REMARKS ON BATTEY'S OPERATION.

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III.—ANALYSIS OF CASES; AND REMARKS.

BATTEY has now performed his operation twelve times; twice by the abdominal, and ten times by the vaginal section. I have done it seven times; thrice by the abdominal, and four times by the vaginal section. In reviewing these nineteen cases, the question naturally arises "Were any of them submitted unnecessarily to operation?" In all of Battey's cases, I think the operation was wholly justifiable. I saw, with him, two of his most moderate cases (IV and V); and in these, all other means had been exhausted without affording the least relief, and he was obliged to operate, or to do nothing. My fifth case (that died of peritonitis on the seventh day), might have been postponed for a while, but it is certain that no other treatment could have relieved her; for both ovaries had undergone cystic degeneration, and an operation would have been necessary sooner or later. In all of my cases, the ovaries removed were in an abnormal or diseased condition; they had all undergone cystic degeneration. Their fibrous structure (stroma) was greatly altered in appearance, and I have no doubt that the microscope would have shown that it had undergone marked organic change, in consequence, probably, of subacute ovaritis, which blends and amalgamates the nerve-filaments, cellular tissue, and fibrous structure of the organ into a sort of neuroma. In no other way can we account for the persistent neuralgic pains; they are the pains of a neuroma, and the poor sufferer almost invariably and necessarily becomes a morphinist.

Ordinary cystic disease of the ovaries, resulting in true ovarian tumours, never gives rise to severe pain, as the experience of Spencer Wells and our other great ovariologists clearly proves. The peculiar pain of these cases must then depend upon some radical organic change in the nerves and stroma of the ovaries, and not upon the mere development of cysts. This view of the case is further strengthened by the fact that the pain persists after imperfect removal of the ovaries where the cysts are broken down and scraped off while a portion of fibrous structure still remains.

In some of my cases, the ovaries were of twice their natural size. In three, the ovary burst in the act of pulling it out of the peritoneal cavity. In Battey's cases, the ovaries had undergone cystic degeneration in every instance in which he examined them. Some were three or four times larger than the natural size. In six of Battey's cases, the ovaries were bound down by adhesions; in one of mine, this was the case. Battey lost two cases; I lost one. All died of peritonitis. The peritonitis in one of Battey's cases was secondary, the result of a small abscess (holding about an ounce) around the ligature on the right pedicle, which broke into the peritoneal cavity on the ninth day after operation, and the patient died in twenty-four hours. In five of Battey's cases that recovered, the operation was followed by some form of pelvic inflammation. One of these had septicaemia, and one pelvic abscess; their recovery was slow. In the other five, there were no complications, and the recoveries were rapid. In five out of six of my cases that recovered, the operation was followed by pelvic cellulitis or pelvic peritonitis, two terminating in abscess. In all of these, recovery was tedious. In Battey's cases, the removal of both ovaries in no way affected sexual desire. In six cases, he removed both ovaries entire; in three, he removed but one. In three cases, the ovaries were broken down and removed piecemeal; in all of those where but one ovary was removed, or where they were broken down, the operation was unsatisfactory, and without benefit. Of Battey's twelve cases, two are marked improved; four, not improved; two died; and four were cured perfectly. Only twenty-five per cent. cured is not encouraging to the advocates of this operation. My own results are still worse. Out of seven operations, one died; in one, the operation was not finished; three were made worse by the operation; one was greatly improved; and but one was perfectly and permanently cured. In reading this record, one may well feel surprised that I advocate the operation to-day with more earnestness than I did at first. I do it, because I now see where mistakes were made, and how they may be avoided in the future. Without operative procedures, these cases are

all hopelessly incurable. We must improve our methods or leave them where we found them. I see no reason why the operation should not be made safe and successful. Ovariectomy was once opposed because it was unsuccessful; but now it is accepted because it is successful. Battey is the originator of this operation. He is the pioneer in an unexplored field of observation. It is not, then, to be wondered at if he should have done some things he ought not to have done, and left undone some things he ought to have done. He is the leader; I the follower. Let us look back over our work, and see if it has always been well done. Our failures will be lost to science if they do not lead to improved methods and to better results. Our errors, like beacon lights to warn others against hidden dangers, should be made conspicuous. Let us, then, frankly inquire where and how we have made mistakes, that we may profit by them in the future.

Battey started out with the theory that the cure would depend upon bringing about change of life by the removal of the ovaries. The experience of all ovariologists proves conclusively that the removal of one ovary will not do this, and yet we find Dr. Battey ignoring his theory at the very outset, and removing but one ovary in the second case on which he operated; and he did the same thing in his fourth and fifth cases. Thus we see him in three out of five operations departing from the rule laid down for his guidance at the start. He lost sight of his theory, and removed but one ovary in each of these three cases, because but one ovary in each was the fixed seat of pain. Nothing could be seemingly more rational, and yet it was most unfortunate; for in all these cases the operation was unsuccessful. In three of my cases, where but one ovary was removed, the operations were utter failures. In all of these, it will be necessary to repeat the operation for the removal of the remaining ovary if they are ever to be cured. In one of Battey's cases, he has already done this; and he expects to do it in the others. It is bad surgery to submit a patient twice to such a hazardous operation as this, to do what ought to have been done at one. My cases prove that in almost every instance the operation is followed by some form of pelvic inflammation; and this, of course, enhances the danger of a second operation. Battey's only successful operations were those in which he removed both ovaries in their entirety, while he failed in every case where he removed but one ovary, or where they were mutilated and removed piecemeal. The inference is clear that, as a rule, both ovaries should be extirpated whole at one operation. There may be exceptions to this rule; but in Battey's twelve cases, there was none. My first (and the only case I cured perfectly) was an exception, and I know of one more: Dr. Sabine's case. Notwithstanding these two exceptions, it is wise to follow the rule in all cases. Battey's operations and my own would have presented very different results if we had not departed from the theory he laid down at the beginning, viz., to bring about change of life by extirpating the ovaries.

Battey started out with the idea that the operation would be easier of execution, and less dangerous, by the vaginal route than by the abdominal; and I did the same thing. If we could determine beforehand that there had been no pelvic inflammation, and, consequently, no adhesions between the pelvic viscera, then there would be no objection to the vaginal incision for the removal of the ovaries. Four of Battey's vaginal operations (IV, VIII, IX, and XII) failed, because he found the ovaries bound down by adhesions, so that it was impossible to remove them entire. He was obliged to break them down with his finger, and to scratch them out with his nail, and the operations were always unsatisfactory and unsuccessful. In all of these cases, if he had operated by the abdominal section instead of the vaginal, he would certainly have removed the ovaries entire, and the result in each would have been just the reverse of what it was. For Battey has never yet failed to remove the ovaries entire by the abdominal section, no matter how great the adhesions may have been. I repeat that the success of these operations, as a rule, depends upon removing both ovaries entire and not by piecemeal. In Battey's four cases "cured", both ovaries were removed in their entirety. In all the failures, they were removed piecemeal, or only one was taken out. In all those removed piecemeal, the incision was by the vagina, and the result a failure. If it had been by the abdominal section, they would have been removed entire, and the result would have been a cure. In the four that I operated on by the vagina, I removed but one ovary in three cases, and they were all not only not improved, but made worse by the operation; and in the fourth case, I failed entirely to reach the ovaries, because they were bound down by adhesions. Now, if I had removed both ovaries instead of one in the first three, they would have stood the chance of being wholly cured; and if in the fourth case I had operated by the abdominal route, there is every certainty that I would have found the ovaries and removed them although they were bound down by adhesions. Battey evidently went

* Concluded from page 882 of last number.

astray in not following out his original idea as to the necessity of removing both ovaries to effect change of life; and I followed him blindly. Again, he was led astray by theoretical ideas in regard to the facility and safety of the vaginal incision as compared with the abdominal. And here again I followed him blindly, and with results more unfortunate than his. Battey's first and eleventh cases were the most unpromising of his whole series, and yet they were the most successful and the most satisfactory of all. These were his only two by the abdominal section. In each, there had been pelvic inflammation and pelvic cellulitis; and in one, the ovaries were bound down by adhesions, but were removed whole, and the cases were triumphantly cured. If he had attempted the vaginal operation in these two cases, his failure to remove the ovaries entire would have been as signal as it proved to be in all of his other cases by vaginal incision where the ovaries were bound down by adhesions and had to be removed piecemeal, with a failure to cure. Fortunately for Battey's first case, the operation by abdominal incision was performed before the maturation of his ideas on the facility and safety of the vaginal incision, or it could hardly have been successful. Fortunately for the eleventh operation (second by abdominal incision) it could not be performed by the vagina because of the atresia vaginæ, which rendered it a physical impossibility; for if the vagina had been normal, he would (at that time) certainly have operated by the vaginal incision; and as the ovaries were bound down by adhesions, he would certainly have failed to remove them entire, and the result would have been as unsuccessful as it was in all of his other cases of the same sort. I failed completely to remove, by the vaginal incision, ovaries that were bound down by adhesions; and Battey has never succeeded in doing it in a single instance in a satisfactory manner, while by the abdominal incision he has succeeded perfectly in every case, even when the adhesions were extensive and well organised.

The inferences that I draw from this analysis of Battey's and my own operations are these.

1. Remove both ovaries entire in every case.
2. As a rule, operate by the abdominal section, because, if the ovaries are bound down by adhesions, it is possible to remove them entire, whereas by the vaginal incision it is impossible.
3. If we are sure that there has been no pelvic inflammation, no cellulitis, no hæmatocele, no adhesion of the ovaries to the neighbouring parts, then the operation may be made by the vagina, but not otherwise.

Battey's views about the relative advantages of the abdominal and vaginal sections for the removal of the ovaries have been lately undergoing a change; for, in a private communication received from him (October 1877) he says: "Three of my cases (by vaginal incision) have been unsuccessful, because of imperfect removal of the ovaries. In every case where both ovaries have been cleanly removed (and the patient recovered), the change of life has taken place, and the maladies operated for have disappeared. The ovaries are more easily removed by the abdominal method. In three cases, it required greater skill than mine to effect clean removal by the vagina. When we cannot be assured of success by the vaginal route, we should elect the abdominal."

Acting upon this principle, I am sure that Dr. Battey's next series of twelve operations will show a widely different result from that of his first twelve; and I hope, with the light now before me, that my next seven cases will be a great improvement on my last.

The following table shows the number of times this operation has been performed, the number of deaths, and the name of each operator.

	Operations.	Deaths.
Dr. Battey, Georgia	12	2
Professor Hegar, Freiburg	2	0
Dr. Trenholme, Montreal	2	0
Dr. Gilmore, Alabama*	1	0
Dr. Thomas, New York	2	1
Dr. Peaslee	1	1
Dr. Sabine	1	0
Dr. Sims	7	1
	28	5

Battey's operation may be resorted to under the following conditions.

1. In cases of amenorrhœa where there is no uterus, or only the rudiments of one, or where there is an incurable atresia uteri, and the menstrual molimen produces such violent disturbance of the whole system as to destroy health and endanger life, the removal of the ovaries is the only means of permanent relief.

2. In cases of prolonged physical and mental suffering attended with great nervous and vascular excitement produced by perturbed

menstrual molimen, whether menstruation be absent, scanty, or otherwise, this operation is justifiable after all the usual remedies fail to relieve.

3. In cases of incipient insanity and of epilepsy depending upon ovarian and uterine disease, this operation is justifiable after all other remedies have failed to cure.

4. In cases of fibroid tumours of the uterus attended with incurable hæmorrhages that endanger life, when the tumours cannot be safely enucleated and removed, this operation may be resorted to with the hope of arresting the bleeding and the prospect of diminishing the tumours.

5. In cases of chronic pelvic cellulitis and of recurrent hæmatocele, when the attacks are traceable to the disturbing influences of the menstrual molimen, we may have recourse to this operation as a *dernier ressort*.

Dr. Trenholme of Montreal, and Professor Hegar of Freiburg, have performed Battey's operation in cases in which severe and exhausting hæmorrhage in connection with the menstrual period had resisted all other treatment.

Professor Hegar has performed Battey's operation twice to bring about the menopause, and thus to arrest hæmorrhage from uterine fibroids. In each case, the bleeding ceased entirely and the fibroids appeared to diminish. One of Dr. Trenholme's operations was to arrest hæmorrhage from an uterine fibroid, and, in the end, it proved successful.

Professor Hegar and Dr. Trenholme each had the happy thought of removing the ovaries, as being less dangerous than the extirpation of the uterus with the fibroid. And thus they have enlarged the area of Battey's operation by opening up a field for its use that was hardly contemplated by its author.

The operations of Professor Hegar and Dr. Trenholme triumphantly vindicate the truth of the theory upon which Battey based his operation; viz., that the removal of the ovaries would bring about the menopause. It has been urged that his theory is not in accord with the fashionable physiological doctrines of the day; and, therefore, his theory being wrong, his practice cannot be right. His theory may not be technically correct; but it does not follow that his operation is unjustifiable; for practical results and theoretical opinions are two very different things. And, moreover, we may differ about what really constitutes a *de facto* change of life. The popular idea of change of life is the cessation of menstruation. If we accept this as the definition of change of life, then the removal of the ovaries will not always bring it about. The extirpation of the ovaries will generally arrest the menstrual function, but not always. Now and then, the menses recur at regular intervals for months, and even for years, after the removal of the ovaries. This has occurred at least once in the practice of Dr. Battey; and he quotes Atlee, Peaslee, and Storer as giving illustrations of the same thing. Their cases are mostly examples of what Spencer Wells aptly terms *metrostaxis*. But Atlee gives some cases of *bonâ fide* menstruation after extirpation of both ovaries. In one case, in which Dr. Atlee removed both ovaries at the age of 35, the patient menstruated regularly and normally till the age of 45, when change of life, so-called, occurred in the usual way.

In October 1857, Dr. Atlee extirpated the left ovary for a married lady twenty-seven years old. In November 1864, he extirpated her right ovary. She continued to menstruate regularly; and, in 1870, six years after the last ovary was extirpated, she was "regular as to time, quantity, quality, etc., and free from any abnormal symptoms". In 1846, Dr. Charles Clay of Manchester removed one ovary from a married lady, and Dr. Atlee removed the other in 1861. Two years after this, she writes to Dr. Atlee: "Courses all right every month." So we see that extirpation of the ovaries is not always followed by cessation of the menstrual flow. But is cessation of the menses actual change of life? By no means. In 1859, I knew a poor woman who had had nine children in fourteen years, and, during all this time, she had menstruated but three times. She had three or four labours consecutively without a single menstruation. Thus we see that ovulation went on regularly, although there was no menstruation. Again, we see cases of complete amenorrhœa, where ovulation, as indicated by the menstrual molimen, occurs regularly every month. In Dr. Battey's first case (of Battey's operation), the patient had menstruated but twice in sixteen years, yet she suffered fearfully every month with the menstrual molimen, and, when he extirpated the ovaries, "each ovary presented a recently ruptured Graafian vesicle, in one of which the blood had not yet coagulated, as if the ovum had but just escaped". There are many cases on record where *post mortem* examinations have shown corpora lutea and recently ruptured Graafian vesicles when there was no menstruation, because there was no uterus. Is it correct, then, to say that change of life consists in the disappearance of the menses?

* Dr. Gilmore's case died of yellow fever nine months after the operation, while convalescing from a peritoneal pyæmic abscess, the result of the operation.

Certainly not; for then the woman who ceased to menstruate while she bore half a dozen or more children had change of life, although she was ovulating and bearing children all the time; and the young woman who menstruates two or three times, and then ceases to menstruate, has change of life, although she may have for years a most violent menstrual molimen every month, and, when the ovaries are extirpated, they may present numerous corpora lutea and recently ruptured Graafian vesicles, which are the real signs of ovulation, and the real evidence that there has been no change of life. It is absurd, then, to say that change of life consists in the disappearance of menstruation. The real change of life consists in the cessation of ovulation, whether this be accomplished by Nature at the climacteric period, or artificially by the operation of ovariectomy, or Battey's operation, which was projected expressly to arrest ovulation and the accompanying menstrual molimen.

When Battey reasoned out his operation of extirpating the ovaries to effect change of life, he reasoned out a truism, for the removal of the ovaries must necessarily stop ovulation, which constitutes a *de facto* change of life, whether the menses recur afterwards or not. The cessation of menstruation may then be regarded as the sign of change of life, but not the actual change. From this point of view, Battey was right in theory; and experience shows that he is right in practice. His operation of extirpating the ovaries to arrest the menstrual molimen is based upon sound physiological doctrine; and in practice it accomplishes what it proposes to do. For we find that, when both ovaries are neatly and cleanly removed, the menstrual molimen ceases. But, if they be imperfectly removed, the menstrual molimen recurs as regularly as it did before the operation.

The term normal ovariectomy, applied by Battey to his operation, is a misnomer; for, in all cases requiring operation, the ovaries are never found in a normal state. This term has been much and justly criticised; and Battey asked me some time ago to give his operation a name. I have always thought that "Battey's operation" would be the most appropriate name for it; and I cherish the hope that the profession in Europe will unite with us in America in giving it the name of the man who originated the operation, and who has, by the most indomitable courage, succeeded in proving its usefulness.

PUERPERAL SCARLATINA.

C. M., AGED 28, primipara, was delivered of a living female child of ordinary dimensions on October 21st. The labour was tedious, lasting about thirty-six hours. The head presented; the placenta and membranes came away entire in twenty minutes, the uterus remaining firmly contracted. The mother did uninterruptedly well until 10 P.M. on the night of October 26th, when she complained of sore-throat and slight shivering, and vomited repeatedly. On October 27th, she was feverish, restless, delirious during the night; the vomiting continued; the lochia were very scanty and extremely offensive; there was total suppression of milk. Light-coloured offensive stools were passed two or three times during the day. Her mother, at 10 A.M., noticed her face and hands to be of a bright scarlet colour, but omitted to examine her body. On October 28th, she had been very delirious the preceding night. Her medical attendant saw her at 11 A.M., for the first time after the morning of October 26th, and found her covered all over with a well marked scarlatina rash. Temperature 103.2 deg.; pulse 128. At 7 P.M., the temperature was 104.6 deg. On October 29th, she was very restless and delirious in the night until 2 A.M., when she became quiet.

I saw her for the first time at 9.30 A.M., and found her lying on her back, with dilated pupils, face pinched, lips bluish; the tongue was dry and brown; the throat dusky red; the whole of her body, with the exception of the face, was covered with a scarlatinal rash of a dusky scarlet colour; there were purpuric spots about the extremities; the hands and legs were of a bluish colour; the muscles were very soft and flabby. There was no tenderness or distension of the abdomen. She was pulseless. Temperature 105.2 deg.; respirations 48, shallow, laboured, and sighing. She was conscious, and, when asked if she felt any pain, answered in the negative. At 11.45, she was unconscious, the whole of her body assuming a livid colour. Temperature 107.4 deg. She died at 12.20. No *post mortem* examination was allowed.

REMARKS.—It is an interesting case, inasmuch as the woman lived long enough for the scarlatina to fully develop itself. It bears out the opinions of Drs. Snow Beck, Meadows, and others, "that scarlatina does not change and produce only 'malignant puerperal fever,' but it retains its specific characters in the parturient woman".

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ABSTRACT OF HARVEIAN LECTURES

ON

THE MECHANICAL SYSTEM OF UTERINE PATHOLOGY.

Delivered before the Harveian Society of London.

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LECTURE II.—Thursday, December 20th.

In this lecture, the relations of the various uterine symptoms to alterations of shape of the uterus were considered. First, it was pointed out on *à priori* grounds that the best form for the uterus must be that originally given to it.

Pain is the commonest uterine symptom, and, as a rule, the one most seriously complained of. 1. Spontaneous pain, not very common, is produced by cancer, tumours, acute inflammation, etc. 2. Pain on motion is very common. It has been far too little noticed by pathologists. Formerly, the author spoke of it as "uterine lameness", but the designation now proposed is "*Dyskinesia*". Uterine dyskinesia may be slight or severe. The characteristic is that it is produced by motion, and in not a few cases patients are entirely incapacitated by it and entirely invalided. This cause is distension of the uterus. Two propositions might be given: 1. Uterine distortions and displacements invariably give rise to dyskinesia; 2. Dyskinesia (referable to the uterus) is invariably associated with uterine distortion or displacement. Proposition 1 is more nearly true than is generally imagined. Proposition 2 is almost absolutely true. Chassaignac (in 1862) attributed such pains to jars (*ballottements*) of the displaced uterus. In mere descent of the uterus, with no flexion, there may be little pain. In cases of slight version and slight flexion conjoined, there may be little pain; but severe distortions, with few exceptions, are always painful. Sometimes, again, congestion is absent, or the uterus has been long in the distorted condition and has acquired tolerance of it. Particular positions of the body, exertions, lifting weights, stooping, riding, even sitting, often give rise to this pain, uterine distortion being the probable cause. Why do these movements produce pain? Dr. Graily Hewitt attributed them to temporary aggravation of the flexion, produced by the exertions. The pain is relieved by whatever prevents aggravation of the flexion. Compression and condensation of the uterine tissues at the seat of the bend, and the effect of this compression on the nervous fibres at this point, is the probable cause. Increase of the congestion, generally also existing in cases of flexion, may account partly for the pain. But the compression and condensation theory is the best. Certain sensations, as sinking, bearing down, are probably due to stretching of the uterine ligaments. Displacement of the ovary, a rare event, occasions dyskinesia to a severe extent.

Undue Sensitiveness of the Uterus to the Touch was next considered. Observation shows that the uterus is almost never painful to the touch, unless uterine distortion be present. The more severe distortions, accompanied by congestion, are those in which the tenderness is greatest. But severe antelexion may also give rise to exquisite tenderness to the touch, and to a somewhat similar train of symptoms. Acute sensitiveness to the touch is sometimes located at the internal os in chronic flexions, and is found out by the use of the sound.

Menstrual Disorders.—The uterine secretions, fluid, and mucous membrane *débris* have to pass through a small aperture, surrounded by dense tissue. This, the smallest part of the uterine-tube, is at the middle of the uterus. The escape should be continuous. The capacity of a tube is greatest when circular; flatten it, and it becomes less. Such flattening occurs even to the extinguishing of the tube, when the uterus is strongly flexed, and usually the flexion has effect at the middle of the uterus, where the tube is smallest. The condensation of tissues at this spot also renders less easy any dilatation of the tube. The horizontal position alone, the prone or supine, according to circumstances, has great effect in relieving dysmenorrhœa, because it partly relieves the existing flexion. The pain indicates expulsive effort of the uterus, due to the retention. This effort partially opens the tube, and partial escape of fluid occurs. Scanty discharge and brown fluid mean retention and partial disintegration of the menstrual blood. Further, gushes of puriform or watery fluid, observed between the catamenial periods—purulent leucorrhœa in gushes—are another result of dysmenorrhœal retention. This is a very characteristic symptom of chronic flexion, though not one always present. These cases are com-