

**Fourth series of twenty-five cases of completed ovariectomy / by G. Granville Bantock.**

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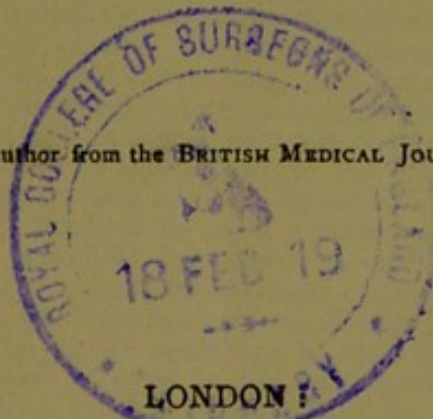
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FOURTH SERIES  
OF  
TWENTY-FIVE CASES OF COMPLETED  
OVARIOTOMY.

BY  
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FOURTH SERIES

TWENTY-FIVE CASES OF COMPLETED

OVARY TOMY

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Surgeon to the Westminster Hospital for Women and Children

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#### FOURTH SERIES OF TWENTY-FIVE CASES OF COMPLETED OVARIOTOMY.

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IN looking over the one hundred ovariectomies which this series completes, I cannot but congratulate myself upon the resolution which I formed, as early as the date of my seventh operation, to put aside the clamp as the means of securing the pedicle, and give the ligature a fair trial. My expectation of greater success from this proceeding—not theoretical, but based on observation—is by this time fully realised, and the practice is justified by the results. The objections then urged against the ligature were, chiefly, that it was not so safe as the clamp, that it was likely to cause pelvic abscess, and that it might become the source of hæmatocele from the escape of blood at the menstrual periods. The danger of the ligature as compared with the clamp was imaginary, and is not now heard of; and, so far as my experience goes, the fear of abscess was equally unfounded. In one case, No. 97, where both ovaries were removed, there were no fewer than seven ligatures on one pedicle and three on the other. Both pedicles were very short, and the inner ligatures actually included the respective cornua of the uterus. All this complicated tying was inevitable, as, on the right side, the tumour dipped deep down by the side of the uterus, so that the line of ligatures formed a curve, with its concavity looking upwards. Here, if ever, one might expect mischief, but no abscess formed; on the contrary, so little evidence of irritation was there, and so rapid was the convalescence after so severe an operation, that the patient left the hospital on the twenty-eighth day, and at the present time is in excellent health.

The idea of hæmatocele arose from the well-known fact that, in a considerable number of cases treated by the clamp, menstruation does take place through the stump and cicatrix. In a former paper, I gave my explanation of this occurrence, and showed that, from the different condition of the stump under the clamp and under the ligature, there was every reason to believe that such an inconvenience must be averted. I am now able to support this view by stating that, in not one of the ninety-four cases, included in these tables, in which ligatures were used, has there been any appearance calculated to cause a suspicion of the expected hæmatocele, or of a periodical discharge. In one case, as already reported (No. 19, of first series), hæmorrhage occurred from retraction of part of the stump, but there has been no other instance of this accident since I adopted the plan of applying a separate fine ligature to the outer edge of the stump formed by the two layers of the peritoneum. In another case (No. 17, first series), where the pedicle on the left side was very thick and short, besides breaking and having to reapply several of the six ligatures used, I punctured a vessel in the deepest and thickest part. A small extraperitoneal hæmatocele was the result of this puncture, and, from its proximity to the rectum, it soon became offensive. The patient died on the ninth day, with the

usual symptoms. An advocate of the clamp would naturally cite this case to clinch all he has to say about the ligature; but it helps him very little, for here the clamp could never have been put on; the breaking of the ligature will happen, but can be remedied, and no valid objection can be made of the punctured vessel against the ligature, since, with all its liabilities, it was the only substitute for the impossible clamp.

I have now the satisfaction of seeing the clamp almost abandoned, and its place taken by the ligature. The reason assigned for this is the adoption of the "antiseptic system", which is thought to be incompatible with the clamp, and to render the ligature safer. Here it is worthy of notice that, while one party is putting forward the antiseptic system as the explanation of their greater success, we find, on the other hand, Mr. Lawson Tait reversing the proposition, and urging that this supposed antiseptic success is due to the coincident use of the ligature. As for myself, I am not prepared to assent to the doctrine that the antiseptic system does enable me to carry out the intraperitoneal method (ligature) with greater safety. The fact of the disuse of the clamp alone concerns me, and not the reasons given for it, regarding which there is room for difference of opinion. The only method that is truly in rivalry with the ligature is the actual cautery. Remembering the excellent results obtained with it by Baker Brown and Keith, and that Wells's cases, so far as they go, are in favour of it, and although desirous of completing a hundred cases with the ligature, I have several times made arrangements to try it; but, as often as I have done so, I have encountered a very short pedicle, with no room for the clamp between the tumour and the uterine cornu. Thus it has happened that my third case remains the solitary example of the use of the cautery in my practice. As I have already stated, no true comparison between the clamp and ligature has ever been made, nor are the data for such a comparison even now forthcoming. It is manifestly unfair to compare the results of the two methods in the hands of one who always treated the long pedicles with the clamp, and the short (and otherwise bad) cases with the ligature, as a *dernier ressort*.

I began to practise the antiseptic method in my thirty-first case, at the time when thymol was brought to our notice with a great German reputation as a germicide. My first five cases did very well with it; one was a case of acutely inflamed tumour with recent vascular adhesions, and required a drainage-tube; another was a case of sessile tumour—operation tedious, with nine ligatures; the other three were either simple, or only moderately complicated. The sixth was a case of colloid tumour in a bad subject, and ended in death on the fifth day. The *post mortem* examination revealed a slight amount of peritonitis, the presence of a small quantity of thin colloid and ascitic fluid in the peritoneum, and advanced fatty degeneration of the heart and liver. "Cheyne-Stokes' respiration" was well marked before death, and it was noticeable that the ice-cap, instead of reducing the temperature, actually caused it to rise. I have since regretted not using a drainage-tube, for it was just one of those cases in which the extra elimination provided for by it would probably have averted the fatal termination. Although I could not attribute this death to the failure of the antiseptic, but rather to the degeneration of important organs, and although the other cases yielded results which could scarcely be improved upon, I changed the thymol for carbolic acid. At first, I was by no means satisfied; for the second case (aged 51) died of acute pulmonary congestion, and

it appeared as if, by attempting to protect my patients against a presumed danger, I was exposing them to a greater risk of death from a new and recognisable cause. In fact, I could not do otherwise than account for the death by carbolic acid poisoning, or chilling effect of the spray; for the parts concerned in the operation presented no evidence of morbid action. The fourth case (aged 41) was one of very short and thick pedicle. I had great difficulty in drawing the ligatures (of which there were seven) tight enough to control the bleeding thoroughly, and did not feel quite comfortable in mind when the operation was finished. I adhered as closely as possible to all the details of Lister's method, and, relying too much on it, I omitted to use a drainage-tube. As I feared, there was some oozing from the stump, and my patient died of septicæmia on the fifth day. So far, then, I had lost one in six with thymol, and two out of four with carbolic acid; and it could not be said that my experience of the antiseptic method, especially with carbolic acid, was very encouraging. It is, however, only fair to state that all the cases were more or less complicated. Before I had completed fifty cases, I had lost three more, viz., one from obstruction of the bowel (case given in detail in last paper), one from acute bronchitis, and one from shock. As to how far the exposure to the cold spray in the two latter cases should be regarded as a factor towards the fatal result—in the one acting in the way of chill, and in the other as a direct depressant in a patient already moribund—opinions will differ. That it had some influence, I am convinced. In the former case, the symptoms of bronchitis set in as in an ordinary case of exposure to cold. The peritoneum was perfectly healthy. Thus, after the first twelve operations with carbolic acid, there were five deaths; and now, with a wider experience of its toxic and irritating effects, I cannot but rest in the belief that this agent decided the fate of at least two of the cases.

In my fifty-first case, I substituted absolute phenol for Calvert's No. 1 carbolic acid, hitherto used, and then I had a run of seventeen cases with only one death. The case, which under any conditions would probably have done badly, was shortly as follows: acutely suppurating cyst; very close adhesion of about a foot of ileum; adhesion of opposing raw surfaces producing partial obstruction; acute gastro-enteric catarrh with the formation of several ulcers, one of which, situated several inches above the injured portion, opened a way into the peritoneal cavity by a large rupture. Another series of ten recoveries was followed by a death in a case of soft sarcoma of the ovary in a young woman, aged 25, which I should have let alone as a hopeless case, but for the result of a consultation, and the urgent solicitation of the patient to have something done to relieve her suffering. An idea of the nature of the case will be obtained from the fact that a very short journey to the hospital was sufficient to raise her temperature to  $101.6^{\circ}$ , and her pulse to 140. With rest in bed, the temperature came down, but the pulse underwent very little change. The operation was long and difficult, and, the adhesions being extensive and vascular, much blood was lost. Four hours after the operation, the temperature had risen to  $101^{\circ}$ ; for the next three hours, it rose at the rate of  $1^{\circ}$  an hour; and, at the end of seven hours, the pulse was 180. The patient died in eighteen hours. *Post mortem* examination showed that the lumbar glands were extensively infiltrated, and that there was not the shadow of a chance for the poor patient. Two years previously, I had been prevented from operating by a diagnosis given adverse to my own;

TABLE of Fourth Series

The Capital letters, R. and L., in Tables

No.	Name of Previous Medical Attendant.	Age.	Condi- tion.	No. of Children.	Previous Tappings.	Date of Operation.	Adhesions
76	Dr. W. H. Platt, Kilburn	59	W.	7	0	May 14, 1879	None
77	Mr. P. Swales, Sheerness	39	M.	9	0	" 21, "	Extensive om
78	Dr. Wynn Williams	71	W.	0	0	" 24, "	None
79	Mr. Moxon, Northampton	39	M.	3	0	" 29, "	Omental (5 liga
80	Dr. Wynn Williams	53	M.	14	0	" " "	Extensive pan
81	Mr. Brooke, Langport	23	S.	0	0	June 5, "	None
82	Mr. G. A. Brown, Tredegar	50	M.	0	0	" 11, "	Broad attac (many ligatur
83	Dr. Stewart, Nottingham	49	S.	0	0	" 18, "	None
84	Dr. Browning, Rotherhithe	34	M.	0	1	July 6, "	Extensive pan
85	Dr. Campbell, Calne	35	M.	2	0	" 9, "	Parietal, om recent and v
86	Dr. Lake, Teignmouth	55	W.	0	1	" 12, "	Parietal
87	Mr. Kershaw, Middleton	49	S.	0	0	" 17, "	Omental and Fallopian tu
88	Dr. Clifford Allbutt, Leeds	22	S.	0	0	" 23, "	Parietal, om pelvic
89	Mr. Jonathan Hutchinson	40	S.	0	1	Sept. 24, "	Filamentous p
90	Dr. Tyacke, Chichester	22	S.	0	0	Oct. 1, "	None
91	Dr. Wynn Williams	35	M.	0	1	" 2, "	Parietal, om pelvic, very v
92	Mr. Ashenden, Hastings	30	M.	3	1	" 8, "	Parietal, oc (many ligatur
93	Dr. Tuckwell, Oxford	22	S.	0	0	" 9, "	Omental, pelv
94	Dr. Maclachlan, Old Kent Road	57	W.	8	0	" 15, "	None
95	Dr. Hope, Chobham	58	M.	0	0	" 16, "	Parietal
96	Dr. Burnie, Nottingham	34	M.	0	0	" 29, "	None
97	Dr. Granville Bantock	33	M.	2	0	" 30, "	None
98	Mr. Hynes, Nottingham	30	M.	0	0	Nov. 5, "	Parietal, oc (several liga
99	Mr. Alban Doran	53	M.	0	0	" 11, "	To whole ex parietes, to tine, and om (9 ligatures)
100	Mr. Phillips, Southend	23	S.	0	0	" 12, "	None

of Completed Ovariectomy.

mean respectively Right and Left Ovary.

Weight of Ovary.	Duration of Operation.	Drainage.	Result.	Cause of Death.	Remarks.	No.
8lb	25 m.	..	Recov.	..	Intermittent cardiac action. Ether.	76
8lb	45 "	..	"	..		77
1/2 lb	25 "	..	"	..		78
8lb	40 "	..	"	..		79
10lb	45 "	..	"	..		80
10lb	30 "	..	"	..		81
10lb	80 "	Glass tube	"	..	Tumour enucleated from broad ligament and mesocolon.	82
1/2 lb	45 "	..	"	..	Very short pedicle. Recovery from operation. Died from otitis 21st day.	83
3lb } 3lb }	90 "	Glass tube	Death	Exhaustion	Patient in <i>extremis</i> . Never showed signs of rallying from condition preceding operation. A "forlorn hope".	84
7lb	45 "	Glass tube	Recov.	..	Inflamed cyst. Internal rupture, with collapse, a fortnight before operation.	85
6lb	45 "	Glass tube	"	..	Double, colloid. Both ruptured.	86
7lb	50 "	..	"	..	Several fibroids in uterus. Metrorrhagia. Pedicle very short.	87
4lb	85 "	Glass tube	"	..		88
5lb } oz. }	45 "	..	"	..	Severe dyspepsia for three years. Immediate relief.	89
4lb } oz. }	50 "	..	"	..		90
4lb	90 "	Glass tube	"	..		91
10lb	45 "	..	"	..	A girl, on August 29th, 1880.	92
10lb	40 "	..	"	..	Dermoid. Pregnant (6 to 7 months) in July 1880.	93
10lb } oz. }	50 "	Glass tube	"	..	Both ruptured, colloid.	94
9lb	45 "	..	"	..	Dermoid. Hair and teeth. Fibroid of uterus as large as goose's egg.	95
8lb	35 "	..	"	..	Small fibroid in back of uterus. Pedicle very short.	96
11lb } 1/2 lb }	100 "	..	"	..		97
6lb	45 "	..	"	..		98
3lb	90 "	Glass tube	Death	Acute nephritis.	Fatty heart. Obstruction of gall-duct.	99
7lb	25 "	..	Recov.	..		100



and now, from the progress of the disease, and the reduced condition of the woman, the circumstances were much less favourable than at the previous consultation. The loss of this patient was not unexpected, but the next case was a sore trial. The patient was a married woman, aged 39. The tumour was a doubtful one, having a close connection with the uterus, but I was able to confirm my opinion in favour of its ovarian origin by the use of the aspirating needle. At the operation, I found it necessary to remove both ovaries. The left side furnished the tumour, which had large adhesions in the pelvis and to the back of the uterus; and, as I could not bring into view the source of some oozing still going on in the bottom of the pelvis, I had to close the wound, and trust to the drainage-tube. The operation lasted about an hour and a half. Within two hours after the operation, the catheter was passed, and two ounces of urine were drawn off. At the end of five hours more, the patient was unusually drowsy, the pulse was between 150 and 160, and no more urine could be obtained. At this time, the temperature was only  $100^{\circ}$ , but, in four hours more, it had risen to  $103^{\circ}$ . She lived twenty-five hours. On *post mortem* examination, the kidneys were found enormously congested, and the heart was small and fatty. There had been very little oozing, and Douglas's pouch contained only a small quantity—a fluid drachm or two—of coloured serum, which had collected there between the last dressing and her death. If there were any room for doubt as to the part played by the carbolic acid in the preceding cases, there could be none here; but I was destined to meet with still more convincing evidence. Fourteen days after this, I operated upon a patient, aged 55 (No. 72), who had been tapped ten times before she came under my hands, and whom I had again to tap in order that she might get a few nights' rest, by relief of the dyspnoea under which she laboured. The tumour, which consisted of a few small cells and one large cyst holding about fifty pints of fluid, adhered to the parietes from a little above the pubes to the ensiform cartilage and diaphragm, and from flank to flank. There was also an adhesion of omentum to the extent of several inches. The operation lasted only thirty-five minutes. The same train of symptoms set in, and it was only by the most heroic measures, such as the wet ice-pack, etc., that she was pulled through. In about seven hours, her temperature was  $107.2^{\circ}$  in the vagina. She was restless, only semi-conscious, and becoming comatose. There was then complete suppression of urine. (This case has since been published in detail in my paper "On Hyperpyrexia after Listerian Ovariectomy", read before the Royal Medical and Chirurgical Society on December 14th last.)

These two instances, occurring in such close proximity, seriously attracted my attention, and I could not but attribute death in the one case, and the very grave symptoms in the other, to the action of the carbolic acid. I had for some time been under the impression that a solution of carbolic acid, which, by its action on the epidermis, kept my hands in a constant state of desquamation, and which, in a great measure, destroyed delicacy of touch, was not unlikely to be attended with some risk when applied freely to the peritoneum, and especially to a large extent of raw surface. To independent lookers-on, the evil was plain enough, and I must say that I was forewarned of what would happen. At that time, I was using the standard solution of 1 in 40. With the view of obviating these unpleasant results, I resolved to reduce the solution to 1 in 50; and I soon found that, as far as my hands were concerned, this was an improvement. It remained to be

seen what the effect would be on my patients. Until a recent period, when I still further reduced the strength to 1 in 60,\* I continued to use this solution; and the table now shows the result. It will be seen that of these twenty-five cases with the diluted solution, eight were so severe as to require drainage. Of these, two died. The first was a very severe case of double ovariectomy in a patient greatly emaciated, and already *in extremis* after tapping, with a temperature of 101°, and a pulse of 140. The smaller tumour was pushed down into the pelvis to such an extent, that the perinæum was depressed as by the advancing head of a child in the second stage of labour; and, chiefly for this reason, operation had been refused three months previously. Shortly before coming under my notice, she had been tapped, and she was now the subject of irritative fever. The operation was a severe one, in consequence of adhesions, and the breadth of the pedicle, especially of the larger tumour. She died on the fourth day, without having shown any signs of rallying. Such a case says nothing for or against the method. The other patient had a fatty heart, got obstruction of the gall-duct from calculi after the operation, and, later on, acute nephritis; and she died at the end of four days. Here, again, the nephritis appeared to be the immediate cause of death, as it was not till the urine had become scanty that serious symptoms set in. Before death, the urine was totally suppressed.

It is not my intention in this paper to enter at greater length into the question of carbolic acid poisoning, as the subject would occupy too much space. But it will probably be expected that at a time when "Listerism" is the question of the day, I should express my opinion on the value of this method in reference to ovariectomy. And lest it should be thought that I have not sufficient data, I may here state that my present views are based on the results of nearly one hundred cases of such operation done under this method, either without or with material modifications. It is necessary to a right understanding of my position, that I should indicate in what manner and to what extent I have departed from the practice of Mr. Lister. I presume it will be proper to ignore, or at least to relegate to the class of non-Listerian cases, the six cases which I treated with thymol, inasmuch as Mr. Lister regards this substance as non-antiseptic, or at least as unworthy of confidence. I began the carbolic acid, then, in my thirty-seventh case, and I adhered to all the details of the method with this exception, that I did not use the "protective". Preferring the dry system of treating wounds, from which I had seen such excellent results, and especially in the preceding six cases under thymol, I placed a pad of thymol gauze between the skin and the carbolic gauze. The results in the healing of the wound were, from the beginning, so satisfactory, that I continued this practice to a recent period, when it gave way for a time to a series of experiments. What particularly enamoured me of this gauze, was its eminently absorbent and non-irritating quality, as compared with the carbolic. At first I employed only a four-fold pad, about one and a half inches wide, followed by carbolic gauze; but the frequent occurrence of a pustular eruption, which in some instances was really troublesome, induced me to extend its use by interposing a single piece between the skin and carbolic gauze over the whole abdomen. I have seen nothing of the kind since. I presume no objection can be made

\* Since the above was written, I have gradually reduced the solution to 1 in 80, in 100; in the last three cases of ovariectomy, to 1 in 150; and in one of oöphorectomy, I have dispensed altogether with the Phenol, using plain water. I have every reason to be satisfied with the result.

to my testimony on the ground of such a modification, and I may observe that Mr. Spencer Wells, who has given in his adhesion to "Listerism", has, I believe, never used anything but thymol gauze. Nor will it be urged, that the absence of the "protective" reduces my cases to the class of non-Listerian, inasmuch as some of his most ardent followers have now abandoned it in favour of some form of dry dressing. At first I used to change the dressings under the spray, but after a time, my common sense rebelling against a double waste of time and material, I discontinued the practice, and not only have I seen no cause to regret doing so, but I believe it has been positively advantageous; for the dry method is thus rigidly adhered to; the patient is saved from the risk of a chill; and the wound is not exposed to the irritation of the carbolic acid, so fatal to the healing process. If, at any time, from imperfect adaptation of the edges of the skin, it has been necessary to depart from the dry treatment, I have been content to wash the wound with a weak solution of sulphurous acid (1 in 20), before re-applying the thymol gauze. This acid possesses the advantages of equal, if not superior, antiseptic and disinfecting properties, and the absence of any irritating quality. Under this system of dressing, I have exhibited wounds at all stages, from a few days to three weeks after operation, and if a total absence of suppuration or even irritation be taken as the test, then I undertake to satisfy Mr. Lister. In other respects, I observed all the details of "Listerism" up to the period already indicated, viz., my seventy-second case.

There can be no doubt that we are all very much under the influence of first impressions. Thus, with a particular, and especially a new method of treatment, a case turns out favourably. This case, from the novelty, makes a great impression; the recollection of previous cases is obscured, and future observations are modified by this impression. In this way I suppose it happened, that I allowed myself to imagine ovariectomies done under "Listerism" presented a less degree of pyrexia than the others. Moreover, when one is continually hearing or seeing it stated that one of the greatest characteristics, if not the greatest, of this method, is the absence of fever, it is not to be wondered at that one who has failed, from want of leisure or any other cause, to make a close and comparative investigation of all the details of his cases, should glide into the same way of thinking. Not long ago, in conversation with a Listerite (and I use this term in a complimentary sense) I asked him to show me a chart of what he called a normal temperature, after an antiseptic operation. As I anticipated, from my own experience, I found the curve reaching as high as 100.4°. I presume this is what Mr. MacCormac means when he says, "with very few exceptions, antiseptic operations were followed by a very slight (if any) rise in temperature". How then does it happen that Volckmann has had to invent a new name for that state of pyrexia, which in his experience so frequently follows antiseptic operations? He calls it "aseptic fever". Now, I have taken the trouble to go over the first thirty-six cases, and to compare them with the same number, from thirty-seven to seventy-two, done antiseptically, the exact number done by me under strict Listerism, with the exception of the modifications as to dry dressing already explained. The first and most striking fact, and one that was startling to myself, is that the lowest temperature curve is found in a non-"antiseptic" case, in which it never passed beyond 99.6°; and the highest in an antiseptic case in which it reached 107.2° (No. 72 already referred to). Nor can it be said that the non-antiseptic case was of the

most simple kind. The tumour weighed thirteen pounds ; there were several pints of ascitic fluid in the peritoneal cavity, and the intestines were considerably injected ; the pedicle was secured by a double ligature of hemp, but the abdominal sutures were of silkworm gut. Comparing the clamp and ligature non-antiseptic cases, the former show the highest temperature, in one reaching as high as  $103.8^{\circ}$ .

In order to institute a fair comparison between individual instances, I have selected cases as nearly alike as possible, viz., as regards the age of the patient, and the nature of the operation. I have been able in this way to bring together eight cases in four sets of two each, and I have placed the register of the corresponding cases on the same chart. I am well aware that this cannot be held as convincing evidence one way or the other, for patients of the same age, and undergoing operations of the same severity, may yet differ in the tendency to disease. But I give it for what it is worth, and the second fact brought out is, that in three of the sets, the advantage is slightly in favour of the non-antiseptic cases, and that in the fourth, the advantage is greater by more than two degrees.

Then, not only were the cases in the first series of thirty-six worse in character, but they were treated with the disadvantage of my want of experience. Thus, among them the drainage tube was used in ten cases, while in the second lot it was used in eight cases only. This is a test of the comparative severity of the two series, and is in favour of the second ; for in my earlier cases I did not resort to the tube as often as I did in my later, and it was not till I had lost a patient, under circumstances in which I would now consider it indispensable, that I saw the importance and value of this instrument. After all, notwithstanding the much greater severity of the first series, the mortality is only 1 more, or as 9 to 8. One would naturally suppose that, under the circumstances, the first would show a much higher range of temperature than the second. Taking the average temperatures for the first four days, I find the difference is only four-tenths of a degree in favour of the antiseptic and milder cases. This cannot be said to be an important difference, nor such as we had been led to expect.

Still further, of the nineteen cases of recovery, without the drainage tube, in the first series, the ice-cap was used for the reduction of temperature in six cases ; of these, the three clamp cases furnished two. Of the same number of recoveries in the second series, the ice-cap was used in five. The latter were, however, all ligature cases. While, then, the antiseptic cases yield an average temperature, four-tenths of a degree lower than the non-antiseptic ; they, on the other hand, include a case unequalled in the severity of the pyrexia, which is clearly attributable to the treatment. It might be thought that the exclusion of this case would leave a much lower average temperature in the other thirty-five cases, but this is not so, as the average temperature for the four days was only  $101.2^{\circ}$ .

There is another matter not to be overlooked. I have stated that after my seventy-second case, I reduced the solution to one in fifty. Latterly I have still further diluted it, viz., to one in sixty. Now, comparing the temperature curves, I find a still further improvement in the latter cases, along with a generally diminished mortality, the mortality such as it is being due for the most part to non-preventible causes. Thus from the seventy-third to the one hundred and twentieth inclusive, I have used the ice-cap in only four cases ; in the last thirty cases only twice ; and of these, in one it was required by car-

bolic poisoning.\* Thus the reduction of the solution to one in sixty, though giving better general results, has not saved me from this accident, regarding which, in the latest instance, the evidence is most complete.

There are two important deductions from these facts.

1. In similar cases the "antiseptic system" in my hands has not yielded a lower temperature curve.

2. The reduction of the strength of the carbolic acid solution has been attended with a proportionate lessening of the temperature curve and a decrease in the rate of mortality.

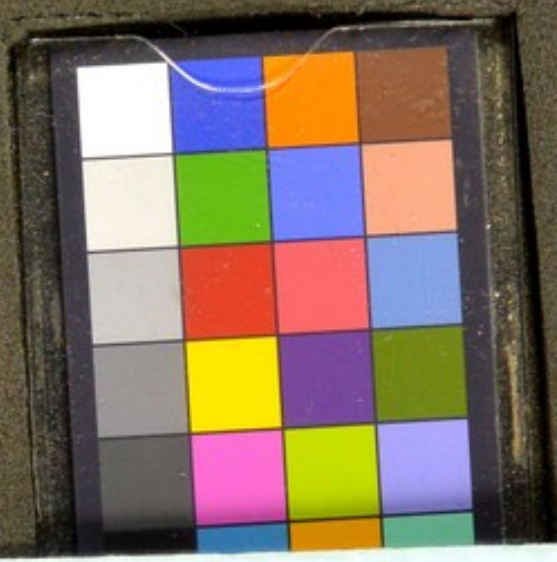
The length of this paper forbids me to add much more. A few words, however, are needed. I claim to have demonstrated that Listerism may be considerably modified, to such an extent, indeed, as no longer to secure the acknowledgment of its author, not only without worse, but with better results. How far this modifying process may be carried remains to be seen. Whether we shall arrive at the contradictory position of Thiersch, who, believing "Listerism" to be theoretically perfect, yet prefers to use salicylic acid instead of carbolic acid, as less irritating, etc.; or like Galezowski, raise a laugh at the whole system by vaunting the virtues of a solution of carbolic acid containing a thousandth part; or, steering that middle course which is proverbially the safest, settle down to a modified practice, something in the form of a purifying irrigation, the result of experience tempered by common sense, the future alone can determine. That the carbolic acid in the proportion and in the manner prescribed by Lister is a necessity few will now contend, any more than they can dispute the disadvantage of the chill from the spray in ovariectomy; but that the cleanliness which is thus attainable, viz., by the spray and frequent ablutions, plays an important part, few will deny. The "antiseptic system" may not irreverently be called "the gospel of cleanliness", for where there is perfect cleanliness there can be no septic process. I have faith in the virtues of cleanliness, but I do not believe in the specific antiseptic properties of carbolic acid thus applied. "The practice has been founded on a theory which has not been proven, and is probably not true". Though I fear this explanation will be far from satisfying the originator of the system, yet I cannot but believe and feel that, if there be found more truth in it than he would be now willing to admit, Mr. Lister will still hold a place amongst one of the benefactors of his age.

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\* Since the reduction of the solution to 1 in 80, I have used the ice-cap only once in thirty-one cases for a few hours. Even in this, it was scarcely necessary, for the temperature had just begun to fall when it was put on.

*12, Granville Place, Portman Square.  
May 1880.*





TABLE(S)  
RUN INTO  
GUTTER

