A Clinical Lecture

ON

GASTRIC AND DUODENAL ULCER.

DELIVERED AT THE LONDON HOSPITAL,
FEBRUARY, 1910.

BY

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GENTLEMEN,—I have elsewhere given at length what I believe to be the correct pathogeny of gastric ulcer. The word "pathogeny," I may say, means simply the pathological origin, as it were, of gastric ulcer; it is just a mixture of pathological physiology and the resultant pathological anatomy; it is the connecting link between the two. I have there stated my belief that in all cases, as met with in ordinary practice, a gastric or duodenal ulcer arises originally as a tiny focus of inflammatory necrosis in one of the numerous, practically innumerable, small collections of adenoid tissue met with in immediate proximity to the epithelial cells in the stomach and duodenum. I told you that I based my belief on, first, the very simple observations, made to fit no preconceived theory, by Dr. Beaumont on St. Martin's stomach; and, secondly, on the microscopical work of Dr. Charles Miller, carried out on stomachs in our pathological department, and given in detail, with drawings of actual specimens, in the BRITISH MEDICAL JOURNAL, volume ii for 1906. I then proceeded to show how Dr. Miller's explanation would fully explain every single one of the clinical and *post-mortem* findings hitherto recorded in connexion with gastric and duodenal ulcer.

I have said, "in all cases met with in ordinary practice." This wants a little explanation. In the first place, it would be an injustice to omit entirely the experimental work that has been done on gastric ulcer, proving that emboli and poisons which cause small patches of local death in the mucous membrane of the stomach are certainly capable of producing the *post-mortem* figure which belongs to the acute peptic ulcer; the clean punched-out "steppy" appearance, an appearance which is in fact produced by the digestion of the embolized spot, or at least disintegration aided by digestive processes. I am not quite sure that it is a question purely of digestion of a piece of dead meat in the stomach, or whether there may not be also some active living inflammatory processes going on as well; these are, at any rate, manifest enough when the ulcer has lasted some time. Dr. Turnbull, the Director of our pathological institute, informs me that he has seen these acute ulcers in cases of septic death, ulcerative endocarditis, etc., due, in his opinion, entirely to minute septic emboli, and he gave me on Monday this particular reference:

P. M. 115, 1910, February 1st. A little child, female, aged 3½, a deep, subacute, duodenal ulcer, ½ in. in diameter, with shelving border touching the pylorus on the posterior surface. The case was one of an accidental death from gangrene and purulent ulceration of the right knee, that is to say the right knee was damaged by external violence, and the child died of septic trouble arising from wound infection, the small septic emboli invading the gastric arteries.

That is a case in point indicating how emboli can cause these ulcers. So I have used the words "in ordinary cases"; for though we must recognize that an embolic origin is a possible one, yet I would urge that in the ordinary case of gastric or duodenal ulcer we have generally no evidence that there is any infective endocarditis, or, indeed, any other source from which emboli could arise. But we do have septic matter, whether bits of ordinary food (which are certainly not aseptic) or small doses of pus from the gums, etc., which, as well as the mechanically hard portions of food, are quite capable of setting up the local focus of inflammation which my theory demands. The results are the same whichever cause is at work; there may be an embolus which blocks an artery and so destroys the part by cutting off its blood supply, or, in the more ordinary way, the *materies morbi* comes into the stomach and gets at the part and inflames it by direct contact instead of killing it by the embolic method. The difference is merely in the process.

I feel obliged to labour this point of the origin of the ulcers, because it is a crucial one in my argument, which, briefly stated, runs as follows, namely: That gastric or duodenal ulcer is what we commonly term a diathesis, that is, a natural proneness of the body to a given disease, and that the actual visible ulcer itself is an acute, unusually virulent attack of the disease or outbreak of the diathesis, just as an acute attack of gout or rheumatism is an acute outbreak or attack of gout or rheumatism in a person prone to those diseases. And just as we are ignorant of why one man is more prone than another to gout—we know a good deal about what will induce an attack in one so predisposed—so we are ignorant of why one person, or indeed one sex, should be so prone to an attack of gastric ulcer, or why the duodenal form, primarily at least, should be practically the only form seen in male subjects. (In ten years, 1899–1908, there were 113 duodenal ulcers admitted to the hospital, and of these 97 were in males.) Those of you who were with me in the *post-mortem* room a week or two ago will remember a man with duodenal ulcer who had five or six gastric ones also. The well recognized liability of some people to slight or even severe attacks of tonsillitis is a highly suggestive fact in my favour, and certainly such liability does not cease on removal of portions of the tonsil. There are plenty of analogous diatheses also which, if not supporting my views, at any rate prevent them from being considered ridiculous or far-fetched. I think they strongly support my view, namely, that this tendency to gastric ulcer is definitely a diathesis, attacking some people, or hereditary in some people, and not present, or very slightly so, in others.

We must now pass on to consider what, in my opinion, is the most important point about gastric and duodenal ulcer, if not, indeed, the only question which the patients care about—namely, whether the treatment should be by medicine or by surgery. Last week, you will remember that in order to clear the ground I gave you a complete, full, and detailed account of what medical treatment in my practice means—that is to say, my own methods of treatment.

There can be no question but that certain *results of ulcer* must be treated by surgical means, but it is to my mind equally certain that the diathesis—and, indeed, a simple acute attack of the disease—can be, and indeed must be, treated by the physician; and it is mainly to oppose what I consider the exaggerated claims of surgery recently put forward by Mr. Moynihan that I have taken up your time to-day with the subject. I listened to Mr. Moynihan's paper, and I subsequently read it in print, and also his article on appendix dyspepsia; and whilst I am bound to agree with a great deal of what he has said and written, I feel obliged to protest on general grounds and on experience against his main conclusion, which it is no exaggeration to say amount to this—anyone who suffers from the symptoms of *painful indigestion* should have the abdomen opened. I do not honestly think I am doing him any injustice, judged by his own words, when I say that that is his meaning. If you have painful indigestion you have an ulcer, and if you have an ulcer it ought to be dealt with surgically. I certainly do not agree with that opinion. Equally emphatically must I protest against the decision of my colleague, Dr. Robert Hutchison, who in the discussion admitted in so many words—and I have it from him directly also—a duodenal ulcer diagnosed with certainty must be treated surgically. That is a statement which I for one cannot admit to be correct in any way or shape, and I am going to speak against it and act against it to the utmost of my power. In order to support my protest by stronger means than my own personal instincts and experience I appealed to the registrars and to our pathological department for information with statistical details of the disease as seen in our wards and *post-mortem* rooms over a series of years; and I would like here to express my thanks to Dr. Zundel and Dr. Turnbull for their trouble in the matter.

Taking first the statistics of the wards, I find that for the ten years 1899–1908 we have had in the hospital for treatment a total of 1,255 cases of gastric and duodenal ulcer, out of which there have been 113 duodenal, the remaining 1,142 being gastric. These patients arrived in the hospital as follows: 757 of them came in on the medical side, and of those 757, 141 were transferred to the surgeons

to deal with. The surgeons had 357 originally admitted under their care *plus* 141 transferred to them from the physicians. I maintain that those figures, though not very large in themselves, and certainly not large enough to completely satisfy the statistician, are certainly quite large enough to give us important clinical information. Subtracting 141 from 757 leaves us 616 which were treated entirely by medicine. What happened to those 616? (I may note in passing that through some arithmetical error they are reduced to 606.) Out of 606, then, 57 died, a percentage of 9.4.

I am not concerned, just at this moment, when talking of the distinction between medical and surgical treatment, with the precise cause of death; for, admitting the fact of death, whether they died of exhaustion simply or from vomiting, or from hæmorrhage, or from perforation, is more or less immaterial. Now in indicating the fate of the remainder—those who did not die—our residents have three categories wherein to put them on discharge—relieved, unrelieved, and cured. I have no doubt that putting the term “cured” to a case of gastric ulcer rather represents the enthusiasm of youth than a proof of the occurrence of a complete healing of the gut; while “relieved” refers to the more cautious attitude of expectancy adopted by a man who thinks himself more conscientious. I am not very much concerned with the C’s and R’s, and I have lumped them together, and I find that not one of those 549 left the hospital unrelieved. I am bound to admit—and it may be considered an important admission, but I do not think it is from my present point of view—that the 141 who were transferred to the surgeon might have died; that is a possibility, and indeed a probability, had nothing been done under the circumstance indicating operation. Assuming, as we have a right to assume, that “relieved” means that the symptoms of pain and vomiting had disappeared (when it comes to a question of accurate diagnosis of what is ulcer and what is indigestion, the line is very indistinct, but vomiting and pain are certainly suggestive of ulcer, but I am not concerned to traverse the diagnosis of my colleagues, who in all these cases believed that ulcer was present), we have 549 out of 606, or in other words 90.5 per cent., which were cured of their symptoms. Notice I do not use the term “cured of their diathesis” nor “cured of their ulcers.” I say it is impossible to be certain that the ulcer has healed simply because the symptoms ceased. (What the meaning of that cessation of symptoms is I am not clear in my own mind, though I throw out one possible suggestion—namely, that the mucous membrane gets thrown into folds, and that a fold covers in an ulcerated surface, and therefore no fluid or anything else can come in direct contact with the ulcer.) All the same, there is some fairly strong evidence from the *post-mortem* room that these people may be cured of their ulcers, for scars of old ulcers in the stomach and duodenum are met with with sufficient frequency to warrant the statement that ulcers do frequently heal under non-operative conditions—that is to say, under medical treatment. We have a number of cases in the *post-mortem* room in which scars are found and in which it is perfectly certain that operation was not undertaken. Dr. Turnbull furnishes me with the following statistics for the one year 1909 (as a matter of fact, he has given me three years of statistics of perforations, and other facts, but it is only in the year 1909 that his attention has been drawn to the subject, and in that period he was particularly careful to see that every stomach and duodenum which passed through the *post-mortem* room was carefully examined for scars): In 1909 he found 10 cases of ordinary still existent duodenal ulcer, in one of which the scar of an old healed second ulcer was present; he also found 5 cases of scars of old ulcers in the duodenum, the possessors of which died of something totally different. Thus, small as these figures may be, they prove to complete demonstration the fact of the possibility of the healing of duodenal ulcer without surgical interference, and that is the one point which I wish to prove and to insist upon: that an ordinary gastric as distinguished from a duodenal ulcer can heal is, I think, admitted by every one without requiring further proof.

Now, think of what I have been proving. I have been combating my colleague Dr. Hutchison’s statement that if a duodenal ulcer is present he thinks surgical interference

is imperatively called for, if you can diagnose it. My point is that while it may be called for for other conditions, yet simply for the fact of ulceration it is not called for, because I have shown that in one single year we have got 6 cases found on *post-mortem* examination with healed duodenal ulcers that have never been operated upon. I cannot therefore plead guilty to any proved exaggeration when I claim that 90 per cent. of ulcers treated medically are as surely cured as when they are treated surgically. Note the comparison I make, “as surely cured.” I am not certain that when we treat them medically ulcers must necessarily heal, but I equally claim that when treated surgically there is no greater proof that the ulcers heal beyond the cessation of symptoms, and I have proved that under any circumstances ulcers can heal.

It is tacitly assumed, at any rate by those who have not specially studied the question, amongst whom must be included every one of your patients, that after medical treatment relapse is the rule, and the patients are really no better, whereas after surgical treatment they are completely cured, and need fear no relapse. It is often stated that if a patient undergoes surgical treatment he will be completely cured; if he be put to bed and treated with medicine he will be relieved of symptoms, but the ulcer will recur. That is definitely a fallacy, as I shall now show by the statistics of relapses. These figures will have to be very much revised, not that they are incorrect, but I cannot positively assert that they represent 606 different persons, how far they may include relapses—a point that must be ascertained before the figures are used for other purposes. But taking them as Dr. Zundel has given them to me, out of 606 cases medically treated, 75, or 12.3 per cent., were known to have relapses at a less or greater interval after the cessation of treatment. What I do not know in connexion with them is how many of these 606 cases would represent relapses from attacks which have occurred before they came to the hospital. So that I must admit that the proportion of relapses under medical treatment may be rather larger, and quite probably is, and I am not concerned to deny it, because I have never raised that point; I have never said that if you will give me a gastric ulcer to treat I will guarantee to cure it so that the patient will never be ill again; that is not my position at all. I am surprised that the number is so low as 12.3 per cent.; I think probably it will be found to be nearer 20 or even 30 per cent. of relapses if one knew the previous histories (Mr. Moynihan’s paper was only read last month, and in one month we certainly have not had time, and I could not throw on our registrar the work of investigating all these 1,255 cases from this point of view, because it means reading the history of every one of them, and tabulating the number of times they have been laid up in bed at home or in other hospitals with gastric ulcer, or with symptoms which are those of gastric ulcer). Now out of 321 cases surgically treated, 28, or 8.9 per cent., relapsed; so that after surgical treatment it may be that the liability to relapse is not quite so great, but they do relapse, and in a very unsatisfactory percentage, and the difference between the medical and the surgical treatment is not so great as would lead a patient, or should lead a doctor, to ignore the chances of getting no improvement after submitting to the risks of operation.

I do not want to lay very great stress on those risks of operation, but I do wish to point out that they are not a negligible quantity, because I find, taking the statistics for the years from 1903 to 1907 on the surgical side, that they had 314 cases to operate upon, and I have deducted at once all cases of perforation, because the prognosis of perforation is such that it is not fair to surgeons to include them; but taking the cases which do not perforate, but in which the symptoms are of sufficient magnitude to make it advisable that the surgeon should interfere—and he has had a favourable chance of interfering, a reasonable surgical chance—of the 314 such no less than 55 died; in other words, there was a mortality of 17 per cent. I do not want to say that that is a very big mortality, because it is not, and gastric ulcer, if left to go on after surgical interference seems desirable, is highly fatal; and I think it can fairly be urged by a surgeon, “It is true I have killed 17 per cent., but I have saved the remaining 83 per cent. from

nearly certain death." But my present point is that surgical interference is not without its own risks, and it has to be compared with the death-rate of those who are absolutely left alone, which rate I gave you just now, 9.4 per cent. You cannot strictly compare those by subtracting one from the other.

These statistical details require a great deal of work and elaboration to enable us to arrive at complete answers to many subsidiary questions surrounding the subject of ulceration. I shall get Dr. Zundel to work them out more elaborately in the next six or eight months. But, partly from these statistics and partly from general clinical experience, I feel justified in giving you for your guidance the following propositions as the guiding indications for surgical and medical treatment respectively.

The first point is this: that a first simple attack of ulceration should invariably be treated by medical means for a period of at least a fortnight, to be extended if necessary to a month. You will have gathered the arguments from what I have said. Secondly, I lay it down that if at the end of a month there are no signs of improvement as regards pain when the patient has resumed active life, we may begin to entertain the idea of surgical interference, because we may be reasonably certain that the ulcer has not healed. But even here I would myself repeat the medical treatment with rest in bed for another month, and that is a proposition which I feel certain it is impossible to controvert. My third proposition is that the above two hold, no matter what the age or sex of the patient may be, but that age and sex have a good deal to do with deciding for or against surgery. Considering age alone, there is very little, in fact no advantage in resorting to surgical interference under the age of 30, unless indeed signs of stricture are too evident to be misinterpreted, in which case surgical interference is imperative on account of and to cure or to obviate the stricture. But the chances of cure of the ulcerative diathesis are *nil*, for all the relapses after surgery, or nearly all of them, are in young subjects under 35. We know, however, the evils we are dealing with, and can estimate very nicely the means we have for dealing with them, and can act accordingly—that is to say, the evil we are dealing with is the stricture; we can estimate its extent fairly accurately by observations on the nature and the amount of the vomit, and the amount of loss of flesh. We have the clinical data, and we can wash the stomach out and see how much it will hold, and so on. So that, except for a stricture, do not operate at or under the age of 30. After the age of 30, and the later after 30 the better, the greater the advantage of surgical interference.

We have every reason to believe that as age advances the adenoid tissue spoken of above (and here is the importance of my original thesis) atrophies and becomes less vulnerable. But if an ulcer do occur after 30, the tissue has proved itself to be vulnerable, and it seems to have a less tendency to heal, and the ulcer a greater tendency to spread, and to produce serious results. Hence a greater readiness on our part to consider surgical intervention, because (a) there is less tendency to relapse, so that a complete cure may result; (b) there is a greater danger of serious results while trying medical treatment; (c) we know that medical treatment is less likely to prove of service because of the intractability of the ulcer. After the age of 30, then, I am not by any means averse to operative interference from that point of view; but still, remember what I have said about my first two propositions, that for the first attack, no matter what the age or the sex, do not operate until you have given at least two months' treatment.

My fourth point is obvious to anybody, that for stricture and perforation surgical interference is absolutely imperative. Nothing more need be said about them; the necessity for the surgeon is too obvious. The fifth point has regard to haemorrhage alone as an indication for surgical interference. Here we are on very delicate and debatable ground; but it would seem again that age has chiefly to be considered and the nature of the bleeding. Take the very common case in which a girl of about 20 years of age, anaemic, is suddenly seized with a very profuse haematemesis. Operation is not indicated in these cases as a matter of urgency; it is practically useless to think of it to check the bleeding, because long before her stomach can be opened she will have bled to death if she is going to do so. In these young subjects, I think

I might definitely tell you, I have never seen a fatal case of haematemesis. I expect that is not strictly true, as I may have seen such a case and forgotten it, but such cases are so rare that at this moment I do not remember one. Should the case prove rebellious to medical treatment, the fact of this haemorrhage has to be borne in mind when we are considering operation at a later stage. But that is another matter. I say that in young subjects with profuse haematemesis it is no good to call in a surgeon because of the haematemesis. Call the surgeon in later because of gastric ulcer. Taking the other extreme and, I think, equally clear case where surgery is of use, it is the subject between 30 and 50 years of age who has had five or six moderately severe haemorrhages in a week. I have known several cases of that description: sick on Thursday, perhaps a pint of blood; sick again on Friday, with about a pint; sick again on the Saturday, with perhaps rather less than a pint; sick again on Sunday, with another pint; getting very exhausted, repeated haemorrhage, in a middle-aged subject. I say that in that sort of case operation is distinctly indicated and urgent for the haemorrhage; we may hope to get into the stomach and secure the bleeding point, which obviously is prevented from closing by some local process, but is not sufficiently open to cause one big fatal haemorrhage, such as occurred in a man we saw the other day who was under my care with duodenal ulcer, in which we found a hole into which a good big probe could be passed into an artery the size of the radial; he died twenty-four hours after admission—the puzzle is how he lived so long. Those are two extremes—the young girl with her one profuse haemorrhage, the middle-aged person with repeated smaller but severe haemorrhages. Decision in them is easy. But between these you will get a number of intermediate cases, in which I must admit that the balance in favour of or against surgical interference is very nicely arranged, and I can only say you must first of all put the patient to bed and treat her temporarily with complete starvation while you are making up your mind. And then I think you should endeavour to place that patient under conditions in which an operation can be promptly performed, if you make up your mind to do it. I think that your opinion will be in doubt many times. You will, of course, see the patient at least twice a day; that is imperative. On the first haemorrhage you will say, "I will have a surgeon to the case." In the next twelve hours you will find there has been no further haemorrhage, and then you will say, "We will wait a bit, as she is not very bad." You will, of course, estimate the pulse in frequency and volume, the rate of breathing, and so on, as well as the degree of restlessness. In other words, you will see what is the effect of the loss of blood on the patient. During this time you must treat them with the free doses of opium, by which I mean 10 m of the tincture every two hours, until you have the patient somewhat sleepy. This helps both the mental and the vascular condition. You will go the next twelve hours and find the patient tolerably happy and able to talk cheerfully, not looking very pale and not being very thirsty; she does not think she is seriously ill, and is not much frightened. You may say, "The patient is going on nicely, and I think we will wait a bit. I will come back again in another twelve hours." And next time you come you may have a vessel shown to you containing a pint of blood. Then you say, "I must have an operation now." But the parents say, "Do you think it necessary, doctor? We would rather it was not done." You say, "We will wait another twelve hours." You will keep on oscillating in your opinion. The only thing for you to do is to keep a very strict watch on the pulse and respiration. If you find the pulse getting more frequent, over 105, for instance, if you find your patient getting more thirsty, and more alarmed, and readier in her own mind to think of operation, then I say call in the surgeon at once.

The next point has regard to the question of relapses. How many relapses should you allow a person before you call in a surgeon? I would say certainly six or seven, as a working rule; but a good deal depends on the patient, whether she will consent to operation or not. I can tell you of a patient who was under my own care. I brought her here under the hope that she would consent to operation, for I knew that she had had at least seventeen severe attacks of haemorrhage, and had never been free from

indigestion since about 1889. I took her in about four years ago. I tried to persuade her to have an operation done, but she declined. She was very ill, and I said you must have an operation done; but again she lost her symptoms with the rest in bed and medical treatment, and she persistently refused to have the operation. What has become of her since then I do not know; I can give you that as an experience, but it would be very unsafe to trust to her case for laying down rules; and I think six or seven relapses in a period of about five years should be the limit in regard to allowing medical treatment to go on. Unfortunately four or five years' duration and six or seven relapses will not always be allowed you. This is probably the meaning of the death-rate in those medically treated; they have died of perforation or haemorrhage under circumstances in which it was impossible to do anything, and surgeons will say, why do you not, then, have them surgically treated in order to anticipate these catastrophes? I do not hesitate to say, strange though it may appear, that it is absolutely impossible to be certain that one of these catastrophes is going to occur. I go further, and I say that even if the surgeon be called in on the first occasion, you cannot guarantee that the catastrophe will not occur after that, because the death-rate after relapse and after operation has been something considerable. Therefore, lame as the conclusion may seem, the fact remains that you cannot be certain a catastrophe will not occur in any given case of gastric ulcer. Still, provided there is no catastrophe, I say five or six relapses over 30 years of age, and then *qua* relapses only, provided the patient's consent be obtained, operate.

Lastly, is there anything to be felt in the shape of a lump in the abdomen which means either simply a scar without adhesions, or a scar adherent to the anterior abdominal wall, or to the liver, or somewhere else? Shall you operate? I must answer that for you by saying, Yes; generally speaking, you should operate if you find these big organic lumps, because it means chronic ulcer which will not heal without treatment. I should have made this statement without hesitation if it had not been for my experience of a case in the last month.

The patient came to me a month or six weeks ago, when I found a big organic lump, and I said, This ulcer has gone too far for medical treatment; you must come in and be operated upon. She consented and came in. In hospital one is never in a violent hurry; we can always get surgical treatment for any emergency in half an hour or so, and so we were not in a hurry. One cause of delay led to another in this case, and she seemed to be improving very much. We got her teeth removed for one thing, and I believe that they were poisoning her, for she had very bad pyorrhoea alveolaris. The result is she had no operation, but she steadily improved under medical treatment and her hard lump disappeared.

Such a favourable result in such a case is not to be commended to you as a thing to expect, but it certainly should be mentioned to you as a fact; so I shall still have to repeat that if there is an organic lump present, operate. It must be taken as one of the indications for operation, but again I insist that even this is not absolutely infallible. The only infallible indications for operation are stricture or perforation; those are the only ones which I know of to which there is no possible exception. The perforation may, of course, be so bad that the surgeon will not operate, but that does not really affect the point which we are discussing now.

WE learn from the *New York Medical Journal* that on February 1st, 1910, Senator Owen, of Oklahoma, introduced into the United States Senate a bill providing for the establishment of a Department of Public Health under the supervision of a Minister of Public Health, who is to be a Cabinet officer, with a salary of 12,000 dollars a year. The bill provides that all departments and bureaus belonging to any department, excepting those of War and of the Navy, affecting the medical, surgical, biological, or sanitary service, or any questions relative thereto, shall be combined in one department, to be known as the Department of Public Health. The work of the department will embrace every agency of the United States for the protection of the health of the people of the United States, or of animal life. The bill provides that the secretary shall establish bureaus of biology, chemistry, veterinary service, and of sanitary engineering. The bill was referred to the Committee on Public Health and National Quarantine.

A Lecture ON DYSMENORRHOEA.

DELIVERED AT THE MEDICAL GRADUATES'
COLLEGE AND POLYCLINIC.

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GENTLEMEN,—The subject we have to discuss is one which ought to be of interest to every practitioner of medicine, for this affection is apt to be the cause of suffering from the time a girl is about 12 or 14 during the many years up to the menopause; and although the worst cases come under the observation of the gynaecologist, yet before doing so they pass through the hands of the family physician, so that there is really nothing more important in the ordinary treatment of the diseases of women, or for which advice is more frequently sought, than the pain suffered at the time of menstruation. It so frequently shatters a girl's health before she has become a woman that too much attention cannot be paid to the careful study of this disease. I am certain that the judicious management of dysmenorrhoea prevents, in many cases, girls and women drifting into chronic invalidism. Although many regard dysmenorrhoea as a symptom of something occurring internally, it is certainly a disease. Healthy girls never have pain with menstruation; it occurs practically with no symptoms whatever, and except for its appearance might be disregarded as far as in any way affecting the health or well-being of the individual. Unfortunately, the minority of women menstruate in this manner, suffering no pain. It has been estimated that only about one-third of all women menstruate without pain. This shows how important the subject is.

I propose to deal with the subject principally under the head of etiology before passing on to what is, of course, the most important consideration for us—namely, treatment; but in the first place I shall merely mention the divisions into which dysmenorrhoea has been classified before giving my own view of them. It is usually said that the forms of dysmenorrhoea are idiopathic, congestive, membranous, spasmodic, obstructive, ovarian, and even neurasthenic. Some believe there is a form of dysmenorrhoea which is purely nervous, and which may be due to faulty innervation of the uterus, be it of an anatomic or a functional defect. This variety they speak of as neuralgic dysmenorrhoea.¹ Others think that it is easy to divide the various forms clinically into uterine and extrauterine;² and others, again, have classified them into primary and acquired.

The greater the experience one has, the more convinced one is that these divisions are entirely unnecessary. I know of nothing to make me alter the view I have long held—that there is only one form of dysmenorrhoea, that is, spasmodic, and that therefore all other varieties, or so-called varieties, merely produce confusion. If the word "dysmenorrhoea" were confined to the disease to which, in my opinion, it ought only to be applied—namely, painful contractions of the uterus during menstruation—confusion could not arise, but because it is so frequently associated with affections of the ovaries, Fallopian tubes, or certain conditions of the nervous system, so many forms of the disease have been described. To speak of neurasthenic dysmenorrhoea means only that a woman suffering from neurasthenia has superadded to her condition dysmenorrhoea, and in such a case it is quite possible that the doctor attending her, or prescribing for her, may think that dysmenorrhoea has supervened in the neurasthenic woman, and that therefore neurasthenic dysmenorrhoea is a legitimate name for the affection. But, as a matter of fact, pain is relative, and anything which lowers the tone of the nervous system, such as the onset of menstruation, is apt to bring on pain in certain parts, or to aggravate the sensation from very slight to severe. The most important division of dysmenorrhoea, and one which demands a considerable amount of attention, owing to the fact that it is always so constantly referred to, is obstructive or membranous dysmenorrhoea. It is considered that membranous dysmenorrhoea produces obstruction on account