dose only of castor oil had been given. In these four cases, it is manifest that our treatment was evacuant in intention and not in effect. It was simply ineffectual, and could not be considered in any way injurious.

The fifth case of death during collapse occurred in a very feeble woman, aged 62, and having the look of being 70. She had been living badly, and had been profusely purged for three days before her admission. She was in collapse when admitted. She was ordered to have castor oil at first every three hours, then every four hours. At length, as the purging went on very actively, the oil was discontinued. She died in collapse, sixty-three hours after her admission. Looking back at this case, and considering the amount of purging which was going on spontaneously, I think it would have been well if no oil had been given, or at the most only an occasional dose at long intervals. It happened that I did not see her until the second day after her admission, and then I discontinued the oil. When the discharges from the intestinal canal are profuse, and when there is no evidence of accumulation within the bowel, no evacuant medicine is required, and therefore none should be given. I am sure, however, that no treatment could have saved this poor woman. She was very aged; and she was greatly enfeebled by a poor diet and by three days of profuse purging before she fell into collapse. In such a case there was no ground for a hopeful prognosis.

The sixth and last case of death during collapse occurred in a man, 35 years of age, who had been purged from ten to twenty times daily for three days before his admission on Sept. 16th. When admitted, he was in full collapse; pulse extremely feeble. Temperature 94½°. The face was blue, and the eyes sunken. He was ordered to have the oil at

* Another fatal case has occurred since this lecture was given. The history of that case will be found in a postscript.

first every two hours, then every three hours. Several doses were vomited, but it brought away a moderate amount of rice-water secretion. His face became less dusky, and for a time he felt relieved; but he relapsed into profound collapse, and died twenty-seven hours after admission.

Now, with respect to this case, I would remark, that the most unfavourable circumstance was the fact that he had been profusely purged for three days before his admission. This must have weakened him, and rendered him less able to bear up against the disease when he fell into collapse. The same source of exhaustion had occurred in the case of the old woman last mentioned.

I have sometimes been asked how it happens, if the vomiting and purging be a conservative eliminative process, that a patient falls into collapse after having been profusely purged for several days. This question is a reasonable one, and it is not difficult to answer. In the first place, the transition from prolonged diarrhoea to collapse does not occur in the case of patients who from the first are treated within the hospital. I have not yet seen one instance of the transition from choleraic diarrhoea to collapse under the evacuant treatment in the wards of the hospital, although many of the cases in 1854 appeared to be on the very verge of collapse when they were admitted. The continuance of a diarrhoea day after day in the cases under consideration is probably to be explained by the fact that the patient is continually exposed to the exciting cause of the disease, either in his home or in his place of labour. He daily breathes the same poisoned air or drinks the same poisoned water, and thus the symptoms are perpetuated by renewed doses of the poison, just as in those notorious cases of slow poisoning by arsenic or antimony mixed with the food. At length, when the accumulative effect of the poison has induced collapse, the patient, exhausted by his prolonged diarrhoea, has less power to combat the disease, than he would have had if a single dose of the poison had sent him at once into collapse. The only safeguard for a patient in whom a diarrhoea is kept up by a continual recurrence of the exciting cause, is to discover and to avoid the cause, whether in the air, or food, or drink. To give astringent and repressive drugs, while the patient is constantly inhaling or imbibing the poison, is only to increase the peril. It is like closing the safety-valve of the boiler while the furnace is fiercely burning below. With such opposing forces in continual operation, it is manifest that sooner or later an explosion must occur.

Passing on now to the six cases of death during the reaction stage.

The first case was that of an infant 15 months old. The child rallied from extreme collapse; then quickly became drowsy, with hurried breathing; then convulsions occurred, and death forty-eight hours from the commencement of the illness. Such a result in a child of that age is very common, and very difficult to avert.

The second case occurred in a woman, aged 47, who had for years been a great invalid. For seventeen hours before admission there had been profuse vomiting and purging. When admitted, she was pulseless. Temperature $94\frac{1}{2}^{\circ}$. Vomiting and purging had ceased; but the bowels became painfully distended, and it was with great difficulty that any evacuation could be obtained. First, an enema

brought away a very offensive rice-water stool, after four doses of castor oil had been given without effect. As the bowels were becoming more distended, we gave two doses of croton oil, two minims for a dose, combined with castor oil. The result was seven scanty stools, with some relief; but the bowels remained distended, and during the last seven hours of her life there was no further purging. The last stools were bilious, and the temperature rose to $98\frac{1}{2}$; but there was an evident want of rallying power. The respiration became gasping; and she died twenty-six hours after her admission.

The next three cases were sisters, aged respectively 5, 6, and 9. The parents had come from the country two years ago, and lived in a dirty court in Newport Market, and the whole family had been ill-fed. In the youngest child, the collapse was moderate in degree; in the two elder girls, there was extreme collapse with pulselessness for many hours. In all three cases, the purging was very moderate in amount and frequency. All three rallied from collapse, and the stools became bilious; and they all passed into the same condition. There was drowsiness, grinding of the teeth, with hurried breathing. The youngest died thirty-six hours after admission, the second in sixty-four hours, and the third in forty-eight hours.

In the two youngest there was extreme fatty degeneration of the kidneys. I have not for a long time seen so much oil in the kidneys. The glands were white and wax-like, and the degeneration was evidently of long standing. In the youngest there were, in addition, patches of local fatty degeneration in the liver; and the lungs were much gorged. Reaction had occurred, and there was bile in the intestines in each case. In the eldest child, the kidneys were healthy; but many of the mesenteric glands were considerably enlarged.

In these three cases, it is evident that the poor children, being ill-fed and strumous, had not the requisite rallying power to carry them through the disease.

Our last case was an equally unsatisfactory subject—a young man, aged 23, of weak intellect, who had a chronic ulcer on his leg in a most offensive state. He had no regular occupation. He lived in a lodging in Clements Lane; and was dependent on his parents for supplies of food. His mother said that he got meat once a week, and sometimes it might be twice. He was in extreme collapse for many hours; and repeated doses of castor oil excited only a very moderate purging. We made an unsuccessful attempt to inject hot water into his veins. He made an imperfect rally, and died twenty-four hours after his admission.

These are the six cases of death after reaction. An infant, 15 months old; a sickly woman, who had been profusely purged before she came in; three half-starved, strumous children; and an ill-fed, unhealthy young man. It would appear as if the choleraic influence in this part of London, being very feeble, had seized chiefly upon those whose weakness and unsoundness had rendered them least able to resist the poison.

Now, gentlemen, I am not here to apologise for the deaths of these twelve patients. My object is to place before you the facts upon which you may base your own judgment. Is there any reason to suppose that our treatment was injurious? Would a different,

would an opposite mode of treatment, would a let-alone treatment, have been attended with more favourable results? I think not; but let each man form his own opinion after a careful weighing of the evidence.

With regard to this question of the treatment of cholera, we stand upon very firm ground. We maintain that no drug has any direct antidotal or curative influence in cholera; that there is only one method of cure for the disease in all its stages, and that is the method by which Nature herself accomplishes the cure. The most that art can do is to assist and, if need be, to regulate the natural eliminative process.

I will not here stop to inquire whether there is an eliminative effort in cholera. The denial of this doctrine appears to me to involve merely a discussion about words and terms. There are, at any rate, foul morbid secretions which in cases of recovery are cast out of the bowel, and which are poisonous to others. This fact must suffice for plain practical men, whose business is not that of hair-splitting.

The mortality from cholera will vary greatly at different times and in different localities. At one time the deaths may be eighty per cent., at another twenty per cent.; but be the recoveries few or be they many, and whatever may be the method of treatment employed, it is indisputably true that the actual cure is effected by the same natural eliminative process in every case. I have asked for a detailed history of any case of recovery from collapse in which there has not been a continuance, more or less, of discharges from the alimentary canal while reaction was taking place. I believe that no such case has ever been seen; certainly, none has been recorded. Yet, if collapse were caused by loss of fluid, an arrest of the discharges must necessarily precede recovery from collapse in every instance. Happily, there are now few pathologists who defend the theory, that collapse is a result of the fluid drain from the blood. It is now very generally admitted, that the facts of the disease are utterly inconsistent with that theory; and with the abandonment of this theory, there is of necessity implied the condemnation of the brandy and opium treatment of collapse.

But now we are told by some that the best that we can do for a patient in collapse is nothing, but to let him lie in bed, drink cold water, and recover if he can. I have seen something of the treatment of cholera by coloured water. I have seen patients thus treated lying in full collapse, having had neither vomiting nor purging for hours, and suffering agonising pain from over-distension of the bowels. It was evident that all the natural expulsive efforts had ceased, and that without some artificial aid death was inevitable. It appears to me that to look on at a patient in such mortal agony, and to make no attempt to relieve him, is as unjustifiable and as cruel as it would be to leave a patient with retention of urine unrelieved. This routine system of *laissez-faire*, when there is such an obvious indication for treatment as distention of the bowels by offensive morbid secretions, is an abuse of the so-called "expectant method", which threatens to extend itself to the treatment of more than one acute disease. A routine system of doing nothing in acute diseases is as unscientific and, it may be, as mischievous as a routine abuse of active drugs.

The advocates of the do-nothing system in cho-

lera, to be consistent, should adopt this method from the commencement and in all stages of the disease. It is thoroughly inconsistent and unreasonable to treat choleraic diarrhoea by opiates and astringents, to arrest the discharges, thus induce collapse, and then say to the unhappy victim, Now you shall be left alone to recover—if you can. This is like pushing a man from a shallow into deep water and telling him to swim for his life. He who did this would certainly not deserve the medal of the Royal Humane Society. This combination of a constringing practice in choleraic diarrhoea with an expectant treatment in collapse is like repressing the rash of scarlatina by exposure to cold, and then treating the renal disease and the dropsy which are thus provoked by rest in bed, and nothing more. The *vis medicatrix* may suffice to cure the patient, but he will owe small thanks to his doctor.

There are some cases of cholera of so malignant a type, that no treatment can be of any avail—they are obviously hopeless; there are others of so mild a character that they will recover by the unaided efforts of Nature. Then there is an intermediate class of doubtful cases in which the result may be much influenced by the treatment employed; and it is only in the management of cases of this kind that the actual effect of treatment can be determined. Mere statistics are quite useless; they give only the deaths and recoveries; but what we have to learn is the actual influence of treatment upon the progress of the disease and upon the mortality. This knowledge we can acquire only by the careful observation of each case that comes before us.

If we cannot ascertain the real influence of treatment on the progress of this disease by the careful observation of each individual case, we shall never arrive at this knowledge by the careless noting of a large number of cases. What would a chemist say to the proposal to determine the ultimate composition of a substance by taking the average results of a large number of incomplete and inaccurate analyses? The attempt to ascertain the influence of treatment in cases of cholera by a mere statistical statement of results is in no degree less absurd than would be such a proposal. We ought to be able to determine, and I believe that we can determine, with some degree of probability in each case, whether our treatment has been beneficial or the contrary, or whether it has been simply inefficient.

I am quite sure that I saved at least one life during the present epidemic.

At the beginning of August, I attended with Dr. Halse a youth aged 15, who had been only a few days in London, and who had drunk water from one of the pumps in the Temple. He had diarrhoea, which was stopped by two doses of opium. Then, after a few hours' cessation of diarrhoea, he was seized with cramps and purging, and he rapidly passed into collapse. When I saw him, his pulse could scarcely be felt; his eyes were sunk; his skin and tongue were icy cold; the cramps very severe. There had been no purging for a considerable period, yet his intestines were full of liquid. There was evidently no natural effort to expel the contents of the bowel; and I believe that, under a merely expectant treatment, he would have died with distended bowels. He got about eight half-ounce doses of castor oil in twenty-four hours. The bowels acted freely, the discharges being at first foetid rice-water, then bilious.

He rallied; had slight drowsiness for a few hours; then an abundant rash of roseola; but he made steady progress towards complete recovery. I consider this life to have been as unquestionably saved by the treatment, as if we had pulled him, in a state of unconsciousness, out of deep water.

Henceforth no reports of the results of treatment in cases of cholera will be of any value which do not give some particulars as to the character of the cases, the age and general condition and habits of the patients, the degree and the duration of the collapse, and the actual operation of the remedies employed. In my work on *Epidemic Diarrhœa and Cholera*, published in 1855, I have given all these particulars respecting fifty-four cases which were under my care during the last epidemic. I take this opportunity of correcting a misstatement which has frequently been made, to the effect that the evacuant treatment was tried here in 1854 only towards the close of the epidemic. The first case thus treated was admitted on the 10th of August; and the same plan of treatment was continued during the whole period of the epidemic, which reached its height in the early part of September.

To tell us that the evacuant treatment of cholera has failed in any set of cases is worse than useless, unless we are informed whether the cases were such as afforded hope of benefit from treatment of any kind; also whether medicines were given in such a way as to ensure their evacuant action; and what other means were employed at the same time. During the height of an epidemic, it would be easy to select twenty cases or more, which, under any plan of treatment, would certainly give a mortality of 100 per cent. Some time since I saw, in one of the hospitals, a very old woman, pulseless, voiceless, and toothless. She was evidently dying; but she had been ordered to have a dose of castor-oil on admission, to be repeated in four hours. It was certain that she would not live to take the second dose; but the case would probably be returned as one unsuccessfully treated by castor-oil. I am told that, in one hospital, a considerable number of cases were treated by castor-oil, but with the addition of brandy and hot baths. Now, according to my experience, this would not be a fair trial of the evacuant method. I am sure that brandy, even in small doses, is injurious during collapse; and I believe that warm baths, in spite of the great temporary relief which they may sometimes afford, are, on the whole, rather hurtful than wholesome.

Dr. McCloy has given in the *Lancet* (Aug. 18th) some interesting particulars of cases of cholera treated in the Liverpool Parish Infirmary. He has compared the results of what he calls the "eliminative" treatment, chiefly by means of castor-oil, with other methods of treatment. He declares that "*the eliminative treatment has been most successful*." It has been a success which those only who have seen and compared the relative severity of the cases can appreciate—a success which statistics cannot show." Out of fifty cases thus treated, there were seventeen deaths; and of these seventeen, "nine were cases in which there was no radial pulsation, and in which neither emesis nor purgation could be produced." I learn that this method of treatment has since been continued in the same institution with equal success, and that at one dispensary in Liverpool about two thousand cases of diarrhœa have been treated on the

evacuant plan with uniform success. I hear, too, that many private practitioners, in various parts of the country, have had most favourable experience of the plan. A highly intelligent gentleman in large practice, who had been very sceptical about the evacuant treatment of diarrhœa, was induced to give the plan a trial. I met him a few days since, and he was enthusiastic about its success. He said to me: "Well, you have established one grand principle; that is, in the treatment of diarrhœa, to free the bowels from morbid secretions before you lock them up by astringents." My reply was: "Common sense would have taught us this, if we had not permitted ourselves to be blinded by an erroneous theory."

Amongst the out-patients of the hospital, many hundred cases of diarrhœa have been successfully treated during the last few weeks, and in nearly all of them a single dose of castor-oil has been the only medicine given.

I should detain you for another hour, if I were to narrate to you my recent experience of the ill results of the abuse of opiates and astringents in the treatment of diarrhœa and cholera. I trust that before long practitioners who have had experience of the evacuant method of treatment will publish the results. I find, however, that some men, whose experience has been most favourable to the plan, are unwilling publicly to face the tide of prejudice which sets in against this safe and simple method of treatment—prejudice as unreasonable and as violent as that which, two hundred years ago, opposed the admission of cool and fresh air into the rooms of small-pox patients. The simplicity of the treatment appears to the minds of some men to be its great fault; they believe in no remedy which does not, as they suppose, exert some specific curative influence. It has been said by some that, if castor-oil have any good effect in choleraic cases, this is due, not to its evacuant action, but to some "alterative" influence on the mucous membrane. A facetious friend remarked to me, that many practitioners, who are now sceptical, would believe in castor-oil, if they could be assured that it exerts a peculiar "castor-oilative" influence.

POSTSCRIPT. Since this lecture was given, another fatal case of cholera has occurred in the hospital. A woman, L. B., aged 48, was admitted on October 12th. She was a widow, and had worked very hard to support her seven children. She had been ailing for a fortnight. For five days there had been very frequent vomiting and purging; but there had been no action of the bowels since 1 P.M. on the day of her admission. She was admitted at 6 P.M. She was in extreme collapse, pulseless, and cold; temperature 96½; features much sunk. There was great dyspnoea, and severe pain, at each inspiration, over the bottom of the sternum. When I saw her at 8 P.M., I found that four half-ounce doses of oil had been given; these had excited neither vomiting nor purging. There was evidence of a moderate amount of liquid in the intestines. She was still pulseless; the dyspnoea very urgent; and each inspiration was abruptly stopped by a sharp pain, apparently having its seat in the over-distended right cavities of the heart. After watching her for a time, I felt sure that to assist the circulation by hot injection into the veins afforded the only hope of rescuing her; and accordingly we slowly injected

forty-two ounces of water, at 118°. During the injection, the pulse became perceptible, the temperature rose to 98°, the pain over the heart and the dyspnoea somewhat diminished; but the relief was much less than I had hoped for, and I suggested that probably a clot had formed in the right side of the heart. She vomited several times after the injection. No more medicine was given; and there was no action of the bowels from the time of her admission until her death, which occurred at 4 A.M. on the following day—i. e., ten hours after her admission.

Shortly before her death, there had been some appearance of improvement in her condition; and, on examination *post mortem*, there was evidence that reaction had just commenced. The evidence was great engorgement of the lungs, such as we have not seen in any case after death in the stage of collapse; and the upper part of the small intestines, for a length of about eighteen inches, contained bright yellow bile. The secretion of bile, then, had recommenced just before death. The small intestines below contained a considerable amount of the usual choleraic secretions, but tinged of a dirty pink colour by admixture with blood which had escaped from the abraded mucous membrane. The stomach contained a small quantity of the castor-oil, but none had passed on into the bowel. In the right ventricle of the heart there was a firm decolorised clot attached near the apex of the heart; it was of considerable length, and extended in a tapering form about four inches into the pulmonary artery. This clot had evidently formed during life, and probably before we injected the vein; it must have obstructed the circulation, not only by the space which it occupied within the artery, but by preventing the closure of the semilunar valves. Hence, probably, the painful distension of the right side of the heart, and the small amount of relief which the hot injection afforded.

Our treatment in this case was thoroughly inefficient. Would any other treatment have been more efficacious? I know of none that would. It was evident after death that, if she had completely rallied from collapse, she would have died exhausted by hæmorrhage from the bowels.

THE CLIMATE OF PODOLIA is probably the healthiest in the world. During the last five years 354 persons have died at from 95 to 110 years of age, 320 of whom were under 100 years old, 27 between 100 and 105, and seven between 105 and 110. Of these 141 were women.

DEATH OF DR. CONQUEST. The death is reported of Dr. Conquest, at Shooter's Hill. The deceased, who was 77 years of age, was formerly lecturer on midwifery at St. Bartholomew's Hospital. He wrote a pamphlet on *The Use and Abuse of Money*, the publication of which led to his giving a prize of a hundred guineas for the best essay on the subject. This resulted in the publication of *Mammon*, by the Rev. Dr. Harris, a work which excited some interest and obtained a large sale. He also published a work entitled *Outlines of Midwifery*, which has passed through six editions in this country, and has been translated into the French, German, and Hindustanee languages. Dr. Conquest was also the author of a revised edition of the Bible, generally known as *The Bible with 20,000 Emendations*.

Original Communications.

TWO CASES OF FURUNCULAR ACNE IN WHICH THE URINE CONTAINED AN INORDINATE QUANTITY OF UREA, "AZOTURIA."

By THOMAS BALMAN, M.D., Liverpool.

In the first edition of his work on *Stomach and Renal Diseases*, published in 1821, Dr. Prout has directed attention to a peculiar condition of the urine characterised by a superabundance of urea, the quantity of water and other ingredients being unaltered. Subsequently, Willis mentioned a similar disorder, to which he, I think not inappropriately, has given the title of azoturia. (*Urinary Diseases and their Treatment*. By Robert Willis, London, 1838.) As the independent existence of this malady has since been called in question by some more recent authorities, and believing that it is of no unfrequent occurrence and entitled to a more prominent position in our nosology than it has yet obtained, I have ventured to bring before the members of the British Medical Association the two following cases, which I take to be examples of the disease in question. Both of the cases which I shall presently read to you, and in which this condition of the urine was observed, were suffering from a somewhat severe cutaneous affection, "follicular acne."

Dermatologists have described several forms of acne. Acne simplex, though oftentimes of trifling importance, is known to be of very frequent occurrence, especially in young and phlegmatic persons of both sexes, and is generally amenable to treatment. The cases in which I have noticed ureous urine have, however, seemed to belong to the third species of Willan's classification, viz., Acne indurata; though it has a strong resemblance to, and is doubtless often taken for, the next species, acne rosacea. The several forms of acne, originating as they all unquestionably do in the sebaceous follicles of the skin, I agree with Mr. E. Wilson might severally be classed under two heads: Acne simplex, and acne rosacea; the first three species, acne simplex, acne punctata, and acne indurata, being mere modifications of the same form of disease. Acne rosacea, however, independent of the erythematous rash, which is its main characteristic, often leads to a dilated and tortuous condition of the superficial capillary bloodvessels of the face, is generally met with in persons of more advanced ages, and is more frequently the result of intemperance, when not of hereditary origin, than the cases I am about to mention. Acne furunculosa would convey a more exact idea of the anatomical element of the eruption, and as the term is not altogether new in connexion with the disease, I propose to retain it in this place.

The different forms of acne, affecting as they invariably do the face, are known to be sometimes of an extremely obstinate character, resisting for years the most skilful treatment even of some of our most eminent physicians. The disfigurement these eruptions often occasion, and the mistaken and erroneous notions commonly entertained as to their origin, are a perpetual source of annoyance to females especially, who are ready to make any sacrifice of personal comfort that may seem to hold out the slightest prospect of cure. These complaints, therefore, should have the best attention of the medical practitioner.

The first case occurred in 1852, and the following