to a very great extent, but very little fermentation occurs; on the contrary, if it be put into a deep vessel, the development of the yeast-plant does not go on so rapidly, but more fermentation occurs. He explains it this way: that the yeast-plant requires oxygen for its nutrition; if it get it easily, as it does in a shallow vessel in the air, it produces comparatively little effect in breaking up the sugar into its constituents. So here, in the deep vessel, the carbonic acid accumulates, supposing any to exist, as in a well. Here the bacterium lactis had but little opportunity for getting oxygen. Accordingly here, just as in M. Pasteur's experiments with the sugar, the lactic ferment produced more rapidly its effect of lactic fermentation.

But this, you say, is assuming that bacterium lactis is the ferment. Now we are coming to that point. I say that this fact demonstrates, as I believe, that the bacterium lactis is the cause of the lactic fermentation. I must add one point by way of fact. For the satisfaction of others rather than for my own, I went through the laborious process of investigating portions of all these vessels, and I found that, in every one in which the lactic acid fermentation had taken place, where there was curdling and souring, the bacterium lactis was present; and in no instance in which there was no lactic fermentation was any bacterium of any sort to be discovered. I believe that demonstrates that the bacterium lactis is the cause of this very special lactic fermentation. But, let us assume for a moment that there did exist some other particles besides bacterium lactis in the milk capable of causing a fermentation; that the lactic ferment were not the bacterium at all, but some chemical ferment. First of all, you will please to observe that we have from this experiment absolute evidence that the ferment, of whatever nature, is not in solution, but in the form of suspended insoluble particles. If the ferment had been in solution, every drop would have produced the same effect. The fact that some drops were destitute of the ferment proves that that ferment was not in a state of solution. That is absolutely demonstrated. Now, suppose we admit, for the sake of argument, that the lactic acid ferment was some non-living substance, capable of self-multiplication as rapidly as the bacterium, but not living; a strange hypothesis, no doubt—but suppose we admit it. Suppose we admit that these chemical ferments, the chemical lactic ferment and the bacterium lactis, were merely accidental concomitants of each other, it would be absolutely inconceivable that these two accidentally present things should be present in exactly the same number. But, suppose you admitted that—that there were exactly as many of the bacterium lactis as there were of the ferment, the true fermentative particles—suppose you admitted that inconceivable thing, I say it would be again inconceivable that they should accompany one another in pairs, that invariably where there was bacterium lactis there should be a ferment particle, and where there was no bacterium lactis no ferment particle. That would be as inconceivable as the other. Therefore, we have two inconceivables, one of which would have been sufficient to show that we cannot admit any other hypothesis than that bacterium lactis is the cause of the lactic acid fermentation.

But the experiment tends to even more than this. Where we find the effect so exactly proportioned, as regards the number of glasses affected with fermentation, to the adult bacteria that we count, we are led to infer that this particular bacterium, at all events, has not any spores—that there are no spores existing in addition to the bacteria. People seem often to assume that these bacteria must necessarily have spores or germs. It seems to me an unlikely thing that they should. If they are, as it were, a generative apparatus *per se*, they are constantly multiplying; why should they have spores? I do not say that bacteria may not have spores. There are very different kinds of bacteria; some may have spores, and some may not; but this sort of result seems to indicate that this particular bacterium has no spores; because, if we had, besides the bacteria that we can count, spores of bacteria disseminated through the liquid also, we should have the effect more than in proportion to those bacteria that we have. The only fallacy here is that it may be that the bacterium has not been diffused uniformly through the milk. Therefore, I do not say that in this case it is absolutely proved yet. At all events, this experiment gives us a line of inquiry, by means of which we may probably settle that point with regard to any individual case of bacterium. This, however, I do not desire to urge upon you; but, what I do venture to urge upon you, is that you will seriously ponder over the facts which I have had the honour of bringing before you to-day; and, if you do so, I believe you will agree with me that we have absolute evidence that the bacterium lactis is the cause of this lactic acid fermentation. And thus I venture to believe that we have taken one sure step in the way of removing this important but most difficult question from the region of vague speculation and loose statement into the domain of precise and definite knowledge.

ON HÆMORRHAGE FROM THE RETROFLECTED UTERUS, AND ITS TREATMENT.*

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AT the risk of being thought to occupy your time on a subject already worked out, and again to tread over well-trodden ground, I have ventured to bring up a subject which is very clinical; and one which, though apparently well discussed both for and against, has not yet been decided, at least by many, so as to be to them a rule for practice.

When discussions on the treatment of the retroflected uterus have taken place, it has generally happened that two opposite opinions have declared themselves: one strongly in favour of mechanical treatment; the other, if not actually repudiating it, yet considering it the *dernier ressort*, not to be tried till constitutional management and local treatment have failed. Between these two extremes, there are various shades of opinions, inclining to the one or the other according as experience, education, and other circumstances have affected the possessor. I shall perhaps be not far wrong when I rank the majority of the profession as forming the intermediate class.

The difference of the extreme opinions appears to me to be owing to these facts.

1. Women are not unfrequently found with completely retroflected uterus, without the least uneasiness or complaint, all their lives.

2. Women complaining of certain symptoms, and even crippled by them, are found to have retroflected uterus; all of these symptoms subsiding upon the restoration of the uterus to its normal position.

3. It is often observed that, before the retroflexion is confirmed, the uterus sometimes is retroflected, and at another time in normal position.

4. That temporary engorgement of a retroflected uterus, which had before given no trouble, frequently subsides under horizontal posture and appropriate treatment.

According as these conditions have presented themselves, in respect of number and intensity, to the various observers, so have their opinions inclined in one direction or another.

It is not my intention, nor is it suitable at the present time, to occupy this meeting with a full discussion on retroversion and retroflexion; but there is one clinical state of it, by no means unfrequent, upon the treatment of which as much difference of opinion has existed, and in a measure still exists, as on any part of the subject. Upon this subject I am desirous of adding the result of my experience. Hæmorrhages may occur from the retroflected uterus in various degrees; the remarks I shall make apply to all of them, more or less, but it may be well to take a typical example.

The uterus is completely retroflected, the body two or three times the normal size; sensitive to the touch, especially at the fundus; the os and cervix are large and patent; there are much leucorrhœa and menorrhagia, with greater or less bleedings between; coagula passing often, sometimes with pain; pain in the back, bearing down; uneasiness in the groin; and indeed all the direct and reflex symptoms produced by retroflexion exaggerated.

It has been contended that in this state it is unadvisable to handle the uterus, so as to cause its restoration to its normal position, and to retain it by vaginal pessaries. It has been argued that this state is inflammatory, and that the disturbance will increase it: that the proper thing is to subdue the inflammation, first, by depletives locally and by postural treatment, and then to replace the organ, should it not have recovered its position by the rest.

This line would be doubtless correct, were the premises upon which it rests correct also. Let us consider for a few moments these data. The effects of retroflexion of the uterus have been well recognised, and may be divided into those which are derived, first, from the obstruction produced by the flexion; and secondly, from the dependent position of the fundus. By both of these the secretions are retained beyond their usual time, a certain amount of irritation is set up, which increases the secretion, and this accumulating, distends the cavity; and thus, in the case of hæmorrhage, a larger amount is permitted to flow. Besides this, a clot, often forming, has to be expelled by uterine contractions, which cause pain, bearing down, and backache; these clots, by breaking up, by a species of decomposition, set up increased irritation and flow of blood and serum, etc. Besides this, the level of the fundus being three to four inches lower than normal, there is a greater pres-

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sure on its vessels by gravitation, thus facilitating increased bleeding; and at the same time, a certain amount of dilatation of the vessels themselves, with frequently exudation into the tissues they supply. And thus we find thickened walls, increased in thickness also by the action necessary to expel contents abnormally retained. This fluid pressure cannot exist without the nervous structure being pressed upon, and thus tenderness and uneasiness are felt, which no doubt are increased by the causes of irritation to the mucous surface already alluded to. When miscarriage has occurred in a retroflected uterus, then these effects are very much increased; and continue for a long time, so long that, as Dr. Barnes has remarked, the original causation is forgotten. But this position also renders the expulsion of the ovum more imperfect and prolonged, especially where there is, as is frequently the case, a firm adhesion between the villi and the uterine wall; and this is well shown by my own experience, for in nearly every case where I have been called in consultation for difficult abortion, with violent hæmorrhages, the uterus has been found retroflected. Other effects succeed to retroflexion, such as external infiltration of serum, or even blood, besides the reflex neurotic symptoms; but at the present I am alluding to the actual clinical state of the uterus itself, the result of its retroflexion especially, accompanied by hæmorrhages. And the question which arises from the consideration of these facts is: Whether the large and tender state of the uterus is dependent upon the retroflexion, or a coincidence with and accident to the state? If the organ have become so in consequence of its malposition, then the line of treatment seems clear, namely, to restore it. But, say some: If you interfere with the uterus in this condition, you will cause an increase of the inflammation and make matters worse.

But, suppose it can be shown clinically that the uterus *can* bear the handling necessary for its restoration to position, that it is not made worse, but, on the contrary, gradually loses all its bulk and tenderness, then the objection just mentioned is without ground. But in truth the term "inflammation", as applied to the sensitive irritable state, is a misnomer: not but that much care should not be exercised in the restoration, lest we unnecessarily increase its irritability by our manipulation. To assist in the solution of this problem, I have ventured to add briefly the results of my own clinical experience, and this is as follows.

1. In no instance where I have restored the uterus, and kept it *in situ* by a vaginal pessary, have I found any inflammatory symptoms follow.

2. In no case have I found other than improvement, the uterus gradually becoming smaller, less sensitive, and the discharges from it less, till it gradually resumed its normal state, so far as was compatible with a certain amount of permanent thickening, the result of the former engorgement.

3. In the case of hæmorrhage, and this especially the sequel of abortions, the restoration of the uterus to its normal position has always been attended by a diminution of the loss rapidly, and in a few months the cessation, except when other causes for hæmorrhage, as fibroids, abrasions, etc., coexisted. An exception also must be mentioned, namely, that the first period after restoration is more free not infrequently, although the second shows clear signs of abatement. This is probably owing to the ease with which the fluid can escape from the uterus now in normal position, while the causes of the hæmorrhage in the uterus are not yet much lessened.

I may add that, in restoring the uterus enlarged and tender, I do not employ the sound, at least very rarely. By gently lifting up the fundus out of Douglas's pouch (in which it is somewhat firmly retained by the recto-uterine ligaments, as shown by Dr. John Williams) by the finger introduced by vagina or rectum, and hooking down the os, assisted by the supine posture, and then introducing a Hodge's pessary, I have generally succeeded in bringing the fundus into its natural position.

If abrasions be present at the first, I generally reduce the uterus and wait a little time; if the abrasions be not cured, I apply the usual remedies. However, we may, in the less irritable organs, apply them earlier, the pessary remaining *in situ* or removed each time. Frequently, however, the abrasions and catarrh pass off as the general improvement takes place.

If the facts which I have noted agree with the experience of others, which I think to a large extent they do, then it may be laid down, as a rule of practice, that notwithstanding the tender and bulky state of the uterus we may, when we find it retroflected, especially associated with metrorrhagia, without fear restore it at once to position, and employ a Hodge's pessary to retain it there. If the restoration be difficult to complete at once, the uterus still inclining somewhat backwards, it will, so far as I have noted, fall into its place shortly by the action of the pessary, assisted by the various movements of the body. If the patient complain of uneasiness, it is very easy to neutralise all pressure

by the adoption of the horizontal posture, assisted by the prone position. If this be carried out for a few days, I have found that the uterus tolerates the instrument well, for in a short time the sensitiveness is lessened by the restored position as before mentioned. Of course, sometimes the uterus is adherent in its retroflected state; care must, therefore, be taken lest we carry on our attempts at restoration too vigorously. Gentle tentative trials should, of course, be made at commencement.

I need hardly point out that care must also be taken in our diagnosis, lest we mistake retro-uterine hæmatocele for the bulky tender retroflected fundus. I may give one case, as illustrative of the advantage of at once restoring the uterus to its normal place in severe hæmorrhage.

A. B., married, mother of six children, was admitted under me into Guy's Hospital, April 4th of this year, in consequence of very severe uterine hæmorrhage, which led her medical man to suspect malignant disease. She was very exhausted and blanched. The history of the case was as follows. She menstruated regularly up till last November. After this, she saw nothing for nine weeks, when severe hæmorrhage set in, but it was shortly checked. She kept her bed for three weeks, but upon getting up the bleeding at once returned, and continued daily up to the time of admission, being very profuse whenever she exerted herself, altogether having had nine or ten severe hæmorrhages. I found the cervix patulous, more anterior than normal, with a swelling behind. This proved to be the body of the uterus, retroflected, somewhat more bulky than normal. I essayed to restore the uterus to its proper position; but the bleeding which took place, on the use of the sound, was so profuse that I had to desist, and plug the os with a sponge-tent. After ten minutes, this was removed and the bleeding was found to be arrested. I then introduced a Hodge's pessary, which, assisted by digital pressure on the fundus *per vaginam*, restored the organ. A dose of ergot was then given. The bleeding at once ceased, with the exception of a slight flow for three days, followed by a little watery discharge. She gradually regained strength and colour, and finally menstruated normally on May 13th, about five weeks after her admission. To the present time, she has continued regular. She had only one drawback from rapid recovery, a slight local inflammation of two of the veins in her legs.

CASE OF INDURATED MEDIASTINO-PERICARDITIS.

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BAUER, in his article upon Diseases of the Pericardium in Von Ziemssen's *Cyclopædia of Medicine*, relates some interesting cases of indurated mediastino-pericarditis: I propose to give an abbreviated account of a case which has been under my care.

Ellen B., aged 20, a delicate-looking girl of blonde complexion, and below the middle height, by occupation a shirt-maker, was admitted as a patient under my care at the Eastern Dispensary on August 21st, 1873. A year previously, she had been under treatment at the dispensary for pleurisy of her right side; there was no effusion. For the past three months, she had been losing flesh, and had ceased to menstruate. On August 16th, she gave up work on account of pain in her chest. She looked very ill; the expression of her countenance was anxious; the skin feverish; pulse quick; tongue dry, with the edges and tip very red; the dorsum covered by a yellowish white fur. She had anorexia and thirst. The bowels were loose; the urine was of specific gravity 1020; no albumen. There was no sign of any pulmonary mischief. The heart's apex could be felt between the fifth and sixth ribs, half an inch inside the nipple-line. The vertical percussion-dullness began at the fourth left cartilage; on the right side, it extended over the fourth, fifth, and sixth cartilages; transversely on a level with the fourth rib, it commenced nearly an inch to the right of the sternum and ended just within the left nipple-line. On auscultation, loud grating friction could be heard all over the præcordial region. The hepatic and splenic dullness were normal.

From this date, all the usual symptoms of enteric fever gradually declared themselves; raw-beef tongue; bowels loose, four or five evacuations daily; abdomen tub-shaped; tenderness and gurgling in the ileo-cæcal region; pink spots disappearing under pressure on the abdomen and chest; sudamina; increased area of dullness over the spleen, which eventually projected two inches and a half from the left costal margin, and was markedly tender when handled. This occurred when there were signs of effusion into the left pleural cavity. In addition to