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THE

PATHOLOGY AND TREATMENT

OF

DISEASES OF THE OVARIES

(Being the Hastings Essay for 1873)

BY

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HONORARY FELLOW OF THE AMERICAN GYNECOLOGICAL SOCIETY.

Fourth Edition, Re-written and Greatly Enlarged

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TROW'S
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DEAR DR. MARION SIMS,

I Dedicate

THIS VOLUME TO YOU AS A TOKEN OF THE VALUE I PLACE UPON YOUR FRIENDSHIP, AND AS AN ACKNOWLEDGMENT THAT MUCH OF THE NEW WORK DESCRIBED IN IT IS THE OUTCOME OF YOUR INGENUITY.

YOURS TRULY,

LAWSON TAIT.

PREFACE TO THE FOURTH EDITION.

Since the first edition of this book appeared in 1873, my experience of the subjects it deals with has greatly increased, and necessarily the present work has grown in size.

I have found reason to change my opinion on some points, but they are of much less importance than I thought they would be when I first sat down to write these pages. I have seen far more reason for extending what I had to say; for the marvellous success which now attends the efforts of those who practise abdominal surgery has fallen largely to my lot, and therefore records of things accomplished, and opinions expressed upon them, will be found in these pages, which, eight years ago, were certainly beyond the limits of acceptance. Since then, however, great advances have been made, but much yet remains to be done, and all must rest upon the patience of the workers and the forbearance of their critics.

For many imperfections of my book I have to offer the apology that I have but scant leisure for research, and fear I may not have done justice, in many instances, to the work of others. My writing is chiefly the outcome of my own experience, and of necessity is freely and unavoidably scattered with the pronoun of the first person.

To avoid needless and cumbersome reference notes, I have given, at the head of each chapter, a list of works which I have consulted on the subjects dealt with.

LAWSON TAIT.

Birmingham, *October* 3, 1882.

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THE PATHOLOGY AND TREATMENT

OF

DISEASES OF THE OVARIES.

CHAPTER I.

ANATOMY AND PHYSIOLOGY OF THE OVARY AND OVIDUCT.

An Extra Ovary. Société de biologie. Paris, 1875. Dr. De Sinéty.

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Zur normalen und pathologischen Histologie des Graafschen Blaschens des Menschen. SLAWIANSKI. Virchow's Archives. Band LI.

WITHIN the last ten years there has grown up a taste—perhaps I had better say a fashion, lest it should not prove permanent—for popular instruction in biology, and this tendency has had results of the best kind. Among others, it has enabled us to teach women, even girls, a great deal which deeply concerns their welfare, in a way which cannot, or, at least, ought not to offend any. It must ever be regarded as a misfortune that the most important functions of life, those of reproduction, and the most important relations of society, those of marriage, have usually been shrouded in mystery and darkness, have been wilfully misrepresented to the inquiring and innocent mind of youth, and have been left entirely for their solution to the personal speculations or experiment of each adolescent.

Now, by simply telling the life-history of a flower, and by the gentlest hint that what is true there is true all through life up to its highest developments, we may convey all the instruction that is needed, and all that is demanded in the interests of humanity. Teach a child the functions of the anther, the stigma, the pollen, the ovary, and the seed-capsule; let him or her see the conjugation of the *spirogyra*, and the child will be armed with a knowledge which will do much to prevent mischief both moral and physical.

Between the simple mass of protoplasm enclosed in a structureless capsule of cellulose, which forms the ovum of the *alga*, up to the complex ovum of the mammal, with its vascular follicle, there is a marvellous difference in elaboration of detail, but no difference in principle. The cellulose capsule is ovary and uterus in one, and the conjugating buds are at once vagina and oviduct; and from this simplicity the complexity arises only from specialization of structure, and not from the introduction of anything new in principle.

In the *algæ*, and in many other instances even in animal life, as the *aphides*, we have two principles of reproduction, or rather of continuation: the first is the production of the *zoöspore* (swarm-spore), which is effected without the conjunction of two cells, and of which we have the solitary trace, in mam-

mals, of the so-called dermoid tumor of the ovary; the second method is the formation of the *zygospore* (resting-spore) by the conjunction of two elements, male (pollen-grain, antherozoid, or spermatozoid) and female (ovule, oöspore, germ-cell, germinal vesicle), and with that process alone the human ovary has to do in its complete function. It must not be forgotten, however, that the zoöspore and the female part of the zygospore are essentially the same, that their fundamental functions are exactly the same, and that the properties introduced by the addition of the sperm-cell seem rather to be an extension of those already existing than the creation of new ones. How far this analogy is extended to mammals, and especially to man, and how far it has been curtailed, is one of the most interesting questions of biology; and yet it is one upon which we have as yet absolutely no information.

Much unnecessary confusion has been introduced into physiological writing and teaching by the use of different names for the same thing in different places. I must here repeat the protest I have frequently made, in my lectures on biology, against this practice, and my prediction that the whole of our nomenclature will have to be revised and this confusion reduced to order. For example, why should the male element be called a pollen-grain in the foxglove, an antherozoid in an alga, and a spermatozoid in a mollusk? It would be much better to call it an antherozoid in every instance, and still better would it be to drop our old-fashioned names, as *Graafian follicle, discus proligerus,* in human anatomy, and give to these structures names like *oögonium,* which would indicate their common and real biological significance.

It is, I fear, beyond my power to introduce such a reform, yet in the following pages I shall do my best to make such terms more familiar to the purely medical reader.

It is wholly impossible to discuss the pathology and treatment of the diseases of a structure like the ovary without a full understanding of its anatomy and physiology; and here we enter upon a field vast and as yet unexhausted. During the last twenty years perhaps no organ in the body has been so much written about as the ovary; yet much remains to be told, and still more to be discovered. To the naked eye nothing could look more uninteresting and unimportant than a human ovary; and yet upon it the whole affairs of the world depend. As far as the individual owner of the gland is concerned—certainly for her comfort, and, if we take with it its appendages, for her life as well, it is the most important organ in her body.

The descriptions given of the rough anatomy of the organ

coincide, of course, closely enough; but between those of its minute structure, its development, and the processes carried on in it, there is considerable diversity of opinion.

From 1870 to 1875 I was much engaged in these investigations, but since then I have been too much engaged in practice to follow them out as fully as has been done by others, more particularly by Mr. F. M. Balfour, the distinguished embryologist. With his conclusions and descriptions, my own work, so far as it has gone, most closely agrees, and therefore, in this part of my subject, I am greatly indebted to his papers for my descriptions; and while I do not desire to depreciate the efforts of other workers, I am bound to say that Mr. Balfour gives by far the most consistent and complete results.

The ovaries are, like most other organs in the body, bilaterally symmetrical; that is, they are similarly situated, one on each side; yet here the usual rule of differences occurs, for I never have seen two ovaries from the same person exactly alike in situation, size, shape, or appearance. Infinite variety in all such details are to be observed, and any description can only be one which is applicable to a particular instance, or one of the average appearances.

The size of the ovaries varies with the different periods of life; and, to a less extent, so does their distance from the uterus. Henning's table of measurements is given below, the chiefly noteworthy fact given there being that the ovary is largest in the first six weeks after parturition. This may have been due to some pathological condition in those examined; but in connection with this it is curious to note the statements of horse-breeders, that a mare is more readily impregnated soon after the birth of a foal than at any other time.

Henning's Table of the Size and Position of the Ovaries at different Periods of Life and in various Social Conditions, in Centimetres.

		Childhood.	Virgins.	Unchaste.	Married.	Multipara.	Puerperal.	Widows.	Divorced.	Menopause.	Old Age.
Length of the ovary.	Right..	1.3 to 3.2	3.8	3.4	3.0	2.5	4.4	3.5	3.5	3.1	2.9
	Left...		3.7	3.8	2.8	2.4	5.5	3.2	3.1	2.5	2.7
Breadth " "	Right..	0.2 to 1.4	1.9	1.8	1.7	1.2	1.3	1.6	1.4	1.5	1.1
	Left...		1.5	1.7	1.5	1.2	1.4	1.7	1.4	1.4	1.0
Thickness " "	Right..	0.2 to 0.6	1.0	0.9	1.0	0.8	0.8	0.8	0.9	0.8	0.8
	Left...		1.0	0.9	0.9	1.1	0.9	0.8	1.0	0.8	0.9
Distance from the uterus	Right..	1.0 to 4.0	3.4	4.4	4.7	5.5	8.0	3.8	4.0	4.0	4.0
	Left...	1.2 to 3.7	3.3	4.5	4.7	5.0	7.0	4.2	4.2	3 7	4.5
No. of cicatrices	Right..	0	6	14	21	22	8	24	17	15	14
	Left...	0	9	13	21	21	8	26	18	24	11

The color of the ovaries when perfectly healthy, and in the living subject, is of a pinkish, pearly hue, with here and there a hazy blueness showing through the tunic when a follicle is either getting ready for the discharge of its nucleus or is disappearing after having fulfilled its function. When a follicle is either about to burst or has just burst, the site is of a purple-brown color. The glands are oval in shape, and flattened from before backward, the anterior surface being shorter and less convex than the posterior, which is more rounded. The outer extremity is also rounded and bulb-like, whilst the inner is somewhat pointed and thinned off into the broad ligament. By these appearances the ovary of one side may be recognized from the other, if the glands are healthy. The average weight of the ovary is about ninety grains (Farre).

The glands are usually situated on a level with the inlet of the true pelvis, behind the Fallopian tubes and round ligaments. Looking down upon the broad ligament from above, whilst it is on the stretch, it may be seen to be formed of three folds, of

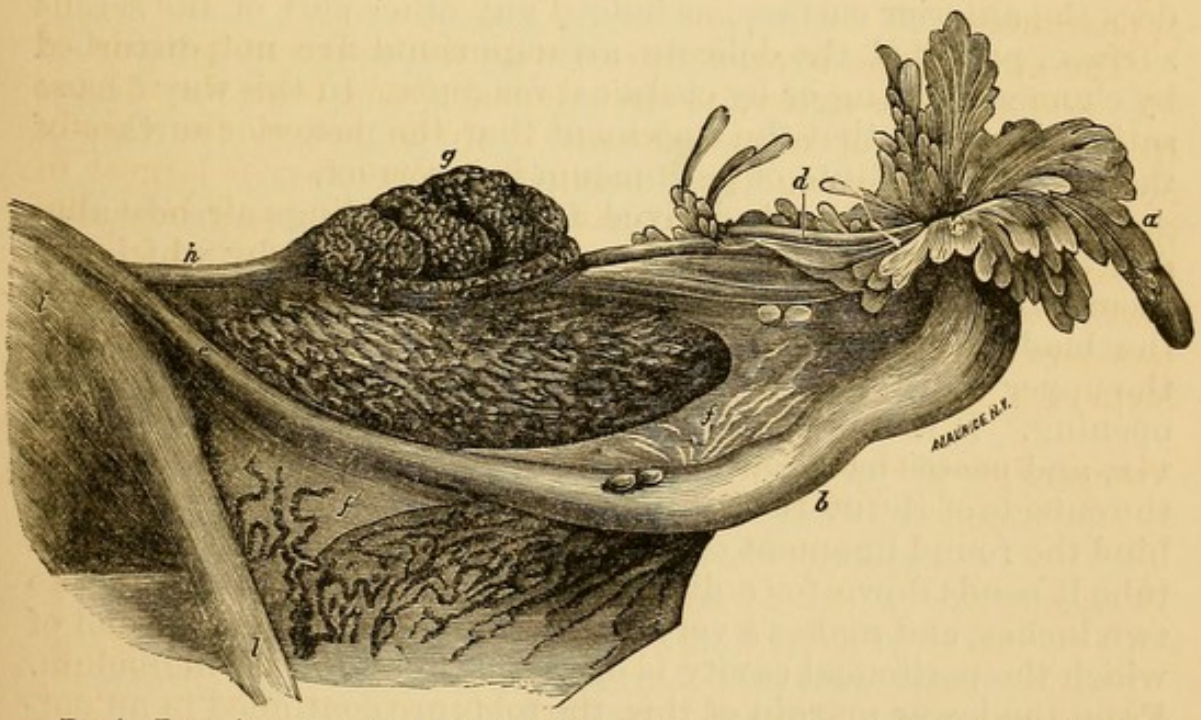


FIG. 1.—Front view of left broad ligament (after Richard): *a*, pavilion and fimbriæ; *b*, body of the tube; *c*, opening of infundibulum; *d*, tubo-ovarian ligament (one of the fimbriæ); *e*, uterine end of tube; *f'*, meso-salpinx; *g*, ovary; *h*, utero-ovarian ligament; *i*, fundus uteri; *l*, round ligament; *h*, *d*, *b*, and *l* are the three points of folding of the broad ligament.

which the ovaries occupy the posterior, the Fallopian tubes the middle, and the round ligament of the uterus the anterior, all these structures being enveloped by folds of the peritoneum in the same way as is the uniform distribution of this most interesting serous membrane.

Recent German writers, especially Waldeyer and Leopold, have asserted that on the posterior surface of the ovary the peritoneal layer does not exist. If so, it has become incorporated with the underlying coat, the tunica albuginea of after-life, for it must obtain a peritoneal covering during its developmental transition. If we consider these facts for a moment, we must conclude that the ovary must be enveloped by both an anterior and posterior layer, just as a piece of small intestine is, for the mesovarium always has two distinct layers.



FIG. 2. — Diagrammatic section of broad ligament: O, ovary; B, Fallopian tube, showing meso-salpinx; D, mesovarium.

Although in the after-life of the gland this posterior layer cannot be removed by the scalpel, it is represented by a layer of squamous epithelium, which covers the whole surface of the gland.

I have undertaken a special research on this subject, and find that the posterior surface, when treated by silver and other staining methods, displays the same stomata and stigmata as does the anterior surface, or indeed any other part of the serous surface, provided the delicate arrangements are not disturbed by clumsy handling or by chemical reagents. In this way I have satisfied myself that the statement that the posterior surface of the ovary is destitute of peritoneum is incorrect.

The broad ligament, derived from the foldings already alluded to, is composed of a process of peritoneum by which the membrane, leaving the anterior abdominal wall and the base of the bladder, bends upward over the fundus of the uterus and the upper margin of the Fallopian tube, as far outward as its opening. It then dips down behind the uterus as far as the cervix, and passes backward and upward over the rectum. Just to the outside of the uterus it bends upward, over, and then down behind the round ligament of the uterus. Then over the Fallopian tube it bends down for a distance varying from half an inch to two inches, and makes a very distinct meso-salpinx, at the end of which the peritoneal cavity is opened into by the infundibulum. From the lower margin of this, the folds are continued in an outward direction to the lateral parietes. From the posterior surface of the meso-salpinx the posterior fold bends upward over the anterior surface of the ovary in very many instances, though in others it passes straight over the gland from its upper margin on to its posterior surface—in such cases no mesovarium being found.

In a few exceptions I have seen a crescentic double fold of the posterior layer of the broad ligament pass down behind the

ovary, covering it like the hood of a "Nepenthes" gland. In all such cases the women have been sterile, probably because this hood has prevented the application to the ovary of the opening of the oviduct. I have seen this arrangement give great trouble in the removal of small ovaries.

From the lower margin of the ovary the peritoneum passes downward to the flexure of the recto-uterine cul-de-sac. Between these two folds, besides the tubes and the ovaries, are to be found the parovarium on the outer side, the utero-ovarian ligament on the inner side, and some irregular and faintly marked bundles of muscular fibre, besides a quantity of loose connective cellular tissue.

Behind the right ovary lies the small intestine, and behind the left is the rectum—a fact of great importance in some of the pathological features of the gland. The ovaries, the parovarium and the Fallopian tubes, and the vessels which supply them, lie outside the peritoneum really, and this also is a most important fact in their various diseases. The blood-vessels are the utero-ovarian and ovarian arteries and veins—the former derived from the internal iliac vessels, the latter from the aorta and vena cava. These latter vessels possess so much practical interest that a few words of special description must be given concerning them. The arteries, which are the homologues of the spermatic arteries in the male, arise from the aorta just below the renal branches, and pass obliquely downward over the psoas muscle. When they reach the brim of the pelvis they turn inward and forward (centrally toward the middle point), and run up to the ovaries between the folds of the broad ligament. They give off branches to the Fallopian tube and to the side of the uterus, where they anastomose freely with the branches of the uterine arteries derived from the internal iliac.

The veins have an analogous distribution. They arise from a venous plexus lying below the ovary and between it and the uterus—the so-called bulb of the ovary (Rouget), which has a free communication with the venous plexus at the side of the uterus. From this the ovarian veins have a direction corresponding with that of their arteries, with this important distinction, that the vein on the right side enters the inferior vena cava at an acute angle, and on the left side the vein joins the renal vein at a right angle.

It has long been known that, in the male, varicocele is much more frequent on the left side than on the right, and the explanation usually given of it was the pressure which is, or may be, exercised on the left spermatic vein by a loaded rectum. A much more exact explanation has resulted from a careful study

of these veins by Dr. J. H. Brinton, of Philadelphia. His deductions from a carefully made series of inquiries result in the following:

1. That the causes hitherto assigned are insufficient to account for the rare occurrence of varicocele on the right side.

2. That the cause of this non-occurrence is to be referred to the existence of a very perfect valve, hitherto unnoticed, at the termination of the right spermatic vein in the vena cava.

3. That no such valve exists upon the left side, at the junction of the spermatic with the renal vein.

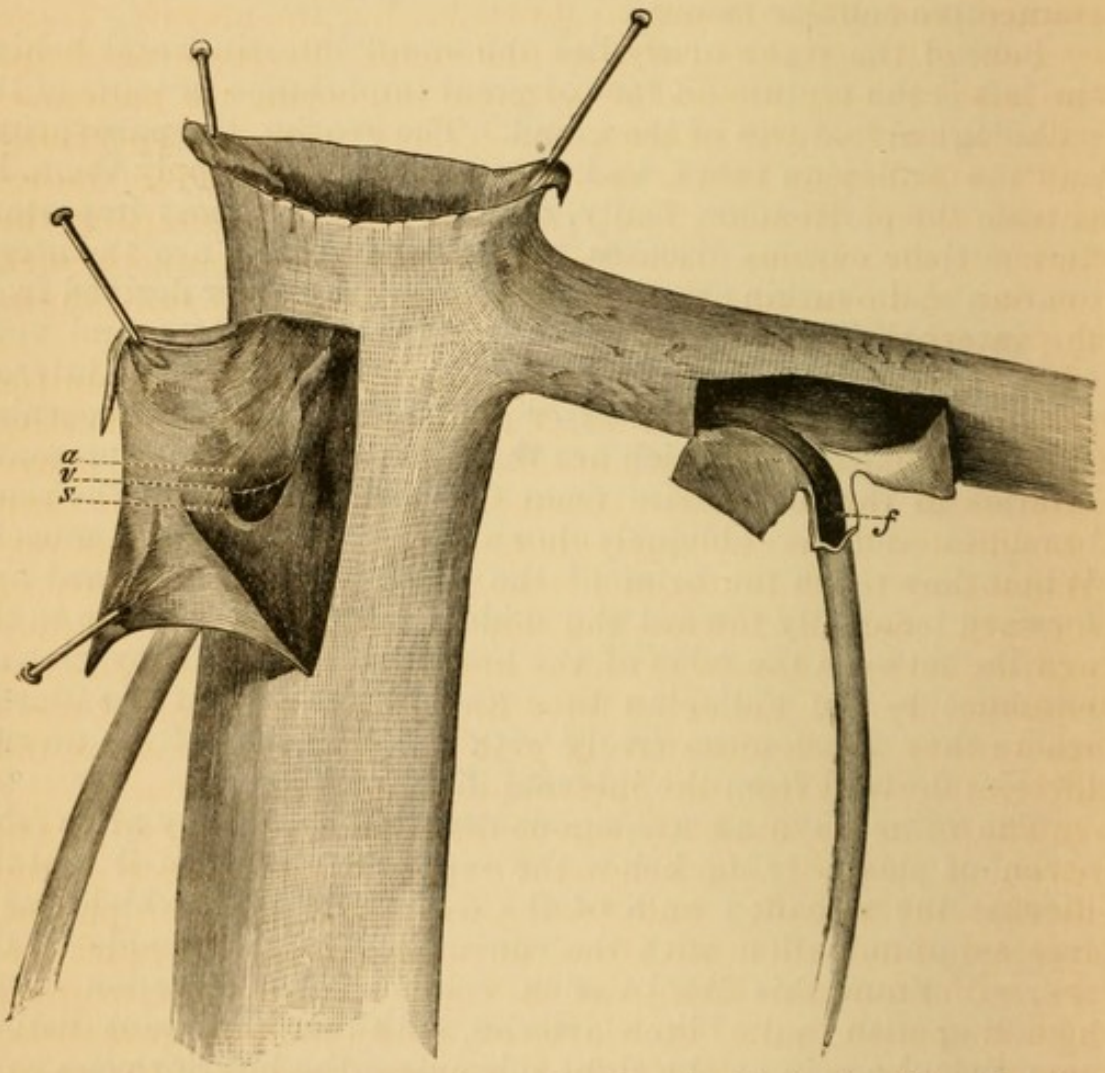


FIG. 3.—Dissection of the vena cava, emulgent and ovarian veins, showing the right ovarian valve: *a*, right ovarian vein; *f*, left ovarian vein, without valve; *v*, valve; *s*, sinus in front of valve.

4. That a similar valve exists in the analogous vein of the female—the right ovarian vein—but that there is none on the left side. (See Fig. 3.)

In this I think there is only one error to be noted: that is, that Dr. Brinton believes these valves have not before been no-

ticed. This is not correct, for in the third edition of Gray's "Anatomy" (1864), now before me, it is distinctly stated that the spermatic veins have valves. Dr. Brinton's merit consists rather in showing that the left vein has not a valve, whilst the right vein is provided with one.

The physiological fact we have further to bear in mind concerning these veins is that during pregnancy they increase enormously in size. These facts explain those distressing cases of chronic ovaritis and ovarian hyperæmia which often start after a first confinement, and which are most frequently characterized by the greatest suffering being in the left ovary, which is always enlarged, and often dislocated down behind the uterus. These cases are often so intractable as to demand the removal of the ovaries as the only method of permanently curing the patients.

The nerves of the ovaries are derived from the spermatic plexus, which in its turn is derived from branches from the renal and aortic plexus. The spermatic nerves accompany the arteries to the ovaries. The Fallopian tube has a special branch from one of the uterine nerves—in this, as in other details, showing its method of development.

Dr. Elischer, from investigations made in the laboratory for embryological research of Prof. Michalkovics, in the University of Buda-Pesth, has satisfied himself that the nerves of the ovary in mammals enter into the substance of the organ in the form of medullated fibres accompanying the looped and tortuous vessels that pass to the hilum, and run also in the proper ligament of the ovary. Some of the fasciculi branch dichotomously till they reach the follicular layer of the periphery, where they lose their medullary sheath, and form loops around the follicles. Others form a coarse plexus around the vessels. The more mature the follicle—that is to say, the thicker the membrana granulosa—by so much the more distinctly can a still somewhat coarse plexus be seen of tolerably thick nerve-fibres in the substance of the follicular investment; and from this plexus another plexus, composed of more delicate fibres, forming a more elongated network, with numerous knots and varicosities, can be seen to arise, which is applied to the outer layer of the membrana granulosa. Some of the branches, he thinks, penetrate the cells of the membrana granulosa, and run up to the nucleus. He recommends the ovary of the sheep as the object best adapted for investigation.

Besides the normal pair of ovaries, accessory glands—or perhaps, to speak more correctly of most of the cases, separated cotyledons—are met with. I have not seen an example of these, and my description is taken chiefly from the observations of Herman, De Sinéty, and Beigl.

Herman was, so far as I know, the first author to notice these interesting structures, and he describes the appearances in the body of a newly born child, where, on the border of one of the ovaries, there was a small, pedunculated body, which proved to be composed of normal ovarian tissue, with its follicles and epithelium, and having in the centre an ovum with its *macula germinativa*. Dr. De Sinéty rather spoils his interesting observation by suggesting that his case is especially noteworthy from the probability that, if the patient had lived, she would have been the subject of extra-uterine pregnancy. This is, of course, nonsense; but if she had had both ovaries removed for disease, she might have gone on menstruating if this adventitious structure had been left; and it is possible that some of the cases of continued menstruation, after the removal of both ovaries, may have their peculiarity from some such cause as this. Similarly she might have been the subject of a third ovarian tumor.

Dr. Beigl has found similar structures eight times out of three hundred and fifty examinations. They were always situated at the hilum of the ovary, at the line of demarcation of the peritoneum (Waldeyer's line), and they varied in size from that of a hemp-seed to that of a small cherry (about 8 mm.). They generally were set upon slender pedicles, and as many as three were found associated with the same ovary. The subjects in which they were found were of all ages, and the substance of the structures was true ovarian tissue.

Waldeyer has described one instance in which as many as six of these additional or accessory ovaries, as he calls them, were found; but he regards them, in some instances, as outgrowths from the ovary in the later stages of its development. He names them "Nebeneierstöcke," which is evidently open to the objection that German writers have already applied that name to the parovarium. In doing so they are, however, mistaken, and I think Waldeyer right upon this point.

These accessory ovaries show, by the active growth of their follicles, that they have a distinct physiological importance.

Before entering upon the difficult and complex subjects of the development and minute structure of the ovary, a few words must be said upon the oviduct and that representative structure, the parovarium, as both of these have great importance in the diseases which truly belong to, or which may simulate the true diseases of the ovary.

In some of the lower orders of fishes (ganoid) the ovaries shed their ova, as soon as they are ripened, into the peritoneal cavity, whence they escape by the abdominal pores, to be fecundated

outside by the shed sperm of the male, as in all fishes. In such cases there is free communication between the peritoneal cavity and the outside water. In higher orders the ovaries are tubular glands, the tubes being continued, as oviducts, to the outside, opening above and behind the anus. In all other vertebrata there is a break between the oviduct and the ovary; and the higher we go in the animal scale, the more complex does this oviduct become, till we get to the marsupialia and mammalia, when a part of it is specialized for the retention of the embryo till it is less or more ready to maintain an independent existence.

At an early period of embryonic life in the mammal the primordial kidneys (Wolffian bodies) are each symmetrically provided with a duct—the Wolffian duct—which passes backward along the outer side of its corresponding gland, and opens posteriorly into the sac of the allantois.

Somewhat later another duct appears on the anterior surface of each Wolffian body, but remains throughout its whole extent distinct from this gland, and never functionally connected with it. Traced backward from the gland it soon comes in contact with the Wolffian duct, and together they form the genital cord. The Müllerian duct opens at its anterior extremity into the pleuro-peritoneal cavity, and posteriorly into the sac of the allantois. In the male the Wolffian ducts persist, and ultimately form the *vasa deferentia*, whilst the Müllerian ducts atrophy, with the exception of a small portion, which persists as the *vesicula prostatica*, or male uterus.

In all animals but the didelphous and monodelphous mammalia, the Müllerian ducts undergo no further modification of any great morphological importance, save in birds, where the right duct is atrophied at an early period and the left only is developed. But in the monodelphous mammalia the two ducts become united at a short distance from their posterior openings, and then, by the disappearance of the coalesced wall, form a vagina with two uterine openings; or, by a further coalescence, form a single vagina and a single uterus, into which two Fallopian tubes open, these tubes being the survivals, in the higher mammals, of the two Müllerian ducts, retaining their openings into what was the pleuro-peritoneal cavity before its division by the diaphragm. In some didelphous mammalia the two tubes remain separate throughout their length, giving two vaginæ, two uteri, and two Fallopian tubes, and instances of all the varying conditions found in antecedent animals are found occasionally in women as reversions.

In female mammals the Wolffian ducts disappear almost entirely in most species, being permanently and constantly repre-

sented only by the apparently functionless organ of Rosenmüller (Figs. 4 and 5). When further survivals of them persist they are known as the canals of Gaertner, which in a few mammals,

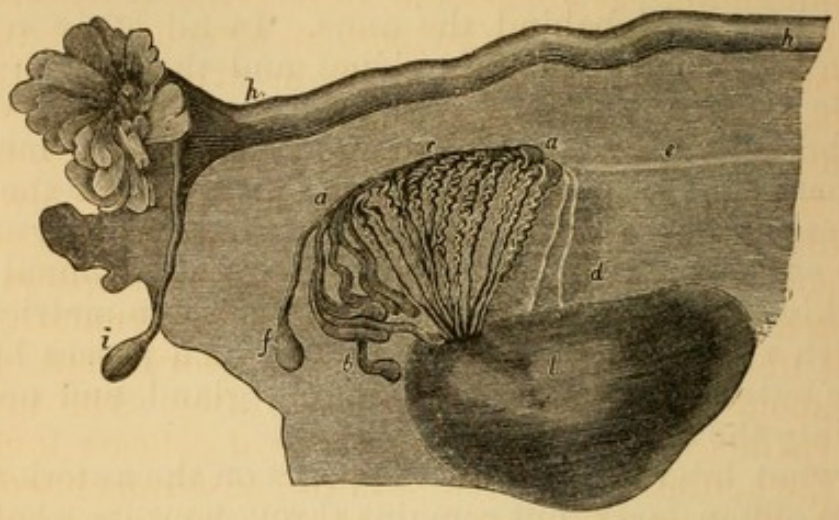


FIG. 4 (after Kobelt).—View of oviduct and parovarium from behind: *a*, *a*, inverted pyramid formed by convoluted tubules of parovarium; *b*, outer tubules, flask-shaped, and often dilated into cysts; *e*, atrophied Wolffian duct, or canal of Gaertner (lower down); *f*, terminal bulb of Wolffian duct, known as organ of Rosenmüller; *h*, Fallopian tube, or altered Müllerian ducts; *i*, terminal bulb of same, known as the hydatid of Morgagni in the male.

as the cow and the pig, retain a large size, but serve no purpose, so far as is known. They commence above, lying in close relation to the organ of Rosenmuller (*e*, Fig. 4), and run down either in the substance of the uterus, or close to it, between the layers of the broad ligament. They open into the uro-genital sinus on either side of the meatus urinarius. In exceptional cases they are found in women, and even during life their openings in the position indicated may be clearly seen.

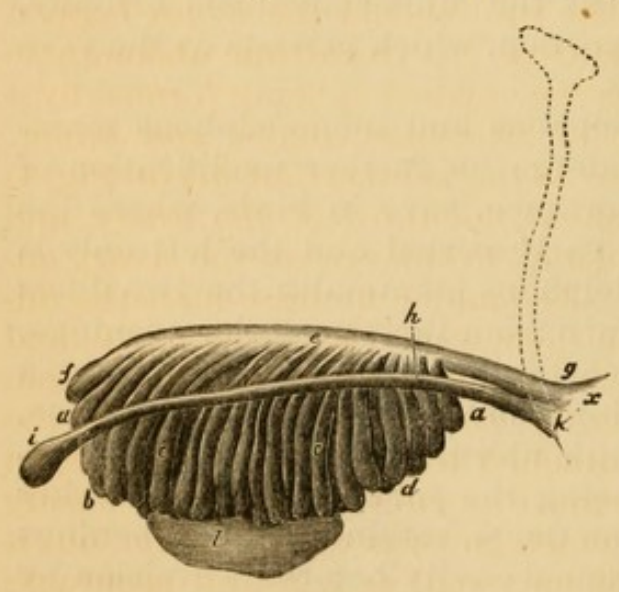


FIG. 5 (after Kobelt).—Wolffian body and ovary of embryo of the sixth week: *a*, *a*, tubules of Wolffian body; *e*, excretory duct; *f*, terminal bulb (organ of Rosenmuller); *h*, Müllerian duct; *i*, terminal bulb (*v*, Fig. 4); *x*, uro-genital sinus, into which both ducts open. The dotted outline shows bent position of Müllerian duct when it has become the Fallopian tube.

When, in the human embryo, the coalescence of the two tubes has so far advanced as to form the utero-vaginal canal, the remaining part of the tube is bent sharply downward and outward, and thereafter occupies its normal (nearly) horizontal position. It leaves the uterus at the cornu (ostium internum), at this part of its course through the uterine tissue being

of very narrow calibre. From this point it extends outward for a distance varying in the adult from three to five inches, its diameter increasing slightly as it leaves the uterus, and contracting again at the ostium abdominale, where it opens out into the infundibulum. It consists of three coats, one derived from the folding over it of the peritoneum, as already described. The greater part of its wall is therefore in direct contact with the outside surface of the peritoneum. The lesser portion of the wall is in contact with the cellular tissue, which occupies the space between the two folds of the broad ligament, at the lower aspect of the tube (meso-salpinx) (Fig. 2). The middle coat is muscular, and consists of a faint layer of longitudinal fibres externally, and a much thicker layer of circular fibres internally. The longitudinal fibres, according to my own observations, disappear entirely about the menopause, or soon after. The internal or mucous surface is thrown into a series of delicate longitudinal folds by the action of the circular fibres, an arrangement exactly similar to that which obtains in the œsophagus and urethra. The mucous surface of the tube is lined with ciliated epithelium, the movements of which are directed toward the uterus, and the function of which is certainly to prevent the passage of spermatozoa up the tube. If this were not so, tubal pregnancy would be much more common than it is. The movement of the cilia also undoubtedly aids the passage of the ovum down the tube, and prevents its adhesion to the wall should the ovum happen to become occupied by spermatozoa. At the ostium abdominale the tube expands trumpet-like, the expansion being formed by a series of fimbriæ, or laciniæ, of two sizes, major and minor. This infundibulum (known also as the *morsus diaboli*) (*a*, Fig. 1) is large enough to embrace about one-third of the ovary, and seems to have a curious tendency to enlarge as the ovary enlarges in diseased conditions. The major fimbriæ have the minor fimbriæ arranged between them somewhat irregularly, and when a Graafian follicle is nearly ready to burst, the infundibulum is said to be applied over the part of the ovary where the ripe follicle is and becomes attached to the surface by a slight cellular adhesion. If so, there must be some peculiar and wholly unknown selective influence which governs this adhesion, but it clearly is a mechanism not of universal or constant accuracy; for I have frequently, during abdominal sections, seen follicles just on the very point of bursting, over which the infundibulum was not fixed. In such a case the ovum must fall free into the peritoneal cavity, and there probably dies in the great majority of instances. There is reason to suspect, however, that, in exceptional instances, it there undergoes cystic expansion.

The infundibulum is covered by transitional columnar epithelium, and at the margin of the fimbriæ it meets the squamous epithelium of the peritoneum, forming the only instance of the union of a mucous and a serous surface—in fact, the unique instance of an opening into a serous cavity.

Under ordinary circumstances, when the tube is healthy, its cavity is occupied by a small quantity of viscid mucus, and during menstruation this is replaced by blood of the usual dark, fluid character. Inflammation may occlude both ostia, and convert the tube into a cyst, occupied by serum (hydro-salpinx) or by blood (hæmato-salpinx), or by pus (pyo-salpinx), of which conditions I have seen quite a number of cases.

Normally the tube lies loose in front of the ovary, and rather below its level, coiling around it till the infundibulum turns toward the middle line and backward toward the posterior surface of the gland. This bend of the tube may be actually below the ovary, but, as far as the structures of the broad ligament are concerned, it is, of course, above it. From the lower margin of the mouth of the tube extends the tubo-ovarian ligament, formed by one of the major fimbriæ (*d*, Fig. 1), and which seems to serve as a guide for the tube in its movement toward the ovary. From the posterior lip of the funnel depends the terminal bulb of Müller's duct, though it is by no means always present.

The parovarium is the remains of the tubular structure of the primordial kidney, or the Wolffian body. It lies between the two layers of the broad ligament, between the upper and outer margin of the ovary and the Fallopian tube. It has the shape of an inverted pyramid (Fig. 4), the apex being applied to, but not attached to, the ovary. The tubules vary much in number—from three or four to thirty. They have always cæcal extremities, and those on the outer side are always best marked, the outermost one forming the terminal bulb or organ of Rosenmüller. If a good example of the structure be carefully dissected, it will easily be determined that the tubes are lying loose in the cellular tissue of the broad ligament, and are not attached to either of its layers or to the ovary. This explains a characteristic feature of those Wolffian cysts which require operation. From the inner and upper angle of the parovarium runs the atrophied Wolffian duct—that is, when it is visible, which is not often the case. This duct is so thoroughly atrophied that I do not think any of the tubules have intercommunication, as they would have if it were not.

Of the three layers of the blastoderm which form, by various

and most curious plications and developmental changes, the many organs of the body, only two—the mesoblast and hypoblast—take part in the formation of the organs we are considering.

The first change consists in an arrangement of cells which, radiating from a centre which forms a lumen, is found to travel down through the mesoblast from its dorsal surface, immediately under the epiblast, just outside the protovertebræ, between them and the pleuro-peritoneal cavity. This cavity is then lined by the epithelium, which ever afterward is its marked characteristic, and which then is known as the germinal epithelium. In the chick, as early as the second day, this cellular track can be traced downward as a distinct ridge (Balfour), and it forms the primitive Wolffian duct. In other animals the changes are probably much the same, but for obvious reasons they have not been traced in sequence, and their dates are unknown. The next change is the appearance of a cell-mass outward into the pleuro-peritoneal cavity, in which the Wolffian body is formed, consisting, like the permanent kidneys, of convoluted tubules, commencing in Malpighian bodies with vascular glomeruli, and opening into the duct. Upon this cellular mass lies the germinal epithelium of the pleuro-peritoneal cavity, from which the ovary is formed on the inner side of the Wolffian body—that is, the side looking toward the splanchnopleure. The germinative epithelium retains its columnar character, and becomes thickened to several cells deep, the mesoblast below it becoming also thickened, so that a distinct eminence is formed as a fusiform white patch or streak, extending, in its early stages, along the whole length of the Wolffian body, but subsequently becoming restricted to its superior portion. In the cells of the germinal layer are found the primitive

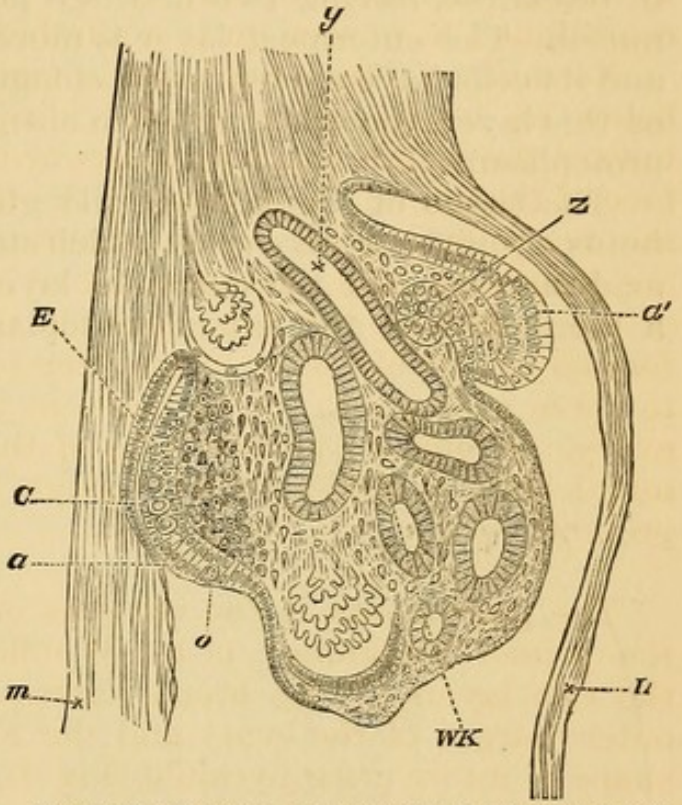


FIG. 6 (after Balfour).—*m*, mesentery; *L*, somatopleure; *a'*, portion of the germinal epithelium from which the involution to form the duct of Müller (*z*) takes place; *a*, thickened portion of the germinal epithelium in which the primitive ova (*C*) and (*o*) are lying; *E*, modified mesoblast, which will form the stroma of the ovary; *WK*, Wolffian body; *y*, Wolffian duct.

ova, developed by differentiation from the epithelial cells. This change is effected in the chick about the ninetieth hour of incubation, at which time it is quite possible to determine the difference of sex. In the human embryo the difference is not discernible till between the fifth and seventh weeks, authorities differing materially as to the exact date.

The structure of the early ovary consists of a superficial layer of the germinal epithelium (*g*, *e*, Fig. 7), and of a tissue internal to this which forms the great mass of the gland.

The germinal epithelium is a layer about 0.03 to 0.04 mm. in thickness, having two or three layers of cells with granular nuclei. The outermost layer is more columnar than the others, and its cells have nuclei rather elongated than round. The cells of this layer, though varying in size, have a larger provision of protoplasm.

The tissue of the body of the gland consists mainly of columns of epithelial-like cells, which stain more deeply with osmic acid than those of the germinal layer, having round nuclei and a more limited amount of protoplasm. Between its columns runs up vascular stroma, formed of spindle-shaped and nucleated cells (*t*, Fig. 7). This tissue continues visible through the whole course of the development of the ovary, till comparatively late in life, and during all the earlier stages it might be easily supposed to be playing some important part in the development of the ova, or to be a part of the germinal epithelium, from which it has only occasionally any well-marked line of demarcation. In this tissue and at the base of the ovary are seen a number of canals which have given rise to the view advanced by Pflüger, that the ovary was developed as a tubular gland. This view has, however, been almost universally abandoned, and, in my own researches, I have seen no evidence which entitles it to serious consideration. These tubules are clearly derived from

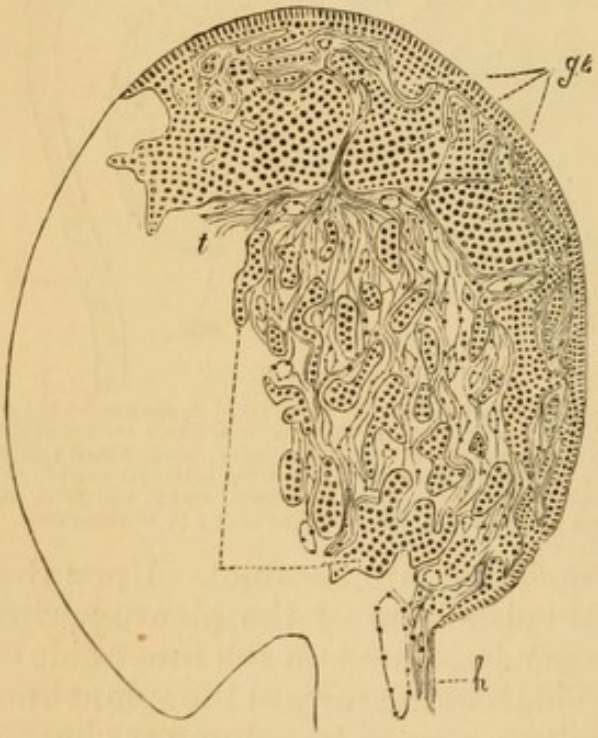


FIG. 7 (after Balfour).—*g*, *e*, germinal epithelium; *t*, trabeculæ; *h*, hilum, with canal.

the Malpighian bodies of the Wolffian structures, and are mere survivals.

The germinal epithelium grows rapidly in thickness by the division of its cells, and the vascular stroma greatly increases in quantity, so that the epithelial tissue is honeycombed by the vascular trabeculæ, which are so arranged as to divide imperfectly the epithelium into two layers, separated by a space occupied by connective tissue and blood-vessels. The outer part is relatively thin, and is formed of a superficial row of columnar cells, and one or two rows of more rounded cells, among which can be recognized the primitive ova (*p*, *o*, Fig. 8) by their size, their granular nucleus, with the characteristic reticulation, and their abundant protoplasm. The inner layer is much thicker, and formed of large masses of rounded cells, and the two layers are connected by numerous trabeculæ, the stroma between which eventually gives rise to the connective-tissue capsule, or *tunica albuginea* of the adult ovary.

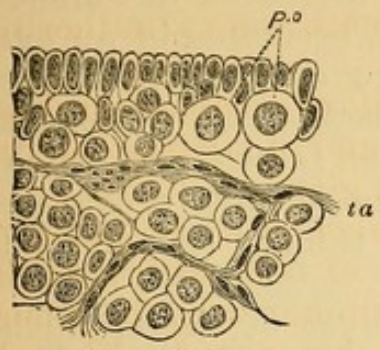


FIG. 8 (after Balfour).—*p*, *o*, primitive ova; *t a*, tunica albuginea; *c*, *e*, central epithelium.

Subsequently in the course of development the germinal epithelium becomes still more thickened to .38 mm., and becomes marked into three distinct layers (Fig. 9, *g*, *e*). These consist of an outer epithelial layer, having an average thickness of .03 mm.; a middle layer of small nests, about .1 mm. in thickness; and an inner layer of larger nests, which has an average thickness of .23 mm. In these three layers the epithelium has undergone important modifications. The greater part of the granular contents of the nuclei of the cells has become clear, the other part remaining as a mass taking the color of staining materials very darkly, and somewhat later taking a stellate figure, these two forms being spoken of as the granular

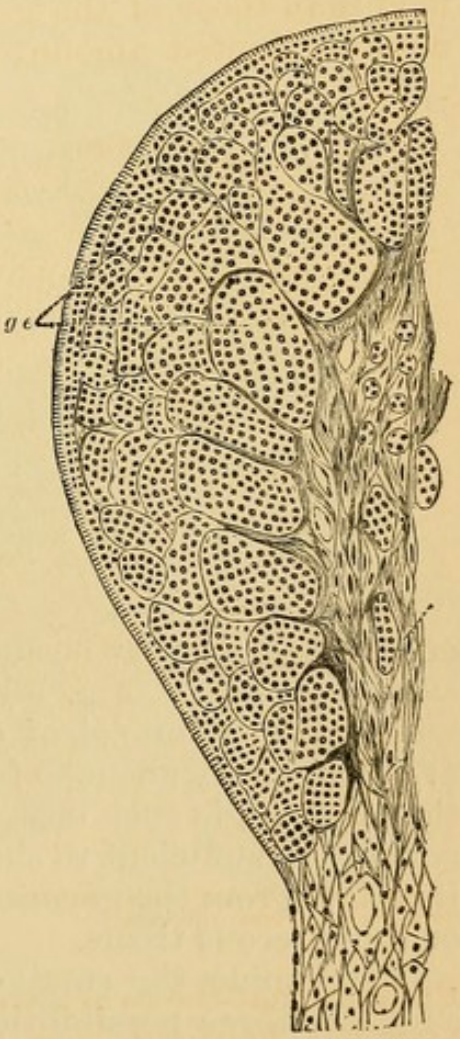


FIG. 9 (after Balfour).—*g*, *e*, germinal epithelium in three layers; *h*, hilum, with canals, *c c*.

and stellate stages of the nucleus. Still later the nuclear mass forms a beautiful reticulation, as seen in the spores of the algæ.

As already said, some of the cells enlarge, and are recognized as the primitive ova, and these are now increased in number. Others of the cells again diminish in size, becoming of an oval form, the nucleus retaining its primitive character, and not going through the changes above described. The cells subsequently form the epithelium of the Graafian follicle. They may be seen arranging themselves around the primitive ova just formed. At the hilum of the ovary the tubules (*c*, *c*, Fig. 9) have by this time almost disappeared.

As the ovary grows, the outermost layer of the epithelial elements becomes more and more separated by the fusiform-cell stroma, and the nests of the middle layer become smaller, and finally the arrangement and formation of the Graafian follicles become completed, and in the typical epithelial nest are to be seen fully-formed follicles with the permanent ova, completely

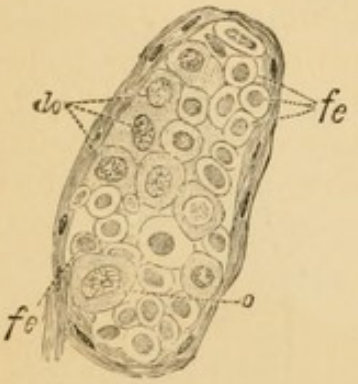


FIG. 10 (after Balfour).—Nest from middle layer, showing formation of follicular epithelium: *o*, primitive ovum; *fe*, cells forming follicular epithelium; *do*, cells which disappear.

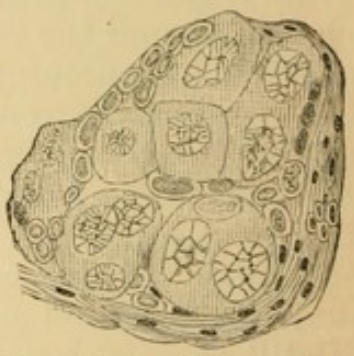


FIG. 11 (after Balfour).— Typical epithelial nest.

enclosed in a cavity occupied by fluid and lined by epithelium; smaller ova (*d*, *o*, Fig. 10) not so enclosed; smaller cells (*b*, *c*), with modified nuclei of doubtful destination; and small cells (*f*, *e*) obviously about to form follicular epithelium. The inspection of a single such nest, says Mr. Balfour—and in this I fully concur—is sufficient to show that the follicular epithelium takes its origin from the germinal epithelium, and not from the stroma or tubuliferous tissue.

Concerning the small cells with modified nuclei, Mr. Balfour suggests three possibilities, and thinks they may have all three destinations: that they become cells of the follicular epithelium, are developed into ova, or are absorbed as a kind of food by the developing ova.

The isolated follicles are now formed by ingrowths of the connective-tissue stroma cutting off fully-formed follicles from a nest. They occur only at the very innermost border of the germinal epithelium. This is in accordance with what has so often been noticed about the mammalian ovary, viz., that the more advanced ova are to be met with in passing from without inward.

In the further growth of the ovary the pseudo-epithelium is formed of a single layer of columnar cells with comparatively scanty protoplasm. In it there are present a considerable number of developing ova. A layer of connective tissue, the albuginea, exists below the pseudo-epithelium, which contains a few small nests with very young permanent ova. In the layer of medium-sized nests internal to the albuginea the ova have all assumed the permanent form, and are provided with beautiful reticulate nuclei with a nucleolus and smaller granular bodies. The majority are not provided with follicular investment, but among them are numerous small cells, clearly defined from the germinal epithelium, which are destined to form the follicle (Fig. 12). In the innermost layer of the germinal epithelium the outlines of the original large nests are still visible, but many of the follicles have been cut off by ingrowths of stroma.

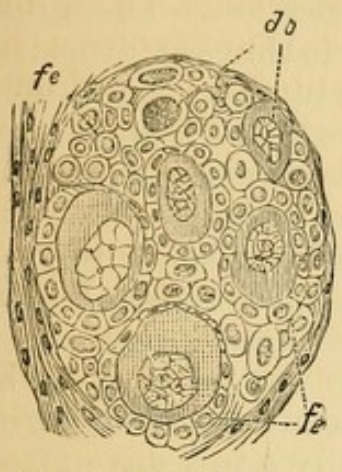


FIG. 12.—Further development of processes seen in Fig. 10.

The general conclusions from Mr. Balfour's researches are that the whole egg-containing part of the ovary is really the thickened germinal epithelium, and it differs from the original thickened patch or layer of germinal epithelium mainly in the fact that it is broken up into a kind of meshwork by growths of vascular stroma.

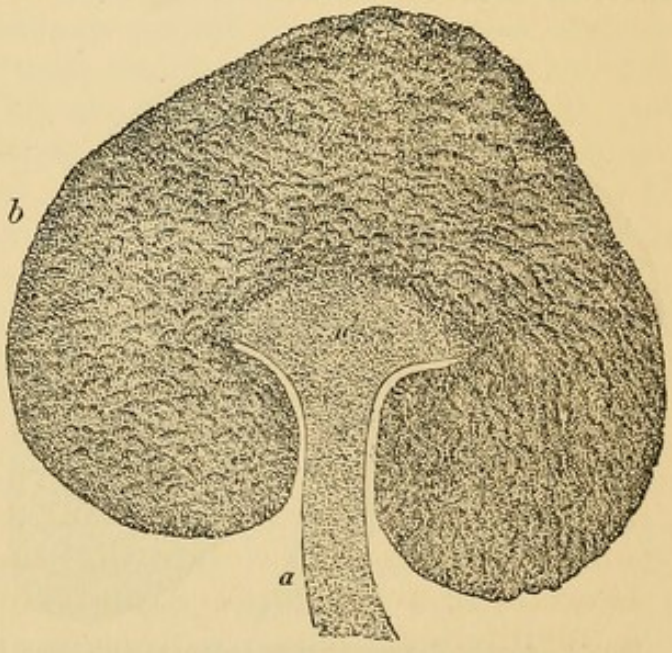


FIG. 13.—Section of ovary of human embryo at third month (after Kölliker), slightly magnified: *a*, mesovarium; *a'*, vascular stroma of hilus; *b*, gland-substance. × 50.

It will be seen, therefore, that the formation of true Graafian follicles goes on very early in the life-history of the ovary, long before the birth of the child, a fact which was pointed out by Valisneri in 1733, but which received but little attention until the writings of Carus (1837) and Ritchie (1842) attracted attention to this most interesting subject. Since then it has been most exhaustively discussed by many observers, and finally, and, as I think, conclusively unravelled by F. M. Balfour. The great practical interest which it has for surgeons is that the dropsical distentions of these follicles produce ovarian tumors occasionally in very young children; and Mr. Cullingworth, of Manchester, has placed on record a most interesting observation where an undoubted ovarian tumor existed in a newly born child, and Virchow alludes to similar instances. By Mr. Cullingworth's courtesy I have been able to examine his specimen, and I have satisfied myself of the perfect accuracy of his description.

A most interesting observation is made by Dr. De Sinéty, who has been struck with the frequency of apparently cystic ovaries in children at the time of birth, and especially a few days after, and who has found, in the great majority of the ovaries of children near the full term, or who die a few days after mature birth, that Graafian follicles are visible to the naked eye, if sections of the ovaries are made. He also says that these large follicles, having arrived at a certain stage of development, begin to disappear, and that the processes of their retrogression and the different phases of the cicatrices which they leave behind them can be followed. He draws attention to the fact, familiar to every one, that it is no unusual thing to see slight swelling of the breasts of newly born children, not only female, but male, and the secretion of a milky fluid. This generally ceases in a few days, and the gland remains quiescent till the puberty of girls, and throughout life in boys. He thinks the ovarian activity is associated with the premature mammary effort, and he quotes Merkel ("Ueber die Entwickelung im inneren der Samenkanälchen,"

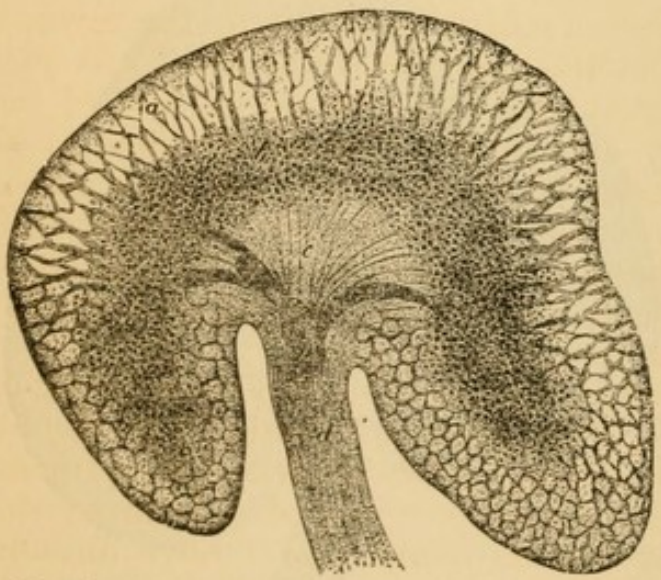


FIG. 14.—Section of ovary of human embryo at sixth month: *a*, external epithelial layer; *b*, internal epithelial layer; *c*, vascular stroma of hilus; *d*, mesovarium. × 50.

Archiv für Anatomie und Phys., 1872), to the effect that in the new-born male a considerable proliferation of the epithelium of the testicle is to be observed, together with the appearance in that organ of round cellules similar to those from which spermatozoa are derived in later life, and that these cellules disappear shortly after birth, and are not found again till after puberty. Similarly, De Sinéty has not found developed ovarian follicles in the years of childhood.

Haussman's observations, made upon eighty-four examinations, completely substantiate De Sinéty's conclusions. He found premature development of the Graafian follicles in ten per cent. of the ovaries examined, and he puts forward the important suggestion that such a condition, by exhausting the stock of the ova, or by prematurely discharging the activity of the ovaries, may be a possible and hitherto unsuspected cause of amenorrhœa and sterility.

These observations cast a most interesting light upon the parthenogenetic theory of the development of dermoid cysts which originated with Ritchie, and which I have more completely elaborated in its appropriate chapter.

This ovarian activity seems to cease about the third month, for after that time, though mature Graafian follicles can be discovered by microscopic examination, they rarely are large enough to be visible to the naked eye; the bands of connective tissue, with the fusiform cells or nuclei, increase in size, and the tunica albuginea becomes more marked, so that by the seventh month the ovary presents all the appearances which it has just before puberty, and up to that time little else can be said of the history of the gland. The only point which yet remains to be settled, and for the settlement of which I have not come across any evidence, is whether or not the premature Graafian follicles ever rupture and discharge their nucleus into the peritoneal cavity. There is some probability that they do, for reasons that I shall give when speaking of wandering ova.

That most of the Graafian follicles thus produced—that, in fact, a very large number of those produced in adult life, including many which reach almost to maturity—die without rupturing and discharging their egg-nucleus, is rendered certain by the observations of every writer on the ovary, and I have already indicated, from Balfour's writings, the possibilities of their ultimate fate.

In a very remarkable paper by Dr. Creighton, of Cambridge, published in the thirteenth volume of the *Journal of Anatomy and Physiology*, that author sets himself to answer the question, What becomes of Graafian follicles within which the ovum has

decayed? The conclusions he comes to are not such as I can yet see my way to accept, for among many hundreds of sections of the ovaries of various animals in my possession I cannot find in one indications of the structures which he describes, and the appearances he figures seem to me to be such as may arise from

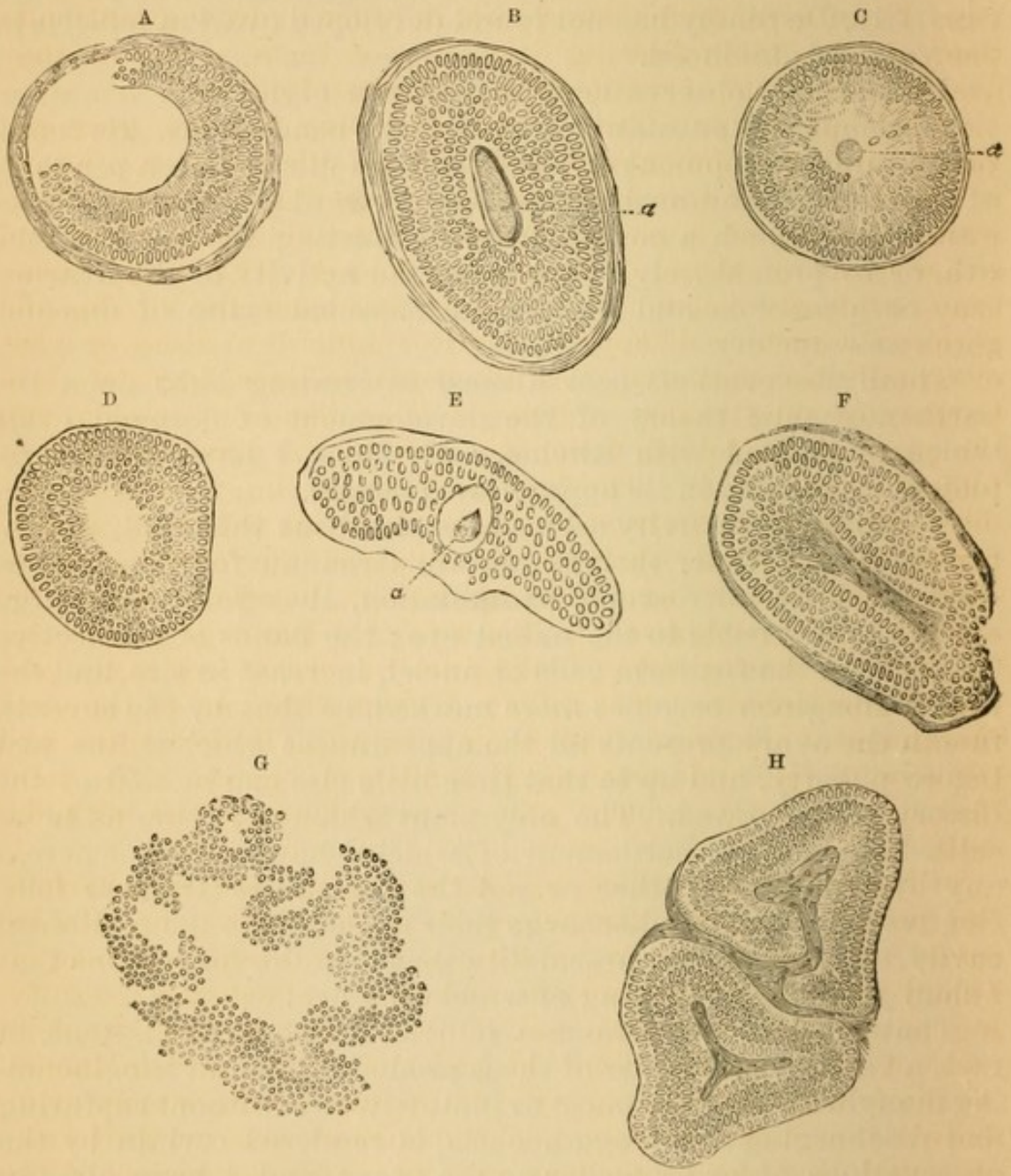


FIG. 15 (after Creighton).—Obsolescence of Follicles, various stages.

peculiarities in the preparation of the sections, and in their direction. I can, for instance, easily accept the appearances at A, B, C, and D, Fig. 15, as being sections, or slices off the top of a Graafian follicle, and the drawing at G may similarly be a slice out of a corpus luteum, as may also be the figure at F; but I

must say that I completely fail to follow the reasoning which would bring us to regard these forms as being homologous in any way with the cortical substance of the supra-renal body figured at H. The paper itself is well worthy of perusal, as being full of information of undoubted value, and it may be that some of Dr. Creighton's conclusions may receive a fuller acceptance than I can give them; wherefore, in order to draw attention to them, I give the following summary of them, so far as they deal with the fate of the decaying Graafian follicles :

"The substance of the ovum, including vitellus, germinal vesicle, and spot, disappears, and the zona or vitelline membrane is found more or less empty and collapsed, as a strong, thick-walled vesicle, of homogeneous structure, yellowish color, and either ovoid in shape or somewhat folded. This tough membrane evidently resists the influences that cause the vitellus and germinal vesicle to disappear, and it is difficult to discover what eventually becomes of it. At all events, in later stages of obsolescence of the Graafian follicle, it is no longer to be seen, and the place of the follicle is marked only by the persisting belt of follicular epithelium. The fate of the enclosing zone of epithelium is in marked contrast to that of the ovum within it; in proportion as the latter shrivels and collapses, the former assumes certain determinate and fixed characters, by which it may be always easily recognized in the midst of the ovarian stroma. The follicles drawn in Fig. 15 illustrate various stages in the process of obsolescence. The follicle A exemplifies one of the most fundamental changes. The ovum is wanting in the centre, and the zone of follicular epithelium persists on one side; the point that it is of importance to observe is the form of the epithelial cells.

"The follicular epithelium does not, in the earlier periods of life, present the usual characters of an epithelium; the cells are round and almost nuclear, or without cell-substance. Under ordinary methods of preparation, and under a moderate magnifying power, they look like naked nuclei, just as the lymphoid cells of a lymphatic gland do under the same circumstances. As the follicle becomes riper the epithelium becomes more cylindrical; it is at the two poles of the nucleus, and not uniformly all round, that the protoplasm collects. This elongation of the epithelium, which is never very pronounced in the follicle destined to extrude its ovum in the ordinary course, becomes quite obvious where the ovum decays within the cavity. The cells are then seen (as in A, Fig. 15) to be greatly elongated, cylindrical cells. In B and C the elongation of the epithelium, and the corresponding shrivelling of the ovum, are seen together; at *a*, in

the centre of each follicle, is the thick, structureless zona of the ovum, which appears to be as if compressed or encroached upon by the lengthening radial cells of the follicular epithelium. At the same time the ends of the cylindrical cells that abut upon the zona appear to have acquired a common bond of union, like a basement-membrane. At D is represented a belt of follicular epithelium, broken at the upper side, and without any traces of the ovum within it. The follicle E exemplifies perhaps the most common appearance of the belt of epithelium surviving after the abortion and disappearance of the ovum; the originally circular belt (in section) has become almost straight, and the shrivelled zona of the ovum lies toward its under surface and almost clear of it. The different forms of the belt of follicular epithelium depend partly on the plane of section; but there is little doubt that the originally circular belt (as it appears in section) unbends, and becomes a slightly curved cylinder, the shallow concavity of which corresponds to the original central space where the ovum lay. At F is shown the belt of epithelium doubled up, and with a stalk of connective tissue issuing from its concavity. These various surviving conditions of the follicular epithelium appear to belong to follicles which had not become greatly expanded; the ovum had filled the central space exactly, and there had been no development of *liquor folliculi*. There are, however, aborted follicles of a much wider circuit, such as that represented at G; in this case the extensive belt of epithelium is thrown into folds, and it is further noticeable that the elongation of the epithelial cells is hardly perceptible, and that there is no uniform basement line either on the outer or inner surface of the belt.

"The peculiar cortical structures of the supra-renal bodies, and their position relative to the rest of the organ, may be readily made out in any good section of a well-prepared supra-renal of the horse. Fig. 15, H, is a low-power view of such a section, made perpendicular to the surface. The outermost stratum is a zone of connective tissue of considerable thickness. Next to that comes the zone of peculiar structures above referred to."

These peculiar cortical bodies of the supra-renals Dr. Creighton regards as homologous to the remains of Graafian follicles in the ovary, from which the ova have not been discharged, but have decayed. His conclusion is that "the morphological resemblance between the ovarian and the supra-renal structures is not only close, but it is complete." I must, with all due deference to the work of an observer of so much distinction, say that, so far as I can see, this position has yet to be proved.

The Ovaries at Puberty.

What has already been said about the growth and ripening of Graafian follicles before puberty constitutes one of the many arguments in favor of the view that menstruation and ovulation are wholly distinct processes, and abundant examples can be given of them being carried on each independently of the other. The statement made by Dr. Robert Barnes, that, if the ovaries are extirpated or become atrophied, menstruation does not reappear, is not accurate; and equally incorrect is his assertion that the first ovular dehiscence corresponds with the first appearance of the menses. It is perfectly certain that ovulation is by no means a periodic process, in the sense of being monthly, and the fact that a periodic flow from the uterus is almost confined to the human race is sufficient to show that it is not in the ovaries that we have to look for the cause of this curious and objectionable phenomenon, for which no one has ever yet suggested a useful object. Where this cause does exist we do not know, but it is quite certain that, as it continues for months, in some cases, after the removal of both ovaries, it cannot be in those glands. Nor is it in the uterus, for in most cases removal of both ovaries arrests the function immediately; and in the somewhat short experience I have, up till now, had of the removal of small ovaries, this has been always immediately effected, and completely, when I have been obliged to remove the tubes as well. I have, therefore, a growing suspicion that we shall find in the monthly movement of the tubes, or in their structures, at any rate, the real source of the monthly discharge from the uterus.

It is perfectly certain that no one has yet recorded one instance in which the tube has been seen fastened on to the ovary before or after the menstrual period of life, as it is during that period. Yet ovulation goes on before puberty and after the climacteric freely. The change in size and vascularity of the tubes at puberty, and their diminution at the climacteric, and the beginning and cessation of their movements, form the most curious of all the remarkable features of those functional changes, and are quite enough to show either that the tubes are most markedly under the same periodic influence as that which produces the menstrual flow, or that they themselves are its cause. Finally, I have, during the last few years, had the opportunity of seeing the ovaries of a number of women, whose abdominal cavities I have had to open for various conditions not connected with diseased ovaries, and I have always found that

during menstruation the tube is fastened on the ovary, whether there be a ripe follicle at the point of adhesion or not; that both tubes were generally fastened to their respective ovaries, though in one ovary there may have been no appearance of a ripe ovisac; that I have very frequently seen an ovisac on the point of bursting, or just burst, when the patient was midway between two menstrual periods, this being a very frequent experience, as I always selected, when I could, a time midway between the periods for my operations, and in these cases I never found the tube fastened on the ovary. Finally, I have removed, in two cases, ovaries with the tubes fastened on them, during menstruation, in none of which were there any ovisacs approaching ripeness.

From these facts, and from others which will be detailed in another chapter, I am persuaded that ovulation is wholly independent of menstruation, and *vice versa;* that the most important feature of the menstrual period is the movement toward the ovary of the trumpet-shaped opening of the tube, and the grasping of the gland by it; that this grasping continues nearly throughout the period of menstruation, and that it is only a matter of the chance of there being a ripe ovisac within the grasp of one or other of the tubes that true ovulation—that is, the passage of the ovum into the uterus—takes place, and there is a possibility of conception. If this be not so, it is quite impossible to see how many married women escape having progenies immensely numerous, seeing the numbers of ripe ova which are produced, and the regularity of menstruation. If ovulation were coincident with menstruation, the probabilities of a woman with healthy organs, who married at twenty, and ceased to menstruate at forty-eight, would be to have eighteen children, instead of six, which is her average as at present. Besides this, the number of sterile women would be greatly diminished, and the increment of the human population would exceed all management. Diminishing our death-rate, or—to speak more accurately, as we should, for we all must die—increasing the average death-age as we do, by the abolition of wars and zymotic diseases, it is not difficult to see that some other agency must step in to aid civilization. If we produced as many young as do the lower animals, civilization would be an impossibility; the life-struggle would be so keen that barbarism must prevail. The inevitable law of evolution has, therefore, secured some process—we do not yet know what—by which the proportion of reproduction is limited, and we are Malthusians despite ourselves. Look at the myriads of young procreated by fish, only to serve for food to themselves or birds. Between them and ourselves there is a change gradu-

ally effected through the whole scheme, till we produce, as a rule, one child at birth, occasionally two, and very rarely three or four. These exceptions are clearly atavic. In some of the higher apes there are signs of something like a menstrual period, but as all such features in animal history become perverted in confinement, we do not know much about them. But in the lowest human races the signs of menstruation are very faint, their labors are very easy, and their whole sexual history different from that of the highly civilized races. It is abundantly proved that, just as civilization (and I use the word in its most literal sense) advances, so does the increment of sexual trouble among women. The flexions, and atrophies, dysmenorrhœas and menorrhagias which affect town-bred women are comparatively unknown to their peasant sisters, and the healthily abundant procreative power of a country laborer's wife is a frequent source of envy to the patrician dame.

In all this menstruation is the chief factor, and I suspect the want of synchronism between the embracement of the ovary by the oviduct and the discharge of the ovum, perhaps also the incomplete maturation of the ovum, will be found to be the most important features of the change.

That menstruation is a new feature in sexual life, introduced high up in the scheme, and has no analogy to the œstrus or rut among the lower animals, is surely proved by the close reasoning of Arthur Farre (article *Uterus:* Encyclopædia of Anat. and Phys.).

The changes effected upon the general system by the accession of puberty do not concern us here, and therefore I shall limit myself to those concerning the ovary and oviduct, and for the observations on which my conclusions are based I am indebted to my own researches entirely.

The structure of the ovary does not seem to be changed in the least by the accession of puberty, save in its vascular arrangements. Before puberty the mesovarium is thin and transparent, occupied by arteries and veins probably as numerous as they are afterward, but straighter and much smaller, the veins especially being but slightly pronounced, and quite different from the appearance of a bag of purple worms which they often have in after-life. After puberty these vessels become convoluted and distended. The ovary itself is slightly increased in size, but no very marked alteration in this respect is to be made out, a matter upon which Henning's measurements are confirmatory. The chief alteration consists in a greater size of the arterioles, a thickening of their muscular coat, and their assumption of a helicoid form, which I have been wholly unable to recognize in the

ovary prior to puberty. This, however, is an extremely difficult point to decide, for I greatly suspect that this helicoid arrangement of the arteries in the ovary, and perhaps in other glands, may be due to the degree of tension at which they are injected. It is at best difficult to make out, for the thickness of a transparent section seems but rarely to contain a complete coil, and however numerous and complete the coils may be, it is not difficult to see that the method of examination is such that they may be missed. I have, however, so often seen sections of such helices in ovaries after puberty, and especially in the glands of multiparous women—though I have often entirely failed to find indications of them in similar ovaries—that I cannot help thinking that there is some reason in their presence, which will be found to affect the condition resembling the engorgement of erectile tissue which the ovary assumes during menstruation, but more particularly during pregnancy (v. Heming). In normally erectile tissue these helices have been regarded as the intrinsic mechanism of the engorgement—a view which I have never been able to accept, for I have never realized that they can be more than a means of permitting the elongation of the vessel when the diameter of the organ is increased. When it is empty, they are coiled up like the slack of a rope, and therefore it is that I have already said that the helices present in a microscopical section will greatly depend on the tension of the injection. If the organ is greatly distended, the arteries will be straight. If slightly distended, they will or may be coiled, and I think it quite possible that my not having found them in ovaries before puberty may be due to the fact that the tissue of the gland is then more easily affected by distention, and that their absence may be from faulty preparation. Still it is likely that the altered hæmic condition of the gland after puberty may produce them.

To inject an ovary before puberty is not an easy task, and to get the opportunity of examining one is rare. I have therefore not been able to get perfectly satisfactory results as to the condition of the vascular supply of the ovisacs. What evidence I have, however, shows that there is little difference in it from what is found after puberty beyond this, that it is possible the vessels are all larger, though upon this point I can say nothing positively. But of this I am certain, that all the phenomena which occur in the rupture of an ovisac and the closure and healing of the cavity are in common occurrence before puberty, and that the characteristic arrangement of capillaries in what is called the *corpus luteum* can be seen long before the occurrence of menstruation.

Spiegelberg says (*Monatschrift für Geburtskunde*, 1867) that he has seen the inner layer of the ovisac distinctly marked off, and possessed of a yellow color, as early as the second year of life. I certainly have seen, in one ovary of the ninth year, an appearance which I could not have told from an adult *corpus luteum* of about fifteen days after the rupture of the ovisac.

In fact, the whole process of ovulation goes on before puberty, and the only difference then made is the important addition of the carrying the ovum into the uterus, and the possibility of its being there impregnated. These additions, however, do not affect the function of the ovary, which was complete before that, as is proved by the parthenogenetic production of ovarian tumors, and, in comparative anatomy, by complete parthenogenesis.

It will serve our purpose, then, if we now complete the description of the normal anatomy of the ovary by detailing the history of an ovisac after its formation.

The mature human ovum measures $\frac{1}{120}$ of an inch in diameter, and its germinal vesicle probably about $\frac{1}{800}$ of an inch, though its exact measurement, free from yolk-substance, has probably not yet been made. The nucleolus, or germinal spot, is about $\frac{1}{3000}$ of an inch in diameter. The ovum lies at first centrally in the ovisac, or Graafian vesicle, but in the ripening of the latter the ovum moves toward the periphery of the sac, and is always to be found close to the surface of the ovary when the sac ultimately bursts. This movement is variously explained, but the more certain processes are, the formation of a *liquor folliculi* by the solution of the epithelium, especially in the outer part of the sac, and the effusion of the fluid on the other side of the ovum, pushing the *discus proligerus* against the thinned wall. This wall is of two layers, the outer being formed of the stroma of the ovary and the peritoneum, and the inner, at first non-vascular, from the follicular epithelium. This inner layer rapidly thickens, becomes vascular, and takes on a distinct yellow color, long before the follicle is ready to burst. It has been called the *membrana granulosa*, but this extra name does no more than describe one of its characters, and leads to confusion. It is the epithelial layer, and it thickens everywhere but at that point where the ovum lies in contact with it, ready to issue forth at the moment of rupture, and the ovum is lightly retained in its place by a cellular attachment to this layer at the indefinite margin of the *discus proligerus*. By the time the ovisac is ready for rupture this layer has become very thick and vascular, and is composed entirely of large, round, and rapidly growing epithelial cells. The vascularity of the walls of the ovisac is most

marked at the point of impending rupture, and its vessels are visible to the naked eye on the surface of the ovary. The rupture takes place at last, and the ovum escapes either into the peritoneal cavity, where it perishes, save in exceptional cases, or into the pavilion of the oviduct, whence it is conducted to the uterus. I believe that the ovum falls into and perishes in the peritoneal cavity in by far the greater number of cases, and that the passage of it into the uterus occurs only in a small minority of the ova produced. The vessels ruptured in the act of the escape of the ovum bleed slightly, and this hemorrhage occupies the emptied cavity, and must, in many cases, along with the *liquor folliculi*, also pass into the peritoneal cavity. I have repeatedly seen a clot hanging from a ruptured ovisac into the cavity of the peritoneum, and on one occasion I saw the follicle rupture before I had touched the ovary, which lay exposed on a uterine tumor. Spiegelberg (loc. cit.) tells us that in women this hemorrhage is very insignificant, as it is also in the cow. In the mare and sheep it is entirely absent, and is most marked in the sow. I have seen nothing to make me believe that in women it is ever so severe as to be pathological, but I can easily imagine that sometimes it may really be so, and may explain those rare and mysterious cases of recurrent pelvic hæmatocele of limited extent, of which I have lately seen a most curious instance in the wife of one of my professional brethren.

After the rupture the follicle collapses, and the thickened inner coat is thrown into a series of convolutions strikingly resembling those of the brain, though I am by no means sure that these convolutions are not indicated before the rupture occurs. I have seen them in an unruptured ovisac before puberty, but as the ovary had been pickled in chromic acid for section-cutting, they may have been produced by preparation. I have also seen them in an unripe sac opened immediately after removal of the ovary. Here again they may have been produced instantaneously by the relief of tension. On a favorable opportunity I shall freeze a fresh ovary before cutting it, and settle this interesting question.

The rent in the ovisac soon heals, and the cavity is again closed, with a small clot in its centre, and this clot was formerly accredited as the source of all the phenomena of the much-discussed corpus luteum. It deserves no such distinction, and it soon becomes decolorized and is absorbed, so that the points of the convolutions come into contact, ultimately coalesce, and finally form the stellate cicatrix which marks for a long time the site of the ovisac. The capillaries of the inner or yellow coat are very regular, and, in a well-injected section, resemble

very much those of a villus of intestine. They spring from a small helicoid arteriole, having very thick muscular walls and two layers of fibres exactly like the arterioles of the kidney, in the outer wall of the ovisac. This arteriole breaks up at once into a ramifying meshwork, which seems to lie between the two coats of the sac, and which probably provides the vessels seen on the outer surface of the ovary at the point of rupture. From this meshwork straight wide capillaries run down in the centre of each lobe to its apex, giving off small branches to each side. At the apex of each villus or lobe (from a section it is quite impossible to say which of these words is correct, though I think *lobe* would be the proper one) comes a vein which runs down between the lobes to the point of vascular origin, and between these interlobular veins and the intralobular arteries there are universal systems of capillary communication. Along the free margins of the lobe seems to run a system of communicating canals, and this it is which causes me to regard this second system of vessels as the veins, together with the fact that I cannot make out that they have thickened muscular walls. In the absence of a successful double injection, which I have never accomplished, this interpretation of these structures may be inaccurate. Within the meshes of vessels are seen the regular round epithelial cells, so arranged as to give a general contour of convolution, and in the cavity are the altered blood-corpuscles.

The lack of greater precision in this description is to be explained by the fact that the preparation of an ovary fortunate in all respects is very difficult, and I have but seldom succeeded, and that for some years now I have been too much engaged with practice to follow up the research, which requires abundant and uninterrupted leisure.

The disappearance of the red color of the central clot is the first change observed in the contracting cavity, and with this the whole forms a yellow convoluted patch. As it contracts the yellow color disappears, this change being effected in about two months in the non-pregnant state. The microscopic characters of the change consist in the disappearance of the cells, said to be brought about by fatty degeneration, though of this I have seen no evidence. This absorption is accompanied by the shrivelling of the blood-vessels and their final disappearance, so that, in eight or ten months, nothing is left but a star-shaped cicatrix extending into the substance of the ovary; but this probably in time entirely disappears in a young ovary. After impregnation these changes are effected much more slowly, owing to the altered nutrition of the whole organs, so that the yellow color may not disappear for twelve or fourteen months (Farre), and the

shrinkage of the cicatrix may take two years to be effected. It by no means follows, however, that an ovisac thus delayed in disappearance has been the seat of an ovum which has been fertilized, for I have seen three such corpora lutea in the ovary of a woman who had been confined only seven months before my operation, of one child—her only one. Farre says that during pregnancy in such ruptured ovisac there is a special increase in the thickness of the epithelial lining, and a larger deposit of oil-granules, but I have failed to get confirmation of his statement. During pregnancy, and for some time after it, the ovaries are especially increased in size by enlargement of their vessels, and so are the contained corpora lutea. The distinguished author just quoted says: "The *true corpus luteum* is the follicle in its largest condition of growth, as it appears after impregnation; whilst in all other conditions, when it has not been stimulated to full growth by impregnation, and whether before or after rupture, it has been called a *false corpus luteum*, so long as it possesses the yellow color." The differences are therefore only questions of degree, and cease to give any special characters long before other indications of pregnancy have passed away. To elevate this structure, therefore, into a medico-legal importance is not to be justified; and after a very extensive acquaintance with ovaries I would not venture to give an opinion, from any number of corpora lutea, as to whether they indicated past pregnancy or not. The great battle on this point has been singularly barren of results.

At puberty a very marked change is effected in the appearance and functions of the Fallopian tube or oviduct. Before puberty the tube is small and straight, and the size of the fimbriæ insignificant. When injected, its vascularity is not a very leading feature, and certainly presents a most marked difference to the peculiarly abundant blood-supply visible in its large-meshed network of capillaries in adult life. The muscular fibres of the fimbriæ are also very ill-defined before puberty, and no evidence is offered by any one that it ever makes any kind of functional movement. In adult life, as every one knows, one or both tubes seek their respective ovaries, and become attached to them for a time, which I believe to be concurrent with menstruation, by cellular adhesion. Permanent adhesion, the result of peri-oöphoritis, is often met with in women who have led lives of prostitution, and in them a sort of permanent metrorrhagia is by no means rare.

In the tube of a girl under the age of puberty I have been unable to find any of the ciliated epithelium which afterward lines it.

My conclusions are, therefore, that the changes in the ovary

at puberty are entirely vascular; that in the tube they are vascular, muscular, and epithelial. But that the most important change of all is the functional movement of the tube, the absence of which alone makes pregnancy before puberty impossible. Otherwise I do not believe that puberty has much to do with procreative power in women.

During the climacteric period a series of changes are effected in the sexual apparatus which make themselves felt throughout the system, but the results of which are not apparent in the structures themselves for some considerable time after the menopause. Ritchie and others have shown conclusively that the formation of true ova goes on long after this event, and I have seen in ovaries of very old women structures which I could not have decided as being in any way different from those seen in the ovaries of women at the prime of life. It is quite certain that the growth of ova persists till the end of life, though with advancing age it gets feebler, the cells become less numerous and less mature. The ovaries, however, continue to be the seat of cell-growth, and pathological cysts are formed in them sometimes even at the very extreme of old age, at a time when operative interference becomes hopeless on account of the age of the patient. The general atrophy which accompanies senility affects,

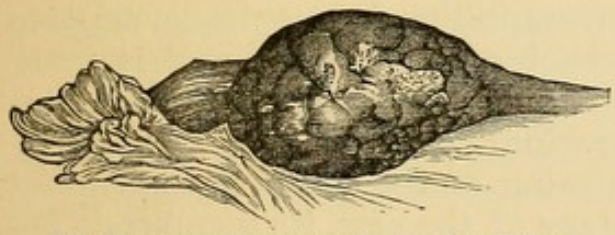


FIG. 16.—Ovary at menopause. (Arthur Farre.)



FIG. 17.—Senile ovary. (Arthur Farre.)

of course, the ovaries, and late in life they are usually small and shrivelled, abundantly marked by scars, and having all the appearances of having been worked out. But even then they exhibit traces of all their old products, and I have seen an ovary from the body of a woman nearly seventy years of age, which it would have been impossible to say might not have been removed from the body of a woman of thirty.

The changes which are most apparent are those effected in the uterus and tubes. These structures rapidly diminish in size, and the tubes are straightened and cease their movements. Here we have further proof that ovulation and menstruation are wholly independent, that menstruation is not dependent on the ovaries or on ovulation. I think also that there is additional evidence in favor of the view to which I am inclined—that menstruation is wholly a function of the Fallopian tubes.

The ovary, then, is simply a gland, developed as other glands, and formed of similar elements; its peculiarity is, that its cell-nuclei have special powers during a certain time of life; and this simplification of its physiology does much to simplify its pathology.

A few sentences from Balfour, concerning the phenomena observed in the maturation and impregnation of the ovum, may here be fittingly introduced. I take them verbatim, as it is a point upon which I have made no research.

"Every ovum, as it approaches maturity, is found to be composed of (1) a protoplasmic body or vitellus, usually containing yolk-spherules in suspension; (2) of a germinal vesicle or nucleus, containing (3) one or more germinal spots or nucleoli. The germinal vesicle, at its full development, has a more or less spherical shape, and is enveloped by a distinct membrane. Its contents are for the most part fluid, but may be more or less granular. Their most characteristic component is, however, a protoplasmic network, which stretches from the germinal spot to the investing membrane; but especially concentrated around the former germinal spot is a nearly homogeneous body, with frequently one or more vacuoles, occupying one of the eccentric positions within the germinal vesicle, and it is usually rendered very conspicuous by its high refrangibility, is sometimes capable of amœboid movements (Auerbach and Hertwig), and is more solid and more strongly tinged by coloring agents than the remaining constituents of the germinal vesicle.

"During the further maturation of the ovum the germinal vesicle moves toward the surface of the egg, its membrane becomes absorbed, and it is metamorphosed into a spindle-shaped body, this being done at the expense of the germinal spot. One end of this spindle enters a protoplasmic prominence at the surface of the egg, the spindle itself dividing then into two, one half remaining in the egg, the other in the prominence. This prominence, at the same time, becomes nearly constricted off from the egg as a polar cell, and a second polar cell is similarly formed. That part of the spindle remaining in the egg is converted into a nucleus—the female *pronucleus*—and this is moved toward the centre of the egg. On the entrance of one spermatozöon into the egg the head of the sperm is converted into another nucleus—the male *pronucleus*. Around this latter radial striæ immediately appear, and these travel toward the female pronucleus. The fusion of the two pronuclei, through the connecting striæ, form the first segmentation nucleus."

CHAPTER II.

ERRORS OF DEVELOPMENT AND DISPLACEMENTS OF THE OVARIES AND OVIDUCTS: SALPINGITIS, HYDROSALPINX, PYOSALPINX, HÆMATO-SALPINX, AND FALLOPIAN PREGNANCY.

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Memoir on Extra-Uterine Gestation. WM. CAMPBELL. Edinburgh, 1842.
Extra-Uterine Pregnancy. JOHN S. PARRY. London, 1876.

THOUGH congenital defects of the ovaries and oviducts are of comparatively rare occurrence, yet, as might be expected, in the case of a gland which performs functions so important as those of the ovary, and whose functions are of universal existence, complete absence of the gland is extremely rare. So far as I have been able to find, there are only three preparations in this country indicating complete congenital absence of the ovaries. Of these, two exist in the cases of malformed fœtuses, in which there may be said to be an almost complete absence of the whole genital apparatus. The third occurred in the case of a girl who died at twenty, without ever having menstruated; and in that case also the whole genital apparatus was extremely defective.

Like every other organ in the body, the ovary is liable to arrests of development, but from what cause or causes these arrests arise, it is not very easy to say. This is a question which has not yet received sufficiently careful attention; but be the cause what it may, it is quite certain that whatever arrests the development of the ovaries equally affects the development of the whole of the genital organs. In the case of the *aves*, both ovaries and oviducts are equally developed in the embryo, but on one side an atrophy occurs early in life which leads to a total suppression of the organs on that side, and the sexual functions are carried on by the left side only. What the explanation of this, and what its cause, is wholly unknown. I am not aware that any similar condition is known to be of even occasional occurrence in woman. I have found only one reference to the existence of a unilateral arrangement of mature organs, quoted by Busch from Chaussier, in the case of a woman who had borne ten children, and who, on *post-mortem* examination, was found to have an entire absence of the tube and ovary of the left side, and apparently an absence of the corresponding side of the uterus. When the ovaries are defective it is almost invariably

the case that the other organs are equally ill-developed; but what is the order of the occurrence is not in any way clear.

I have already pointed out at some length, and I shall discuss in detail in a subsequent chapter, the effects which certain diseases of the zymotic type have upon the sexual organs—more particularly scarlet fever. As this disease is peculiarly incident to childhood, I have a very strong impression that a large number of the cases of incompletely developed sexual organs in women arise from the effects of this disease in childhood. In such cases we find that the occurrence of menstruation is unduly protracted, or it may not be apparent at all; that, at the time when the disturbance should occur, a number of vague symptoms make their appearance, sometimes trifling, at other times extremely serious. If, under these circumstances, the patient be examined, the uterus will be found to be infantile in size, anteflected; and if a *post-mortem* examination should be made, the ovaries will be found small, possibly somewhat puckered, the mesovarium but slightly indicated, the tubes extremely small; and should the patient have reached mature life without the occurrence of menstruation, the organs will be found to present all the appearances of those of a child between five and ten years of age.

In such extreme cases the development of the whole sexual apparatus is generally imperfect, the sexual appetite is in abeyance, and there may be comparatively little suffering after the first few months, during which an effort seems to be made by the system to establish the change. This is provided epilepsy does not supervene, which is, however, only too common an accompaniment of arrested sexual development in women. Women who are thus affected have frequently an absence of those external peculiarities of their sex evident in roundness of form, a *prononcé* bust, smooth and hairless skin, and highly pitched voice; and they often partake, in some slight degree, of the characters of the opposite sex, especially in the growth of straggling tufts of hair on the upper lip and on the chin in a line with the canine and premolar teeth.

A greater number of cases have the arrest at a later stage, and in them menstruation is established, after much difficulty and suffering, between sixteen and nineteen years of age, and, though it may last with fair regularity, but deficient quantity, for four or five years, it then ceases completely. In many of these cases, however, if marriage should occur during the time that menstruation is in action, and if the patient should be fortunate enough to become pregnant, a cure may result; that is, her periods will become more abundant, and her suffering less;

her health will be improved, and she may go on menstruating for many years, and may even have a number of children. Even without the occurrence of pregnancy, marriage often establishes the health of a woman afflicted with arrest of ovarian development.

The great bulk of cases of this kind are those which are afflicted to a less degree, but whose sufferings are nearly always sufficient to require medical assistance; and it is a singular fact that a very large percentage of the patients are found to be women of splendid physical development, who, to any but one well acquainted with such cases, look the most likely to possess capacity for procreation. In these women menstruation is established later than the normal time by a few months or a year or two. They have, at first, irregular times and much pain, but after a while the flow is established with normal quantity and regularity, and with but little suffering. In this way they go on for eight or ten years, and, if they marry in the interval, their menstrual career may run an ordinary course. If they remain single, however, they begin to suffer from ovarian dysmenorrhœa between twenty-five and thirty, and, after about ten years' suffering, they undergo a premature climacteric change. It is also noticeable in these women that their menstrual function is suspended on slight provocation. Any chronic disease—even of an unimportant nature—any occupation which necessitates an overstrain on their system, mental anxiety, or sudden fright, will check their menstruation for months or years, or perhaps forever. In fact, this slight excess of functional power which the ovary became possessed of at their puberty is readily and soon exhausted, and its extruded cells, on slight provocation, assume an immature form, and the systemic conditions become correlated. In fact, in such cases of amenorrhœa, and similarly to a less extent in those of dysmenorrhœa, there is a temporary resumption of the infantile condition of the ovarian functions; or it may be a complete and premature assumption of their senility. The amenorrhœa of pregnancy and lactation are also partial resumptions of the infantile condition. This view has been admirably expressed by Dr. Charles Ritchie: "In early infancy, extreme old age, and long-continued organic disease, the ova are minute, transparent, and structureless; and in advanced childhood, soon after the critical age, and during pregnancy and lactation, they are more or less organized, larger, and in the latter stage are often so well matured that about one-third of the renewed pregnancies of married women take place while they nurse."

In these slighter cases of this kind of dysmenorrhœa the uterus

is generally normally developed, and it is frequently so in some of the most severe cases. There is a converse condition where the uterus is infantile and the ovaries normal, much more rare and far more severe in its symptoms.

In all such cases the general symptoms are pretty constant and distinctive. Besides the menstrual irregularities and deficiency, there is almost always a persistent, sickening, and well-marked pain, occurring in the less severe cases only at the menstrual periods, but in others being seldom absent, and always greatly increased at the periods. It originates in the ovarian region, and shoots down the thigh, often also down the leg and around to the back. There is also often present, especially on the accession of atrophy, the peculiar submammary pain of ovarian disease, generally felt in the left side only. Headache, nausea, or even sickness and great general discomfort, are always present more or less.

In the milder cases treatment is generally successful in mitigating the sufferings, and often the ovary may be made, even in some very well marked cases of arrested development, to fulfil its functions completely. First of all therapeutic remedies there stands iron, which will be found in such cases to be of great use, even though there should be no general indications for its employment. There can be no doubt that many forms of this remedy have a specific power over the sexual organs, male and female; for, in a case of chronic metritis or subinvolution, smart hemorrhage may be induced by large doses of iron. In ovarian and tubal dysmenorrhœa it is best given during the intermenstrual periods in small doses, one to five drops of the liquor ferri perchloridi, well diluted, and increased suddenly to fifteen or twenty for a day or two previous to and during the menstrual flow; or, quite as good, is the substitution of an iron and aloes pill for this large dose, there being few better combinations in the pharmacopœia than that old-fashioned remedy. Hot hip-baths and leeches to the perineum at the period are often useful additions, with an occasional blister on the sacrum. To such as this, the treatment of delayed or difficult menstruation at puberty, due to inefficient ovarian development, must be confined; for the other means are only allowable in very obstinate cases, after the patient has been married, or when there are indications of premature ovarian atrophy. Marriage is, perhaps, the most efficient remedy, and one we may, under certain circumstances, recommend; for, even if the patients should not have children, they will have better health, and they may even become pregnant if they marry early enough and are not mismanaged.

The last and most powerful aid is mechanical irritation of the

uterus; but, as it is not free from risk, and therefore requires careful use, it is not always to be recommended. It is, besides, in the class of cases where the uterus is most at fault that it is least risky and most serviceable. The method of irritation I generally employ, as the most convenient and least troublesome, is the insertion of Simpson's galvanic pessary. This instrument has by some writers been very much decried, but I think by those only who seem to have used it indiscriminately, and without reference to a proper selection of cases.

The irritation set up by the presence of a galvanic stem in the uterus is communicated indirectly to the ovaries in a manner that is not as yet explicable, but that it has an influence is beyond doubt, and, if it remain within bounds, it is in a large number of cases beneficial. A large experience has shown me that it is only in occasional instances that the stem cannot be borne, and that, if carefully watched during the first few weeks of its use, these cases are easily eliminated. In a case where I have been led to regard the use of the stem as advisable, I always begin with a small size, and after this has been worn for two or three months I change it for a larger one. For the first week after its introduction it is not unusual for the galvanic stem to give rise to considerable discomfort and even positive pain; but this usually passes off if the patient keeps her bed for a few days, and there is no further trouble save from the leucorrhœal discharge, which is a part of the process. The action of the stem is not purely mechanical, as has been stated; for, very soon after its insertion, the zinc becomes coated with an albuminous deposit, from which the copper is free, and the zinc becomes corroded. It is certain, therefore, that there is a galvanic action set up, and the stimulating effects are due partly to this, and partly to the interior of the uterus being constantly bathed in a weak solution of chloride of zinc. However produced, it is certain that the uterus rapidly enlarges under the action, and there is every reason to believe that the ovaries take part in the increased activity. If once the uterus becomes accustomed to the presence of the galvanic stem, it may be worn for many months, and the longer it is retained the more permanent will be the benefit; but if, after a trial of a few months—say four or five, there is no apparent alteration for the better, the attempt should be given up, and the case considered as hopeless.

In a very large number of cases of incompletely developed ovaries another remnant of infantile life is met with in an exaggeration of the normal curve of the uterus, amounting sometimes to complete anteflexion, and in this class of cases the galvanic stem is especially serviceable.

The results of my attempts to arrest premature atrophy of the ovary from any cause, when once begun, have been far from satisfactory; and this has been more especially the case when that atrophy has been due to a constitutional disease, such as tubercle. Sir James Simpson had a belief that the pretubercular amenorrhœa, so often seen in young women, was a cause of the subsequent disease; and he therefore directed his attention to the restoration of the utero-ovarian function as a means of treatment or prevention of the consumption. From the views previously expressed, it will easily be seen that I consider his theory to be based on error, though in some cases his treatment would seem to have been successful; but how much of his success was due to local and how much to general treatment cannot now be determined. It is not, however, a practice likely to meet with many followers.

It will be seen, therefore, that not only is the ovary extremely liable to be arrested in its growth from childhood to adolescence, but it is also liable to have induced upon it a condition of premature senility. It is unlike most of the glands of the body in that it has its functions limited to a particular period of life, or at least the period during which these functions are complete is so limited—and therefore it seems possible, and to be of not unfrequent occurrence, that the period of complete functional activity is very materially shortened. This I think we shall afterward see to be by no means unusual after first confinements, more particularly after miscarriages which occur early in sexual life; for the number of women who come under my care is large in whom the history is uniformly given of an attack of pelvic inflammation after the first pregnancy, and who have suffered from distress from that date, and have never again become pregnant. The explanation of this will be found in the adhesions formed by the tubes, to be afterward described.

The most common displacement of the ovary is dislocation downward into the retro-uterine pouch, to which the name of prolapse of the ovary has been given, as I think, somewhat improperly. I have very little doubt that a large number of women go about with marked dislocation of their ovaries downward, without any kind of suffering; and there is equally no room for doubt that this dislocation is in many cases a source of suffering so great as absolutely to prevent the woman from fulfilling her duties in life, and to render her life a prolonged misery.

The origin of this peculiar dislocation is very various. I have no doubt that, in some of the cases I have seen, the position of the ovaries in the cul-de-sac was congenital; in others the dislo-

cation has probably arisen from some accidental strain; but in by far the largest number of cases it has been due to some accident during the process of involution of the uterus after a confinement or a miscarriage. In a large number of cases it is associated with retroversion or retroflexion of the uterus, but in others the uterus is nearly normal in direction, and then we can only assume that there has been some relaxation of the peritoneal investments of the ovaries, by which they have been allowed to drop downward and give rise to the trouble.

There can be no doubt that by far the larger number of these cases arise in a condition which is practically that of subinvolution, and in two anatomical facts we have a complete explanation of this result. Turning to Henning's table (p. 4), it will be found that the ovary of the puerperal woman is extremely large—indeed, nearly twice as large as it is at any other time. It is also extremely remarkable that the left ovary increases in the puerperal woman to a much larger extent than does the right, a circumstance which I have no doubt is fully explained by the want of a valve in the left spermatic vein (v. p. 8). As the ovaries rise in the abdomen with the pregnant uterus, their ligaments, their tubes, and everything connected with them rise in proportional degree. It is not, therefore, to be wondered at that any incident which interferes with the subinvolution of the uterus after parturition should also affect the ovary. One of the most common results of subinvolution of the uterus is retroflexion, and therefore it is that we have a large number of these cases of dislocation of the ovary downward associated with this uterine displacement; and my experience is entirely in accord with that of Professor Goodell, when he says that if we find a dislocated, or, as he calls it, a "prolapsed" ovary, it is almost sure to be the left. It is therefore practically a subinvolution of the ovary with which we have to deal; and as in the uterus we have hyperæmia of the organ gradually passing into chronic metritis, so we have a similar process occurring in the ovary, and in several remarkable cases, in which I have been obliged to remove the ovaries on account of extreme suffering, I have found the organs in a condition of chronic inflammation and greatly enlarged, associated with chronic fundal metritis and enlargement of the whole body of the uterus. In these cases intractable menorrhagia has been a leading symptom, and the monthly engorgement involved by the process of menstruation leads, of necessity, to an increase of the symptoms and an exaggeration of the pathological condition.

The history of such a case as this will generally be that of some disturbance after confinement, followed by a prolonged

convalescence from childbed, a speedy resumption of menstruation, great difficulty and pain in locomotion, almost always pain on defecation, and pain during sexual intercourse. Besides these local symptoms, there will be very often a number of more or less distinct reflex symptoms, such as headache, pain in the breasts, pain in the back, and pains travelling down the thighs. The loss at the monthly periods will increase until it may amount to absolute flooding. The patient becomes anæmic, dyspeptic, and suffers from symptoms of extreme mental depression, and in course of time she will become an absolute invalid.

On examination, the uterus will be found to be markedly retroverted or retroflected, or both, the fundus usually being much enlarged. It may happen, however, that the uterus will retain its normal direction, though it will be rarely found that it is of normal size. Great care must be taken in the examination to ascertain the position of the fundus, because it is quite possible to mistake an enlarged and dislocated ovary for a retroflected fundus, and *vice versa*. But the fundus may easily be recognized with a little care, by finding that the tumor felt is continuous with the cervix. The temptation may be great to replace this by means of the sound, but I would strongly urge, especially on the beginner in gynecology, not to yield to this temptation. The sound is a most dangerous implement, and in the record of more than one of my cases it will be found that an immense increase of suffering has resulted from this practice. The experienced gynecologist should generally be able to replace the depressed fundus by the point of his finger, and if he is unable to do this in any particular instance, he may suspect that there is some adhesion which will make it much safer not to use the sound. The leverage of the sound in such a case will exercise an amount of force of which the operator may have no exact knowledge, and which is likely to do more harm than good. If I may here venture to sum up my experience of this instrument, extending over more than twenty years, I would say that it has done an infinite amount of mischief, and that probably we should have lost nothing if it had never been invented, and that the more experience grows in practice the less will this instrument be used.

If the tumor in the cul-de-sac be found not to be the fundus, then the probability is that it is an ovary; and if it be an ovary, and not adherent, it may easily be pushed upward in the direction of its proper place, and this will generally be found to be toward the left side. If it be an ovary, the peculiar, dull, sickening pain evinced by pressure will at once declare its nature, and if it cannot be easily replaced by the finger, it may be assumed that it is

adherent. The conditions may be fully established by the bimanual method of examination, and in all probability this will not be done with perfect satisfaction without the assistance of an anæsthetic, and ether is by far the best agent to use. By this method of examination it should, first of all, be ascertained whether or not the ovaries are in their proper place on each side of the uterus. If they cannot be found, it is most probable that the retro-uterine tumor is an ovary, and more particularly if it be adherent I would recommend the greatest caution in dealing with it, for I have more than once seen a smart attack of pelvic peritonitis set up by too rough handling. If the tumor be a fundus, it will probably easily be dealt with; but if it be an ovary, very great difficulty indeed may be met with in treating the case satisfactorily. If the gland is not adherent, it may be replaced by a pessary, adapted so as to keep it in place, or at least far enough up to be out of harm's way; but, if it be adherent, it may be taken as certain that no pessary can be borne. The best pessary for this purpose is one which I introduced many years ago, under the name of the "wedge pessary," and which is here figured. I have frequently had cases brought to me in which the sufferings of the patient had been greatly increased by well-intended efforts to replace by pessary an adherent ovary.

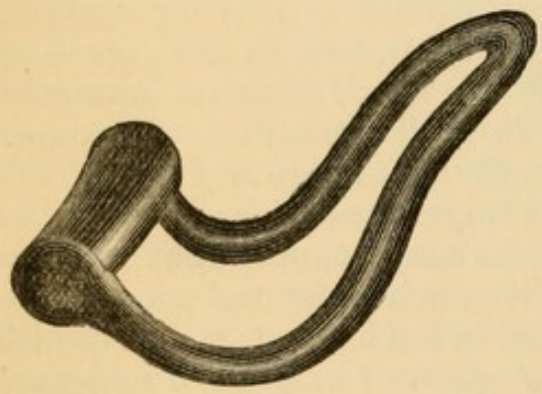


Fig. 18.—Wedge Pessary.

The general treatment should consist of absolute physiological rest; that is to say, that during the menstrual period the patient should be confined absolutely to bed, and that there should be a cessation of intercourse. Any kind of treatment which will tend to improve the patient's general health should be employed, and by far the most effectual remedy will be a judicious administration of ergot and the salts of potash. What has proved in my experience to be the best method of giving these drugs is to put the patient on a prolonged course of the bromide and chlorate for alternate months, in doses of from five to twenty grains twice daily, and taken continuously; and to this is to be added a pill containing from half a grain to two grains of ergotin, to be taken for a few days before the appearance of menstruation, and during the whole of the period. I am bound to say that no other treatment by drugs has seemed to me to be of the slightest use. Professor Goodell speaks in high praise of

a combination of the ammonic and mercuric chlorides, but I have not found them of much use. He gives them in the following formula:

℞. Hydrargyri chloridi corrosivi..... gr. j.
Ammonii chloridi..... ʒ ij.
Mist. glycyrrhizæ co..... f. ℥ vj.
M.

S.—One dessertspoonful after each meal, in a wineglassful of water.

In addition to this, Professor Goodell recommends treatment by the genu-pectoral position, as introduced by Dr. Campbell; and in some cases of dislocated ovaries which were not adherent, accompanied by retroflexion and subinvolution of the uterus, I have found this plan to be distinctly effectual. It is, however, very harassing to the patient, for it requires prolonged use, and I have not found many women sufficiently persevering to give it an extended trial; the misfortune in these cases being, like very many others in this line of practice, that almost any treatment requires to be continued for so long a time that most sufferers are apt to lose patience, and seek other treatment at the hands of some fresh practitioner. I take the following description of this postural treatment from Professor Goodell's writings:

"A very excellent way of keeping up the ovaries—one which, in every case, I adopt, and one which I shall now teach this patient—is the knee-breast posture, devised by Dr. C. F. Campbell, of Georgia. Two or three times a day, or more frequently if needful, this woman will unhook her dress, loosen her underclothing, and kneel on her bed as she now kneels on this table. Her body is then bent forward until the breast is brought down to the surface of the bed, while her head is turned to one side and supported in the palm of her left hand. Her knees should be about ten inches apart, and the thighs perpendicular to the bed. If she now refrains from straining, and breathe naturally, a reversal of gravity will be established. With the fingers of her free hand she will next open her vulva. Air will rush in, and the abdomen and its contents will at once sag down. This will, of course, draw up the womb and the displaced ovaries out of the pelvic canal. As it is rather awkward for a woman, while in this posture, to free one hand and reach the vulva, Dr. Campbell advises that, previously to taking this attitude, she should insert into the vagina a small glass tube, open at both ends, and long enough to project externally. This will leave an air-way, and dispense with the use of the fingers. With such tubes as I

now show you I furnish each one of my patients; but you will find a good substitute in the empty barrel of the old-fashioned cylindrical "female syringe," as it is called. After staying in this posture for a few minutes, the woman will remove the tube and slowly turn over on her side, where she will lie as long as she can. Such constant replacements are of great service, for they lessen the throbbing, they give the limp ligaments a chance of shrinking, and they teach the ovaries good habits of staying at home."

It will, however, often happen that, after all kinds of treatment have been employed, and many practitioners consulted without the slightest improvement, or even, it may be, only with the result of increasing the patient's sufferings, that she settles down as a permanent invalid, her life being rendered absolutely miserable, and she being hopelessly invalided by her dislocated ovary. Then there remains as her only hope the operation of ovariotomy. The discussion of this important subject is by no means yet complete, and it is unfortunately one in which much unnecessary and most unfair criticism has been introduced. I shall in another chapter consider more fully the arguments upon this question, but here it will probably be quite enough to quote again the words of Professor Goodell, with the remark that I endorse every word which he says:

"Once in a while, however, such lasting tissue-changes take place in the ovaries as no medication can reach. The hypertrophied glands keep heavy, and refuse to float up. Now, must the unfortunate owner of these organs drag out the rest of her menstrual life burdened with the distressing ovaralgia, the crippled locomotion, and with all those aches, and pains, and throbs which I have described to you? No, indeed! The source of all this mischief—the ovaries themselves—must be removed. Nor need you fear that such an operation will unsex a woman. In the cases in which it has been performed by myself and by others it in nowise changed the voice, the appearance, or the character of the woman. It merely brought on, more abruptly than nature does, that change of life which every woman longs to reach, and which, while taking away all hope of future offspring, makes her no less a mother or a wife."

A great deal of discussion has taken place concerning the merit of having first proposed this operation, and Dr. Marion Sims has thrown the whole weight of his justly great authority in claiming for it an American origin; and for Dr. Battey he asserts the credit of it, having named it "Battey's operation." I must, however, in justice to myself, protest against this; and I have to point out that, so far as the records of this operation

are known, Professor Hegar, of Fribourg-im-Breisgau, was the first to perform it, and that my first case preceded that of Dr. Battey by several days—all three operations having been performed within a fortnight. Further, I have to point out that in the first edition of this book, written in 1872 and published in 1874, the essay to which was awarded the Hastings gold medal of the London meeting of the British Medical Association, the following passage occurs :

"The ovaries are liable to certain displacements, which may give rise to many disagreeable symptoms without any actual disease of the gland. Thus, one or both ovaries may, by a relaxation of their peritoneal investments, drop into the retro-uterine *cul-de-sac*, and there be a source of great trouble. This will be especially the case if there be at the same time retroflexion or retroversion of the uterus ; for I have known such a displacement of an ovary utterly to prevent the application of any apparatus for the replacement of the uterus, and cause so much suffering as almost to make us discuss the question of ovariotomy."

The details of the history of this operation will be fully discussed in a special chapter.

In cases where the pelvic viscera are displaced downward so as to form more or less complete protrusion, the ovaries of course share in the dislocation, and they may incidentally increase the amount of distress caused by such a condition ; but as this belongs more to diseases of the uterus, I shall not dwell further upon it ; and similarly may dismiss the displacement of the ovaries involved in the inversion of the uterus. A more rare displacement of the ovary occurs as the result of an excess of embryonic transition, the gland being carried downward and forward in the direction taken by the testicles of the male in the course of descent. I have not been fortunate enough to have had any experience of this peculiar form of hernia, and what information I can give upon it must be by quotation. It is, however, a matter of such importance that I feel perfectly justified in quoting at length some of the more notable cases which have been placed on record. The instance which has attracted most attention is that narrated by Mr. Percival Pott, of a patient, aged twenty-three, admitted to St. Bartholomew's Hospital, who had two small swellings, one in each groin, which had been for some months so painful as to entirely prevent her following her occupation as a domestic servant. Her menstruation had been perfectly regular, and the tumors were more painful at that time. Mr. Pott found the tumors to be the ovaries, which had come down through the inguinal canal. He removed them suc-

cessfully, and the patient's health and comfort were speedily and completely re-established; and menstruation never reappeared, the last observation on this point having been made several years after the operation. In the record of the case no mention is made as to whether or not Mr. Pott removed the tubes, or any part of them. In the work of L. C. Deneux many very remarkable examples of displacement of the ovary are given, such as its appearance under the crural arch, through the ischiatic notch, as part of the contents of an umbilical hernia, and of various eccentric ventral and vaginal protrusions. Dr. Busch gives seventy-eight similar observations which he has collected, including fourteen cases in which there was more or less pronounced absence of the uterus, thirteen cases of various kinds of spurious and true hermaphroditism, and four of unicornual or bicornual uterus. These observations go to show—and I think all the facts of comparative anatomy indicate it—that the male organism is an advance upon that of the female, and that these cases would have to be regarded more as arrests of development in the direction of the male organs than hypererchesis of the female.

Kiwisch narrates an instance of an ovary forming part of a hernia through the foramen ovale. When the ovary is displaced in this way, it is, of course, quite as apt to undergo cystic degeneration as an ovary in its normal position; and therefore it is not surprising to find that there is at least one case on record where a cystic ovarian tumor has been removed from outside the inguinal ring. One of the most remarkable cases on record is that published by Mr. W. M. Jones, in the *British Medical Journal* for 1877, in which the patient seems to have had double congenital hernia of the ovaries, and yet became pregnant.

"A. E. C——, aged twenty-three, came to the out-patient room on August 1st, complaining of dragging pains about her abdomen and a swelling in both labia. She was married, and had one child. Ever since she could remember, when she stood up a small lump descended into each labium, going back again on lying down. She had always suffered from pains in the abdomen, and at her menstrual periods the lumps themselves were painful. On examination, a small, roundish tumor was found in each labium, feeling like a testicle, and quite easily returnable into the abdomen through the inguinal canal. It was perfectly dull on percussion, and there was no impulse on coughing. Double ovarian hernia was at once diagnosed, and the diagnosis was confirmed on her coming the following week, during her catamenial period, with both tumors swollen and tender. An ordi-

nary double inguinal truss was given her, which effectually prevented the descent of the tumors, and she reported herself last week as being quite well, and entirely free from her abdominal pains. The great interest in this case lies in its analogy to the descent of the testicle in the male, and in the fact that, notwithstanding the malposition of both ovaries, she had actually been impregnated, and given birth to a living child."

Dr. Werth, of Kiel, narrates an instance of removal of both ovaries on account of double ovarian inguinal hernia. The patient was twenty-three years of age, and had never menstruated, but at each monthly period there was severe abdominal pain. The vagina was occluded, the clitoris was unusually large, and was furnished with a large prepuce; and under the skin, over the inguinal canal on each side, was a body about the size of a pigeon's egg, resembling a testicle in shape and consistence. These bodies lay symmetrically in the axis of the inguinal canal, the upper end corresponding to the inguinal ring, while the lower and inner end corresponded to the anterior margin of the labium majus. At the upper part of each body was an ill-defined substance of the size, form, and consistence of an epididymis. They were both quite irreducible, and looked so very like testicles that the sex of the patient was extremely doubtful. An operation was undertaken for their removal, which was completely successful, and on subsequent examination they proved to be unquestionable ovaries, for they possessed the characteristic follicles containing ova.

Weinlechner (*Wiener med. Wochenschrift*, 1877) relates a case of inguinal hernia produced by a stumble, and for which a truss had been worn for eighteen weeks, when it came down again and became irreducible. This was followed by vomiting and acute pain; as taxis failed, and the symptoms of incarceration became more severe, she was admitted into hospital. In the right groin was a tumor the size of a goose-egg, which was marked into two divisions by Poupart's ligament. The symptoms of incarceration were not quite perfect, and the patient asserted that the hernial tumor increased at the menstrual periods, that the probability of an ovarian hernia was recognized, and it was confirmed by operation. The swollen and irreducible ovary was removed after ligation of its pedicle, and the patient recovered well.

Dr. J. H. Balleray has written an extremely interesting paper upon a case of this kind, to which he appends some valuable references, and I therefore here insert the whole of his observations.

"The hernial tumor was large, and seemed to be divided into

two portions by a sulcus; the skin covering it was somewhat inflamed and tender to the touch. There was something very peculiar about the feel of the tumor, especially at its lower portion. The sensation communicated to the finger was such as to satisfy me at once that I had to deal with something out of the usual order of things, but as to what the real character of the hernial tumor was I had no definite idea. I therefore requested my friend, Dr. E. J. Marsh, to see the case with me. He did so, and seemed to be as much puzzled as I was. He suggested, however, the possibility of the ovary having found its way into the hernial sac. Taxis having failed to effect reduction of the hernia, and the patient's condition being critical, both Dr. Marsh and myself were convinced that an operation was imperative, and that it should be performed without delay.

"Having informed the patient's husband of the result of our deliberations, he requested that we should proceed with the operation at once, if in our judgment it was necessary. Accordingly, with the kind assistance of Drs. Marsh and Rogers, I proceeded to operate. Having cut down to the sac, this was cautiously opened, and about four ounces of brownish yellow fluid escaped, when, to my surprise, there was neither intestine nor omentum to be seen, but the left ovary was found lying near the lower portion of the sac, and tightly strangulated by a firm, fibrous band, which extended from one wall of the sac to the other, and constricted the ovary at about its upper third. This band was divided, and the ovary liberated. It was found to be very deeply congested, but as its vitality did not seem to be destroyed, I decided, after consultation with my *confrères*, to return it into the abdominal cavity. The wound was then closed in the usual way, a pad and bandage applied, and the patient put to bed.

"She rallied well from the operation, and at the end of the third week she was convalescent. The enterocele returned, however, after she began to walk about, and she has, therefore, been obliged to wear a well-adjusted truss, which enables her to attend to her household duties with a greater degree of comfort than she had enjoyed for years before.

"In January, 1864, Mr. Holmes Coote reported, at a meeting of the Royal Medical and Chirurgical Society, a 'case in which the left ovary was found in the sac of an oblique inguinal hernia.' A young woman was brought into St. Bartholomew's Hospital with a swelling in the left groin, and suffering from the symptoms of strangulated hernia. In the course of a few hours the usual operation was performed, when the ovary and Fallopian tube were found in the sac. The left ovary was removed, some thickened omentum cut away, and the patient was put to

bed; but the sickness and constipation continued, and she died four days after the operation. The cause of the sickness, etc., was displacement of the stomach and transverse arch of the colon. In the discussion which followed the report of this case, Mr. Cæsar Hawkins stated that he had met with two cases in which the ovary was found in the hernial sac. In one of these the patient was an elderly woman, and died of peritonitis. In these cases he thought the better practice was to leave the ovary in the sac, as its removal was attended with danger.

"Dr. Frank H. Hamilton, of New York, assisted by Dr. Terry, collected reports of twelve cases of ovarian hernia occurring in the inguinal region, most of which were operated upon before a diagnosis was made. These cases were published in the 'Bellevue Hospital Reports,' 1870, p. 159. Dr. Hamilton himself has seen one example of congenital ovarian inguinal hernia. The late Dr. J. C. Nott met with a case of ovarian hernia at the inguinal ring, in a lady sixty years of age, which, being strangulated, he was able to reduce by taxis. A very interesting case is also reported by Dr. Alfred Meadows, in the 'Transactions of the Obstetrical Society of London,' vol. iii., p. 438.

"In cases of strangulated ovary, the question as to whether the ovary should, after division of the stricture, be returned into the abdominal cavity or left in the hernial sac, ought, in my judgment, to be determined by the condition of the organ itself. The rule by which the surgeon is governed in the management of strangulated intestine or omentum is, I think, applicable to these cases.

"According to Hamilton ('Principles and Practice of Surgery'), Neboux, Mulert, and Krieger returned the ovary into the abdomen, and their patients got well. Deneux, on the other hand, cut away the ovary, and the patient was well in twenty-nine days. Bérard found both the ovary and Fallopian tube in a sac, which he supposed to be a serous cyst. Having opened it, suppuration ensued and the patient died.

"The method of dealing with the ovary adopted in my own case was, I think, justified by the result, and in similar cases I would recommend similar treatment. But in cases in which, from long continuance of the strangulation, or excessive tightness of the stricture, the tissues of the ovary either are or are likely to become gangrenous, removal of the organ is, in my opinion, the proper course to pursue."

One of the most remarkable cases of ovarian hernia is the following, narrated by Dr. Leopold, in which the left cornu of the uterus was included in the protrusion, and removed with its corresponding ovary.

A woman, aged twenty-eight, the issue of parents who had had seventeen children well formed, experienced for the first time, at the age of fourteen, the menstrual molimen. This molimen reappeared regularly every twenty-six or twenty-eight days, but was not followed by any loss. It was accompanied with pains which were localized in the left inguinal region, lasting several days. In process of time the patient remarked that, from the first day of the molimen, a body of the size of a plum rose in the left groin, and that this body became larger every day, and only resumed its former volume several days after the period. At length there resulted from it an excessive irritability and a grave alteration of the nervous system. Married at twenty, she, with the advice of her husband, had recourse to a gynecologist, who, finding the vagina absent, endeavored, by incision and dilatation with tents, to form a passage to the uterus, in order to remedy a supposed retention of the menstrual blood. The treatment was fortunately interrupted, but there occurred afterward, and especially in 1877, vicarious hemorrhages from the nose and lungs.

In March, 1878, she placed herself under the care of Dr. Leopold, who, after having treated her for more than twelve months, published this remarkable observation :

"The breasts, pelvis and vulva were well conformed, but the vagina terminated in a cul-de-sac 3 ctm. in depth. In this place there was no indication of a vaginal portion, and above there was no trace of either uterus or ovaries.

In the left groin, on a level with the external inguinal ring, was perceived an uneven tumor of about the size of half a hen's egg, painful, hardly movable, almost on a parallel in its great axis with the inguinal fold, and resembling an ovary abnormally situated. On the right side the inguinal region was normal ; but on deep pressure a small body was felt, resembling that met with on the left side, but more movable, less painful, and much smaller.

The pain caused by the tumor on the left side became at length so acute that an operation was performed on February 15, 1879. The tumor was removed. It was not, as had been diagnosed, *an ovary*, but a *rudimentary uterine cornu*. At the same time the neighboring tube and ovary were removed. After the abdominal cavity had been thoroughly cleansed, and the ligatures cut short, the T-shaped wound was closed by five deep sutures of silver wire, including the peritoneum, and by several superficial sutures of silk thread, and at the point of union of the two incisions a small drainage-tube was introduced to a depth of about one centimetre.

No fever followed, and fourteen days afterward the wound had perfectly healed. The time of the menstrual epoch passed without the least trouble, only that there were some contractions in the muscles of the left leg.

The amputated cornu uteri, of the size of the thumb, presented the histological structure of the uterus; that is, smooth fibres, conjunctive tissue, vessels, and glands. The ovary presented all the characteristics of normal structure, with yellow bodies and vesicles at various stages of development. The tube has a pavilion beautifully fringed, but there was no canal continuous with the infundibulum."

Dr. Alfred Meadows has also placed upon record, in the "Transactions of the Obstetrical Society" (vol. ii.), a case in which he removed a hernial ovary.

I have also met with a very singular case of ovarian displacement, where the condition was evidently congenital, and was discovered only when an operation had to be performed for the removal of the misplaced ovary on account of cystic degeneration. The tumor was of very large size, and for its removal the usual median incision was made between the umbilicus and the pubes. No difficulty was encountered until I attempted to drag the upper part down through the incision, when I found a broad band of union extending upward from the umbilicus. The peritoneum passed from the abdominal walls on to the tumor, just as it does on to the rectum, and the union was evidently not merely inflammatory adhesion. On dividing the peritoneum, I found that the common tendon formed part of the cyst-wall, and that the fibres of the rectus abdominis muscle were inserted into the cyst. The round ligament of the liver ran through the cyst-wall to the umbilicus, and, on being cut through, the umbilical vein contained in it bled profusely, and had to be tied. Very careful dissection had to be made to remove the cyst, and when it was completed it was found that a large triangular gap was left in the abdominal wall, covered only by skin, and having its base at the umbilicus and its apex at the xiphoid cartilage. This gap was closed by subcutaneous stitches of silver wire, and the patient made a complete recovery, and has since been safely confined of a living child. Careful examination of the tumor satisfied me that the only explanation which could possibly be offered of these unusual conditions was that the ovary had become attached to the cleft in the visceral arches during early embryonic life, and had subsequently been affected by cystic degeneration.

Klob has described a twisting of the ovary on its axis, which is probably congenital, and has not yet been found to be of any pathological importance in an otherwise healthy ovary. In the cystic ovary a similar twisting has been observed to a more complete extent, and with disastrous results, as will afterward be described. The ovary is said sometimes to be completely detached from its normal position and relations, and forms new attachments elsewhere. This occurs with the healthy ovary, and, as Mr. Spencer Wells has shown, also probably after it has undergone degeneration. How and when it occurs have not yet been satisfactorily explained, but in all probability the curious axial rotation to which ovarian tumors are subject, as described in a subsequent chapter, has something to do with it.

In some rare instances we find the peritoneal layers so deficient that the ordinary mesenteries and ligamentous folds are completely absent. I have described several cases of congenital defects of the peritoneum (*Dublin Quarterly Journal of Medical Science* for February, 1869), but the most interesting I have met with is one I published in the *Obstetrical Journal* for October, 1876. There the peritoneal sac was wholly absent, the intestines being connected together by an abundance of extremely loose cellular tissue. In the pelvis it was absolutely impossible, on post-mortem examination, to identify any organ but the uterus, from the entire absence of any of the usual peritoneal limitations. Thus, the bladder was torn open in removing the uterus, under the impression that it was some of the loose areolar tissue, and its nature was recognized only by the escape of urine. Two masses close to the uterus, one on either side, when cleared of the abundant connective tissue and laid open, proved to be the ovaries, and in the left there was the clot of a recent Graafian follicle, the ovum of which, if it ever were extruded, must have been arrested in the surrounding tissue. Over the right ovary the Fallopian tube seemed to course in a normal direction, but it became lost in a mass of connective tissue, and I could find no appearance of the fimbriated expansion. On the left side there was an appearance of a rudimentary tube in a fold of tissue.

The menstrual history of the patient, as ascertained by my friend Dr. Hickinbotham, in consultation with whom I saw the patient during her life, was in no way abnormal, and she was twenty-five years of age. The cause of her death was the obstruction of scybalous masses in a bunch of coils of intestine, along which they could not pass, apparently because the intestines were unable to move about.

Another class of remarkable errors of development of the ovary consists of those to which I have given the name of hy-

pererchesis. So far as I know, they are limited to the development of fetal structures in the ovum before it has left the follicle, this taking place probably during the fetal existence of the ovary, and constituting, in after-life, the variety of ovarian tumor known as "dermoid," under which head this remarkable change will be found fully discussed. They are also seen in the peculiar growths resulting, as I believe, from the extended life of ova which have been dropped out of the follicle into the peritoneal cavity, and there have continued their existence, becoming developed into huge cysts, instead of perishing, as they usually do. In the chapter upon ovarian tumors several instances of these will be described.

The errors of development of the Fallopian tube partake very much of the same character as those affecting the ovary. When the gland is insufficiently developed, its tube will be found correspondingly defective. I have, however, already narrated a case in which, coincident with defective development of the peritoneum, the growth of the tubes had been apparently arrested, while the ovaries had not suffered in this way. Sometimes, also, we find the tubes are displaced by congenital defect, being placed either too low, or being too short, or having their infundibula too small to enable them to acquire their periodic relations to the ovaries properly, and in these cases there is of necessity a resulting sterility. Conversely, we sometimes find that a badly developed ovary, or, it may be, an ovary which has suffered from inflammation, is displaced downward and outward, beyond the reach of the normal tube; and here again sterility is produced.

In some instances of arrested development of the tubes they are found to be occluded at both ends, and distended by the fluid into cysts. I have found, in several instances, that this occlusion at the outer extremity of the tube was formed by an adhesion of a permanent kind to the infundibulum of the ovary, perhaps of a congenital origin, but more probably the result of inflammation. In these cases extreme menstrual pain has resulted from periodic distention of the tubes, the patients have been sterile, and, when married, have been wholly unable to perform their marital functions. They have wandered about from one practitioner to another, and from hospital to hospital, vainly seeking relief, and the only means of giving it to them is to remove the ovaries and the tubes. Two or three characteristic examples of this condition I propose here to describe.

Such inflammatory affections as spread into the uterus are apt to pass along the tubes and produce ovarian or peritoneal

mischief. In this way the inflammation of the tubes is of immense importance, and it may be suspected after the appearance of indications of the more serious extension of the disease. It may also have an important result, in addition to the extension of the inflammation, in the form of destructive desquamation of the ciliated epithelium which lines the tubes. The function of this ciliated epithelium, as well as that of the peristaltic movements of the tubes, is evidently chiefly for the passage downward of the ovum; but it also seems to me likely that it is to hinder the contact of the spermatozoa with the ovum until the latter has reached the cavity suited for its maturation. The statement that impregnation takes place before the ovum has reached the true uterus seems to me an assumption based on insufficient evidence—indeed, on no evidence at all. *A priori*, we may safely say that, if it is the rule, Fallopian pregnancies and the disasters which follow them ought to be much more common than they are, and I believe it to be more than likely that the real cause of this accident is the coincidence of a set of circumstances, the most important of which is the destruction or insufficiency of the ciliary movement. Inflammatory desquamation may then be a cause, and probably is not an infrequent one, of tubal pregnancy. Destruction of the tubal epithelium may also, and undoubtedly often does, cause atrophy or occlusion of the tubes, and occlusion of the apertures of the tubes may be the cause of another disease of the tubes, of which I have seen a considerable number of cases—dropsical distention. The fact which is mentioned by many authors, that both tubes are usually affected, is suggestive that tubal dropsy is generally the result of inflammatory action. The distended tubes seldom reach a large size, and the majority of the cases, where they are described as having reached such a size as to rival and demand the treatment of ovarian tumors, are open to the suspicion of inaccurate description. There is, however, one case given by Dr. Peaslee in his book on ovarian tumors, about which there can be no doubt. It contained eighteen pounds of fluid, and would have been removed if the patient had recovered from the tapping.

In some six or seven cases where I have found the Fallopian tubes distended with fluid, and where I could not remove them, I have drained them by the process I have described elsewhere as applicable to cysts of the liver and kidney and to pelvic abscesses. I first of all expose the cyst, then empty it by the aspirator, and then enlarge the opening into and stitch its edges to the edges of the parietal wound by a continuous suture, so as completely to close the peritoneal cavity. The cavity of the

cyst is carefully drained in front, or by a tube passing both upward and downward, as well as into the vagina. I have in this way cured cases of hydro-, pyo-, and hæmatosalpinx, but the results and the rate of progress are not nearly so satisfactory as when the uterine appendages are completely removed.

This occlusion of the Fallopian tubes, which is certainly of very frequent occurrence, is facilitated by the relations of the infundibulum to the ovary, this being far more intimate than is generally imagined. Fig. 1 (p. 4) gives a perfectly exact representation of the organs, but in order to display them their relations have been destroyed, for the fimbriæ are always in close relation to the ovary, and the tubes, as I have said, curl over and around the ovary, so that the infundibulum is in contact with the lower and posterior surface of the ovary, the axis of which is often nearly vertical generally, but not always. Adhesion, as I have said, occurs at the menstrual periods independently of ovulation, and I think it more than probable that not more than one in ten of the ova shed by the glands really enter the tubes. The rest drop into the peritoneum and die there.

The largest collection of fluid which I have seen in an occluded Fallopian tube occurred in the following case, the more remarkable in that the disease was unilateral.

E. E. T——, aged twenty-eight, was placed under my care by Mr. Watkin Williams, of this town. She had been married, but had been obliged to divorce her husband for misconduct. It is more than probable that gonorrhœa had been communicated to her about five years before I saw her. From that date she had suffered from intense pain during the menstrual period, and had become very much emaciated. She had been under a great many doctors without obtaining relief. I discovered a small cystic tumor behind and to the right of the uterus, freely movable, but very painful when moved. I advised its removal, and this I undertook on May 23, 1879. I found it to consist of the right Fallopian tube, distended by about a pint of clear serum. The infundibulum was glued on to the right ovary, and the uterine part of the tube was distended like a tortuous sausage, the greater part of the cyst being made from the outer half of the tube. I removed the tube and left the ovary. She made an easy recovery, is now in robust and perfect health, and has married again.

Dr. Saundby examined the fluid removed, and gave me the following report upon it: specific gravity, 1014; reaction alkaline, pale greenish color, clear, with scanty grayish deposit; contains about three-fifths of its volume of an albuminous body, having

all the characters of serum-albumen. After removing the albumen, the filtrate precipitates with mercuric nitrate (urea ?) and with argentic nitrate (chloride of sodium ?). The microscopical examination showed only a few indifferent cells.

E. C——, aged thirty-two, was married at seventeen years of age, and had her first child when she was eighteen, and her second in the following year. She was quite well until 1876, when she had a smart attack of inflammation of the pelvis, and ever after that she had extreme pain at her periods, when she had to remain in bed for several days; and she described her sufferings as amounting to agony, and resembling labor-pains more than anything that she knew of. She was seldom free from pain in the back, and for the last three years she has been utterly unable to endure married life. I found the uterus slightly retroverted, and on each side of it there was a distinct mass in the position of the ovary, large, fixed, and extremely tender. She had been under a great variety of treatments, without the slightest benefit. On October 5, 1880, I made an exploratory incision, and found both ovaries adherent in the cul-de-sac, the infundibula of both tubes occluded, and the tubes themselves distended into cysts. The whole of the organs were matted together, and the operation for their complete removal was extremely difficult. The amount of fluid in each tube was about two ounces. She made an uninterrupted recovery from the operation until the monthly period after, at which time she had a small hæmatocele on the right side, coincident with a slight menstrual appearance. From this, however, she speedily recovered, and on February 17th last I found the uterus perfectly free and normal in direction. I last saw her on March 26th, and found her in perfect health, absolutely free from pain, and she told me that she had seen no appearance of menstruation since November, and that marital functions had been resumed without the slightest pain.

H. S——, aged thirty-seven, had been married seventeen years, and had only one child, fifteen years ago. She did not recover well from that confinement, and ever since had menstruated too often and too profusely, being rarely a fortnight clear. I found the fundus large and tender, somewhat anteverted, and what I regarded as the ovaries formed two large masses low down, and somewhat behind the uterus. For a long time past sexual intercourse had been quite impossible on account of the suffering it caused her. Dr. C. H. Phillips, of Hanley, who placed her under my care, had exercised a large amount

of ingenuity in her treatment, without any benefit, and from February till August, 1880, we conducted further treatment equally in vain. On August 3d I opened the abdomen, and found the ovaries large, completely adherent in the cul-de-sac, covered with lymph, and having the infundibula of the tubes occluded. The tubes were distended into large cysts, each containing from four to five ounces of clear serum. The organs had to be very carefully detached, as the adhesions were extremely firm, and the hemorrhage during the operation was tolerably profuse. Her recovery from the operation was rapid and easy, and the only distresses she encountered were the climacteric flushings. In May last Dr. Phillips sent me a most satisfactory account of her condition.

A. S——, aged thirty-eight, had been twice married, and had had five children by her first husband, the youngest being twelve years of age. She has had no children by the second husband, to whom she has been married six years. After her second

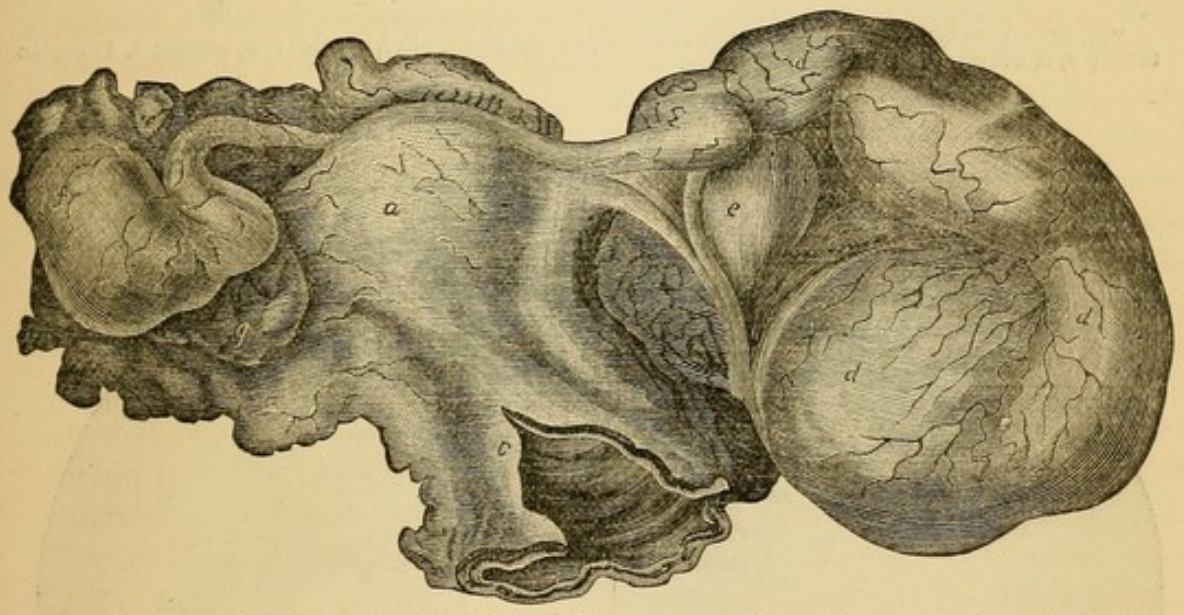


FIG. 19.—Bilateral hydrosalpinx (after Hooper, from Arthur Farre, Encyc. Anat. and Physiol.): *a*, uterus; *b*, vagina; *c*, os uteri; *d* and *f*, Fallopian tubes; *e*, ovary.

marriage she seems to have had an attack of pelvic inflammation, and ever since she has had intense pain at her periods. She referred this pain distinctly to the region of the ovaries. For somewhere about three years she had been wholly unable to submit to intercourse, and her domestic life was thereby rendered extremely uncomfortable. I found the uterus to be normal in position, and on each side of it I found a mass situated quite low down, and having characteristics exactly like those in the two cases given above, so that I had no hesitation in making up

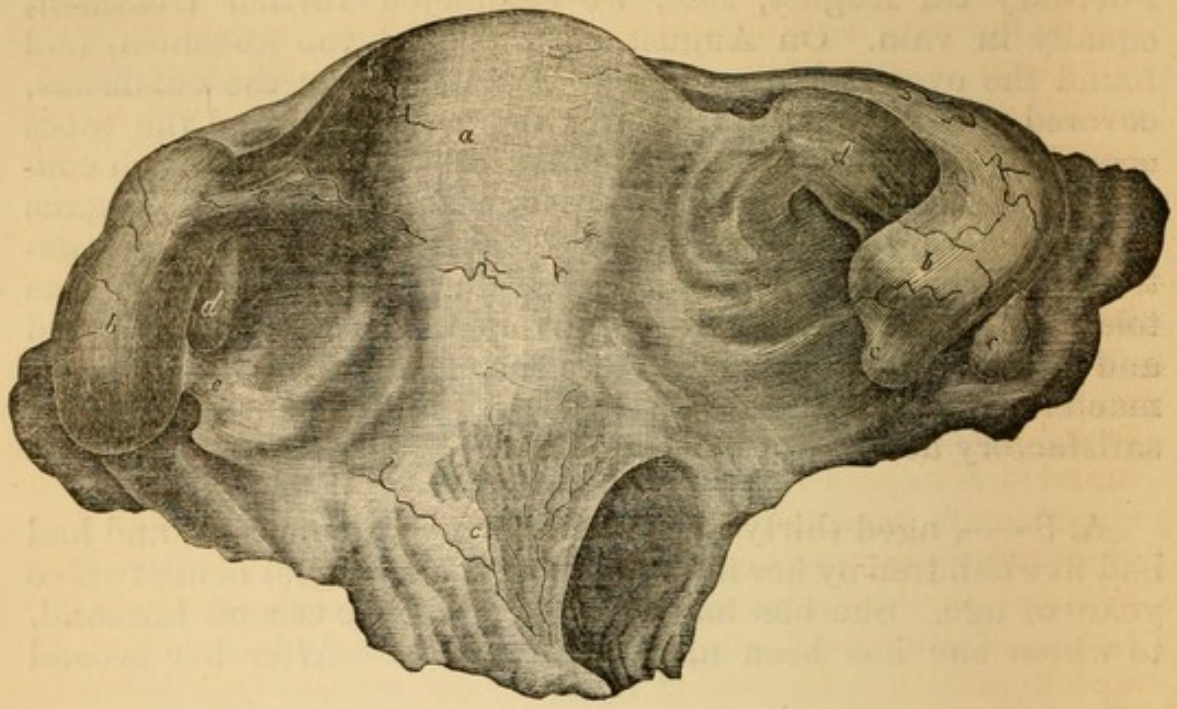


FIG. 20.—Occluded and adherent Fallopian tubes (Arthur Farre, after Hooper): *a*, uterus; *b*, Fallopian tubes; *d*, *d*, ovaries; *e*, *e*, bands of adhesion.

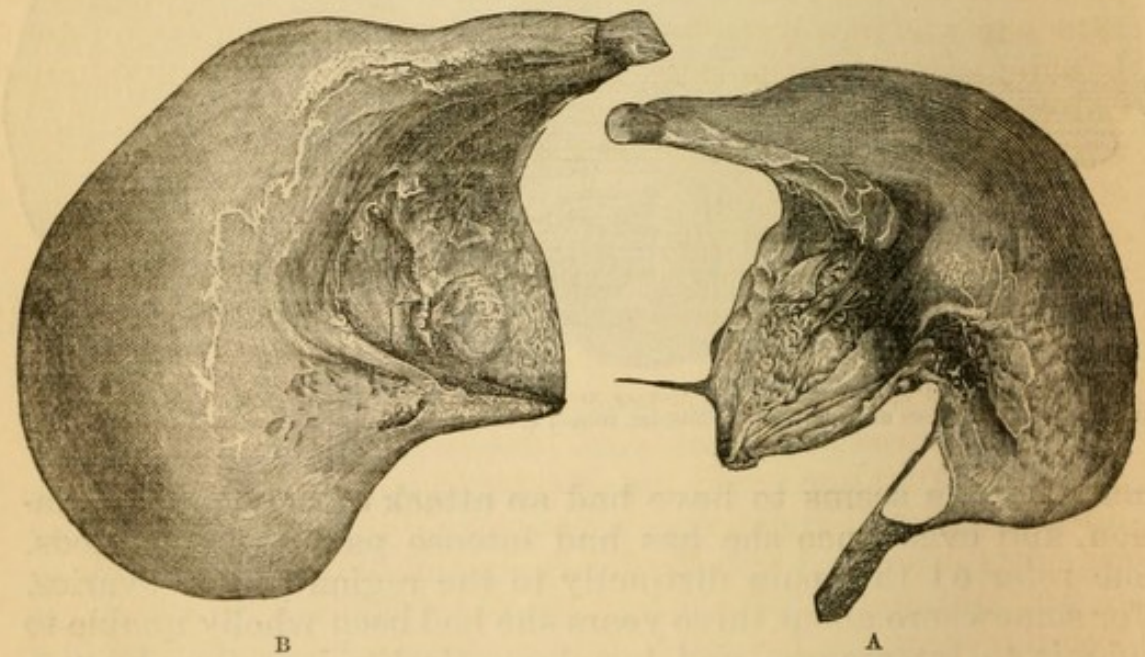


FIG. 21.—Right (A) and left (B) Fallopian Tubes and Ovaries, removed by abdominal section from a patient aged thirty, who had suffered early in her married life from gonorrhœa. The large bulbous masses are the Fallopian tubes occluded and distended with serum (hydrosalpinx), the shrivelled ovaries being shown in the convexity of the tubes. (From a photograph slightly reduced. Preparation now in Museum of Royal College of Surgeons.)

my mind that she had occlusion and distention of the tubes. Dr. Cameron, of Bilston, who had placed her under my care, sent her to me with a statement to the effect that he was perfectly sure that nothing but an operation would relieve her. This I performed upon May 21st, and found matters exactly as described in the last case. Her recovery was rapid, and the relief immediate and complete.

In some cases we find that the contents of these closed tubes consist of pus or menstrual fluid, both of which I can instance from my experience in the following cases.

Mrs. L——, aged thirty-four, was sent to me by Dr. McLinctock, of Church Stretton, in September, 1878. She had been married four and a half years, and had one child. Since her confinement she had never been well, and had suffered from symptoms which clearly were those of subinvolution and retroflexion. She had profuse menstruation, and on consulting a physician, in July, 1872, she was told she had a tumor behind the womb, and that the womb must be dilated and the tumor removed. For this purpose a sponge-tent was introduced, and inadvertently left in, according to her reiterated statement, for nine days. She had suffered from an attack of "acute inflammation of the bowels, and was in her bed for seven weeks." This incident occurred, I am pleased to say, neither in Church Stretton nor in Birmingham. When she came to me on September 24, 1878, she was in a wretched state of exhaustion and emaciation. I found a mass on the right side of the uterus, which fluctuated indistinctly. The uterus was retroflexed and fixed completely by perimetric effusion. I tapped the mass, and removed about four ounces of pus from what I now know was an abscess of the right ovary. She was much relieved, and returned to Church Stretton under Dr. McLinctock's care, and slowly gained some strength. She did not make anything like a recovery, however, and her medical attendant sent her back several times to me, and together we carried on a variety of treatment, without much benefit. The uterus remained fixed, and all efforts to replace it were so painful that she could not endure them. She could not bear intercourse, and she never was a day without pain, and her life was fitly described as a burden to herself and her relatives.

Dr. McLinctock sent her down to me again at the end of last February, and in his letter he told me he was sure something more must be done if our patient's life was to be saved. I found that the mass on the right of the uterus was just as I had left it, that the uterus was still fixed and retroflexed, that there was now a more clearly defined mass to the left of the uterus, and

that the whole roof of the pelvis was exquisitely tender to the touch. Her temperature went up at night, and she had night-sweats; and although I could feel no fluctuation, I had no doubt there was pus somewhere. I therefore advised and performed an exploratory incision on March 6th. I found the pelvis roofed over by adherent coils of intestine, which I lifted with much trouble. Below this the whole of the organs were matted together, and their identification was a matter of the greatest difficulty. Finally, I succeeded in recognizing the right Fallopian tube, distended into a cyst with greatly thickened walls, and full of pus. Below it, and intimately adherent to it, lay the ovary, as large as an orange, and containing some old cheesy matter, the remains, probably, of the abscess which I tapped two and a half years before. The uterus was bound down in the cul-de-sac by old adhesions, and from these I relieved it. I found the left ovary adherent below the fundus, and from it the left Fallopian tube ran a circuitous course, like a sausage in appearance, and adherent to the brim of the pelvis, the uterus, and a piece of small intestine. It contained about two ounces of pus. I removed both ovaries and both tubes, cutting the latter off close to their uterine attachments. The hemorrhage during the operation was very troublesome, but was controlled by sponge-pressure. Mr. J. Raffles Harmar assisted me, and Mr. Wright Wilson gave ether.

The patient has recovered without a bad symptom, and is perfectly free from pain for the first time since the incident of the sponge-tent. The uterus is now perfectly free, and any movement of it gives her no pain. My only regret about this case is that I did not operate two years before.

J. H——, aged thirty-one, came under my care in April last, after having been under the treatment of many well-known practitioners. There was no history of any acute illness, but for many months she had been ailing, unable to walk, and constantly in pain, and her sufferings during the menstrual week were very great. She was desirous of getting married, but, as she was quite unable to get about, this was out of the question. She had been told by all her doctors that she suffered from a displaced womb. On either side of the uterus was a large, fixed mass, the slightest pressure on which produced pain of a sickening kind. No fluctuation could be determined. She used iodine blisters and took bromide of potash for two months, without any benefit. At the end of June I proposed an abdominal section, but only with the result that she consulted another practitioner, greatly my senior, and under whose care she had previously

been, who told her that she ought never to submit to any such proposal.

She came back to me, however, in August, and I took advantage of the presence of Dr. Battey in my house as a guest to get his advice. He agreed with me that it was a case urgently demanding interference. This induced her friends to take her to an eminent specialist at a distance, who characterized my proposal as absurd; but yet the poor girl got no better, and she returned to me on October 18th, determined to have the operation performed.

One matter of importance was the question of her affianced husband's knowledge and consent, and this I desired should be secured. For that purpose I had an interview with him, and fully explained the case and my proposed treatment. He raised not the slightest objection, and expressed himself as desirous only of obtaining relief for his *fiancée*. The operation was performed on October 21st, and I found the pelvic organs completely matted together. After I had separated them—a matter of great difficulty, taking a long time in its performance—I found the left Fallopian tube distended and as big as an orange. Unfortunately it burst, and the curdy pus with which it was filled was scattered into the peritoneum; and a similar misfortune occurred in the removal of the right tube, which was also distended with pus. I need not say that I took every care to cleanse the pelvis well out, and I used a drainage-tube. The removal of the tubes and ovaries in this case constituted the most protracted and difficult operation I have ever performed. The patient recovered speedily and completely, has not menstruated, and is completely free from pain.

At the meeting of the Société Anatomique, held on January 16, 1880, a case of pyosalpinx was narrated from Dr. Bernutz's service at La Charité. The patient was aged twenty-nine, and was admitted with very severe symptoms, pointing to pelvic inflammation, and subsequently, peritonitis. She died four days after admission, and, on a post-mortem examination, suppurative peritonitis was found to have spread up from the pelvis, having arisen from the rupture of a tubal abscess. The following is a description of the parts:

"The tubes extended one on either side, and were the seat of the principal alterations. The internal half of each tube was healthy, and its direction normal, but the outer half presented three or four dilatations, varying in size, the largest being situated at the outer extremity, being formed by the occlusion of the pavilion, so that there was no opening into the tube, which was distended with pus. These dilatations communicated one with

another, and the internal mucous surface was smooth and softened, but otherwise normal. There was no communication between the uterus and the tubes. The ovaries did not occupy their usual situation, being both displaced downward and embraced by the concavity of the tube, making with this a largish mass. On the left side there was a peculiar arrangement; a cyst occupied the pavilion of the tube, of the size of a hen's egg, which seemed to be directly continuous with the cavity of the ovary, and the two cysts were entirely empty. The internal surface of the tube was smooth, while that of the ovary was very rough and much reddened, the difference being distinctly marked by a line of division of the two structures. The ovary was not greatly enlarged, and upon its posterior surface, toward the middle, was found a small rupture through which the contents had been extravasated into the peritoneum."

M. Bernutz remarks that in all probability the suppuration of the tubes and left ovary was of ancient date, and that the fatal peritonitis was undoubtedly due to the perforation of the abscess into the peritoneum. He does not give any explanation or history of the pyosalpinx. The case, however, is to me an extremely interesting one, for it illustrates exactly the same conditions as those seen in the case narrated above, and I think there is little reason to doubt that, if the patient had been seen earlier in her history, the symptoms would have been found sufficiently severe to warrant an abdominal section; and, if that had been done before the rupture, not only would the patient's life have been saved, but her disease would have been cured. Even after the rupture of the cyst and the onset of peritonitis, had the case been under my care, I would have opened the abdomen without the slightest hesitation, have cleaned out the cavity, and removed the cause of the disease. I have had numerous cases in my recent practice where such a proceeding—which would have been regarded as madness three years ago—has had the most brilliantly successful results.

As another instance of pyo-salpinx, I may give the following:

M. F——, aged twenty-six, had been, ever since the age of seventeen, living an immoral life. About three years ago she suffered from gonorrhœa, which was followed by severe pelvic inflammation, and ever since that time she had suffered from severe menstrual pain. About six weeks previous to my seeing her she had been exposed for a whole night to extreme cold, and after that suffered from great pelvic pain. She was placed under my care in March last, by Mr. John Green, of this town. I found her suffering from all the symptoms of pelvic suppuration, and

there was a fluctuating pelvic tumor on the left side of the uterus. This I diagnosed to be the left Fallopian tube distended with pus. I opened the abdomen on March 28th, and found my diagnosis correct. It was, however, quite impossible to remove the tube, and I therefore had to content myself with emptying it, dragging it up to the wound, securing the two openings together by a continuous suture, and fastening in a drainage-tube. This was kept in for some weeks, and she made a satisfactory recovery. Her menstrual suffering, however, is quite unrelieved, and therefore the cure is only partial. It could only have been made complete by the removal of both tubes and ovaries, but this was made quite impossible by the dense adhesions formed by the previous inflammation. In such a case, of course, there is not the slightest hope of her ever becoming a mother, though, as she has left her irregular life and has been married for about a year, this would be desirable. As it is, she will certainly remain a sufferer until she reaches the climacteric.

A few days ago I operated on a patient sent to me by Dr. Standish, of Cradley, on account of persistent pelvic pain, greatly aggravated at each period, and which no treatment relieved. I could find nothing on examination, and I had very great misgivings about operating in a case where the conditions were purely subjective. I opened the abdomen, however, and found both ovaries and tubes adherent. The tubes were occluded, and the outer extremity of each was occupied by a small chronic abscess, which amply accounted for all the symptoms. The preparations are now in the museum of the College of Surgeons. The patient has made an excellent recovery.

The features common to these cases are (1) a history of severe pelvic inflammation, though sometimes this cannot be ascertained with precision. Its origin is variously ascribed as from gonorrhœa, a chill, or sudden stoppage of menstruation, and (most frequently) inflammation after labor or a miscarriage. There is always (2) pain, which comes on after exertion, and especially after intercourse, and generally becomes intensified when menstruation appears. At this time the pain is often described as excruciating, and it lasts throughout the period. In the majority of instances there is (3) irregular and profuse menstruation, often amounting to hemorrhage.

The physical signs are (1) swellings at the seat of the ovaries, which are always tender and generally quite fixed. Distinct fluctuation can often be felt, and their peculiar sausage-like shape has frequently enabled me to diagnose correctly the condition previous to the operation.

No treatment whatever relieves these cases, save removal of the uterine appendages.

Most of my cases had been in the hands of some of our most eminent specialists before they came to me, and an infinite variety of treatments, both by drug and operation, had been used fruitlessly. They had all been treated by pessaries, and many of them had had their cervical canals dilated and cut.

Tapping the dilated tubes is of no use and is extremely difficult.

At the operation the organs are nearly always found matted to the pelvic wall and to intestines, and their removal is often extremely difficult—far more difficult than removal of an ovarian cystoma. I always remove the ovaries along with the tubes, as without the ducts the glands are, of course, useless.

All my patients—twenty-two in number—have recovered, and of those in which a sufficient time has elapsed since the operation I can say confidently that they are all completely cured.

All the cases had been, of course, rendered sterile by the disease, and in most of them the marital function was also destroyed. I have not found that the operation has had any other effect than that of restoration of sexual activity where it has been lost.

Menstruation has, in most cases, been arrested immediately, but in a few it has lingered for a month or two.

The pathological condition of most importance is practically the same in all these cases, and arises, I think, from an attack of acute or subacute oöphoritis or peri-oöphoritis. During this process the trumpet-shaped extremity of the tube approaches the ovary for its normal temporary attachment; and this, by the inflammatory process, becomes permanent. Certain it is that, in nearly all the cases, permanent attachment of the tube to the ovary is to be seen. Probably, after the attachment has occurred, the inflammatory process extends to the tubes, there is a desquamation of the ciliated epithelium, and occlusion of the tube at its uterine extremity occurs.

The nature of the contents of the tube is determined by causes which I do not understand. The most common of the varieties of this disease is hydrosalpinx, and the rarest is hæmatosalpinx.

In connection with these cases I may refer to a curious series of phenomena which has repeatedly fallen under my notice, and which, I gather from the writings of other observers, has also been noticed by them. Thus, in several cases where I have opened the abdomen for the purpose of removal of the ovaries on account of severe and intractable pelvic pain, I found the organs matted together with all the appearances of old pelvic

peritonitis—so much so that I have been unable to complete the operation. I have also, as I have already stated, found in very many instances the Fallopian tubes occluded and distended with fluid which, in several cases, was purulent. Looking back upon the histories given by some of the patients in whom I was unable to complete the operation, I have found distinct statements made of recurrent attacks of severe pelvic inflammation. I have also noted, in some of them who had been under my care for prolonged periods, that at some of my examinations I found distinct tumors on one or both sides of the uterus, while at other examinations no trace of these tumors could be seen. Speaking of an absolutely similar experience, Dr. Mathews Duncan says: "After a time the tumor disappears. Frequently its disappearance is accompanied by adhesive perimetritis. Now, what has happened in these cases? Of course it may be said that it is a case of mere ignorance, or that the cysts were parovarian; but, to this latter explanation there is, for me, the great objection that the cases occur more frequently by far than to admit of their being justly so explained. We must suppose, therefore, the bursting of the not infrequent small follicular dropsies. I would further add that the bursting of simple parovarian cysts does not appear to me to be followed by perimetritis, or accompanied by it, so generally in the case of the disease of which I have been speaking."

In only one of my cases have I seen reason to accept Dr. Duncan's explanation, that is to say, in only one of them have I found the small cyst of the ovary (which was probably on the point of bursting), and elsewhere in the ovaries, distinct evidence that similar ruptures had occurred before. I feel perfectly certain that in some of the cases there can be no doubt that Dr. Duncan's explanation is the correct one; but I have so much more frequently found the Fallopian tubes at fault, that I am inclined to believe that in the great majority of these cases of recurrent pelvic peritonitis, due to rupture of a cyst, it is in the tubes that we shall find the origin of the mischief.

Looking at some of my preparations of hydro- and pyosalpinx, I find it difficult to resist the conclusion that periodic rupture of the distended infundibulum is a somewhat frequent occurrence in them. It is, of course, very likely that in many cases a cure is effected in this way; but in others the disease is only aggravated, and probably in many the rupture is fatal. In a discussion which took place at a meeting of the American Gynecological Society in 1880, I find a corroboration of my views in some of the remarks of my friend, Dr. James R. Chadwick, of Boston. He speaks as follows: "Now I have had several cases

presenting a similar succession of symptoms, in which I have been able to detect a cyst on one side or the other of the uterus, which I have diagnosticated as a cyst of the Fallopian tube, but have never verified my opinion by operation or autopsy. My explanation of the symptom is this: that fluid collects in the tubes, of which the fimbriated end is occluded until it is greatly distended; as the monthly period approaches, peristaltic action repeatedly occurs in the tube, attended with great pain, which tends to expel the fluid through the uterine end temporarily occluded. This action continues at intervals with increasing violence, until, during the catamenial relaxation of the uterus, the fluid contents of the tube are forced through the opening into the uterine cavity, with immediate relief. If this view be correct, the removal of the ovaries was of no consequence, though the operation was quite as urgently called for, as the best and perhaps the only means of relieving the cystic condition of the tubes, and securing to the patient immunity from her suffering."

A patient who had been under my care for some months died suddenly a few weeks ago, and the pelvic organs were obtained at the post-mortem examination. She came under my care in January last, as an out-patient at the Women's Hospital, with symptoms of chronic metritis, including severe menorrhagia. On April 28th I recognized the presence of a small cyst on the right side, as large as an orange. At the end of May she had an attack of pelvic peritonitis. On June 13th the tumor had disappeared. On August 15th it had returned, and it again disappeared in September, its disappearance being accompanied by inflammatory symptoms. On October 10th it was again to be felt, and about the 30th she died, and Mr. J. Garman, of Wednesbury, obtained the preparation for me when I told him that she had a bursting pelvic cyst. Mr. Alban Doran has examined the preparation, and confirms my opinion that the cyst is the right Fallopian tube. Doubtless many a mysterious case of peritonitis arises from some such cause as this.

Thus it will be seen that recent advances in pelvic surgery, by which we are enabled to deal with a great number of cases which formerly were left to unrelieved suffering, and often to inevitable death, have made it clear that there are very many cases in which the tubes, having become occluded by acute or chronic inflammation, are occupied by purulent fluid—a condition which can be remedied by surgical operation alone.

In its acute stage inflammation of the tubes is a most formidable disease, and so rapidly ends in general peritonitis that we can hardly recognize the necessity for interfering before it is too late to do anything. I have seen several fatal cases of peritoni-

tis which undoubtedly had their origin from inflammation of the Fallopian tubes, and which ought to have been treated by abdominal section. Indeed, I do not think I shall again willingly allow a case of peritonitis to die without an effort to save her by an operation. I am fully persuaded that we might save many such cases by boldly opening the abdomen and cleansing its cavity. In three cases of chronic peritonitis I have done this, and cured the patients completely.

Pyo-salpinx is, however, a more chronic condition, some of the cases upon which I have operated having lasted over several years. Probably they all arise in some acute inflammation which occludes both openings of the tubes, and converts one or both—generally both—into chronic abscesses; yet in the last case I have narrated there was in the history no incident of an acute kind. Dr. S. Wilks has met with two cases where pyo-salpinx caused general pyæmia, one case proving fatal from an abscess in the liver, the other from an abscess in the brain.

Besides pus, we occasionally find that an occluded Fallopian tube may contain bloody fluid (*hæmato-salpinx*) of menstrual origin. It has been completely established, especially by the observations of Bernutz and Goupil, that the tubes generally share in the secretion of the menstrual fluid, and when the clamp used to be employed in ovariotomy, we constantly saw menstrual weeping from the stump. It is not, therefore, surprising that occasionally we should meet with a case of hæmato-salpinx. Dr. Alfred Medows records the post-mortem examination of one in the eighth volume of the "Transactions of the London Obstetrical Society," in which "it was found that both Fallopian tubes were enlarged, not regularly and uniformly, but so as to form a kind of cyst. On the right side there were two such enlargements; on the other, one. There was no evidence of any communication between these dilatations and the fimbriated opening. On the left side there was not even an opening into the uterus, the ostium uterinum being completely occluded. They were all filled with a dark, thick, grumous fluid, of a prune-juice color. It is evident that, in this case, we have an example of what Bernutz and Goupil contend for—menstrual retention within the Fallopian tube. The one fact which is clearly revealed is, that the Fallopian tubes do, as well as the uterus, take part in the menstrual secretion: and hence, when any obstruction occurs to the passage of that secretion into the uterine cavity, and so externally, we get the resulting symptoms of menstrual retention."

The following case has occurred in my practice, and I have

fortunately been able to operate on it successfully. The patient remains now (1881) in perfect health :

Miss M——, aged thirty-eight, was sent to me in the beginning of 1877, by my friend Mr. Alfred Freer, of Stourbridge. In November, 1876, she had an ill-defined illness, during which she had obscure pelvic pains, accompanied by fever. Previously to this illness she had been in good health, and had menstruated regularly. After it, she had severe pain during the whole period of menstruation, and she gradually increased in size until Mr. Freer discovered a pelvic tumor in February last. I found the tumor to be pear-shaped, quite movable, attached to the uterus at the left cornu, evidently unilocular, and about the size of an infant's head. I diagnosed it as a cyst of the parovarium, and advised that it should be tapped after it had increased in size sufficiently to warrant interference. She returned to me in May, with the tumor increased so as to be felt above the umbilicus. I advised her to come again in a month. She came, however, before the expiration of that period, on account of a sudden accession of serious symptoms; and when I saw her, on June 20th, there could be no doubt she was suffering from peritonitis. Her pulse was 130; the temperature was 38.4° C. (101.12° Fahr.), and rose to 39.6° C. (103.28° Fahr.) in the evening ; and there was excessive pain all over the abdomen, with considerable flatulent distention. I administered opium freely, and applied counter-irritation over the epigastrium.

On the morning of the 21st she was easier, but the temperature and pulse had not fallen. I therefore had her placed under the influence of ether by Dr. A. H. Carter, and proceeded to open the abdomen, assisted by Mr. Priestley Smith. The tissues of the abdominal walls were extremely vascular, and it was necessary to use a large number of ligatures to arrest the bleeding. The peritoneum was found to be intimately adherent to the tumor, and, as soon as the latter had been laid bare for a short distance, it became evident that it was not an ovarian tumor, but presented the red, muscular appearance of the uterus. Passing the forefinger of my left hand down as deeply as I could in front of the tumor, with that of my right hand in the vagina, I made out distinctly enough that my original conception of the relations of the tumor to the uterus were perfectly correct. Under the suspicion that it might be a tubal pregnancy, I did not separate the tumor further, as I had not opened the peritoneal cavity, but cautiously opened the cyst in the middle line by means of a knife. As soon as I had reached its inner coat, I passed my small trocar in, and evacuated about six quarts of thick, dark brown fluid,

having the peculiar smell of menstrual fluid. After the cyst was emptied, I passed my finger through the hole made by the trocar, and, to my amazement, I found that the cyst had contracted; moreover, as I kept my finger in the cavity, I distinctly felt it contracting round and grasping my finger. Passing the forefinger of my other hand into the vagina, I made out that what I had opened was, beyond doubt, the left Fallopian tube, and that I must have opened it close to its fimbriated extremity. I could find no canal leading into the uterus, and did not deem it advisable to make one. I washed out the cavity freely with weak carbolic lotion, by reversing the syphon action of my trocar. The wound was closed by four deep sutures, one of which was so arranged as to fasten in a loop of wire drainage-tube; but before this was done I acted on a hint from Mr. Priestley Smith, and snipped off a piece of the cyst-wall for microscopic examination. This fragment proved to be composed of an abundance of unstriped muscular fibre, conclusively supporting the view that this singular tumor was a distended Fallopian tube. After the operation I treated her exactly as a case of ovariotomy. Her temperature fell slowly. The wound suppurated freely, and shreds of what was undoubtedly mucous membrane came away with the discharge in large quantity. The drainage-tube was removed on the twenty-first day, and its track continued to discharge until the beginning of August. It then healed, and she is now (October 18, 1881) in perfect health. She has never menstruated since the operation.

From the fortunate issue of this case much is left to speculation, but of the nature of the tumor there is no doubt. As to its origin, it seems to me that it may be accounted for by the supposition that the illness from which all her symptoms dated was a localized salpingitis, which resulted in the closure of the two ends of the tube. The peritonitis, which she undoubtedly had when I operated on her, I suggest was due to a threatening rupture of the tube, and possibly a slight escape of its contents. If this be so, it is evident that it was only the accident of my determination to act promptly which saved the patient's life.

Arthur Farre quotes a case of this kind, in which the distention by the menstrual fluid advanced to rupture, followed, of course, by death. I think my case was on the verge of a similar fate.

Dr. Mathews Duncan has for a long time insisted that occasionally the Fallopian tube might be patent so far as to admit the passage of the sound through it. I am bound to say that I have never seen any evidence, from my own experience, that

such a condition has occurred, though I have admitted its possibility. The cases in which it is stated to have occurred I have always regarded as instances of the passage of the sound through the fundus, an incident to which Simpson drew attention, very many years ago, as of not infrequent occurrence. In the *Lancet* of 1872, I have reported authenticated instances of this curious fact. In the *British Medical Journal* of March 12, 1881, Dr. Duncan has published a paper in which he gives some very curious information on the subject of "Open Fallopian Tube," and though I am still somewhat sceptical, I give the following extract from his paper, in order to draw attention to the subject. That it is not impossible is certain from the fact that in cases of parovarian cysts, where the tube is often wound around the base of the tumor, and greatly increased in length and thickness, it is sometimes possible, with care, to pass a No. 4 or 5 male catheter through the tube.

"Investigating the pelvic conditions of a case destined by a colleague for ovariotomy, I found the uterine probe pass to the right side of the pelvis, and far beyond the limits of the uterine body, which was easily and certainly felt. When the woman was on the operating-table, I failed to pass the probe again through the tube, probably from the unfavorable circumstances under which the attempt was made. A few days subsequently, in the post-mortem examination, the right tube was observed lying in the route which the probe had taken, and its uterine extremity was patent—not to the extent of being wide enough to transmit a uterine sound, with its large, bulbous point, but to transmit a common small surgical probe.

"Interesting evidence of the patency of the tubes is found in the intra-uterine clots discharged in some cases of metrorrhagia. These, coming away as models of the uterine cavity, bear, at their upper angles, long clots drawn out of the tubes, and found hanging from the main intra-uterine clot. Appendages of the same appearance and origin may be found attached to the decidua in cases of abortion (see the author's 'Researches in Obstetrics,' p. 296); but these are decidual in structure, and have some strength, and are not extracted from the tube, but are part of the tube. They do not indicate patency; but the extraction of a long clot, so delicate and perishable as it is, attached only by the feeble cohesion of coagulation to the main intra-uterine clot, indicates a decided patency of the canal from which it passed. In Pirie's case (*Obstetrical Journal*, January, 1880, p. 5), 'the upper part of the clot was firm, even somewhat tough, and the tubal cords were nearly four inches long.' In the case of C. Rokitansky (*ibid.*, March, 1880, p. 133), the body of the uterus

contained 'a three-cornered coagulum, ending above, on both sides, in a short, thin thread running to the tubes.' Whitehead (*ibid.*, March, 1880, p. 137) says that, in his case, 'small fibrous prolongations from the clot corresponded to the Fallopian tubes.'

"Of the possible evil results of persistent patency of a Fallopian tube, the following is an example, related to me by Mr. Hewer, as having occurred in the practice of his partner, Mr. Calthrop. A widow, aged forty-eight, had a polypus of the cervix snipped off by scissors. On the fifth day after the operation her sister gave her, gently, a vaginal injection of warm water with Condy's fluid. While receiving the injection the patient cried out, 'You have killed me,' and was seized with sudden pain in the right side of the abdomen. She lived for three days, in great pain till within a few hours of death. On post-mortem examination there was found general peritonitis, with flaky lymph on the intestines. The right Fallopian tube was seen to be much larger than the left—twice as broad; it was freely patulous. The section of the pedicle of the polypus was healthy. There were two other polypi in the cervix. Here, as Mr. Hewer says, it was plain that the injected fluid passed into the peritoneal cavity through a canalized tube, and caused peritonitis and death. Of this accident many cases are now on record, the injections being either vaginal, as in Hewer's case, or intra-uterine.

"By the same route I have long held that blood frequently passes from the uterine cavity into the peritoneal cavity, and gives rise to hæmatocele. Indeed, I incline to the opinion that this diversion of blood, whether menstrual, menorrhagic, or metrorrhagic, is the most frequent cause of this not uncommon disease. Of course it is necessary to suppose, what has been well accounted for, that the morbid course of the blood is mechanically easier than the natural or ordinary one through the cervix uteri into the vagina; and there can further be no doubt that, ordinarily, even when a tube is patent, the mechanically easier progress of the blood is through the cervix into the vagina. Were it not so, hæmatocele would be much more frequent than it is. I have often known a woman lose blood from her uterus *per vaginam* while a tube was freely patent.

"Besides the passage of blood, there is the almost certain, but very rare, passage of a lumbricus through a patent tube (Winckel: 'Die Pathologie der weiblichen Sexual-Organe,' S.321). This kind of passage is effected by the movements of the animal.

"Further, openness of a tube is a necessary condition for the accomplishment of the wandering of the ovum in certain cases of extra-uterine pregnancy.

"In conclusion, it is necessary to remember that, besides natural and morbid conditions of patency, there may be unnatural absence of temporary patency, or of occasional dilatation of the tubes; for it is probable that they dilate during sexual excitement, and permit the passage of the semen. Indeed, it is scarcely conceivable that semen can permeate the tubes while they are in their usual closed state. This absence of dilatability of the tubes, or their rigidity, may thus be a cause of barrenness.

"The proposition of Tyler Smith to catheterize the tubes, and thus cure sterility, was brought forward under the influence of different theoretical views from those expressed in this paper. It has, as yet, led to no more practical result than the proposal of Froriep to close them by cauterization, in order to produce sterility."

Of course I need hardly say that I regard such views as Dr. Duncan here expresses concerning the passage of semen up the tube as wholly contrary to fact, and quite irreconcilable with what I have already said concerning the physiology of the tubes.

Simpson relates a case of simple hypertrophy of the muscular coat of the walls of the tubes. Various authors also mention tumors as having been found in their substance, but the majority of these cases are not described with sufficient minuteness of anatomical detail to enable us to accept them implicitly. Myomata of small size, as we might expect from the structure of the tubes, have been repeatedly found, and about their occurrence there can be no doubt. Cancer and tubercle extend into the tubes from the uterus; but we may dismiss all these conditions by saying that their diagnosis is impossible, and that it would be of little importance if it could be made.

Occasionally calcareous concretions have been found in the tubes, possibly the result of old, chronic abscesses. The clinical history of such cases is never given. The organ of Rosenmüller, a small cyst which remains from the ducts of the Wolffian body, is a curious feature of the outer part of the tube. I believe that sometimes it undergoes cystic enlargement, and should be treated as an ovarian tumor. In one of my recent ovariotomies I found it to be about four or five times its usual size, and I removed it.

One of the most important abnormalities of the Fallopian tube is that in which the ovum comes in contact with spermatozoa during its passage through the canal, becomes adherent to its walls, and develops into a Fallopian pregnancy. This accident occurs probably when the ciliary action of the mucous lining is destroyed by some desquamation or other accident, for I

have already stated that I do not believe that impregnation takes place in the tubes save under exceptional circumstances, and when it does occur the probabilities are great that the fertilized ovum will there contract the adhesions which it ought to have in the uterus. When this misfortune does occur, the tube expands to a certain limit, that limit being reached between the second and third months of pregnancy, at which time rupture usually takes place. In the vast majority of cases that rupture is fatal, and I am sure that there is no experienced gynecologist who has not seen at least several instances of it. I have known at least twenty post-mortem examinations of women who have died from ruptured tubes. In not a single instance which I have seen, nor in any of which I have found record, has the pregnancy been anywhere but in the tube. The cause of death in these cases of tubal rupture is invariably hemorrhage, and the source of hemorrhage is the enlarged maternal vessels at the site of the placenta. Unfortunately, it is just here that the rupture nearly always occurs, because the tissues are thinner, more vascular, and more easily torn than elsewhere. These facts I was able abundantly to prove in a case which I attended with my friend Mr. Hall-Wright, in which I removed the parts *en masse*, and succeeded in injecting them perfectly. Occasionally this rupture takes place without hemorrhage, or at least without fatal hemorrhage, and the patients survive the accident. In what percentage this fortunate issue occurs we do not yet know, but it is probably not large. By the rupture the ovum is extruded into the peritoneal cavity or between the layers of the broad ligament, the latter being an exceptional and a very favorable occurrence, because the patient is not likely to die of the hemorrhage.

It was after the dissection of a case of this kind, described by Dezeimeris as "subperitoneo-pelvic" (his second variety "sous-peritoneo-pelvienne"), that I was led to reconsider the whole question of the pathology of this important subject. Up to that time we had accepted the involved classification of the author I have just quoted, who made out ten different varieties. Growing experience and the consideration of a large number of recorded cases have, however, induced me fully to adopt the view of the origin of all cases of extra-uterine pregnancy which I first laid before the Obstetrical Society of London in 1873. Of this view the late Dr. John S. Parry, in his exhaustive treatise on the question, says: "In opposition to this minute anatomico-pathological classification of Dezeimeris, we have the simple one of Mr. Lawson Tait, who asserts that there are only two forms of misplaced conception. In one the oviduct bursts, the peritoneum

remaining uninjured, after which the ovum escapes into the broad ligament, between the folds of which its development continues. In the other variety the peritoneum is lacerated, as well as the walls of the tube, and the ovum finds its way into the cavity of the abdomen. The first is the subperitoneo-pelvic pregnancy of the French authors, and the latter is the secondary abdominal pregnancy of Boehmer.

"Prof. T. G. Thomas, of New York, has recently promulgated opinions in support of those of Mr. Tait. He writes (*New York Medical Journal*, June, 1875): 'I feel inclined to believe that, in the commencement of its development, the impregnated ovum never attaches itself to or draws its nourishment from any other parts than those lined by the mucous membrane of the uterus or tubes. Knowing, as we do, the delicate and subtile connection which the chorion establishes with the maternal tissues, it is certainly difficult to believe that an impregnated ovum, falling free into the peritoneal cavity, or detained within the Graafian vesicle, can, with parts so unlike the lining of the uterus, establish relations almost identical with those which are normal.'"

These opinions of Prof. Thomas are quite in accord with my own, and it has further always seemed to me that the idea that an ovum could be impregnated in the ovary and then pass, not through the Fallopian tube, but into the peritoneal cavity, and then out through the membrane into the tissue of the broad ligament, was alike improbable and far-fetched. It was much more likely, and the dissection in my case made me certain, that this exceptional form arises merely from the rupture of the tube in an ordinary tubal pregnancy, the wall giving way at the lower part, and allowing the ovum to extrude into the connective tissue between the two layers of the broad ligament. This conviction led me still farther. It made me examine other cases of which I had the preparations, or which I met with in practice subsequently, with great care, and I became convinced that in every instance the pregnancy was tubal originally, and that the acquired relations of the ovum depended entirely on the accidents of the direction and extent of the rupture of its envelopes.

Of course some sub-varieties may be made out of the position of the original attachment of the ovum in the tube, but these can be referred to only in specimens of the displacement in an early stage. In the later stages of the pregnancy all such distinctions must certainly be lost, unless it be that which has been termed *interstitial*.

The varieties which may thus be made are three in number:

(*a*), tubo-ovarian, when the ovum has been fertilized in the infundibulum before the separation has occurred between that structure and the surface of the ovary ; (*b*), tubal ; and (*c*), interstitial, where the attachment has been formed to that part of the tube lying in the uterine wall.

There can be no doubt that the former variety has been accurately described and fully established, but of the so-called ovarian pregnancy I shall have something to say in the chapter on ovarian tumors.

The interstitial variety is very likely to be far more common than we have hitherto suspected, for there is little doubt that it can and does end by natural labor at the full term. (See John S. Parry's scholarly and complete treatise.)

This distinction of varieties has, however, but little practical importance, save, perhaps, in being associated with a like distribution of the frequency of disastrous rupture of the structures, and death by hemorrhage. One would expect that the tubo-ovarian variety would be most likely to have this occurrence, and that the interstitial variety would be the least likely ; but there are no established data upon which to make any statement.

It is at any rate certain that when an impregnated ovum attaches itself to any part of the tube outside the uterus, rupture of some of the structures will take place before the fourth month, probably very much earlier. If the extrusion takes place into the abdominal cavity, the membranes may either remain entire and be developed with the fœtus, or they may rupture, and the fœtus will then float loose in the cavity of the abdomen. Meanwhile the placenta retains its old attachment to the inner surface of the tube, which becomes everted, and it likewise acquires new attachments, as it grows, to the front of the rectum, ovaries, various parts of the peritoneal surface, and even to the small intestines. Wherever it may attach itself, it displays a marvellous power of sending villi into the structures, and inducing an enormous enlargement of the vessels in the neighborhood. These enlarged vessels, as I have seen on injection, appear more like sinuses than ordinary vessels. Their walls are very thin and have no distinct muscular layer, a fact which at once explains the disastrous results which have always followed attempts to remove the placenta in operations for extra-uterine gestation, the hemorrhage being quite uncontrollable. It also explains the profuse hemorrhage which follows a comparatively insignificant rupture of an organ not usually very vascular.

It will be seen, therefore, that I maintain that every case of

extra-uterine pregnancy is tubal in its origin, and that it may become intra-peritoneal or extra-peritoneal, just as the tube happens to burst. The intra-peritoneal termination is beyond all question the more common and the more fatal; while the extra-peritoneal development of the ovum is much rarer, less fatal, and, what is of more consequence, far more amenable to treatment.

The diagnosis of extra-uterine gestation in its early stage is

Fatal Case of Fallopian Pregnancy at Eighth Week (after Duguet).

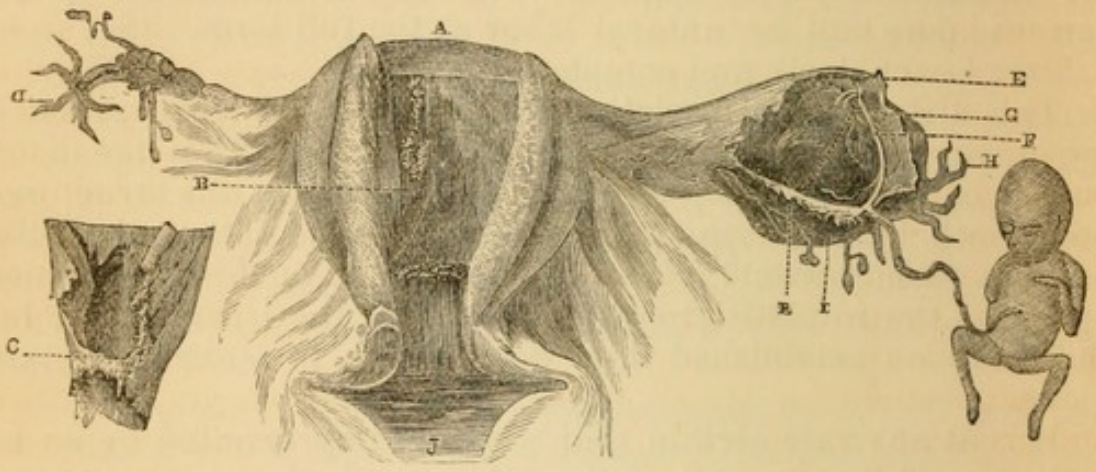


Fig. 22.—*A*, Uterus laid open on the anterior surface; *B*, part of the *decidua* still adherent to the right uterine cornu; *C*, decidua, nearly entire, expelled before death; *D*, right tube and ovary, normal; *E*, *E*, margins of artificial opening in the left tube; *F*, umbilical cord; *G*, placenta; *H*, pavilion of the left tube; *I*, vascular plexus, ramifying over the tubal covering of cyst, from which the hemorrhage occurs on its rupture; *J*, vagina.

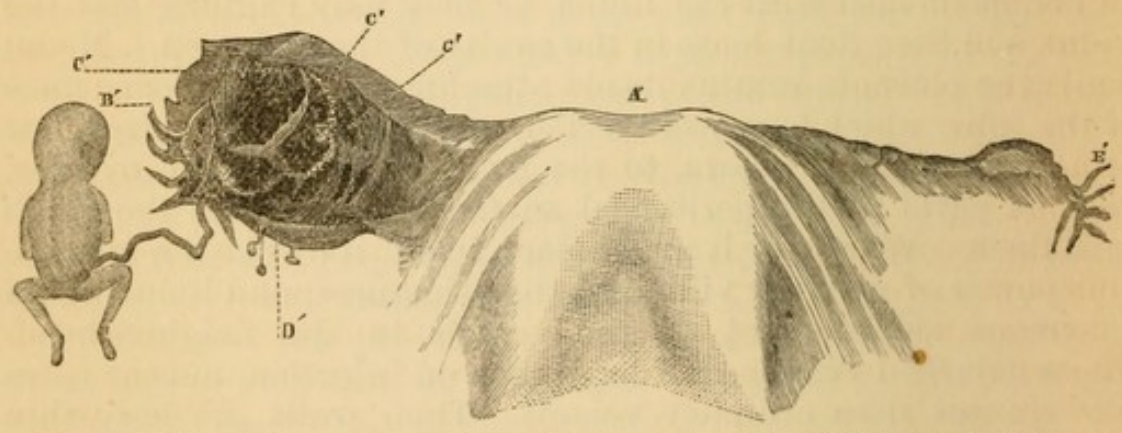


Fig. 23.—*A*, View of the posterior surface of the uterus; *B*, fimbriæ of left tube; *C*, *C*, *C*, rents in tubal covering of cyst, corresponding to site of placenta, from which the fœtus escaped and hemorrhage came; *D*, ovary attached to lower surface of cyst, and increased in size; *E*, right tube.

surrounded with difficulties, and we are seldom called upon to consider it until nearly all hope of successful interference is over. I refer, of course, to the class of cases which we see at the time of the tubal rupture, and which are generally included under the head of intra-peritoneal hæmatocele.

I have very little doubt, however, that many of these cases would be saved by prompt action. The difficulty is, of course, in the diagnosis, some certainty of which is requisite before an abdominal section can be performed. I have twice been on the point of performing abdominal section on account of suspected rupture of a Fallopian tube, and have been prevented by scruples as to the correctness of the diagnosis. In both cases post-mortem abdominal section showed that the suspicion was correct, and I believe both of these patients might have been saved. A hesitation in opening the abdominal cavity was natural enough when we were overburdened with the superstition that it was a very serious step; but now that we know it can be done with perfect safety, I would not hesitate to explore in a case where I suspected a Fallopian rupture. If my suspicion were verified, I would apply a ligature to the rupture after I had completely emptied the sac, or I would completely remove the broad ligament, or perhaps stitch it to the abdominal wound, and drain it, as I have done with pelvic abscesses, dropsy of the gall-bladder, hydrated cysts of the liver and kidney, etc. In this way I think some of these terrible cases might be saved.

I have chiefly to speak of the cases which survive this first and greatest risk of death from hemorrhage. Usually we do not see them until some months after the time of their expected confinement, and after the child has died. In very rare instances our assistance is asked before this period, and in these the utmost care must be exercised before the diagnosis is acted upon. Of course, if the child is found loose in the abdomen and moving about, the diagnosis is as simple as that of a fractured leg, and the mere division of the abdominal walls will end the displacement. Only one such fortunate case is as yet, however, on record —that which recently has been published by Mr. Jessop, of Leeds.

But supposing that the child is still enveloped in a sac of some kind, and alive, how can we determine that it is not in the uterus ? I confess that, short of introducing the sound or the finger into the cavity, I know of no means of certain diagnosis, and that proceeding can be justified only by urgent symptoms. Since I wrote originally upon this subject, I have repeatedly been called to cases where, for some reason or other, extra-uterine pregnancy with a living child was suspected, but in not a single instance did the result justify the suspicion, and my invariable advice to wait for symptoms always resulted in our waiting for ordinary labor. In one case in the practice of Mr. Langley Browne, of West Bromwich, we found a very thin uterus extremely retroverted. In the others the conditions were those of extremely

thin walls, with some kind of displacement, as latero-flexion or retroflexion, and in these patience always solved the doubts. If I met with a case where any urgent symptoms existed, I would not hesitate to use the sound or use my dilators if necessary; for the worst that could happen, in the event of mistake, would be a premature labor.

This condition of extreme thinness of the uterine walls, in a pregnancy perfectly normal in every other respect, is a point which has not yet received any notice, so far as I know. It is, however, of sufficiently common occurrence to be a source of difficulty and danger, and therefore I propose to say here what I have noticed about it, in the hope that it may draw the attention of some one engaged in obstetric practice who may be able to investigate it more fully. I can now recall six cases in which I have been consulted concerning a supposed extra-uterine pregnancy, yet in which there was only an extreme thinness of the uterine walls. I have no record of three of the cases, but of the others I have more accurate data than mere recollection. The features of all, however, had much in common, and the known histories of three quite establish this. The ordinary symptoms of pregnancy were present in all of them, and in only one was there any doubt as to its existence. The question generally was, Is the child in the abdominal cavity? and sometimes I had great difficulty in persuading the gentlemen who brought the patients to me that the position of the child was normal. Save in one case—that seen by me with Dr. Whitwell, at Shrewsbury—there was a marked absence of the liquor amnii, so that the movements of the child could be seen and felt in a most striking manner. In the pelvis the finger comes upon the presenting part of the fœtus, as if it lay immediately under the mucous membrane; and it was only on very careful investigation that the attenuated cervix uteri could be made out, spread over the body of the child.

These cases were all under the seventh month. In the eighth and ninth months the walls of the uterus thickened, the quantity of liquor amnii increased, and the cases terminated in perfectly natural labors.

These facts were given to me in connection with Mr. Langley Browne's case, also with a case which was watched by Dr. Hill Norris, and attended by him in her confinement. In Dr. Whitwell's case, which I saw with him last August, there was a large, thin-walled cyst, through which the child could be felt with the most astonishing distinctness, and it floated about as if it were perfectly free in the abdomen. He writes to me that "the patient went on very well, that some time before the expiry of

gestation the fœtus became much more a fixed body, which undoubtedly showed an increased thickening of the walls of the uterus, as well as enlargement of the fœtus, and that her labor was quick and without any subsequent hemorrhage."

The other conditions with which extra-uterine pregnancy may be confused, before the death of the child, are (*a*) displacement of the normally pregnant uterus during the early months of pregnancy, complicated with fibro-myoma or cystic disease of the uterus; and, more rarely, (*b*) pregnancy of one-half of a double uterus. In a case which I saw with the late Mr. Ross, of Wakefield, I diagnosed either extra-uterine gestation or a double uterus with pregnancy of one side, and it turned out to be the latter. Frequently we have considerable lateral displacements of the normally pregnant uterus, especially in unmarried women, sent to the specialist as something very different from what they really are.

But it is in cases seen after the death of the child, or at least when the time of the expected confinement has passed so long that if there is a child it is sure to be dead, that our most serious difficulties in diagnosis are met with.

The first point to consider is the history given by the patient of her supposed pregnancy, and the events which occurred at and after the time of her expected delivery. It is somewhat remarkable, and I think it is in favor of the views of the pathology of tubal pregnancy which I have advanced, that the majority of the instances of this abnormality occur in women who have not borne children previously, or in those who have had no children for many years. This point in the history of the patient is therefore always noteworthy. The other matters requiring careful consideration are the sudden arrest of the menses, the gradual increase in size, the occurrence of symptoms of labor at or about the end of the ninth month, *and the subsequent diminution in size.* Of all those points, the last is the only one having the importance of a sign; but it must always be borne in mind that no history, however complete, is of sufficient weight to establish a diagnosis unless there be some distinct physical signs in support of it. This I lay down as a rule based upon a remarkable experience, which I published in detail in the "Transactions of the Obstetrical Society of London" for 1874. In this case I had diagnosed double ovarian tumor, but was completely misled by a subsequent history which the patient volunteered. This was to the effect that just three years before she had believed herself pregnant, because her menstruation had ceased for eight months, her abdomen had slowly enlarged, and so had also her breasts. She was also quite sure that she had

often felt movements, and, indeed, had all the feelings that she had experienced in each of her seven pregnancies. One day, when walking in the street, she was seized with pains, exactly like labor-pains, and these lasted for four hours. At these pains she felt no surprise, fully believing that she was in labor. She felt as if a child was about to pass from her, and was aware of the "swelling pressing downward." She afterward felt this "pass back into the belly," the pains ceased, and her size remained unaltered. At this false labor there was no discharge. Up to the time when I first saw her she is quite certain no diminution of her size had ever occurred, and that there had been very little increase, if any.

The physical signs of the case were those of multilocular disease of both ovaries, and on them I need not dwell. I found it was so when I operated, and the operation was successful. The lesson of the case is that we should place very little confidence in the statements of patients, if they are not in harmony with physical signs. I must plead in extenuation, that I never saw a woman farther removed from any taint of hysteria, and, being an illiterate woman, there could have been no cramming up of symptoms from books. The strongest point in her story was the arrest of menstruation for eight months, and I had corroboration of her statement.

The weak points in the story were those I did not attach sufficient weight to, and they were those alone on which we ought to place any reliance whatever. They are, that she had no "show" during the false labor, and that her size did not diminish after it. Having now almost exhausted, I believe, the literature of the subject, I am satisfied that these two circumstances are invariable in extra-uterine gestation which has gone past the period. The first is due to the general excitement and congestion of the organs involved, specially to the enlargement of the uterus, which is always present to some extent; and the second, to the absorption of the liquor amnii after the death of the child. The complete arrest of menstruation during the period corresponding to normal pregnancy is far from being a constant condition. But even though it were, like its accompanying signs, such as enlargement of the breasts, darkening of the areolæ, increase of Montgomery's tubercles, malaise, vomiting, etc., it would help us to do little more than suspect a pregnancy. Sometimes there is metrorrhagia, due to the large size and empty condition of the uterus, a symptom which would incline us to the diagnosis of uterine myoma. After the death of the child, auscultatory signs cannot, of course, be made available; though in one of my cases, where the child was clearly dead, the pla-

cental sound was heard at my first visit, but had disappeared entirely at my second, ten hours afterward—a set of signs which tended to confirm my diagnosis.

The invariable condition of the uterus in extra-uterine pregnancy, whether before or after the death of the child, is that it is intimately associated with the tumor, generally in front of it, movable to a limited extent, always enlarged before the death of the child, and remaining so afterward if the placenta be attached, as it generally is, to the posterior surface of the fundus. The most important point is that the cervix is always quite open—in my cases almost admitting the finger. Under such circumstances, if a fetal heart is audible, the case is clear. If not, then the character of the tumor must be taken carefully into account. If the case is seen soon after the death of the child, the tumor will be soft, more or less obscure ballottement will be felt in it, and possibly a part of the child may be made out by rectal, vaginal, or supra-pelvic examination. It is at this stage that the difficulty between extra-uterine gestation and hæmatocele will occur. Hæmatoceles are not all formed quite suddenly. I have seen several cases where a monthly addition was made to the effused blood. In one such case, during the formation of a large hæmatocele, menstruation was entirely suspended, or rather its external indications were. The tumor subsequently suppurated and discharged through the rectum, and for a while it really was a grave question to decide whether it was a suppurating hæmatocele or the suppurating cyst of an extra-uterine pregnancy. I made an exploratory incision into it from the vagina, and satisfied myself that the former alternative was the correct one, and it is now in process of cure. Periodically increasing retro-uterine hæmatocele may easily be mistaken for extra-uterine pregnancy in the later stages, and *vice versâ*.

After the absorption of the liquor amnii, the character of the tumor in extra-uterine pregnancy alters very much. The uterus may become smaller and more mobile, and parts of the child may be felt, especially in the rectum, such a sign at once pointing out the nature of the case. This will be particularly evident in the instances of the extra-peritoneal variety. These prominences, and likewise the "bosselures," or knobs of the hands and feet, which are often felt above the pelvis, may be closely imitated by the small nut-like cysts of small ovarian tumors, and especially by the hard irregularities of dermoid cysts. These resemblances existed in the case I have narrated above to a considerable extent, but to a very much more marked degree in another patient, where I removed both ovaries—one dermoid—but where the resemblances, fortunately, did not lead me astray.

If the cyst be packed down in the pelvis, the deception may be great, and nothing but an exploratory incision will clear up the case. I would strongly recommend that, in such cases, the aspirator should not be used. In a joint, or in the pleura, where the conditions between which diagnosis has to be made are limited in number, this instrument is doubtless of great use, as it is for treatment as well. But in the abdomen and pelvis it is very different. The aspirator may tell you a tumor contains serum, blood, or pus, but that helps you but little as to the seat of the disease, and nothing at all as to its treatment. Besides, the risk of the aspirator is great, quite as great as the risk of an abdominal section. My use of the aspirator in my special line of practice is therefore diminishing, and in all cases of abdominal tumor, where there seems a reasonable prospect of doing good to the patient, I open the abdomen and make out the condition. I have never had to regret this practice, and I very often have had reason to be pleased with its results.

Slow-growing cancer of an ovary, or in the neighborhood of the uterus, especially behind it, might be difficult to diagnose by physical signs from extra-uterine pregnancy of long standing, but the history would here greatly help us. The increase would probably be steady, and if a rapid accession to the growth took place, a temperature chart would settle the difficulty; for the only condition which could induce rapid increase of the cyst of an extra-uterine pregnancy is suppuration, and this would tell its story on the chart in lines that could not be mistaken. Anything else might safely be set down as cancer. Fibro-cystic disease of the uterus could be determined as a tumor of the uterus. Phantom pregnancy can always be dispelled by an anæsthetic.

After the diagnosis of a case of extra-uterine pregnancy has been satisfactorily determined, the question arises, What is to be done with it? If the child is still alive and near the full term, I believe it to be our duty to operate. If the child is dead, the propriety of operating seems to me quite evident, though it has been disputed by so eminent an authority as Mr. Jonathan Hutchinson. Of course no strict rule can be laid down, and each case must be decided on its own merits; but the records of surgery are so full of instances of the risks which such cases have to run when suppuration of the sac occurs, as it almost always does some time or other, that I think we are in most instances justified in operating. Moreover, the surgical principles on which the operation is to be conducted are now so well established, and its results are so good, that the opponents of the operation seem to me to be in a very illogical position if they still continue to

advocate certain other surgical proceedings, of which the results are notoriously bad. Whether the child be dead or not, the steps of the operation do not vary, and the only condition which would modify my procedure would be a certainty that the fœtus had been developed outside the peritoneum, in the layers of the broad ligament. There can, however, be no certainty of this until after an exploratory incision in the median line of the anterior abdominal wall has been made, so that we may say that, in every case, abdominal section is the first step; and here the same strict precautions must be observed as in ovariotomy.

After the peritoneum has been opened, a careful inspection of the relations of the ovum must be made, for the further steps of the operation will differ materially according to the nature of these relations. If the child is loose in the abdomen, it merely requires careful removal, careful avoidance of the placenta, and the closure of the wound in the abdomen save at the lower part, through which the umbilical cord must be drawn, and which must be kept open for the passage of the placental *débris* after it has separated, through a wide glass drainage-tube inserted for the purpose. The discharge must be drawn up by means of a syringe, three or four times in the twenty-four hours, and the cavity occasionally washed out with a five or ten per cent. solution of sulphuret of potassium, or some other harmless disinfectant.

If the fœtus is found in a sac which is not covered by peritoneum, that is, which is not formed by the folds of the broad ligament, the sac must be carefully opened in the middle line, emptied and cleaned out as well as possible, and then its edges must be stitched round to the edges of the wound in the abdominal wall, so as to close the peritoneal cavity as well as possible. The lower part of the wound, communicating with the sac only, must as before be left open, and through it the cord must be brought and the placental *débris* must pass. I have had six successful cases of this kind, most of which are recorded in detail in the "Transactions of the Royal Medico-Chirurgical Society." If, however, the sac be found to be covered by peritoneum, that is, if the case is one of the extra-peritoneal variety, I believe that a different method might be followed if possible. In such a case the peritoneum will be found lifted up from its usual relations, so that it runs on to the walls at a much higher level than is usual. In this way an exit for the fœtus by way of the vagina is possible, subject to certain conditions, which are that the placenta is not to be cut through, and that the passages must be large enough to allow the child to pass. In a case which I operated on by removing the fœtus through an incision from the

vagina, behind the uterus, everything was favorable; but, unfortunately, in ignorance I removed the placenta, and the result was fatal. If these conditions are not possible, then the fœtus must be removed from above, and the sac must be treated as already described. I am bound, however, to say that I am not in any way in favor of vaginal section. I have never resorted to it but once, in the case before alluded to, and my growing experience makes me think that abdominal section is in every case preferable. The golden rule for this operation is to avoid touching the placenta.

CHAPTER III.

OÖPHORITIS AND PERI-OÖPHORITIS—CIRRHOSIS OF THE OVARY—ABSCESS OF THE OVARY.

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THE accession of puberty alters the nutrition of the ovary to the extent that, at the monthly periods, it shares in the general state of hyperæmia and excitement then common to all the sexual organs, and the whole economy seems to participate in the disturbance. Normally, this change takes place in the fourteenth or fifteenth year of life in this country; at an earlier date in hot climates. In strong, healthy girls, especially those engaged in active out-door work—still more those living a life approaching to the primitive state—the moliminal change is effected without suffering; but in girls brought up in refinement, of delicate habit and strumous parentage, there is much trouble. As a rule, this seems to be due to the onset of menstruation, and the

other signs of the change while the ovary is still in its infantile or incompletely developed condition ; that is, while it is forming incomplete cells, whose nuclei are incapable of fulfilling their great functions, and the whole mechanism of ovulation is out of gear. In such cases we find that the menstrual flux comes on either at irregular times or in insufficient quantity; or that, if it comes regularly, it is over-abundant, and it is always accompanied by severe pelvic pain.

There is a large class of ovarian disease due to altered hæmic nutrition of the gland, which clinical experience proves to be far more common than pathological investigation has yet shown. Of the prime factors in these cases we are as yet comparatively ignorant ; but the opportunities now afforded us of seeing the actual lesions of the ovaries, in those cases where they are removed for diseases other than large cystoma, are rapidly opening up some of the most difficult questions of ovarian pathology. Until two or three years ago, when Keith's success in ovariotomy induced us to extend our efforts in abdominal surgery, we knew no more of those obscure diseases of the ovary which make the lives of so many women burdensome, than was afforded by the evidence of a few stray post-mortem examinations. As the clinical histories of the cases in which these examinations were made were usually entirely absent, it is not surprising that we knew little or nothing of the pathology of the ovaries, save in the instance of cystoma.

So far as my own experience goes, I think I can now say with confidence that I know a great deal more than I did three years ago, not only of the pathology of the ovaries, but how to cure the sufferings inflicted by their diseases.

I propose to retain the division of the diseases of the ovaries due to alteration in the hæmic nutrition, which I introduced nearly ten years ago, and therefore I divide them into three groups, differing probably only in degree of severity, save in the cases where acute ovaritis has a specific origin. They are : 1. Ovarian hyperæmia ; 2. Acute ovaritis ; 3. Chronic ovaritis.

It may seem a metaphysical refinement to make a distinction between the first and second of these classes, but I have long satisfied myself that it actually exists. Ovarian hyperæmia is the result of an over-sufficient and generally precocious ovarian activity, and is, therefore, the converse of the condition I have detailed under the terms amenorrhœa and dysmenorrhœa. It is far from being a rare affection, and is invariably well marked in its history, the chief detail of which will generally be found to be menorrhagia. In a typical case which I have now under my care, the following is a summary of the facts : The young

lady is the child of parents of markedly nervous temperament, is well-grown, I might almost say prematurely developed in every way, and, when little over thirteen, began to menstruate. From the beginning her periods were profuse, and at first painless. She enjoyed excellent health for many months after the accession of menstruation, during which time the flow continued profuse, generally lasting for six days or a week, and necessitating the use of from four to six napkins daily. By the time she was fourteen it was, however, evident that her health was suffering. She became listless, sleepy, fainted when at her lessons, gave indications of loss of memory, and, when I saw her first, she was decidedly anæmic. At that time it wanted but two or three days before the accession of her period, and steady pressure over the ovaries gave her great pain, which she described as turning her quite sick. During menstruation this pain was induced by less pressure, but in the intermenstrual period it could not be produced at all. She always seemed better in health during the flow, and it was this very common peculiarity that prevented her parents from applying earlier for the much-needed advice.

In such a case there cannot be a doubt that there is hyperæmia, not only of the ovary, but of the whole sexual apparatus, due to, it may be, or more probably only accompanying, the increased ovarian activity. This of itself is not a source of danger, for that lies in the menstrual loss producing anæmia. I have not yet had an experience sufficiently extended to trace such a case throughout its course; but, meeting with many instances which I have had reason to regard as identically of the same nature in later stages, I believe that their menstrual history is much the same as that of other women after they have had a child, the process of gestation seeming to rectify in great measure the abnormal excitement. If they remain unmarried, they go on suffering from menorrhagia, become extremely anæmic, and have the menopause at the usual time, but marked with abnormal profuseness, as might be expected. I have repeatedly had occasion to observe that marriage, even without resulting pregnancies, often seems to do good in the way of modifying the monthly hemorrhage. In other cases, however, marriage seems to make them very much worse, to induce chronic ovaritis, displacement of the ovaries, and finally to destroy their health entirely.

The treatment of such cases should, if possible, be begun in the first stage. There is no cause of deteriorated general health so certain for a young woman as profuse menstruation due to ovarian hyperæmia. The spanæmic condition induced by a few

years' continuation of it is one over which iron seems to have no control; indeed, all ferruginous preparations ought to be sedulously avoided until the menorrhagia has completely ceased.

In the cases such as I have narrated, my first advice is that the patient should be removed from school; and that, for six months, all instruction, especially in music, should cease. I notice music especially, for I am quite certain that instruction in that art, as carried out in boarding-schools, has to answer for a great deal of menstrual mischief. To keep a young girl, during her first efforts of sexual development, seated upright on a music-stool, with her back unsupported, drumming vigorously at a piano for several hours, can only be detrimental. It is usually the habit of those who superintend the education of girls to make no difference whatever in their physical and mental exercises during their menstrual periods; and, at a time when the great necessity of the system is perfect rest, laborious efforts have to be made. This is most pernicious, and I have repeatedly had to trace to it the existence of serious disease in young ladies. Musical exercises are especially hurtful, for the further reason that music, in those who are devoted to it and gifted with its necessary peculiarities, is a strong excitant of the emotions; while to those not so gifted, and who do not care for it, musical exercises are an intolerable and useless burden. Absolute rest is an essential part of the treatment of the early stage of ovarian hyperæmia, and I need scarcely say that it is in its early stage that the treatment is most likely to be successful. This rest ought to be rigorously carried out by the patient being confined to the prone position for a few days before, during, and for a few days after, the catamenial flow. The application of a counter-irritant over the ovarian region, just before the period, is very useful; but the most potent part of the treatment consists in the administration of ergot before and during the period, and of the salts of potassium continuously during the intermenstrual time. The ergot is best given in the form of ergotin, my favorite formula being half a grain of Bonjean's ergotin made into a pill with sufficient lupulin. The bromide I give night and morning, after meals, in doses from five to ten grains. There is a great deal to be done in moral treatment. It may be only a coincidence, but I have noticed this affection chiefly in girls who have had no brothers, or brothers only younger than themselves; and I am quite certain that great harm is done to many girls by their rigid social seclusion, in youth, from the companionship of boys. Under proper supervision, no wrong could happen from more unrestricted association of boys and girls at their critical periods; and it seems to me that it is a mischievous plan to draw wide

barrier-lines between the sexes at a time when they ought to begin to understand themselves and each other ; and, by harmless intercourse, many of the risks may be obviated which afterward beset them when an unaccustomed association is opened out at an age when instinct has the chief ascendancy.

While upon this subject, I should neglect my duty were I to refrain from speaking on another subject concerning the education of girls. There has grown up a desire to educate women in exactly the same way and to the same extent as men. It would be easy for me to show, were any charge of obstructiveness or want of liberality to be made against me, that throughout my public life I have ever been in the front rank of those who advocate perfect freedom of every kind of instruction for every one who may desire it ; and I have been particularly strong in the expression of my views that there should be restriction of neither class nor sex. But it is useless to disguise the fact that, inasmuch as women have functions to fulfil which men are free from, it is not to be expected that women can, with safety, do the work of men, and at the same time properly fulfil their own special functions as women. The questions raised by the advanced advocates of women's rights are to be settled, not on the platform of the political economist, but in the consulting-room of the gynecologist. This is no place to air political crotchets, but I may own myself an advanced advocate of women's rights; at the same time I cannot help seeing the mischief women will do to themselves, and to the race generally, if they avail themselves too fully of these rights when conceded. It may be, and probably is, a very gratifying circumstance for a young woman to go to a college, and show that she could take as high a degree as a man ; but, considering the fact that she has a monthly disturbance, she would take this degree at a price which a man would not have to pay for it. To fulfil the necessary conditions she will tax herself to such an extent as will, in all probability, make her functions imperfect. To continue the career begun at college, she must deny herself the congenial occupations of a wife and the pleasures of maternity, and thus she robs the human race of what it wants most, brain-power on the part of the mother. To leave only the inferior women to perpetuate the species will do more to deteriorate the human race than all the individual victories at Girton will do to benefit it. This overtraining of young women is wholly unnecessary in the interests of human progress, and it is mischievous alike to themselves and to humanity. To hear an elderly maiden lady read a learned paper on mathematics may be a gratifying circumstance, but it is largely qualified by regrets when we speculate what supe-

rior children she might have produced if she had been a little less learned in books. Those who advocate the equal treatment of the sexes must bear in mind that great culture in a man does not unfit him for paternity, but, on the contrary, will help him, in the struggle for existence, to maintain a family. For women, on the contrary, exceptional culture will have infallibly the tendency to remove the fittest individuals, those most likely to add to the production of children of high class brain-power, from out of the rank of motherhood.

All the cases of ovarian hyperæmia which I have met with at puberty have yielded to the treatment I have detailed, and many cases which I have had reason to regard as of this nature, but in a later stage, have been benefited by it. It is, however, in the perfect fulfilment of the function of the utero-ovarian organs that we have the radical cure.

Ovarian hyperæmia is sometimes met with as the result of marriage, especially when the marital acts have been indulged in to excess, and particularly when pregnancy has not resulted. This, in fact, is only the mildest form of a serious disease which may end in total inflammatory disorganization of the ovaries of newly married women. It is not unusual to find a delicate woman, who had menstruated normally previous to her marriage, suffer from severe menorrhagia for the first three or four years of married life, and to find an explanation of this in the vigor of the husband. In these cases ovarian tenderness is always present, and very frequently there is violent pain and tenesmus, lasting for hours after connection, so that soon the unfortunate sufferer dreads the idea of a marital embrace. The menstrual period becomes prolonged, so that there is left only an intermenstrual interval of a few days. In prostitutes of a tender age this affection is of extreme frequency, and often ends in the chronic ovaritis with adhesion of the Fallopian fimbriæ to the ovary, and the subsequent atrophy of all the sexual structures so often found in their bodies. The recurrent inflammatory attacks thus induced in these unfortunates have been termed *colica scortorum*. The cure depends, of course, on the removal of the exciting cause and the employment of such treatment as has been before alluded to, but in severe and protracted cases it will be effected only by removal of the ovaries and tubes. This step is to be resorted to only after the failure of everything else, but many times I have been obliged to adopt it, and always with the best results. The idea that removal of the ovaries will unsex a woman is founded on ignorance. So far as maternity is concerned, it of course destroys the function completely; but that has already been done by the disease for which the opera-

tion has been performed. A woman who has suffered for years from chronic ovaritis with adherent tubes, and possibly hydro-salpinx or pyo-salpinx, is necessarily barren, so that to remove the uterine appendages is to make her no worse than she was. But such a disease as this will oblige her to suspend marital relations, or to endure them only as a matter of duty, and with great suffering. To remove the diseased structures will be to enable her satisfactorily to perform her marital duties, and the operation, if successful, will be found really to reinstate her in her sexual functions, and not to unsex her.

In very many of the cases of which I now speak, no line can be drawn which will define where simple hyperæmia ends and acute or chronic ovaritis begins. In many of them we get a distinct history of an acute attack, which was probably ovaritis, while in others the symptoms came on gradually, without any noticeable starting-point, and ovarian hyperæmia probably in these passes insensibly into chronic ovaritis. I propose here to give a series of cases illustrating these different classes.

E. S—— was a young married lady, whom I first saw in May, 1879, with Mr. Arthur Newton, of Newhall Street. Her menstruation commenced when she was thirteen years of age, was always so painful that she was confined to bed while it lasted, being wholly unable to get about or sit up. This pain came on invariably two days before the period lasted, so that she began her sexual life with diseased ovaries. She was married in 1876, and marriage made her very much worse. She became pregnant in three months, and it was hoped that this would cure her, but it did not. After her confinement she had an acute attack of pelvic peritonitis, which seems to have been a very serious illness. She became pregnant again, and was confined in January, 1879, and had another inflammatory attack, and from that time she was never out of bed till after the recovery from the ovariotomy which I performed on her on February 9, 1880.

I saw her, as I have said, first in May, 1879, and I then found the fundus very large and retroverted, with the ovaries also much enlarged, extremely tender, and lying down below the fundus. She could bear no kind of pessary; the menstruation was regular and profuse, and the pain during its continuance amounted to agony. I advised blistering, morphia, pessaries, and the abundant administration of bromide of potassium and ergot. This treatment had no effect, nor had the efforts of another specialist under whose care she was afterward placed. I saw her again, with Mr. Newton, in January, 1880, and found her condition much worse. She had all the old symptoms, but

in addition she was feverish, worn, and hectic. Everything had been tried and had failed, and ovariotomy only remained. To this Mr. Newton agreed, and so did the patient, her husband, and friends. I found both ovaries adherent in the cul-de-sac, and much care had to be exercised in detaching them. They were very soft, greatly enlarged, and covered with lymph. She made an uninterrupted recovery, and got up on March 5th. On April 1st she walked about the house for the first time in eighteen months, and had gained greatly in every respect. On July 20th she was able to walk a mile, and had got quite stout, was entirely free from pain, marital relations had been resumed with perfect satisfaction, and this, as she frankly told me, for the first time in her life. On September 9th I saw her get down without assistance from a high dog-cart, and run briskly up some steps, as if she had never ailed. She has not had the slightest appearance of menstruation since the operation, and the climacteric disturbance is quite over (February, 1881), and she is in perfect health.

The patient, her friends, her attendant Mr. Newton, are all quite as well satisfied as I am with the result of this case, and that nothing short of ovariotomy would have saved her life. The only thing I regret is that I did not operate many months earlier than I did. In this case the patient probably suffered from ovarian hyperæmia during the whole of her menstrual life, and this was transformed into chronic ovaritis by an acute attack in the puerperal state.

On February 20, 1880, a lady was brought to me from London, who had been confined to the recumbent position for seven years, and to bed absolutely for nearly four years. Her menstruation began at twelve years of age, was not very regular, and was always accompanied by pain. It continued much the same till she was about twenty-eight years of age, when she had an illness, and, ever since, the pain during menstruation has been much more severe, and had become progressively so for the last nine years. During the four years she had been under the care of Dr. Graily Hewitt, and had undergone prolonged, careful, and various treatments by pessaries, etc., but without the slightest benefit; in fact, she got continuously worse. When I first saw her, the history was given that menstruation was perfectly regular, lasting from six to eight days, and was very profuse. Just before the period severe pain came on, and lasted, with slight intermission, the whole time. The pain in her back was incessant, and utterly prevented her walking. I found the ute-

rus quite bent upon itself backward, and so retroverted as to be almost turned upside down. The fundus was very large and soft, and the ovaries, much enlarged, were alongside and below it. The organs were so excessively tender that without ether examination was impossible, so that I am not surprised no pessary could be endured.

I explained to the lady and her friends that the conditions were such that no effort at rectification by pessary need be attempted ; that, if Dr. Hewitt had failed, I was not likely to succeed ; and that the radical cure of ovariotomy was the only one which promised success. This they accepted, and I performed the operation on the 26th. The ovaries were enormously enlarged, but not cystic ; the fundus was soft and spongy, and nearly three times the size it ought to be in a virgin. There were no adhesions. After removing the ovaries, and whilst closing the wound, I passed a stitch through the fundus, and fastened it up to the abdominal wall. She recovered perfectly, has never menstruated since, is getting fat and well, and can now walk about the house and garden. The recovery of her power of locomotion is slow, but steady, and I need hardly say that, after seven years of their suspension, we can hardly expect any very rapid progress. The uterus is now perfectly straight and normally hung, and it is quite of the senile size.

The next case was that of a lady, aged thirty-three, who began to menstruate at thirteen, was married at twenty, and in eleven years had seven children. Her first child was born prematurely, and she had never been well since, for she got up and undertook a railway journey on the fourteenth day. After this she had continuous hemorrhage for several months. She had several premature and dead children after this, and then one living child, and the seventh dead. Three years previous to my seeing her she consulted a distinguished metropolitan specialist, who, upon his consulting-room couch, "did something to her which gave her immediately a violent pain in the back," and that pain she never lost for an hour, save when asleep or narcotized, till the day I operated upon her. What this was which was done to her, of course I do not know, though I have little doubt it was the rectification of her remarkable retroversion by the sound. If it was, it is another example which we may quote against this mischievous practice. When I first saw her I got the story that ever since this incident the patient's life was a misery to her and her surroundings—that she could not get about—was on the couch all day long—her menstruation so protracted and profuse that it lasted quite half the month—and she had hardly recov-

ered from the exhaustion consequent upon the loss and the increase of her sufferings when she was ill again. She had been under the hands of quite a number of specialists both here and in London; and after reading up her case, and comparing the opinions expressed about it, and having come across one of my cases of spaying, she came to me deliberately, to ask me if I thought I could spay her, and, if I could, if I thought it would do her good. She had been told that the womb was bent backward, but that there was a tumor on either side of it. The tumors in question I found to be enormously enlarged and very tender ovaries lying behind and below a retroflected and retroverted fundus, which felt so large that it really might have been a question whether or not there was a myoma in it. From my previous experience, I was of opinion that fundal enlargement was due merely to chronic fundal metritis, though I was quite prepared to find a myoma at the operation.

I had no difficulty, in such a case as this, in recommending the removal of the ovaries, for the mere names of the gentlemen under whose care she had previously been, without benefit, were sufficient guarantee that everything short of that had been tried. Moreover, the patient, a clever, intelligent woman, knew all about her case, and told me pretty accurately all that had been done. I had, besides, the advantage of the history given by one of her medical attendants.

The immediate arrest of the hemorrhage, which had been uncontrolled even by hypodermic injection of ergotin, would alone have been a sufficient warrant for the ovariotomy, but there were numerous other reasons in its favor. I therefore performed it on April 9th, and found the fundus enlarged from chronic fundal metritis only, the ovaries enlarged from chronic interstitial inflammation, and the displacement as I have described it. I removed the ovaries and stitched the uterus up to the wound as in the previous case. She made an uninterrupted recovery, and has never menstruated since. She is now full of color, stout, and well in every respect but one. She went through the early stage of the climacteria without much suffering, and these disagreeables are passing off rapidly. For six weeks after the operation she was absolutely free from the terrible pain in the back; but as she began to get about it came back, and for a time was as bad as ever, despite the uterus being absolutely normal in position and speedily regaining its normal size. This pain in the back still continues in a modified form, and is, I believe, slowly fading away; and I have not the least doubt it will entirely disappear in time. Why it has returned, and why it has lingered so long, I do not know, for there is no physical reason

for it perceptible. In every other respect the results of the operation fully justify its performance.

In very many cases such as these, there will be found no incident in the history from which it can be said that ovarian hyperæmia was transformed into chronic ovaritis. In others a distinct history can be given of an acute attack, from which the chronic suffering can be dated; and my belief is that the two classes may be more carefully defined by further observation, and that their pathological features are wholly different.

So far as I know, acute ovaritis is the result of four conditions only:

1. Injury;
2. Gonorrhœal infection;
3. Septic poisoning in the parturient condition;
4. Exanthematic fevers and acute rheumatism.

In one woman I diagnosed acute ovaritis following injuries inflicted by her paramour kicking her; and though it may have been general pelvic peritonitis, yet the uterus never became fixed as it does in that condition, and the subsequent permanent disturbance of menstruation, accompanied by other signs of chronic ovaritis, confirmed me in my opinion.

Acute ovaritis from gonorrhœa is a common result of the infection, and is a frequent cause of sterility. It seems to be precisely similar to the acute epididymitis of the male, as was first pointed out by Bernutz and Victor de Méric. In this affection the patient is found with an anxious face, agonizing pelvic pain, generally only on one side, the knees drawn up, and all the signs of a severe inflammatory attack. The patient can lie with comfort only on the back, and micturition and defecation are productive sometimes of excruciating pain. It is often impossible to make a vaginal examination without an anæsthetic, and this had better be used at once, for it is a matter of consequence to diagnose between acute ovaritis and pelvic cellulitis. In the latter the tumor will be found attached to the uterus, and moving with it and with the whole roof of the pelvis, and will be found to be more or less fixed; while in ovaritis the enlarged ovary may, as a rule, easily be made out. The treatment should consist in leeches to the perineum, a blister over the ovary, diuretics, and small, frequent doses of opium. The rectum should be well evacuated by an enema, and the bowels kept quiet for a few days. The great risk of the disease is that of its spreading into general peritonitis. In the event of the attack appearing to threaten the life of any patient under my care, I would not hesitate to open the abdomen, cleanse out the cavity, and possibly

remove the diseased organs. When an ovarian tumor is gangrenous or suppurating, we serve the patient by promptly removing it, and I do not see why this principle should not be extended. The result of the disease is nearly always to destroy the functions of the glands, and therefore, in prospect of a fatal issue of the disease, the argument against an operation, that it will unsex the patient, need not be considered.

Gonorrhœal ovaritis is an extremely treacherous disease, or rather, perhaps I ought to say that gonorrhœa is a disease which in women may be fraught with the most serious and unexpected consequences. Some years ago a gentleman who had been a short time married, visited a neighborhood where he unfortunately met a friend of his bachelor days. Within forty-eight hours he came to me in terrible distress, with the initial symptoms of gonorrhœa, but with the still more terrible dread that he might have conveyed it to his wife, for intercourse had taken place a few hours before his symptoms appeared. Of course I at once cautioned him to refrain absolutely from intercourse with his wife—advice which I have no reason to believe that he disregarded. His gonorrhœa proved very trifling, and passed off entirely in less than a week. Wishing to take his annual holiday, he brought his wife to me to make sure that she was free from disease, and I could not find the slightest trace of vaginitis. I therefore sanctioned their travelling to a considerable distance. But within three days I was summoned to her, and found her suffering from a most severe attack of inflammation of the left ovary. After some weeks she got well, though the ovary could be felt, both by rectum and vagina, as large as a small orange, firmly fixed and exquisitely tender. Suddenly the right ovary became similarly affected; and after a most severe illness, during which she seemed frequently at the point of death, she recovered, with the right ovary similarly enlarged and fixed. She never menstruated after this second illness, and she now lives a semi-invalid life, hardly ever free from pain, and unfit for any great exertion, though as time goes on her sufferings seem to obtain slight amelioration. She is quite unable to endure marital intercourse, and the best thing that could be done for her would be removal of the uterine appendages. She belongs, however, to the better ranks of life, and we find that patients of this class very often prefer a chronic invalidism to the risks of an operation. They can pay for any amount of luxury and medical attendance, and they do what is best for their doctors in a pecuniary sense, but not what is best for themselves.

The history of such a case is undoubtedly that the poison has permeated the uterus and Fallopian tubes, alighting on the ovary

from the tube probably at the time that the fimbriæ were in association with it; but it is somewhat surprising that there was never any trace of vaginitis.

A case of alternating ovaritis, for which I have been unable to discover any cause, has been for some time under my care in hospital practice. The patient, J. K——, aged twenty-five, came to the hospital with well-marked acute inflammation of the left ovary. She had been married for three years, and had never been pregnant. There was nothing in her history to make me suspect that she had suffered from gonorrhœa, nor did she know of her husband having so suffered. The left ovary recovered in a few weeks, but remained somewhat enlarged and very tender, and it was also somewhat fixed. In about two months she came back with the right ovary quite as severely involved, and has since been several times under care with recurrences on one or other side; but both ovaries have never been attacked together, and none of the attacks have been associated with menstruation, which, always irregular, has been gradually getting rarer and more scant. The most probable explanation of this curious case is, that she is exposed every now and then to some infection which travels up her Fallopian tubes, and attacks the ovaries without giving any indication elsewhere of its presence. The possibility of such an event must always be borne in mind, and as a guide to future directions it may be advisable to ask cautiously into the history of an attack of acute ovaritis. Whatever be the explanation, it must ever be borne in mind that ovaritis is a disease peculiarly liable to relapses, and cautions upon this point must be given to the patient.

Of acute ovaritis in childbed from septic causes, as distinguished from general septic peritonitis in which the ovary is involved, my experience is limited to one case in which, like those recorded by Simpson, Bernutz, and others, an abscess resulted. The infection occurred after a miscarriage in the wife of a medical man, and was distinctly limited to the two ovaries, as was readily determined by an examination under an anæsthetic. An abscess formed in the right ovary, and I tapped it in the early stage by means of the aspirator, with a completely successful result. The general symptoms were pain, elevation of the temperature, night-sweats, drawing up of the knees, inframammary pain, and pain shooting down the thighs and legs. The chief remedies employed were counter-irritants, such as turpentine stupes and blisters, and the internal administration of quinine and opium. The aspiration was, of course, performed through the vagina.

Of this disease, Dr. Mathews Duncan describes a case in

which "the right ovary was swollen, renitent, as big as a walnut, and when cut into was found to have its healthy tissue everywhere utterly destroyed, and converted into a yellow, purulent, almost diffluent mass. There was no lymph in Douglas' space. Bladder and uterus normal; no general peritonitis. Of such ovaritis, with suppuration, examples are not rare, because puerperal pyæmia is not rare."

It has long been known that, in certain zymotic diseases, especially in mumps and scarlet fever, male children are apt to suffer from orchitis, and I remember seeing a statement somewhere that such inflammation of the testicle was likely to be followed by atrophy and loss of its function. I cannot, however, verify my recollection by producing the reference.

In 1870 and 1871, and still more in 1874, my attention was drawn to the occurrence of acute pelvic peritonitis in women after attacks of scarlet fever and small-pox, these attacks leaving indications which showed clearly that the mischief began in the ovaries. Accident enabled me to trace the subsequent history of two such cases, and I found that in both the menstruation became greatly diminished in amount, that it was accompanied by severe dysmenorrhœal symptoms, and that in one of the cases it entirely disappeared. From these cases I began to suspect that the attacks were primarily due to inflammation of the uterine appendages, and that this had some kind of relation to the zymotic diseases which preceded it.

The terrible outbreak of small-pox from which this town suffered between 1872 and 1874 gave me the opportunity of following out this line of research, and in the second edition of my Hastings Essay on the "Pathology and Treatment of Diseases of the Ovary" I sum up my conclusions upon this subject, and, up to the present time, I have seen no reason to modify them:

"The occurrence of acute ovaritis in certain of the exanthemata, or as a sequela to them, has never yet, so far as I know, been placed in sufficient prominence. I have already alluded to it, but I wish here to record further experience gained from an epidemic of small-pox of considerable severity, which existed in Birmingham from 1872 to 1874. Though practising exclusively as a gynecologist, it is somewhat curious that I was called in consultation to four cases as instances of pelvic ailment which ultimately proved to be cases of small-pox. One of these gave the clinical features of the exanthematic ovaritis with great clearness. She had been married four years, and had been confined twice. She was pregnant for the third time in September, 1873, when she was seized with a sudden rigor, followed by severe pyrexial symptoms. These rapidly became localized in the

pelvis, the patient complaining of excruciating pain in each iliac fossa. I saw her on the fourth day of her illness, and found her suffering from double acute ovaritis and threatening abortion. She aborted on the fifth day, and then showed a papular eruption of small-pox, which rapidly became confluent. She made a very protracted recovery, and has never menstruated since. The fundus uteri is fixed down on the sacrum, and both ovaries are enlarged and tender, the left being firmly fixed alongside the uterus.

"In hospital practice I met with a large number of cases, of which the following is a good example: H. A——, aged twenty-two, began to menstruate a short time after she was fourteen, and was quite regular till August, 1872. At that time she had an attack of small-pox, which she says was not severe, and which has not left any deep marks. Up till the time of that illness she was strong and robust, and never knew what illness was. During the attack she had a very profuse menstruation at an irregular time, and this was followed by severe abdominal pain, which was treated by hot fomentations. She did not get rid of this pain entirely for some months, and since then she has menstruated at long intervals, the discharge being very scant, and accompanied with great pain. She is now very anæmic, though still stout, is short of breath, and has a loud systolic hæmic murmur at the base. The ovaries are not to be felt at all, and therefore it is probable that they have become atrophied. She obtained considerable relief from small doses of iron, combined with chlorate of potash. I have no doubt that she had an attack of acute exanthematic ovaritis, which has led to atrophy of the organs.

"I have repeatedly seen, on post-mortem examination, cirrhotic atrophy of the ovaries in women who had by no means reached the usual climacteric period of life, but had prematurely ceased to menstruate. In one case only could I get a history of the menstrual life of a patient, which was to the effect that she had not begun to menstruate till twenty years of age, and had ceased before she was thirty; and about that time she had an illness which probably was scarlet fever. The ovaries were small and shrivelled, and a stained section showed that nucleated and banded fibres constituted the bulk of the glands. Here and there, in small loculi whence the bands seemed to radiate, a small group of cells served to indicate the site of a Graafian follicle, but no perfect follicles could be found. This extreme instance was the result probably of two factors—insufficient development and exanthematic atrophy. I think that in such cases it is likely that future observation will establish the existence of an intersti-

tial oöphoritis, distinct in character and perhaps in origin from the ordinary acute inflammation of the peritoneal covering of the ovary, to which latter we might more appropriately give the name of peri-oöphoritis. The results in the two classes seem to be different; for in the second, menstruation does not seem to be suppressed, but, on the contrary, it is sometimes excessive; while, as a result of the supposed interstitial form, we have ovarian atrophy and amenorrhœa of an incurable form; and when it occurs in puerperal women, superinvolution of the uterus."

The views which I have expressed in these sentences have now been fully confirmed by my own experience and further investigations, and I have no doubt now that there is a special form of oöphoritis associated with certain exanthemata, more particularly scarlet fever and small-pox, and that in its results it differs altogether from the form of ovarian inflammation to which I prefer to give the name of peri-oöphoritis.

The most important result of this specific form of ovarian inflammation is that it leads to a cirrhosis of the ovary which may or may not be characterized by general atrophy. It always is indicated, as I have said, by atrophy of the true gland-structure and excess of the fibrous element. How this may be brought about is not yet clear; but, as I shall show you immediately, the facts are fully established, and my own explanation is that it is due to the absorption of the gland-elements after the inflammation, while the fibrous elements are left, just as is said to occur in the contracted kidney and in other instances of cirrhosis.[1]

Whatever be the process, there is no doubt that it is sometimes associated with atrophy of the uterus, resulting in what is known, and was first described by Simpson, as superinvolution of the uterus. At page 119 of my book on "Diseases of Women," I hazard the following explanation of this condition:

"Of superinvolution of the uterus, it must first be said that it is an extremely rare affection, and that all we know about it is due to Simpson. It is a condition perfectly analogous in its details to arrest of development of the uterus, with the difference in history that the superinvolved uterus has at one time been so large as to be pregnant. How the normal involution is

[1] Dr. Saundby, Pathologist to the Hospital for Women, who has given much care and personal work to this subject, tells me that it is still a *questio vexata* of pathology how far the connective tissue of cirrhosed organs is derived from retrogressive transformation of the pre-existent more highly organized elements, *e.g.*, glandular epithelium, etc., into spindle-cells and fibres, and how much is due to cell-migration from the blood-vessels and proliferation of the connective-tissue corpuscles. It is probable that the first of these processes plays a more important part than has been assigned to it in the doctrines which have found most favor during the past few years.

carried on to hypererchesis we do not know; and, so far as I can discover, we have only one description of the post-mortem appearance of a uterus so affected—that given originally by Simpson. The patient was twenty years of age, and had never menstruated after her first delivery; but no history is given of any febrile illness to which might have been attributed the abnormal absorption of the uterine substance. After death the uterus was only an inch and a half long, and its walls were less than half their normal thickness, their tissue appearing dense and fibrous. The ovaries were also much atrophied, and their dense fibrous tissue presented no appearance of Graafian vesicles. In this case it is, of course, doubtful whether the process was truly one of ovarian atrophy, followed by atrophy of the uterus, in obedience to the usual law that all useless organs tend to disappear. Several cases of what I have had reason to believe was true superinvolution of the uterus have come under my care, but in every one there has been some febrile illness, generally of a zymotic character, which occurred at, or soon after, a labor or miscarriage: and my impression is that, of all the cases, those in which a miscarriage was the origin of the trouble were in the majority. In fact, I am strongly disposed to regard superinvolution as a result of an atrophic inflammation occurring at the time when involution is going on. Thus, in a case which I published in the *London Obstetrical Journal* for May, 1873, and which certainly was the most pronounced case of superinvolution I have ever seen, the patient had had scarlet fever during the first week of her convalescence from her second labor. She came under my care in 1871, seven years after the fever, and has remained under observation ever since. When I first saw her the uterus was perfectly infantile, the vaginal portion of the cervix being represented only by a pimple. Her menstrual periods had disappeared, and were replaced by severe epileptiform seizures, as will be found detailed in the journal. I succeeded in getting menstruation restored, and the uterus increased in size by the use of galvanic pessaries, and as her periods became re-established the epilepsy disappeared. But when I discontinued the use of the pessary the menstruation slowly disappeared and the fits came gradually back, and this therapeutical experiment has been several times repeated with uniform results; and that the fits are epileptic is made certain by the severe injuries the poor woman inflicts upon herself during the attacks. Looking back on this case and others, and aided by the evidence of other facts referred to under the head of exanthematic ovaritis, I am led to believe that superinvolution is explained by the occurrence of inflammation, followed by atrophy, during the puerperal month;

and that the uterus merely follows in the steps of the ovary, carrying the process farther, however, because it had been already in action, and stopping it only when, perhaps, there was no more muscular tissue left to absorb.[1] I do not suppose that the exciting ovaritis need necessarily be exanthematic, but peri-oöphoritis, or inflammatory action affecting only the covering of the ovary, does not seem to affect menstruation; it rather inclines to induce sterility only. These views would explain many facts which are otherwise irreconcilable, and, what is most of all remarkable, the rarity of superinvolution. First of all, exanthematic or other interstitial ovaritis, such as leads to ovarian atrophy and is not fatal, is very rare in puerperal women, the great majority of such cases ending in death. The few who recover are likely to suffer from superinvolution. Again, numbers of non-puerperal women who suffer from ovarian atrophy, the result of inflammation, do not at the same time have atrophy of the uterus, because when the ovarian process began the uterus was not already undergoing involution. This explanation is quite in accordance with the history of, and the appearances in, Simpson's case, and also in harmony with the general principles of uterine physiology. Its practical bearing is, that though in such cases we may get temporary relief from the galvanic stem, that relief will cease with the use of the instrument, or when, as sometimes happens, its stimulus becomes insufficient."

The case now referred to is one of so much importance that, at the risk of being tedious, I shall give its history fully from my first acquaintance with the patient. The former part of the case I take from the *Obstetrical Journal* of May, 1873.

"E. E——, aged thirty-five, came under my care in November, 1871, at the hospital. She had been married twelve years, and had two children, the last of which was born seven years ago. She had scarlet fever after this labor, and the menses were long in reappearing. When they did come they were scanty and very painful, and occurred irregularly at intervals of from five weeks to three months, lasting only one day, or two at most. About four years previous to her first visit, slight attacks of an epileptiform nature occurred at each period—almost imperceptible at first, but getting gradually worse as the periods got more irregu-

[1] It is not to be supposed for a moment, however, that the uterus ever can be so absorbed as to disappear altogether, even though it may be so thin that a sound can be passed through it, as in the case recorded in the British Medical Journal for 1872, p. 408, by Mr. Whitehead, of Manchester. At p. 465 of the same volume I offered the more feasible explanation that there had been formed a metro-peritoneal fistula.

lar and scantier. For some months previous to applying at the hospital she had two or three severe fits at each period, each fit leaving her insensible for some hours, and often with severe injuries. On November 5th she had had a period and a very severe fit; 9th, ordered five-grain doses of the bromide of potassium thrice daily, and an aloes and iron pill twice a week. Examined on the 16th, and the uterus found quite infantile; ovaries normal. The uterus was so small that I failed to get anything into its cavity. On the 30th I doubled the dose of the bromide. December 7th, menstruated for one day, and had increased flux and no fit. Menstruated January 4th and 5th, with slightly increased amount, and one severe fit on second day. Had a severe fit on 22d, without any menstrual flow—the first time this has happened. Menstruated February 1st and 2d; no fit. March 11th and 12th, menstruation without fit, but a severe seizure occurred almost immediately after the flux ceased. On the 18th Mr. Jordan kindly put her under chloroform for me, and I got a small tangle-tent into the uterus. I at the same time discovered that there was considerable anteflexion. March 25th, passed in No. 8 tangle-tent, and on the 29th I got No. 8 galvanic stem in. April 5th, got in No. 12 stem; 7th, 8th, and 9th, menstruated more profusely than she has done for years, and without a fit, though one occurred on the 16th. She still wears the stem, and menstruates regularly and profusely, but has no fits."

From this point I continue the case from the hospital record.

On April 26th, 1873, I introduced No. 16 galvanic stem, the largest I have ever used, and from May 3d to 7th she had a period more profuse than she had ever had since her confinement. On June 4th she again menstruated for four days, again in July, and also in August and September, during which time she wore the large stem, and had not a single fit.

The stem was removed at the end of September, having been worn five months with most satisfactory results. In November she menstruated for one day only, and in December there was no appearance of it at all, but a fit occurred at the time it was expected. During the whole of this time she was taking sixty grains of the bromide each day. The fits recurred at each period when menstruation ought to have appeared, so that on May 16th I had recourse again to the galvanic stem. She menstruated from the 20th to the 24th without any fit, and she wore the stem with only very occasional fits, and with perfect and regular recurrency of menstruation, till November, when the stem was removed. By the following March, 1874, the fits had reappeared, and the menstruation was again in abeyance, and just as it disappeared the fits were re-established.

During 1875 I saw her only occasionally, as it was only when she was worse than usual that she came for the bromide mixture. It was quite clear then that her mental qualities were becoming dulled, and she was rapidly taking on the characteristic face of an epileptic imbecile. During 1876 another effort to re-establish the periods by means of the galvanic stems was made, but with results less satisfactory than those made in previous years. On February 5, 1877, I was asked to see her at her own house, and found her in a condition of epileptic mania. I advised her removal to an asylum, but her husband and mother declined to act upon my suggestion, despite its being quite evident to them that the injuries she inflicted upon herself during the fits were of so serious a character as to endanger her life, and from her occasional violence during the delirium it was quite possible she might become a homicide. Every month the fits returned with increasing severity, and the attacks of mania fastened themselves almost wholly upon the week, during which a slight loss, lasting for a few hours, indicated that her menstruation should have occurred then. The bromide of potash was pushed to as much as two hundred grains a day without the slightest effect, and other drugs were tried equally without avail.

In July, 1879, her condition was so dreadful that her friends at last determined to send her to an asylum, and I saw her on the 28th. She was almost completely fatuous, her memory was almost gone, the fits seemed to miss only one week in four, the attacks of mania were irregular and continued for varying periods, and menstruation occurred at irregular times. Yet, on the whole, it was said by her mother that she was at her worst very regularly one week out of the four.

It occurred to me that, if my view were correct, that this was a case of menstrual epilepsy really depending upon exanthematic cirrhosis of the ovary, removal of the ovaries—an operation of very ancient date, and which I performed for the first time in this country in 1872—held out some prospect of curing this unhappy woman. At least it could not make her worse than she was, for, even if she died under it, the release would be a grateful one to all concerned. Her relatives, therefore, gave a ready consent to my proposal when I laid it, and the reasoning upon which I based it, before them.

I therefore admitted the patient to the Women's Hospital, and, with the concurrence of my colleagues, I removed the ovaries on August 11th.

This operation—according to my experience one of the most successful operations in surgery, and likely to prove of infinite

service to suffering women—was first performed in 1872 by Professor Hegar, of Leipsic, and he first published his proposal. Within a very few days after Professor Hegar's operation it was performed here by myself, some months before Professor Hegar's account of his case reached this country. Dr. Battey, whose name it is proposed to fix upon this operation, did not operate till after Hegar and myself, and his publication was also subsequent to both of ours.

The operation in the case of E. E—— was made somewhat difficult by her being extremely fat. A somewhat profuse catamenial flow set in on the third day after the operation, and lasted for three days, but without the slightest appearance of a fit. This pseudo-menstruation is very common after ovarian operations, and often recurs for two or three months after removal of both ovaries.

The stitches were removed on the 18th, and she sat up on the 23d of August, twelve days after the operation.

I went away for my holiday, and did not return till the 29th of September, when I found her an altogether different woman. She had had no fits, no more menstruation, was bright and cheer-

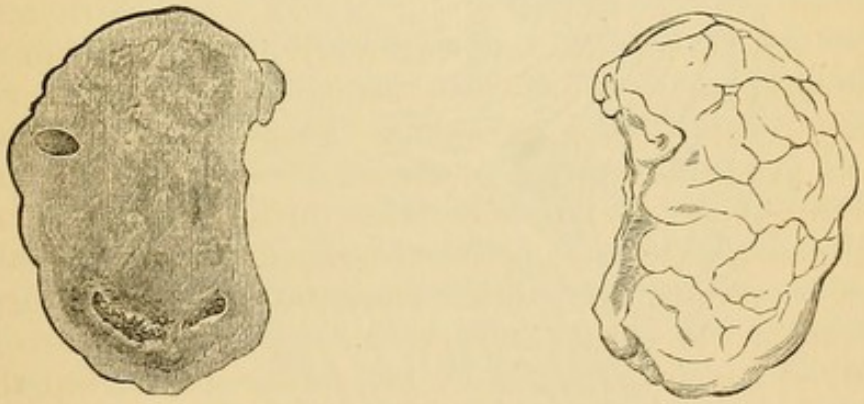


Fig. 24.—Exanthematic cirrhosis of ovary.

ful in her face, her memory returning, and she had altogether lost the dull, heavy, epileptic look which she had before.

I last saw her on October 13th, when she was about to go to her home in Peterborough, and she and her friends were satisfied as to her perfect recovery, and were as grateful as people could be for the improvement in the patient's condition.

One question of course remains: Will the improvement be permanent? I do not know. It seems almost too much to hope for, but I really think it will be. The description of the ovaries by my friend, Mr. Alban Doran, completely justifies my view of the pathology of the case, and my treatment is but a logical con-

clusion from that view. After such an operation one would expect that the fits would probably continue for a few months, and gradually disappear. But here they have disappeared at a blow, and, after nearly three months' absence, I think they may be expected to have finally disappeared.

The ovaries removed were not much smaller than normal ovaries, but they were fissured in a most remarkable manner, so as to resemble in miniature the kidneys of an ox, or the convolutions of the human brain. I sent one up to the College of Surgeons' Museum, and I quote from Mr. Doran's report upon it, as follows :

"There are no signs of 'alveolar degeneration,' but the elongated cells of the stroma are larger than in normal ovaries, and

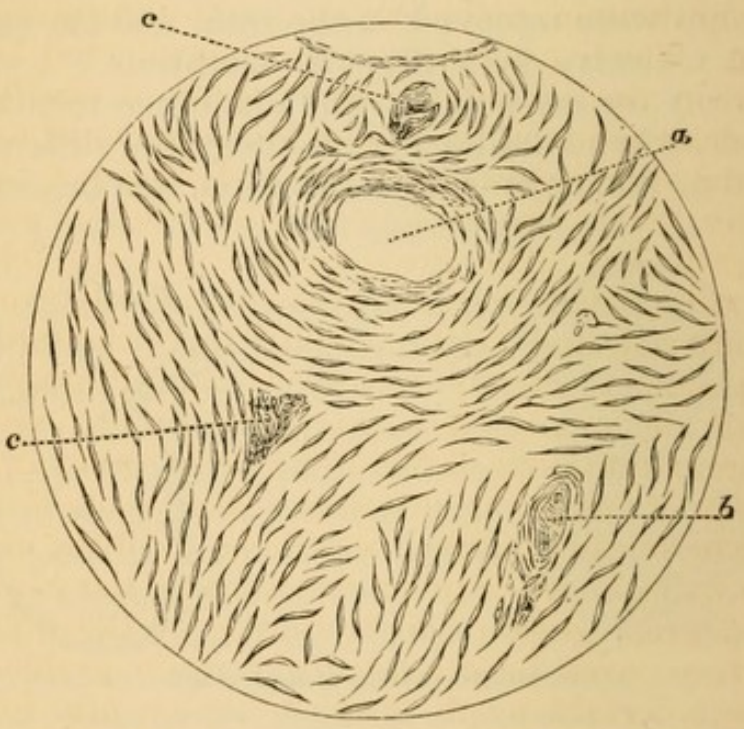


FIG. 25 (drawn by Mr. Alban Doran).—Microscopic appearances of cirrhotic ovary, magnified diameters: *a*, Normal arteriole; *b*, small vessel occluded; *c*, *c*, débris, probably site of vessels occluded by pressure of new cellular tissue.

there are few vessels; the hypertrophy of those that remain, and the bundles of fibrous tissue, point to a cirrhotic change following the exanthematic oöphoritis. There were no morbid cysts, nor extravasation of blood; no pathological breaking down. There were two Graafian vesicles, each about one-twentieth of an inch in diameter, both close to the surface, the periphery of the ovary being slightly denser than the deeper stroma, to the depth of one-eighth of an inch. Near the inner extremity of the ovary is a menstrual corpus luteum which makes a distinct bulge on the surface."

I cannot conclude the notes of this case without restating my belief that, in this operation for the removal of the uterine appendages, we have the means of alleviating an enormous amount of suffering of an otherwise incurable kind.

The conclusions which I have made concerning the influences of exanthematic diseases upon the ovaries have already been confirmed by many interesting observations, chiefly by Dr. Lebedinsky. In the specimens he has examined during the actual process of inflammation, he describes the macroscopic appearances as unchanged both on the surface and in section. He examined the ovaries after having hardened them in Müller's fluid and afterward in alcohol and picric acid. He found, on section, that all the Graafian follicles were in a condition of parenchymatous inflammation, which commenced in turbid swelling of the epithelium, and proceeded to the complete destruction of the cells. The ovarial stroma was not affected beyond being hyperæmic in the neighborhood of some of the follicles. He found the destruction of the glandular tissue most marked in the case of a girl eight years of age, who, during her convalescence from scarlet fever, was attacked by measles, of which she died on the eighth day. The great majority of the follicles in the ovaries of this patient were occupied by a finely granular, structureless material, and in the cortical layer the follicles seem to have been almost entirely destroyed. Lebedinsky regards this affection of the ovaries as being quite analogous to the well-known parenchymatous inflammations of other organs during the progress of infectious diseases. The result is a destruction of a larger or smaller number of follicles, and the consequent interference with the subsequent function of the ovaries in corresponding degree, so that the fecundity of the infected person will, in severe cases, be rendered extremely problematical, and this will be a certain result if the tubes are also found to have been damaged. This is the case in by far the larger number of cases, and the tubes share in the general atrophy of the parts in the subsequent cirrhotic change.

As I have already indicated, chronic ovaritis may be a later stage of moliminal hyperæmia. It may also be the result of acute ovaritis; but the majority of the cases occur from sexual excess and masturbation, or as a sequela of exanthemata and rheumatic fever, and probably of syphilis. I have only once had an opportunity of dissecting a case where I had recognized chronic ovaritis in life, and then it certainly was the result of acute rheumatism. It occurred in the case of a girl seventeen years old, who had suffered from eight or nine attacks of rheumatic fever. In two of them she was under my care as a dispen-

sary patient; and after the recession of the articular affection an attack of pelvic pain came on, which was increased by pressure, and the attack was accompanied by an irregular menstrual flow. The whole passed off in a few days after the application of a blister, but ever afterward her menstruation was irregular, profuse, and painful, and she suffered more or less from the symptoms I shall describe immediately. I regarded the attack as one of mild acute or subacute ovaritis, followed by a chronic stage. She died subsequently of embolism of a cerebral artery, and I found her ovaries large, soft, covered with lymph, and dotted with enlarged follicles, and the peritoneum was thickened around them. The left ovary was partly adherent to the rectum, and it had nearly the whole of the fimbriæ of the corresponding tube glued on to it.

The following case illustrates the same lesion in a more chronic stage of its progress:

H. B——, aged thirty, was placed under my care in September by Dr. Bradley, of Dudley, who told me that when she came under his care she had retroflexion and a variety of somewhat severe symptoms, including pains in the groins, extending down both thighs and into the back, which were much worse just before the menstrual period. He remedied the retroflexion by one of Graily Hewitt's pessaries, but the pains still continued as bad as before, and he sent her to me with a letter containing this sentence: "It seems to me that, in order to completely cure her, it might be necessary to remove one or both ovaries."

The history that she gave was as follows: Her menstruation began at the age of fourteen, and was at first regular and normal. At the age of eighteen, while resident in Paris, she had an attack of rheumatic pleuro-pneumonia, and after that she did not menstruate for seven months. It is not quite clear, but I think it more than likely that at this time she had an attack of ovaritis, because, during her convalescence, she found that she could not for many months straighten herself on account of severe pelvic pains, which existed on both sides, and extended down the thighs, and which prevented her walking any distance for a long time. At the end of the seven months she got somewhat better, and her periods returned, but she suffered intense pain while they were on. Three years before I saw her, while resident in Poland, she had a severe attack of pelvic inflammation, which was at the time regarded as being of a rheumatic character. Since that attack her menstruation has always been extremely irregular and very painful, so that practically for three weeks in every month she has been wholly unable to do any-

thing, or even to walk, and for two years previous to my seeing her she had not been able to follow her occupation of a governess.

I found the uterus fairly normal in position, and down behind it and on either side the ovaries could be felt, large and nodular, just like mulberries. They were extremely tender and evidently adherent. After some further discussion with Dr. Bradley, it was determined to remove the organs, and this operation I performed on October 26, 1880. I found the ovaries adherent, nodulated, studded with minute cysts, and markedly cirrhotic ; they were very friable, and their removal was a matter of great difficulty. With them I removed the adherent tubes. She recovered rapidly, but unfortunately during the process of recovery she had a hæmatocele, and though she has improved steadily since the operation, her progress has been, on account of this accident, neither so rapid nor so satisfactory as I could wish, nor as has been the rule in most of my cases. The condition of her ovaries very well illustrates the result of the interstitial form of oöphoritis which is a result of rheumatic disease.

Speaking of chronic ovaritis, Dr. Mathews Duncan gives the following valuable evidence, which I quote in full on account of the precision of the language, the eminence of the author, and most of all because, as Dr. Mathews Duncan does not practise surgery, he may be expected to give his opinions without surgical bias :

"These cases, indeed, generally resist all treatment. Here is a case : A. H——, aged twenty-four, married a year and a half, never pregnant ; catamenia regular. She complains of painful menstruation. On examination the left ovary is easily felt, and somewhat swollen and tender. The uterus is natural, except extreme sensitiveness of the mucous membrane of its body. The cervix permits easily the passage of only a No. 7 bougie. After some partially successful treatment of the dysmenorrhœa, she left the hospital, but soon returned, saying she was not cured. Now, she privately made known that what she wished cured was not so much her painful menstruation as pain in sexual connection, a pain which delicacy had prevented her from earlier mentioning. With this in view she was re-examined, and now both ovaries, somewhat prolapsed, swollen, and tender, yet freely mobile, were easily felt. Pressure on either of them produced pain, which she recognized as that of her dyspareunia. She is now under treatment. Counter-irritants externally, and small doses of corrosive sublimate internally, are being used. I can only say I hope she will be cured."

Here, then, we are dealing with a disease which one of the

greatest living gynecologists frankly admits is almost incurable. In hospital practice I assert that it is absolutely incurable in by far the greater number of cases. The only means of arriving at so satisfactory a result is limited to the classes possessing wealth and education, for with them alone is it possible to secure the obedience to directions and the perseverance in treatment by which it is possible occasionally to get a cure. More than this, it is only in a life of luxury that it is possible to prevent the relapses to which this disease is so liable.

The symptoms of the disease vary very considerably, yet there are certain features common to all the cases which are sufficiently definite for reliance to be placed upon them for purposes of diagnosis.

Pain is an inevitable feature, and nineteen times out of twenty it is worse on the left side than on the right; and if it exist on one side only, it is almost certain to be the left which is affected. The explanation which I offer of this peculiarity will be found in the first chapter (p. 8).

This pain is always referred to the groin as the point of origin and of its greatest intensity. It is nearly always persistent, and liable to exacerbation when the patient is in the erect position—when walking, but more particularly when being jolted in a carriage. It also becomes more intense as the organs become congested at the monthly periods. When the pain is intensified from any cause, it extends from its habitual seat down the thighs and round into the back, and very often a reflex pain is excited in the breast of the same side. Sometimes the pain is so great as to prevent the patient straightening herself, and obliges her to walk what little she does in a semi-bent attitude. Pressure upon the seat of pain always increases it, and the slightest touch on the ovary from the vagina gives rise to a peculiar sickening sensation which is very characteristic. For this reason, and also from the fact that her chronically inflamed ovary is nearly always displaced downward, marital intercourse is generally a cause of great pain, and, in the majority of instances, is absolutely unendurable. Generally speaking, the pain lasts throughout the whole period of menstruation; but in some instances it varies in this particular, for in some of the most pronounced cases of chronic ovaritis that I have seen, the pain ceased, or at least was greatly diminished, on the appearance of the menstrual flow.

Menstruation itself is, in most cases, profuse, but in some of the cases I have already detailed this profuse metrorrhagia is due not so much, perhaps, to the ovaritis as to the fundal metritis and the inflammation of the tubes with which it was associ-

ated, and it has been the chief cause for interference by surgical operation. Those cases in which hemorrhage is a characteristic are, I believe, those in which the inflammation is of an interstitial character, and probably not of that kind in which the cirrhotic change subsequently occurs. I have found that, in the cirrhotic cases, there is less inflammation of the other organs, and generally an atrophy of them, as of the ovary itself in the later stages; and that, as a consequence, the periods, instead of being profuse, become rather scanty. It is in the former cases that we find the ovary studded with small cysts, whereas in the others the increase in the size of the ovary is of the solid kind.

But as yet there is not, either in my own practice or in that of any others yet published, any sufficiently precise data upon which an absolute conclusion in this matter may be founded; indeed, it has only been of very recent time that we have been able to see these diseased ovaries in cases where the clinical history was accurately known. Our experience is as yet insufficient to warrant us in asserting any positive conclusion.

It is, however, perfectly certain that there are two kinds of pathological appearances produced by chronic ovaritis, and these are probably the result of two wholly different morbid processes. It is in the cases where we have a cyst-production that we have the most adhesions formed, and I think, from what I have already seen, that it is very likely that these adhesions are produced by limited inflammations resulting from the rupture of these small cysts. This phenomenon was originally described by Dr. Mathews Duncan, and I have seen the results of it, I believe, in several instances, and, in one of my cases, I have twice been quite certain, from the completely altered condition of the ovary at separate examinations, that such ruptures must have taken place. I have so often seen these cysts rupture immediately the ovary was touched, that I can have no doubt of the accuracy of Dr. Duncan's description. I exhibited a specimen recently to the Pathological Society of a cyst of the Fallopian tube, which I had recognized as having repeatedly been the subject of rupture, each rupture being followed by an attack of acute peritonitis. The specimen was obtained on post-mortem examination, after the sudden death of the patient.

The physical examination of a case of this kind requires to be conducted with a great deal of care, for nothing disappoints a suffering woman more than to have her pain increased by rough handling. When, therefore, the practitioner hears a narration of such symptoms as I have described, let him be careful how, by his finger, or the sound, or the speculum, he injures a displaced and inflamed ovary or tube. It will, as I have already said, be

easily found behind, and about on a level with, the upper part of the cervix. A careless observer may mistake it for a retroflected fundus, and introduce a pessary for its replacement; but this will prove to be nearly always a source of disappointment; indeed, as I have already said, it may be a source of danger. Besides the symptoms of inflammation of the ovary in a chronic case, there are symptoms of uterine complication, and a very great many of the cases of intractable endometritis met with in practice are really but expressions of the same serious disease.

For the treatment of this disease the most important of all considerations is physiological rest. For this purpose the woman must regard herself as an absolute invalid during her menstrual week, remaining in bed the whole of that time. This condition at once separates the hospital from the private patient, and therefore we find that, while in hospital practice it is almost impossible to permanently cure a case of chronic ovaritis, yet, in many of our cases in private practice, a cure may be accomplished by patience and perseverance. Besides the menstrual rest, there should be complete cessation of marital intercourse. If it be found that the ovary is displaced and not adherent, it may be replaced by pessary or the genupectoral position, as already described (in the chapter on ovarian displacements). Over the groin counter-irritation should be employed by means of blisters of iodine or cantharides, my favorite formula being a mixture of equal parts of the tincture and liniment of iodine (B. P.), to be painted on over the groin every morning, as long as the skin will stand it. When it can no longer be borne, the skin is allowed to peel off and become again quite fresh, and after that the processes are to be repeated for some months. Of internal remedies, the only drugs which I have seen of the slightest service are bromide and chlorate of potash, and nux vomica. These I generally give in combination, or alternately, the patient taking from fifteen to twenty grains of the bromide or of the chlorate for a month, and then the nux vomica for a month.

In these cases I never give iron when there is any tendency to hemorrhage, or, indeed, in any other condition when that symptom is prominent. I have always found iron do harm then; and in support of my views upon this point, which have been very adversely criticised, I cannot do better than quote Dr. Alfred Meadows: "I do not think I at all exaggerate when I say that, in ninety-nine out of every hundred cases of menorrhagia which come before the practitioner for treatment, his first thought is, What form of astringent shall I give? And the answer probably in most cases will be, an astringent chalybeate—either the perchloride or the pernitrate; or some similar preparation of iron

will be almost certainly prescribed. No wonder that such routine practice frequently fails ; for a very considerable number of cases of menorrhagia which come up for treatment are of the kind we have been considering, and for such as these the persalts of iron are worse than useless ; their only effect will probably be to aggravate the complaint." The paper from which I quote Dr. Meadows' words is one on "Ovarian Menorrhagia."

Among our private patients, in spite of every kind of treatment, no matter how long a time it may be continued, we shall find a few cases in which no good result is obtained, and these can only be dealt with by the last resort of a surgical operation. Among our hospital patients, on the contrary, the cures are exceptional and the failures are the rule, solely for the reason that these poor women cannot fulfil the necessary conditions. This part of the treatment I shall discuss at length in the chapter on ovariotomy.

As one of the results of chronic ovaritis, we get pronounced hypertrophy of the glands, and this occurs distinctly in two forms, as it affects the follicles of the gland or its fibrous tissue. There may be, as Dr. Ritchie and Dr. Fox have pointed out, an increased formation of the number of follicles ; this, in all probability, being a pathological feature of the ovarian hyperæmia I have described. Follicular hypertrophy may take the form of increase in size of individual follicles, and constitute, as first shown by Rokitansky, a variety of cystic growth ; and this is, as both Dr. Duncan and myself have pointed out, a frequent character of the ovaries, which have to be removed on account of the suffering inflicted by chronic ovaritis.

In fact, there seems to be a close and hitherto unsuspected connection between cystic disease of the ovary and some of the most severe uterine symptoms that patients suffer from. Thus, I have removed the ovaries of a large number of women suffering from profuse and destructive hemorrhage, due to the presence of uterine myoma, and in the majority of these cases I have found the ovaries cystic. But it may be noticed that these cysts have not always been like the large tumors for which we perform ovariotomy, and the ovaries containing them have very often been no larger than walnuts. In them the ovarian tissue had been replaced by cysts, and when those cysts were emptied there was very little left besides their walls. On the other hand, some of the cystic ovaries in these cases of myoma had attained quite a large size, so that there has arisen a difficulty in deciding as to whether one was operating for the removal of cystic ovaries, or removing the ovaries for the purpose of arresting hemorrhage in cases of myoma. Indeed, the difficulty was to say

whether it was a case of ovariotomy or the so-called "oöphorectomy." The result has been, as I shall state at length in another chapter, that I have completely discarded the use of this latter term, because, unless some kind of conventional distinction is made, it will be perfectly impossible to classify our cases in any logical manner, or for any useful purpose.

These small cystic ovaries very often give rise to extremely severe hemorrhage, even when there is no myoma present, and when there is no suspicion of any chronic inflammation of the glands. The size of the ovaries is not great enough to justify us in calling them ovarian tumors, and it is highly probable they are nothing more than follicular hypertrophies. Of this peculiar condition I propose here to give in detail three instances.

In June, 1880, I was called by Dr. Collis, of Bridgenorth, to see with him, in consultation, a lady of very eminent social position, on account of persistent metrorrhagia. She was twenty-nine years of age. She had been married six years, and before that had suffered always more or less from a white discharge and irregular and profuse menstruation. Nine months after marriage she was confined of a still-born child, and nearly lost her life from hemorrhage. Two years after she had another child, living, and in the following year another child, both labors being characterized by unusual hemorrhage. In 1878 she had a miscarriage, and was alarmingly ill from hemorrhage. In August, 1879, a third child was born, about six weeks before the full time, when again the hemorrhage was extreme.

Dr. Collis has favored me with the following notes of the progress of this most interesting case: He saw her first on May 31, 1880, when he was informed that, up to a fortnight before his visit, she had missed three menstrual periods, but that during the fortnight there had been a continuous flow. Neither she nor her husband thought it possible that she was pregnant. They regarded it as her usual profuse and protracted menstruation; but on examination Dr. Collis found the uterus enlarged. He kept her in bed and gave her astringents, and afterward ergot and bromide of potash. Finally he had to plug the vagina, and then he telegraphed for me to see her with him. I saw her on the evening of June 13th, and found the patient very anæmic, and the uterus enlarged as if by a pregnancy of the third month. The cervix being closed, it was clear that we must dilate, and for that purpose I introduced my instruments, which act by continuous elastic pressure. In a few hours dilatation had proceeded so far that, after placing the patient under ether, I was able to empty the uterus of a large quantity of clot and some villous

cysts. These, I presume, were remains of a chorion of which the villi had undergone cystic dilatation, but nothing in the shape of membranous or placental structure could be discovered. Recognizing the urgent necessity of there being no more hemorrhage, I took great pains to remove everything from the uterus, and I scraped the whole of the inner surface over with a curette. She had no further loss, and made a good recovery till July 10th, when her period came on very profusely, lasted ten days, and left her very anæmic and exhausted. During the whole time she took large doses of bromide of potash and ergot, but with no apparent effect. Hemorrhage again occurred on July 29th, by which time she had been removed to Malvern, where she was under the care of Drs. Pike and Weir. The hemorrhage was extreme, and everything was tried, including hypodermic injections of ergotin, without any avail. I was sent for on August 3d, and found the patient in the very last stage of anæmic exhaustion. I removed a plug which had been placed in the vagina, found the uterus perfectly small and normal, explored it with the alligator-forceps, but found nothing in it, and then I applied solid nitrate of silver freely to the inside. This stopped the hemorrhage for about twenty hours, but after that it came on, and I was sent for again on the 6th. At my visit on the 3d I had informed the husband that, if the nitrate of silver did not check the hemorrhage, I knew nothing short of a surgical operation which would, but said nothing to him as to the nature of the operation I intended to perform. When telegraphed for on the 6th, I replied that I should bring my assistant and everything prepared to operate if it was thought desirable, and for this purpose my friend, Dr. J. W. Taylor, accompanied me to Malvern, in the absence of Mr. Raffles Harmar.

When I reached the house I met the husband, a man of distinguished position and great intelligence, at the door. He greeted me with the remark that he did not know what I proposed to do, that he left it entirely to me, but that he was perfectly sure the only thing which would give either temporary or permanent relief would be removal of the appendages. As this was exactly my own notion, and was readily agreed to by my colleagues in the case, I at once proceeded to carry it out, my only fear being that we had delayed it too long. She was blanched beyond my powers of language to describe, and she had those swollen, waxy lips which are rarely restored to their original condition. There was no difficulty in the operation, and both ovaries were found to be cystic, and about the size of Mandarin oranges. The uterus was perfectly normal in size and consistence when I had it between my fingers. The incision was only

two and one-half inches long, and its bleeding points were indicated by a flow of serum almost devoid of color. For about an hour after the operation I gave up almost all hope of her recovery. Dr. Pike and I were in almost constant attendance upon her for five days, during which she had some ups and downs, but finally she got right, and has never lost a drop of blood since. She has had the usual flushes and other slight indications of the climacteric, but these are wearing off; and in the last letter I have had from her husband, a few days ago, is the sentence: "It only remains for me to express our united gratitude for your skill and attention; for, humanly speaking, I shall always look upon you as her saviour."

Putting aside, as far as possible, all personal gratification at such an expression, I desire only to put in this evidence given by a highly educated layman, fully conversant with his wife's condition and what was done for her, in favor of an operation upon which only those who have not successfully tried it are endeavoring to cast obloquy. The only credit in this case I desire to assume is, that I had the courage of my convictions, and that I proceeded, as a last resource, to a step which, if I had regard to metropolitan opinions, I should not have attempted. Had the case been unsuccessful, the position of the patient was such that the proceeding would have been widely, and I fear adversely, criticised.

Looking at the ovaries of this case, I notice that there is little, if any real ovarian tissue left. There is hardly anything but the thin walls of a number of dilated follicles, from which it is very difficult to believe that a healthy ovum could be sent into the tube. This naturally raises the question as to whether the imperfect ovulation, which was the first cause of my being sent for to her, was the result of this follicular hypertrophy. I think it very likely that it was so. The condition seen in these ovaries must, I think, be something special, and not merely the early stage of cystoma, for I never hear such a terrible story of hemorrhage from the lips of a patient in whom an ordinary cystoma has grown, as I have to narrate about the three cases in whom I found these small cystic ovaries, and from whom I removed them with perfect success.

The second case was in some respects more remarkable than the first, though it is not necessary to occupy so much space with its detail. She was thirty-nine years of age, had been married at fourteen years of age, and was confined of her first child before she was sixteen, her second at seventeen; eight

months after she had a miscarriage, and then for the next ten years had a baby every year. At each confinement the hemorrhage was very great, and two or three times she was supposed to be dying from this cause. As she had had no menstruation for twelve years, being either always pregnant or suckling, she could tell nothing about this matter until she became a widow at twenty-eight. She married again about four years ago, and during her widowhood her menstruation had been far too frequent and too profuse, and she had been almost constantly in the doctor's hands on that account. Since her recent marriage she has had eight miscarriages in forty months, the first being at seven months and the others between four and five. She was admitted into the hospital in February last, when pregnant at the third month. She was put upon chlorate of potash and biniodide of mercury, in order to avoid the repetition of the miscarriage, and she took every precaution to assist us in this, for both she and her husband were very anxious for a living child. In spite of everything, however, she miscarried at the fifth month, and as nearly as possible died from the hemorrhage. During May, June, and July she had most profuse menstruation, though active treatment was employed, and when admitted into hospital again she was a completely broken-down anæmic woman, whose desire was to die if nothing more could be done for her. In this case it did not occur to me to remove the appendages, and that proposal originated with my colleague, Dr. Hickinbotham, at the consultation held on the case. I am bound to say I did not regard the idea with favor at first, and it was only after prolonged discussion with my colleagues, and finally at the earnest and frequently repeated request of the patient herself, that I undertook it. This request was based on her knowledge derived from a patient in the same ward who was recovering from the operation. Here again the ovaries were cystic, just as in the first case, the cysts being small and thin-walled, but occupying the whole of the ovary. We may again ask, Did they account for the repeated incomplete ovulation, as well as the hemorrhage? Such a question needs a much wider experience for its solution. Whatever be the explanation, the result is brilliant, for the woman made a speedy recovery, and now, not yet twelve months since the operation, is in robust health—such health, in fact, as she has never known before.

The third case was sent to me by Dr. Meredith, of Wellington, in Somersetshire, and I give the history in his own words:

"In May, 1877, I had to attend a young woman, aged twenty,

on account of excessive menstrual discharge, which had been going on for some weeks.

"The previous history of the case was briefly this: The patient, as a girl, had always been considered delicate up to the menstrual period—which, with her, began when she was fifteen—although she was well-formed and tall. Once the courses became established, she began to gain strength and fatten. The catamenia were regular, but scanty; only about three diapers at a period; still she felt well, and gave this no particular thought. Her parents are healthy, and so are her brothers and sisters. One day in March of the year mentioned (1877), while menstruating, she assisted in lifting a book-case. She felt the effort affecting her, and the discharge, instead of terminating at the expected time, went on day after day.

"When I saw her she was in an exhausted condition from the loss, and suffering pain, etc., in the lower part of the abdomen, indicating the presence of a certain amount of local inflammatory action. After the administration of opiates this condition of irritability subsided, and after a while I obtained permission to make a digital examination of the vagina and cervix uteri.

"The information I got from this was that there was no appreciable difficulty in introducing the finger; the os uteri was patulous, with a blood-clot in it, and the cervix elongated. There was nothing special to note in regard to the condition of the uterus—no marked version or flexion. Now, a very natural question suggested itself, and I have no doubt it arises at once in your minds, namely, Was not the case one of miscarriage? A question which I put to the patient some time afterward, bearing on this, was met by a negative answer—just what I might have expected. But my duty was to arrest the hemorrhage and bring about recovery, if I could. To this end I administered ergot, acids, bromide of potassium, chlorate of potash, digitalis, and cannabis indica. The last-named three, in combination, seemed to answer well for a while, then there would be a relapse. Cloths dipped in vinegar and water were applied over the vulva and lower part of the abdomen; cold water, vinegar and water, and carbolic acid solution were at intervals injected into the vagina, and, of course, absolute rest in bed was enjoined, with everything cold in the way of food and drink.

"In spite of everything, the discharge continued more or less until July. At times there would be nothing but a pink, sanguineous staining on the cloth. The patient soon learned to dislike this appearance, as she had always a great deal of backache with it, from which she was only relieved after the expulsion of blood-clots. The explanation of this, I take it, was that the clot

formed in the os uteri, and, owing to the flow not being enough at times to carry itself off as a whole, the fibrin separated at the uterine outlet and in the uterus as well, staying there gathering in volume, while the liquor sanguinis escaped, and produced the stains mentioned. The fibrinous part, in thickening, rested upon the walls of the os, distending it, and, as in labor or any other form of tension at the os uteri, the discomfort was referred to the sacral region—the region of backache with many women. After going on in this way for a time, I decided to apply pure carbolic acid to the interior of the uterus. I did this in the usual way, by means of a piece of cotton-wool wrapped around an ordinary uterine sound. The result was satisfactory for the time; the discharge stopped for five months, the patient recovered strength, and was able again to go about and enjoy herself.

"In the beginning of 1878 the menses reappeared, but nothing much to complain of at first; then the loss assumed a more persistent character. Drugs seemed to have very little influence now, nor had the intra-uterine application of carbolic acid the same arresting influence as at first; still it exercised a certain amount of staying power. Thus matters went on unsatisfactorily to all concerned. Toward the end of 1878 the loss was not very great, still it recurred at short intervals; but on Christmas eve she got excited with some of her friends, and then it came on profusely. There was always tenderness over the ovaries, at times more over one than the other, and, of course, the usual sympathetic tenderness along the spine.

"Finding that I was unable to afford the relief I wished, I urged the patient to go to the Women's Hospital at Birmingham, to be under the care of Mr. Lawson Tait, with whom I had had some correspondence regarding the case. Accordingly, on January 15th she went thither. She was thin, weak, and anæmic at the time. A few days afterward I had a note from Mr. Tait, saying that he had dilated the uterus and thoroughly explored it, and could find nothing amiss with it, only that the fundus was a little enlarged, nothing more.

"A few days after admission, nitrate of silver was applied to the cavity of the womb, and repeated three times between that and February 15th. On the 19th the loss ceased, and no further application was made. The patient had mixtures given her, consisting of ergot, bromide of potassium or chlorate of potash, and, after the cessation of the discharge, dialyzed iron.

"She left the hospital, apparently recovered, on March 1st, and went to a convalescent home, where she stayed for some time and was much improved. In due course she returned home to Wellington. On the night of her return the discharge began again.

"I knew nothing of her return, or of the recurrence of the discharge, until she had been at home for some weeks. During the interval she tried the effects of medicines which some neighbors procured for her—getting into a sort of desperation-state, which we can all pardon under the circumstances. I now tried the effects of cold water hip-baths, and with some apparent good results. Mustard poultices over the ovaries were followed by no marked benefit. Swabbing the interior of the uterus produced some relief. After a time I introduced a piece of nitrate of silver into the cavity of the uterus, and left it there. This altered the character of the discharge; but, in spite of all, the loss persisted. I frequently left her alone, desiring her to keep still and take no medicines at all; the result was the same—always losing.

"On July 9th last I gave her a hypodermic injection of ergotine, which was followed by a stoppage of the discharge for about three weeks. On August 5th I again sent her to the Women's Hospital at Birmingham, under the care of Mr. Tait. At the time the patient was anæmic, thin, and weak, and hardly able to stand."

I re-admitted the patient in August, 1879, and removed both ovaries on the 8th of that month. The ovaries were large and flabby, and occupied by a number of distended follicles forming cysts. They were also chronically inflamed, for there was evidence of old lymph here and there on their surfaces, and they were somewhat adherent. The patient went home in a few weeks after the operation, and speedily gained health and strength. She has never menstruated since, and enjoys perfect health (May, 1882).

From these cases I am forced to conclude that between these small cystic ovaries and uncontrollable hemorrhage there is some connection which has yet to be studied, and that in such cases the removal of the ovaries is not only to be justified, but that it is the proper proceeding, the results of these cases having been brilliantly successful.

Besides this follicular hypertrophy, there is a distinct form of fibrous hyperplasia which is probably the result of that form of chronic ovaritis which attacks the fibrous element, and results in follicular destruction or arrest of development of the proper ovarian cells, and produces an excess of the trabecular structure. It is, in fact, the process of cirrhosis in its second stage, previous to the contraction. The following is a case which I have had the opportunity of watching for many years, and now seems to be verging toward the cirrhotic condition. Both the patient and I are agreed that if I could have done for her ten years ago

what I could do now, if it were as necessary, she would elect to have her ovaries removed rather than pass through the prolonged invalidism to which she has been subjected. She belongs to the upper ranks of life, and therefore has had every opportunity of recovering, and no money has been spared to secure her good health; yet she has been an invalid for about twelve years, and is so yet, though enjoying better health than she did three years ago. It would have been cheaper for her, and better in every way, to have had her ovaries removed ten years ago.

She is now about thirty-eight years of age, is a pretty, delicate blonde of nervous temperament and most refined cast of features, and has been married about eight years. She has a history of hyperæmia of the ovaries at an early age, and has had always very profuse, and generally irregular, menstruation until within the last three or four years, when it has been scantier and less frequent. From November, 1871, until she came under my care, she had had only one normal period (in seven months), and another in April, 1872. From the former date a constant, offensive, brown discharge had been present, which was increased by exertion. She had pain and straining after coitus, pain on defecation, loss of appetite, and frequent sickness. Examination revealed a condition of enlargement and tenderness of the uterus, openness of the cervix, and decided retroflexion of the fundus, with a tendency to retroversion of the whole organ. The cavity was not larger than normal, but the passage of the sound gave great pain. The displacement was easily reduced, and then it was found that both ovaries were very much enlarged and tender, the left especially. They could both be distinguished by bimanual touch as quite free from adhesion, readily moving about. I introduced a ring-pessary to rectify the displacement, much to her comfort, and directed the use of iodine-paint in the manner previously described. She also took a tonic mixture consisting of cinchona and angostura, and the uterine cavity was occasionally washed out with a weak solution of neutral acetate of lead. The latter part of the treatment was discontinued after a few months, but the counter-irritation and the pessary were persevered in, along with occasional recourse to tonics. In October, 1875, the brown discharge had almost disappeared, and the right ovary could be felt to have distinctly diminished in size. The uterus was also straight and the cervix closed, and the whole organ of a much less size. Early in November there was a slight menstruation lasting three days, and in January of this year there occurred quite a normal period of four days, followed by rather profuse leucorrhœa. In February, as the expected period did not occur, I ordered her small doses of

iron, in the form of ten drops of Parrish's syrup of the phosphates, taken thrice daily.

For the last five years the treatment has been varied, both by myself and others, but nothing seemed to have any very marked effect, unless it be residence at Kreuznach and the prolonged use of the waters. Nature seems to be working her own cure, and the only question to be discussed is: Would it not be better, in such a case as this, to run a slight risk in the removal of the ovaries, and so effect a speedy and permanent cure? I think it would, and so does my patient.

There is probably a chronic ovaritis of occasional occurrence in chronic phthisis; for, though the rule in that disease is to have ovarian atrophy, evinced first in dysmenorrhœa and finally in amenorrhœa, yet I have seen a few cases where the menstruation was profuse, irregular, and characterized by the other symptoms of chronic ovaritis. I have seen such conditions temporarily after small-pox, and frequently after scarlet fever in adolescent women. One case I have also satisfied myself of in early acquired syphilis. There is a distinct form of syphilitic metritis, as pointed out long ago by Mr. Langston Parker, and no doubt in these cases the ovaries are involved.

Arthur Farre has noticed an intense red coloring of the ovaries in cardiac disease, and I have more than once found that intractable menorrhagia had apparently its origin in valvular disease of the heart, or at least was closely associated with it, in such cases there being often no discoverable lesion of either uterus or ovaries.

I have met with a small group of cases which I can only class under the head of ovarian neuralgia. They have been characterized by acute lancinating pain referred to the region of the ovaries, generally on both sides, coming on paroxysmally, without any reference to the uterine or ovarian functions. No physical signs of disease have been found in these cases, and they have all occurred in women approaching the menopause. They have all been addicted to outbursts of over-indulgence in drink, taken, as they allege, to deaden the pain. Whether this inebriety was a cause or a result of the neuralgia, or whether the neuralgia in some of the cases may have had any actual existence, I am unable to say; though the special character of the pain and its site have been described by the sufferers with a constancy which would seem to vouch for its reality. In connection with this affection I would here urge the necessity for the medical profession combating strongly against the wrong women are often allowed to do themselves by taking spirits to relieve ovarian and menstrual pain. No habit can be more pernicious, or more

likely to lead to the most deadly mischief, both physical and moral.

A singular condition has been noticed by Dr. Priestley, of intermenstrual pain, occurring about midway between the periods, which is almost certainly due to an ovarian condition, though it is not clear of what kind. Since reading his paper I have seen several cases, but have been unable to refer them to any category.

Abscess of the ovary is a condition of extreme rarity, or, at least, it certainly is one which we can rarely diagnose during life, and in the majority of instances probably death occurs from the rupture of the abscess into the peritoneum, without any diagnosis having been made beyond that comprised in the generic term of an attack of "inflammation of the bowels," under which category a large number of cases are entered in the death register, many of which probably might have been relieved if a more accurate diagnosis had been possible. Of the cases of abscesses of the ovary which recover after rupture we of course only find subsequent traces in the indications of old perimetritis which are usual after a great many other lesions as well as this. Probably, however, the larger number of cases which have been published as abscesses of the ovary are nothing more than suppuration in ovarian cysts, and therefore belong to an altogether different category from those of which I am now speaking.

True abscess of the ovary is said to occur most frequently in connection with pelvic suppuration of the puerperal woman, and possibly this may be a condition of pretty frequent occurrence. I have, however, for many years past, carefully avoided attending post-mortem examinations of such cases, and therefore I have seen no instance of this kind. The only cases of abscess of the ovary, in clinical experience, of which I have been certain, are two, one of which I have already narrated under the head of pyosalpinx; and the second is one of great interest because it also shows what immense success has been recently made possible by the advances of abdominal surgery.

The patient was sent to me by Dr. Lycett, of Wolverhampton, and I cannot do better than give the history of the case, which he sent to me in a letter, as being quite a model of what such communications should be. It was as follows:

"She is about thirty-eight years of age, and has suffered for many years from great ovarian pains, rarely free, and much increased at the menstrual period, which are often fortnightly, scanty, and prolonged for a week or ten days. The left ovary seems the one at fault, being tender and somewhat enlarged; the uterus is rather conical, but the passage fairly patent. She

has had a variety of treatment under my hands, and, though able to afford some relief, yet I see no prospect of permanent good, so that at last I am desirous of your opinion as to oöphorectomy, for her health has materially suffered, as you will observe. She is a weakly, nervous, anæmic person, whose life is a misery, and may probably break down before the menopause. She has not had any children. Several times at the periods her temperature has risen to even 102°, marking some local inflammation, and at these times the pain and tenderness is greater."

No history could be more graphic, concise, and complete. The only additions I can make to it are that marital life was absolutely unendurable, and that I found the left ovary adherent in the cul-de-sac.

I quite concurred with Dr. Lycett's views, and with his concurrence and assistance I performed ovariotomy on June 28th. I found the left ovary firmly adherent in front of the rectum, and to pull it off from its attachment was a work of difficulty. It contained about two drachms of pus, and appeared to be just on the point of bursting into the peritoneal cavity. Had it so burst, she doubtless would have had an attack of acute peritonitis, from which she might have died. The right ovary was shrivelled, so I removed that also. She made a perfect recovery, and not only is cured, but her sexual relations are now possible, so that not only has removal of the ovaries not unsexed her, but it positively has resexed her—a statement which I have made about a number of other cases of a somewhat similar kind.

Two cases of abscess in both ovaries, narrated by Mr. C. J. Cullingworth, in the *Lancet* of November 3, 1877, illustrate well this unusual disease, and are equally instructive in showing the disastrous results of delay in the performance of abdominal section in cases of doubt, where patients are suffering from pelvic mischief.

The first was a case of a woman, aged forty-five, admitted on January 13th, with vomiting, severe pain in and enlargement of the abdomen. In the lower part of the abdomen was a fluctuating swelling, reaching nearly as high as the umbilicus, quite dull on percussion, and a soft, rounded swelling in the vagina, to the right of the uterus. The morning temperature was low and the night temperature high, showing clearly the presence of pus, as did all the symptoms.

"January 27th.—Thirty-five ounces of pus were withdrawn by the aspirator, without relief.

"February 7th.—An exploratory incision was made, and a

large abscess opened in the abdominal walls, outside the peritoneum, with a communication into the abdominal cavity. The patient died a few hours after the operation, and the post-mortem displayed that the source of the mischief was an abscess in the right ovary, which had burst. The left ovary also had become converted into a small bag of purulent fluid." The case seems to have had a very chronic progress, and if the abdominal section had been done some weeks before it was, there probably would have been a successful result.

The second case is an even more instructive one. About the middle of 1875 she noticed an enlargement of the abdomen, and was suffering from local distress. In June, 1876, this amounted to constant pain in the left iliac region, where there was a distinct, hard swelling, tender on pressure. The uterus was quite hard, the vagina was encroached upon, and its upper part exquisitely tender to the slightest touch, causing great suffering. An exploratory puncture was made without result, and after some months' residence in the hospital she was discharged on March 31, 1877.

She was readmitted in May following, with the symptoms much aggravated, the abdomen uniformly enlarged and universally tender, and the old, tender swelling could still be felt. The night temperature was always considerably higher than that of the morning. She was kept under observation till August 3d, when she died.

The post-mortem examination revealed old peritonitis. The right ovary was four and three-fourths inches in its large circumference, and three and one-fourth in its shorter, and was a mere shell, filled with offensive, purulent fluid. The left ovary was much larger, and formed the large tumor which was felt during life, and this again was filled with a highly offensive, purulent fluid.

In such a case as this it is impossible to resist the conclusion that abdominal section, performed soon after the onset of the serious symptoms, would have enabled the surgeon to have relieved his patient.

M. C. Darolles contributes some valuable observations concerning the microscopic examination of ovaries, in which ovaritis had resulted in the formation of abscess. He found that the process began in the suppuration of separate follicles, and that these subsequently coalesced, forming abscess of the whole gland. Such cases, he points out, as well as those of suppurative inflammation of the tubes, frequently result in a series of secondary

accidents, such as pelvi-peritonitis and acute general peritonitis, which may have a rapidly fatal issue.

M. C. Salamon has narrated a series of cases of tubercle of the ovary; but, as this condition is always associated with tubercle elsewhere, which is of far greater consequence, it can be regarded only as having an interest of curiosity. I have not heard of a case of tubercle of the ovary only.

Hermaphroditism.—If the law of evolution embraces all organized structures — and its details have now been so fully worked out that we may assume that it does—we must accept Darwin's theory of the descent of man. This acceptance at once becomes the explanation of the occasional occurrence of bisexual vertebrates, and consequently of true hermaphroditism in human individuals. Conversely, the occurrence of such malformations may be offered as one among the many proofs which are being accumulated from every quarter in favor of Darwin's theory, for they must be regarded as reversions of type. In the vegetable kingdom the majority of the species are bisexual, though modern investigations have shown most ingenious contrivances to secure the advantages of cross-fertilization.[1] Even in the more complex organisms of the animal kingdom, bisexuality is met with as high up as the nudibranchiata, while in the next sub-order, the prosobranchiata, most of the groups are unisexual. In the cephalopoda, where other great advances in structure are indicated, unisexuality is the rule. From this point a symmetrically double body is introduced into the schema, though it is met with also in the insecta, and the sexual organs are double, one in each half of the body. But as in the insecta, where unisexuality is the rule, hermaphroditism occurs with some frequency, so it does in the lower vertebrates, the frequency of the malformation diminishing, until in man true hermaphroditism is found very rarely. In all cases of hermaphroditism in animals where unisexuality of the individuals is the rule, the doubly sexed organs are always imperfect, even in insects; and in most of the cases recorded in birds there has been on the male side only a convoluted seminal tube and no testicle, so that the tube might have been taken for an aborted oviduct, had it not been, as in one of Simpson's cases ("Encyclopædia of Anatomy and Physiology"), for the coincident presence of the characteristic epithelial appendages of the male. In Simpson's second case I do not think there was any evidence of true hermaphroditism.

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[1] See Darwin's "Fertilisation of Orchids," "Cross and Self-Fertilisation of Flowers," and Kerner's "Flowers and their Unbidden Guests."

The human testicle and ovary being developed from the same blastema, and being really the same organ, it is not surprising that occasionally reversions of type should occur, so that an immature testicle should appear on the one side, and an imperfect ovary on the other. According to Simpson, the ovary in these cases appears generally on the left side. This distinguished author has collected from many sources a large number of cases, the descriptions of some of which are not above suspicion; but in others, especially that recorded by Dr. Banon in the *Dublin Medical Journal* for 1852, the facts are beyond dispute; for the examination of the textures of the gland on either side by the microscope completely established that one was an ovary and that the other was a testicle, though both were so immature as to contain no perfect products. There was an imperforate penis, the urethra opening at its root, and behind this a genital canal closed by a perfect crescentic hymen, a fact which at once removes the case from the classes of spurious hermaphrodites already described. This genital canal led up to a small, well-formed uterus with normal relations to the bladder, rectum, and peritoneum, and having at its left cornu a perfect Fallopian tube with a corpus fimbriatum. In relation with this there was an ovary. There was neither tube nor ovary on the right side, but a testicle containing the characteristic tubules, and provided with an epidydimis and vas deferens. Simpson calls this true lateral hermaphroditism; and he further describes what he calls true transverse hermaphroditism, that is, where the internal organs, testicles, or ovaries, are alike on the two sides, but the external organs represent appearances somewhat like those of the other sex. But it is not clear in any of the cases he quotes that the malformation was anything more than an extension of the characters of spurious hermaphroditism; and as the glandular element must always be considered as the chief element of sex, it is not a philosophical proceeding to say that both sexes are represented unless both a testicle and an ovary are present. Even when the clitoris is perforated by a urethra as far as the glans, the condition is only that seen normally in the *Loris gracilis*.

This view, which I first enunciated in 1873, has been most fully confirmed by an admirable paper by Prof. Morrison Watson, in the *Journal of Anatomy*, October, 1879. He says: "In the gland alone and its structure is to be found the determination of sex. No arrangement of the passage is absolutely distinctive. Even the prostate gland is absent in the males of some animals (elk, red deer, etc.), and it is occasionally present in many female mammals, even women. The lateral hermaphro-

dites of Simpson are those to whom alone the term *true* can be applied."

In Simpson's third variety, to which he gives the name of "true double or vertical hermaphroditism," he describes the presence of a gland of each sex as present on both sides, or, as he says, "actual sexual duplicity." Without denying the possibility of such an occurrence, I must say that I think it very unlikely, and I have no hesitation in saying that none of the cases he quotes justifies the establishment of this variety. The most complete case is that recorded by Vrolik, and he distinctly states that neither in the structure which he supposed to be testicle nor in that considered to be ovary did he find a trace of histological evidence of the nature of the gland. Mere anatomical position goes for nothing in such a case, for the ovary descends sometimes in the same way as does the testicle, for it also has a gubernaculum. It must also be borne in mind that occasionally appendices both to testicle and ovary are met with, giving the appearance as if the individual had three or even four testicles or ovaries. If such a condition were met with in a hypospadic male who had at the same time an enlarged prostatic utriculus, as many of the cases quoted by Simpson undoubtedly had, and if the testicular appendix had not descended with the true testicle, the appearances would be exactly as described in most of Simpson's cases, and yet there would be not the slightest reason for the statement that both kinds of glands were present. The only satisfactory test is that of microscopic examination; and so far the evidence goes to show that there is only one kind of true hermaphroditism—that in which there is an ovary on the one side and a testicle on the other.

The cases lately recorded by Leopold, of Leipsic, and C. E. Underhill, of Edinburgh, are clearly cases of descent of undeveloped ovaries into the inguinal canal—instances of hypererchesis.

CHAPTER IV.

OVARIAN TUMORS AND CONDITIONS WHICH SIMULATE THEM.

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OVARIAN pathology has always been a fertile field for research, and although the list of writers upon this subject which I have given above is a long one, it by no means fully represents the great number of contributions which have been given to it. The interest which diseases of these organs has always excited has been greatly increased during the last twenty years, owing to the remarkable triumphs which surgery has achieved in the establishment of ovariotomy as the most successful of the major operations.

The pathology of ovarian tumors—especially those of a cystic character—involves a number of questions which have been raised and discussed by observers of the greatest eminence; but I think it is only in very recent times that anything like a reasonable view of the origin of these growths has been advanced. In the first edition of this book I said that, up to the time of my writing it, I had found very little which either harmonized with my own observations, or seemed to me to give any satisfactory explanation of these growths. The conclusions which I put forward were at variance with those held by many previous and

probably much more competent observers; but these conclusions nevertheless seemed to me to be founded on fact, and to have the strong recommendation of affording explanations which were at least simple and intelligible. Since I wrote this a good deal more has appeared on the subject, much of which I have carefully studied, but without finding reason to depart from the views I published in 1873.

What seem to me by far the most important contributions recently made to ovarian pathology are the papers published by MM. De Sinéty and Melassez, in the *Archives de Physiologie* for 1878. I find in these papers a great deal which completely confirms my own previous conclusions; but, on the other hand, it is equally fair to say that much which has been advanced by these authors is quite out of harmony with what I have myself observed. Upon the latter point, however, I think the facts established by Balfour, in his researches upon the development of the ovary, which have been given at length in a preceding chapter, may be taken as conclusive, more particularly upon the alleged development of cystic disease of the ovary from the so-called tubes of Pflüger.

As to the prime causes which lead to any of these diseases, we may at once confess ourselves to be profoundly ignorant, and what little we do know is entirely confined to the processes by which these causes produce their peculiar results. It may be that some day we shall know here, as we desire so much to know in every other field of pathological inquiry, what may be the ultimate causes of any particular lesion; but up to the present we have no instance of any knowledge of this kind.

It is extremely difficult to arrange the discussion of this subject in such an order as will be satisfactory from a general view, and any division of ovarian tumors must be entirely arbitrary. We may take them from their physical characteristics, or from their surgical peculiarities, or from their microscopic appearances, or from their supposed methods of origin, but we cannot possibly take them in an order which will satisfy all these aspects. De Sinéty and Melassez adopt a perfectly arbitrary method of classification, which in itself would not be objectionable were it not that the names they make use of for the purposes of their division are such as—in England at least—involve certain meanings which the authors evidently do not imply by them. Thus, they divide their observations into those upon "cystic ovaries," thereby meaning instances where the cysts are very small, and where the ovary is still easily recognizable. "Cysts of the ovary" they use to designate tumors where the ovary is no longer distinct, but seems to have disappeared. And

then again, when they speak of a case in which the solid parts of a tumor become the predominant elements, they use the term "cystic tumor." It is perfectly useless to take up space in discussing the want of precision of such a division as this; all that can be said in its favor is that it is no more illogical than others which have been propounded. The authors themselves say: "It is very certain that between cystic ovaries, ovarian cysts, and cystic tumors of the ovary, there exists quite a series of intermediate tumors, and that it would be very difficult, even impossible, to establish between them well-defined lines of demarcation. In fact, the difficulty is just the same as has been found, within the last two or three years, to hang round the use of the word ovariotomy; for every one whose experience has extended beyond the removal of large ovarian cystomata, has found it quite impossible to use any such word as oöphorectomy, in contradistinction to ovariotomy, without some arbitrary definition and division of his cases. Therefore I have already indicated my intention hereafter to make use of the word *ovariotomy* to include the removal of an ovary for any purpose or disease whatsoever. In like manner I intend the word *cystoma* to refer to any disease of the ovary in which the existence of cysts is a leading characteristic. Whether I may be more successful than my predecessors in establishing subdivisions of this disease, or whether I may succeed in giving a rational view of their methods of production, must be left to the judgment of my readers.

In the first chapter of this book I have drawn attention to the somewhat numerous observations which establish the fact that at the time of birth the follicles of the ovary are very frequently found to be so much distended by limpid fluid as almost to present the appearances of disease; and from the fact, equally well established, that these cysts do rupture and go through the stages of cicatrization, we may assume as proved, that in the dropsical distention of the Graafian follicle we have at least a very common origin of ovarian cystoma. If we go through the literature of this subject, we shall find numerous instances in which these follicles have become so distended as to have an absolutely morbid appearance. Thus, in the *American Journal of Obstetrics* for January, 1880, Dr. T. G. Thomas describes an ovarian cyst, removed post-mortem from a new-born child, which was sent him by a physician in New Jersey. The peculiarity in the case was that the child was delivered at full term, and nothing abnormal was discovered. About one month after birth a tumor was discovered in one iliac fossa, about the size of a hen's egg. The child was well developed at the birth, but soon began to show signs of impaired nutrition, grew very slowly, became

emaciated, and languished until it was three years and five months old, and then died. The autopsy revealed the existence of an ordinary ovarian (or parovarian ?) cyst filling the abdomen. The remains of the Fallopian tube and ovary were upon one side of the tumor.

At a meeting of the Pathological Society of London, on May 21, 1878, Dr. Leared showed specimens of ovarian cysts from twin infants, who had been under the care of Dr. Macmahon, of Norwood. They were born at full term, and one, jaundiced from birth, died at the age of eight weeks. There was found complete obliteration of the common bile-duct. Each ovary was the seat of a cyst the size of a filbert. The other child died from pneumonia, and it also presented a small ovarian cyst the size of a pea.

The observations of De Sinéty and Melassez, and also those of Hausmann, therefore, lead us to the conclusion—which has been quite established by fact—that ovarian cystoma may be the result of these processes in the young child, and that we may expect, every now and then, to see one of these follicular dropsies reach such a size as to require surgical interference. Thus, Dr. Basil, of Bonn, has performed ovariotomy on a child two years of age, on account of a large cystoma. Mr. Folker, of Hanley, has operated at three years of age. A case is recorded by some American surgeon of an operation at seven, and Mr. Wells has operated on a child of eight years. The probability is that in these cases, and in many others, the cystoma has been due to the extension of the follicular dropsy, which occurs normally during the first three months of life, and which has become pathological by reason of the follicles not rupturing, as in the ordinary course of events.

Waldeyer, in the *Arch. f. Gynœkologie*, tome I., p. 289, also observes that: "In the ovaries of old women are often found small cysts from the size of a pea. . . . They are covered with a cylindrical epithelium, which, under the form of glandular tubes, often penetrates into the ovarian stroma. . . . These little cystomata never contain ovules or remains of ovules. . . . In some cases may be seen a continuation of the epithelium which covers these cystic cavities with the epithelium of the ovarian surface."

Here, then, we have a starting-point, from which we may arrive at an explanation of ovarian cystoma—the only one, in my belief, that can be reasonably advanced. It seems to me it will explain all the specimens which I myself have examined. Indeed, I am quite strengthened by my later researches, as well as by those before referred to, in the views I advanced eight years ago, to the effect that ovarian cystoma is the result of fol-

licular dropsy only. The fact which is now almost universally admitted, that ovarian cystomata are never unilocular, also points to this conclusion. Thus, De Sinéty and Melassez say: "Amongst all the cysts we have examined, we have not found one which was truly unilocular. All those sent to us as such presented, truly, a large principal cyst, but, on examining them with care, we have always found other cystic cavities, sometimes small enough, it is true, to pass unperceived on a first examination."

This conclusion I can entirely substantiate from the examination of a very large number of tumors. There is one possible exception, however, and that is of a small cyst—now in the museum of the College of Surgeons—which I removed as an ovary, in a case of severe hemorrhage due to a uterine myoma. The patient recovered, and the operation had for some months a perfectly successful result; but about seven months after it she died, as I believe, from cancer of the uterus. No post-mortem examination was made, and I have been quite unable to obtain any information concerning her case from the practitioner under whose care she was at the time of her death. It is, therefore quite possible that what I removed on that side was not the ovary; or, it may be that I did not remove the whole of the ovary, but merely an outstanding cyst. At any rate, I do not feel disposed, on account of this isolated and incomplete observation, to depart from the conclusion I have come to from an otherwise uniform experience, which is that such a thing as a unilocular ovarian cystoma does not occur. On *à priori* grounds such a condition would be extremely unlikely, for it is difficult to imagine—whatever be the cause of the distention of Graafian follicles into pathological cysts—that it could or would affect one follicle only, and allow the rest of the gland to remain free from its influence. Conversely, granting the likelihood that follicular dropsy develops ovarian cystoma, then we have at once an explanation of what is the fact—that these tumors are always multilocular.

I think that the term *adenoid* may be retained with advantage in the nomenclature of ovarian tumors, because it conveniently classes them with reference to the tissue from which they originate, and by the hyperplasia of which they are formed, without giving any theoretical explanation of their formation. All non-cancerous tumors of the ovary are, therefore, adenoid; even the dermoid tumors are so to a certain extent, for they are the result of increased growth of one or other normal constituent of the gland, without alteration save in quantity. Cancerous growths, on the other hand, introduce tissue which is either not

found in the gland normally, or they produce it in a form which is immature.

There are two methods of origin for pathological cysts which are universally accepted, and both of which are instanced in the ovary. The first is by occlusion of normal ducts or tubes, as best seen, perhaps, in the kidney and salivary glands, and in connection with the ovary in the form of cystic dilatation of the Fallopian tube or of its trumpet-shaped extremity, after adhesion to the ovary—tubal and tubo-ovarian cysts. The other form of cyst-growth consists in the dilatation of a physiological cyst-cavity by its own secretion poured out to an abnormal extent; and this secretion may either retain its original character and constitution, or be altered by the addition of blood or of some of its usual or exceptional albuminous products. This is the common form of cyst-formation in the ovary, and in the multiple adenoid tumor and in the dermoid we see instances of it. The former illustrates the formation of cysts by the retention of its products in the cavity of the normal sac of the gland, these products being the fluid cell-substance and its nucleus, the ovum. This is accomplished, as far as I could discover from two specimens I have to describe, by hypertrophy of the fibrous covering of the gland—a sclerosis of the ovary; and the nucleus, the ovum, seems to have retained its normal appearance until destroyed by some process not yet understood. It is, however, possible that some defect of action in the nucleus may have to do with the non-rupture of the sac; for, under healthy conditions, it is undoubtedly the maturation of this nucleus that governs the rupture of the wall of the ovisac, and enables the perfect ovum to escape. Upon this point it seems to me that the observations of De Sinéty and Melassez have very great value, for they show that, coincident with the absence of the ovum in the follicle, changes are effected in the epithelium of a very marked kind. Whether they result from or are themselves the cause of absence of the ovum, we cannot say.

The dermoid cysts are due, on the other hand, to an altered and increased activity of the cell-nucleus in early life, the results remaining latent for years, until re-excited by the great systemic change. We may look, then, to some other altered condition of the cell-elements for explanation of the remaining variety of cystic tumor of the ovary which has been termed by Mr. Wells and others "proliferous."

I do not like this term as applied to the compound cysts of any kind, for it assumes, what I am quite certain is not true, that the large cysts are directly the parent of the small ones. Thus, Mr. Wells, at page 25 of his book on "Diseases of the Ovaries," de-

fines: "Proliferous cysts — parent-cysts with secondary cysts growing from the interior of the cyst-wall." These minor cysts are secondary, as far as date of growth is concerned; but they are the younger brothers and sisters, not the children, of the larger sacs. In fact, the very caution which Paget gave his hearers in using the term *proliferous* is disregarded in such an application of it, as may be seen from the following quotation:

"In an ovary, it is not unfrequent to find many small cysts, formed apparently by the coincident enlargement of separate Graafian vesicles. These lie close and mutually compressed, and, as they all enlarge together, and sometimes, by the wasting of their partition-walls, come into communication, they may at length look like a single many-chambered cyst, having its one proper wall formed by the extended fibrous covering of the ovary. Many multilocular cysts, as they are named, are only groups of close-packed single cysts; though, when examined in late periods of their growth, and especially when one of the groups of cysts enlarges much more than the rest, it may be difficult to distinguish them from some of the proliferous cysts." (Paget's "Surgical Pathology," p. 415.)

The formation of a compound cystic tumor in the ovary, whether it be of the multiple variety or of the less complete kind of which I am about to speak, may be very well illustrated by blowing soap-bubbles in a basin. If the fluid be not viscid enough to enable the bells to retain their form, then the normal condition of the ovary is represented, its cells bursting and disappearing. Let us suppose that the cell-growth is constantly going on, and that some alteration occurs in the state of matters which prevents the cell-walls bursting; the fluid in the basin is so viscid that the bells do not break, and bubble after bubble is formed, some larger, some smaller, until a large multicystic tumor is the result. The actual appearances of the cystic ovary may be very well imitated in the basin of soap-lees. A large cyst can be made with little ones crowding into it, looking like its offspring, and the walls between two or three may be broken down, making one larger multilocular—the remains of the intervening walls not being left in the instance of the soap-bubbles. If they had been left, the appearances would be identical with what is represented diagrammatically for ovarian tumors in Mr. Wells's book on page 39. In the ovary we have the continual production of cells, representing the continuous blowing of the bubbles; and we have only to discover what it is that is analogous in the ovary to the increased viscidity in the solution of soap; what it is that keeps the cysts in their entirety, perverting a physiological into a pathological process.

With the exception of two examples of "Rokitansky's tumor" (to which I give the name of multiple cystoma), and which I shall afterward describe at length, I have failed to find anything like ova in any cysts of the tumors I have examined; and I have not seen any reason to believe that the little secondary cysts met with in the walls of some of the larger sacs are what Dr. Ritchie has interpreted them to be—dropsy of the blastodermic vesicles—chiefly because I have failed to see round them anything like remains of the membrana granulosa, and I have seen three or four of them on more than one occasion on the wall of the same sac. In searching for ova in the cysts of the two tumors where I found them, I had seldom occasion to look for them in the walls of the cysts, as they seemed to have been floating about loose, or to have been set free by the opening of the sacs. In other cases, not finding them in the liquor folliculi, I adopted the further plan of first isolating the cyst to be examined, and, having opened it, I searched carefully in every sediment of its contents for the ovum. Not finding it, I turned the sac inside out over a ball of cotton-wool soaked in glycerine, smeared its everted surface with the same substance, and, covering it piecemeal with a thin glass, I carefully examined it over its whole extent by reflected light. I never found anything I thought was an ovum. In one tumor which I removed lately, I found a mass of secondary growth in the position of the ovary at the base of a single large cyst. I made sections of this mass by the freezing process, and found it composed of follicles all lined with their proper epithelium and undergoing cystic enlargement, but in none was there any trace of an ovum.

I cannot reconcile this discrepancy between my observations and those of Dr. Ritchie otherwise than by supposing that, having been one of the discoverers of ova in a certain kind of ovarian tumor, he was too anxious to apply his principle of explanation to all; or he must have had a plan of examination more successful than mine.

My original observations, made in 1872, have been amply confirmed by De Sinéty and Melassez, who say: "no author has met with ovules in cysts larger than a walnut. All the large cysts we have studied, and which we shall describe farther on, possessed an epithelial investment completely different from that of the follicles, a fact equally attested by all recent observers." Rindfleisch has found, only once, one ovule in an ovarian cyst of which the cavity was no larger than a cherry.

The fact that I never found ova in any of the sacs of these multicystic tumors, even the smallest, or in those with the most limpid contents, led me to entertain the opinion that in this we have

an explanation of their formation. The function of the ovary is one of cyst-formation from its earliest existence to its latest, and in its pathology we need not go far away from its physiology. It seems to me, therefore, *à priori*, very unnecessary to resort to the ingenious explanations of cyst-formation advanced by Dr. Wilson Fox, though they may be fitted to exceptional cases. Experience in the examination of the growths themselves has strengthened this view; for my wonder is greater, the more I see of them, that they do not occur more commonly and with more complexity than they do, considering the apparently reckless amount of ovarian cyst-production that goes on throughout life.

The aim and object of this cyst-formation is the production, maturation, and discharge of the ovum. But, if the ovum be not formed, or if it be produced only to a rudimental extent, may it not happen that the cyst will not be ruptured, but go on aimlessly expanding? Whatever be the source of the change, we know that it does not affect one ovisac alone, but may influence them in great numbers, whether it be in a tumor where the ova have been matured and subsequently prevented from escaping by sclerosis of the coat of the ovisac, or in a growth where the ova are not to be found. There is a great clinical difference between these two kinds of tumor; for in the one the growth is very limited and slow, and in the other it may be, and often is, extremely rapid, and is practically unlimited in extent. In fact, the growth of these cysts without ova partakes somewhat of the character of malignancy assigned to them originally by Bright for clinical reasons. Malignancy, apart from any association with cancerous structure, is always indicated histologically by a tendency to the production of a form of tissue which is young and immature, and this is certainly the condition of these ovaless ovisacs.

As a matter of fact, ovarian cystomata are a great deal oftener malignant than has yet been admitted. The recovery from an ovariotomy is generally so rapid and easy that at the end of a month we say "cured," and discharge the patient. But a number of these "cures" die speedily of cancer of the peritoneum or of other organs, and the more our primary mortality from the operation has diminished, the more numerous have become these secondary deaths from cancer, occurring between three and thirty months after the operation.

A few months ago I removed an ovarian tumor from a child aged twelve, and did not see, either in the tumor or in the abdomen, a single trace of malignant disease. She recovered from the operation, but died within the month with cancer in all the large organs. This singular sequence requires explanation, and

I believe that has now been arrived at, the last link being supplied by the work of De Sinéty and Melassez; and if the observations be correct, then a bright light is shed by them on the whole question of cancer. Elsewhere ("Diseases of Women," 1877) I have said: "Histologically, the characters of cancer are essentially those of immature and reckless cell-proliferation, the presence of numerous nuclei, both in the cells and free, suggesting the idea that they have had no time to become full-grown; and I have never failed to find evidence that the primary changes take place in the epithelium." The bearing of this will be seen more fully later on, when I speak of cancer of the ovary, but at present it leads up to the remarkable discovery of De Sinéty and Melassez, to which I have alluded.

"If, then," say these authors, "we cannot absolutely deny the possibility of the formation of a large cyst of the ovary at the expense of a follicle, it may be seen that this formation does not appear to result from a simple dilatation of these follicles, as many have said, and that we must admit a destruction of the ovules and a transformation of the epithelium when the follicles exceed a certain volume." It will afterwards be seen that there is probably an error in the last clause of this quotation, in that the cause is put for the effect; but it is in the establishment of the fact of the change in the epithelium, or rather in the interpretation of it, that the merit lies. I had long been quite familiar with the fact that between the epithelium of a healthy Graafian follicle and that of a large ovarian cyst there was a great difference. I had also ascertained the fact that, in an ovarian cystoma where ovules were to be found (Rokitansky's tumor, or the multiple cystoma), the epithelium remains as it is in the Graafian follicle. I had concluded, therefore, that the cyst in the latter case was merely the result of follicular dropsy, and that therefore probably all ovarian cysts were the same; but I had not seen, in the alteration of the epithelium, the explanation of the marvellous difference in the two kinds of tumors, nor that here we might find an explanation of the growth of ovarian cystomata. Such, however, I now believe to be the case.

Among other facts which lead me to the conclusions which I propose now to discuss were the absolute resemblance between the arrangements of the blood-vessels of a Graafian follicle and those of an ovarian cystoma which has not been altered by rupture, tapping, inflammation, or malignant degeneration. If such a cyst be carefully injected, cut into sections, and compared, in the matter of its vascular arrangements, with a Graafian follicle either before or after its rupture, no difference can be seen. The description of these will be seen in the first chapter. Roki-

tansky, Cruveilhier, Schroeder, Arthur Farre, and De Sinéty all record observations—to which I might add several—of cysts which had clearly been produced in the follicle after its rupture by a continuation and recurrence of the hemorrhage, and which have been called hæmatic cysts.

The theory, therefore, of this method of growths of cystomata, is that there is a reversion to the premoliminal condition of the ovary, so far as the ova of the affected ovisacs are concerned. The whole ovary does not, of course, become simultaneously implicated, and matured ova may still be given off by some ovisacs still unaffected and within reach of the Fallopian fimbriæ. Impregnation may thus occur from a degenerated ovary, though it is much more likely to occur from the one which remains healthy.

We have in these facts the reason that these adenoid tumors occur with greatest frequency during menstrual life, if indeed a future experience may not yet show that they do so exclusively. The menstrual congestion and excitement induces a dropsical distention and growth of a cell which would not be so perverted during childhood or senility, when its blood-supply would be sufficient only for passive nutrition.

The growth of cysts in the walls of the major sacs, appearing sometimes outside and sometimes within in great numbers, depends wholly on the relation of the original adenoid tissue to the cyst-wall; and, as that wall grew primarily in that tissue and surrounded by it, it would be indeed surprising if it did not carry along with it in its expansion some of the cells of the *couche ovigène* from which it sprang. These displaced cells have in their turn a stimulus for development, prematurely perhaps, on account of the increased hæmic activity of their abnormal surroundings, due to the growth of the sac. They also go through the process of dropsical distention, developing no ova, not rupturing, but becoming secondary cysts—perhaps ultimately to rival, or even to excel that which has preceded them, on which they grew, and of which they have been supposed to be the offspring.

Sometimes these intracystic growths line the cavity of the major cyst like an eruption of small-pox. In the case of a tumor which was removed by my colleague, Mr. C. J. Bracey, hundreds of little separate cysts lined the major sac of the tumor, as if the whole adenoid tissue had been spread on the inner surface as the cyst grew, and as if it were degenerating; and I have no doubt that this was really the case, for these little cysts were all Graafian follicles beyond a doubt.

The great change effected within the Graafian follicles, which

is doubtless that to which they owe their subsequent development as cysts, lies in the epithelium. To quote the words of the French writers: "The epithelium in these new formations shows every possible variety; but in none, not even those which simulate the Graafian follicles, have we found any epithelium similar to that of the follicles, neither have we ever found any ovules in their interior." But they very carefully point out that this change is by no means effected suddenly, nor is it extended uniformly over the whole surface of the cyst.

Speaking of the pathogenia of follicular dropsy, they say:

"In all the cases we have just stated the follicular epithelium was normal in its form as in its dispositions, although it must have been the seat of a certain degree of proliferation, since it covered a larger surface, the volume of the follicle being augmented. The ovule could be detected in a tolerably large number of follicles, even in the largest, and it did not appear altered. The lesion appears then to consist in an augmentation of the follicles, and in a greater quantity of follicular liquid. We cannot say whether this liquid is or is not modified in composition, if the secretion is or is not altered."

In those cavities which they regard as pseudo-follicles, that is, resembling in every particular save that of their epithelial lining the follicles they have just left, De Sinéty and Melassez note that "the differences are still more considerable. The epithelium which lines these cavities resembles in nothing that of the follicles. There are found in it cylindrical epithelial cells more or less voluminous, cells with *cilia vibratilia*, sometimes even caliciform cells. The different kinds of epithelium may be observed in the same cavity. In general these cells are disposed in a single row.

"In no cavity did we find anything that in any way resembled an ovule or a proligerous disc. The contents are a transparent liquid, more or less fluid, coagulating and becoming opaque at the contact of alcohol, reassuming its fluidity and transparency in water, and showing in its interior granulations and degenerated cells, proceeding no doubt from the epithelial investment of the walls. In one of these cavities there existed large, radiated cells disseminated in the contents, which gave it the aspect of mucous tissue."

The only points concerning the epithelium in which my own observations do not entirely concur with those of the writers I am quoting are, first, the frequency with which they appear to have met with cells furnished with vibratile cilia. I have found these in only one of the tumors I examined, and that was a dermoid cyst, having abundant indications of immature nerve-

structure; these cells I regarded as probably representing the lining membrane of a cerebral ventricle. Beneke speaks of having found them once or twice, but I do not know that any other observers have seen them frequently. De Sinéty and Melassez speak even of having seen them in a very fresh specimen with the cilia in motion. From the general accuracy of the observations of these gentlemen I am bound to accept these remarkable statements, but I do not see that the occurrence of these cells can be looked upon as having any importance or significance. The second point of difference between us is that in the dropsical follicles containing ova I have found the epithelium quite normal in arrangement and appearance.

We have, however, from them a complete establishment of the facts that I have so often seen, that we may have in the same ovary—indeed, we may have in the same cyst—indications of a reversion of type in epithelial growth; that is, we may have the normally cylindrical cells at one part of the lining membrane of a large ovarian cyst, while at another part we may have round, immature, rapidly growing cells, presenting all the appearances they would possess upon a mucous surface. Such cells as these, however, are not seen in a Rokitansky's tumor. In columnar cells the arrangement is, of course, in a layer single, or nearly single, and the first change from the type is seen when we have cells increasing largely in size, becoming somewhat irregular in shape, and having underneath another layer polyhedral and polymorphous. We may, I think, accept the following description as perfectly accurate:

"Sometimes these are flat cells, with sinuous borders, and occasionally of considerable dimensions. Seen in front, in silver preparations, they simulate a pavement with irregular designs, quite different from the very regular mosaic formed by the cylindrical epithelium. Seen in profile, on sections, their thickness is found to be somewhat variable; it is generally larger on a level with the nucleus. It possesses one and sometimes several nuclei.

"Sometimes, instead of flat cells, we have thick, voluminous ones, of the most varied and fantastic forms. Some are seen presenting a broad base of implantation, and terminating in a narrow, tapering extremity, which causes them to resemble cones. Others, on the contrary, are only attached to the wall by a thin pedicle, more or less long, while their extremity is distended, which gives them the appearance of clubs. There are some that are broad at the base, broad at their free extremity, and of which the two voluminous extremities are united by a more or less narrow neck. We have seen some which presented

successively two constricted portions, and consequently three distended portions; others which were carried, like grapes, upon a ramified pedicle. The free extremities have a tendency to approach the spherical form, while the others are polyhedric through reciprocal pressure.

"The protoplasma of these singular cells is in general granulous, with large grains. It takes a brownish yellow color under the action of picrocarminate, except in the portions constricted or in the form of pedicles, which portions are more homogeneous and more refractive. These generally possess large nuclei, with one or several voluminous nucleoles. Those that present several distended portions have nuclei in each of these portions. It will be perceived that they are elements that proliferate and bud with very great activity."

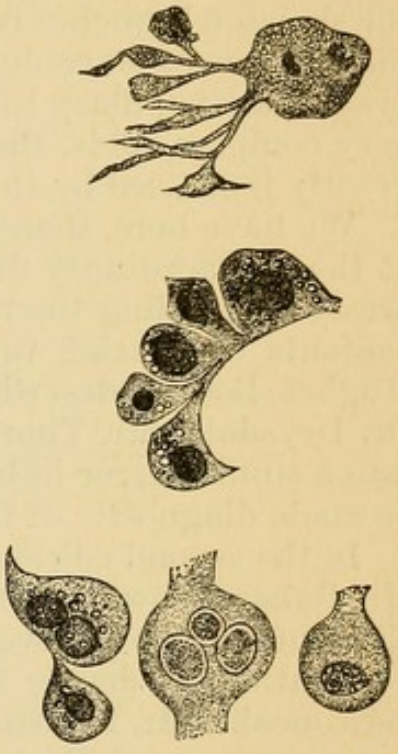


FIG. 26.—Altered Epithelium from Walls of Ovarian Cysts. (After De Sinéty and Melassez.)

The epithelium of a cyst-cavity, after having undergone the changes of type already described, takes on a further growth, and, departing from an arrangement in a single layer, as seen in the normal follicle (Fig. 12), or in the cyst still containing an ovum, it increases in thickness, adding layer upon layer of the immature cells. As these layers grow, the cells alter more and more from the normal and adult forms, and finally cease to have any resemblance to them. Waldeyer, as well as De Sinéty and Melassez, have fully confirmed my observations on this point. The new layers are not uniformly distributed over the interior of the same cyst, and they are often so localized as to form elevated patches, or even tubercles, on the inside of the cyst. To the naked eye these often look very like cancer, and on microscopic examination they have all the appearances which I have already described as belonging to that tendency. The French authorities I have so often quoted say of these masses: "They have the aspect of carcinomatous fungosities, and they appear also to have their malignity." Boettcher and Waldeyer, besides other authorities, fully support my conclusions.

In some tumors we find velvety-looking tufts hanging from the walls into the interior; and these are found, on examination, to consist of a basis of nucleated fibrous tissue—in fact, ovarian stroma—lined on each side of their many branches by regular

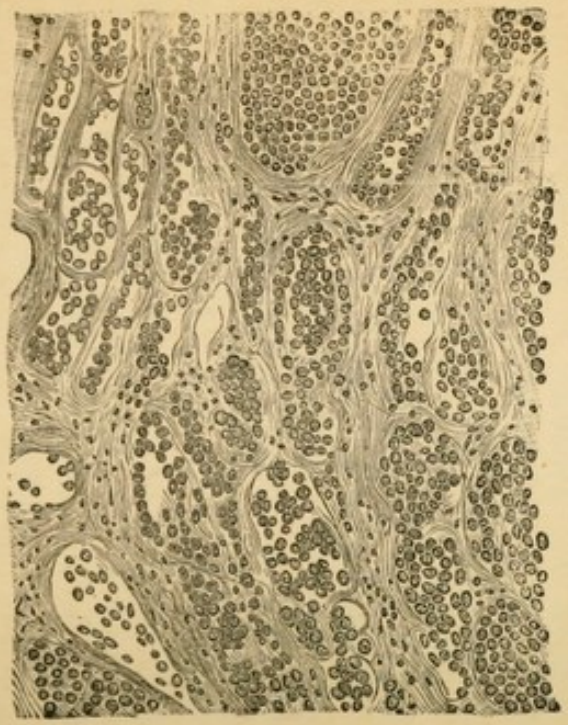
columnar epithelium, or by epithelium of the immature forms. They are sometimes, in fact, transformed into pediculated masses of villous cancer.

As these structures divide and redivide into branches, they very much resemble trees, and therefore have had conferred upon them, among other names, that of intracystic dendritic growth. If a cyst in which they exist be injected and the sections stained, they will be found to consist merely of the remains of follicles which have burst in their efforts to become cystic, the skeleton branches retaining the epithelium of the cysts which formerly were on each side of it. By the growth of subsequent cysts these papillary remains are often forced into irregular and very complex folds, the apparent complexity of which may be greatly increased by the accidents of the section.

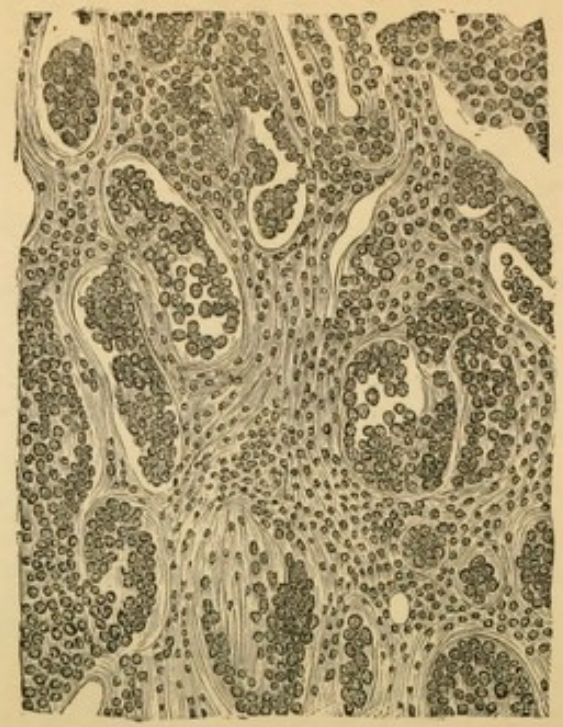
We have here, therefore, among other things, an explanation of the extraordinary differences of opinion which have been expressed regarding the results of microscopic examination of the contents of ovarian tumors. Many years ago Professor John Hughes Bennet described what he termed an "ovarian cell." Dr. Drysdale, Mr. Thornton, and several others, have all fallen into a similar error in believing that any one form of cell could be made diagnostic of these curious growths.

In the second edition (1868) of his book on "Ovarian Dropsy" Mr. Baker Brown discusses the views of Professor Bennet, of Edinburgh, who believed that it was possible, by microscopic investigation, to decide as to whether the fluid was ovarian or peritoneal. Mr. Brown, in conjunction with Mr. Nunn, seems to have gone very fully into this matter, and, in the words of the latter, it seems to have been decided as follows:

"I am inclined to say, as the result of many examinations of different specimens of ovarian fluid, that the most constant characteristic of such fluid is its containing, in greater or less abundance, cells gorged with granules, and, in addition, circumambient granules having the same measurements as those encompassed by the cell-wall. At one time I considered the size of these granules was constant, but subsequent observations have convinced me of the incorrectness of this conclusion. The size of the gorged cells and of the granules varies greatly, even in the fluids from different cysts of the same ovary." "In the present state of our knowledge I do not think we are justified in asserting that the nature of the fluid would be diagnostic of the disease which gives rise to its production. What I believe to be the value of a microscopical examination of the fluid is that it may serve to strengthen an opinion, but alone it ought not to decide one."



a.



b.

a, section of fœtal ovary; *b*, section of rapidly growing cancer of the ovary. Comparison of the two sections show that cancer is a reversion to the fœtal type of growth.

We may also find, from the observations of De Sinéty and Melassez, a confirmation of what I advanced many years ago in a discussion upon this subject: that there was no kind of cell of epithelial origin that it was not possible to obtain from the cyst-cavities of an ovarian tumor.[1]

In the last paragraph quoted from the French authors there is a most concise description of what must also be regarded as a tendency to malignant growth; that is, the reversion of type in the production of these cells toward immature, incomplete, and rapidly growing elements which are practically cancerous.

This of course at once explains the clinical fact which all ovariotomists are quite familiar with, and to which Dr. Keith has especially drawn attention—that the rupture of certain cysts, on the escape of their fluid into the peritoneal cavity, is followed by, or at least is associated with, the infection of the general peritoneal surface with papillary cancer, this accident having a uniformly fatal result. On the other hand, I have seen over and over again the same cells and the same expressions of immature growth in the peritoneum, without the presence of any ovarian tumor; and only on the morning in which this sentence was written I opened the abdomen of a patient placed under my care by Mr. Oliver Pemberton, in which I found exactly the condition so accurately described by De Sinéty and Melassez in the above-quoted sentence, but with no ovarian tumor. Now that we open the abdominal cavity with great frequency, we are constantly coming across this peculiar form of malignant disease, and we find it is by no means confined to the inside of ovarian tumors or to the peritoneal cavity, even when no tumor exists there, for I have had occasion to examine pieces of the pleura and of the pericardium where exactly the same appearances were found.[2]

The conclusion from all this is that to which I have already pointed, that the growth of ovarian tumors is associated with a tendency toward malignant disease, which finds constant clinical expression, and which receives its explanation in the mar-

[1] Dr. Mathews Duncan speaks of this question with his usual force in these words: "In fact, we have, in all departments of ovarian diagnosis, more to admire in the zeal and diligence of histologists, in regard to the fluids, than in the exactness and reliability of the practical results they can show."

[2] But my most recent experience tends to show that there are two forms of papilloma associated with ovarian tumors, one malignant, and one not so. The latter speedily disappears after the removal of the tumor and the patient gets quite well, though its naked-eye appearances are quite indistinguishable from those of the malignant kind. I have had two cases within the last year, aged 57 and 38 respectively, where I have left large masses of papilloma, fixing the uterus in both cases. Since the operations these masses have entirely disappeared, and the patients are both in perfect health.

vellous changes we find produced in the epithelial linings of its cysts. Much more requires to be done in the study of this most interesting question, and doubtless, when the method of its progress is made clear, we may find some indications for a more sure prevention of it. One thing I am certain it clearly establishes, and that is the absolute propriety of removing ovarian tumors at a very much earlier stage of their existence than has been, till recently, the accepted rule in practice. If these epithelial changes are progressive—as doubtless they are, and if they are malignant—as I certainly believe them generally to be, then, acting upon the principles which guide us in the treatment of all tumors, we ought to remove an ovarian cystoma early in its history, before these changes have been effected, and certainly before there is any risk of cyst-rupture. Finally, we ought to regard the operation of tapping as one which ought to be discarded, save under very exceptional circumstances, because not only is it fraught with considerable immediate danger, but it seems to possess a still more important secondary risk, which has until lately been almost overlooked. I may say that in my own practice it is an operation never performed, unless I am certain the tumors cannot be removed.

There is one fact which may be quoted in support of the views advanced by De Sinéty and Melassez, to which they have not drawn attention, but the significance of which becomes apparent; that is, that while, in the cysts of the tumors which I shall afterward describe as possessing ova, the contents are always limpid, whereas in the cysts where the changes have occurred the contents are mucous, viscous, highly albuminous, and often bloody; in fact, they present all the characteristics which the contents of mucous cysts would possess. It might not be inappropriate also to point out here that the arguments I have given above also point in the direction of establishing for cancer a local origin. We have, then, ovarian tumors with well-pronounced and distinctive characters, possessing the power of infecting the system generally with cancer, as they most undoubtedly do when their removal is too long delayed, or when they have ruptured or been frequently tapped; and, on the other hand, we have tumors with the same characters, but which have never been tapped and have not ruptured, but have been removed early in their history. By the early removal of these latter tumors we remove the source of the systemic infection, and prove, it seems to me, a local origin for cancer—as far as the ovary is concerned, at least.

There comes then the question—if we should see any of these altered and sprouting cells in fluid removed from a cyst or from

a serous cavity—Should we set the case down as being hopelessly malignant, and, in the case of an ovarian cyst, refuse on that account to operate? I certainly could not answer these questions in the affirmative. I believe that such appearances indicate the high road to cancer, but it is possible that the goal may never be reached. I think it perfectly certain that removal of the tumor may arrest the progress of the change before a general infection is reached. Dr. Mathews Duncan very well points out that the ovary is in every way the most isolated organ in the body. Removal of a cystoma in which such changes are being effected, at an early stage, may avert a systemic affection; and, as a matter of fact, I have observed all these changes in tumors which I removed many years ago, and the patients from whom they were cut out are alive and well at this date. In one case the period which has elapsed is eleven years.

Upon the question of the origin and diagnosis of this condition a good deal has been written, especially by Dr. Foulis, of Edinburgh, and Mr. Thornton, of London. They both claim the credit of having discovered masses of sprouting epithelium, both in the cystic fluid and in that of the peritoneum, which will enable us to diagnose cases of cancer. After a very large experience, both of microscopic manipulation and of cases of this character, I must absolutely dissent from the views they express. Dr. Foulis goes so far as to say that the absence of these sprouting masses from ascitic fluid is an almost certain sign of the absence of malignant peritonitis and malignant ovarian tumor. But I have had, in at least two cases, reason to suspect malignant tumors, when I found none of these cells in the fluid, and yet I found abundant reason afterward to know that my suspicions were correct. In fact, I place no reliance on the presence or absence of these cells in fluid removed by tapping, and as I never tap removable tumors at all now, I never have any occasion to look for them, or any opportunity. The changes to which their presence is due certainly lead to a malignant condition in their later stages, but their presence is no guide for a prediction that the patient will die of cancer, and it is the best of all arguments for the speedy removal of the tumor.

It is also quite certain that this form of papillary cancer frequently arises long after the removal of an ovarian tumor under circumstances which make it extremely unlikely that the tumor should be regarded as its cause. They are both probably the result of the same condition, whatever that may be. Thus, in the *Lancet* of October 25, 1875, I published a brief note of a case of ovariotomy in a young girl, in which the operation was made to cure a complete protrusion of the uterus. The operation was

performed on August 18, 1875, and after her recovery from it, until about May, 1877, she remained in perfect health. She came to me then with indications of a general failure in her health, obscure pelvic pains, a slight amount of ascites, and a small fixed mass behind the uterus. For a month she took chalybeates, and returned to me much improved in her general health, but with more ascites and with the retro-uterine mass increased. In the beginning of July the increase of the ascitic effusion was so marked that it became evident something must be done. The age of the patient (nineteen) made me hesitate to pronounce it a case of cancer of the peritoneum, to which view I strongly inclined. The only alternative which seemed to me reasonable was that the other ovary had become cystic, was fastened in the pelvis, and was producing the ascites by pressure on veins. I deemed it therefore right to make an exploratory incision, and this I did on July 15th. In the operation the only noteworthy point was the absolute perfection with which union had taken place between the tendinous structures divided in the previous operation—a result which is not always obtained after abdominal section.

When the peritoneum was opened and the fluid evacuated, the lesion was found to be the papillary form of cancer of the peritoneum. Small papillary nodules were scattered over the whole surface of the parietal peritoneum within reach, and on the surface of the small intestines. The pelvis was occupied by several masses, the largest of which, about the size of an orange, seemed to embrace the rectum, and this it was which had been previously felt from the vagina. At the posterior surface of the uterus several nodules were felt, but the remaining ovary (the left one) was perfectly healthy. The right cornu of the uterus was tied up to the lower angle of the wound by a firm band about a third of an inch in diameter, representing the pedicle of the tumor removed nearly two years before. She recovered from this operation, went home, and died after great suffering, on the 27th of August. I have again examined the tumor removed from her with great care, and can discover no appearance of papillary growths about it, and, as she remained in perfect health for at least eighteen months after the operation, I can only regard the access of the papillary growth as an independent event.

There is another condition associated with malignancy in ovarian tumors—hemorrhage into their cavity. I have seen one case where this occurred, yet where the tumor was not cancerous; yet, as a rule, its occurrence must always be regarded as suspicious.

The following is a typical example of a case of cystic tumor of the ovary which has undergone malignant degeneration—one

in which the mistakes I fell into have been of immense service to me since.

On March 2, 1876, I was summoned to the neighborhood of Llangollen to see a patient under the care of Dr. Price Jones, from whom I received the following history: She had been confined of her first child on February 21st. The labor was natural, the child still-born, the placenta somewhat friable, but expelled without difficulty. She did not, however, diminish in size as much as usual after labor, and in a day or two symptoms of peritonitis appeared. On February 28th her pulse and temperature fell to 100 and 99° respectively, and the only matter of note was that the abdomen was greatly distended by fluid. At 2 A.M., on March 2d, I found the abdomen so distended as to have quite a drum-like tightness, the temperature quite normal, but the pulse about 180 and the respirations 50 in the minute. These symptoms were regarded as due solely to the mechanical interference with breathing. The uterus was fixed high up in a solid, doughy mass, which could only be blod-clot. No intestinal resonance could anywhere be discovered. Generally over the abdominal surface a wave of fluctuation could be felt, but here and there it was less distinct. Palpation gave no assistance on account of the tenseness of the integument. The conclusion I came to was that it must be a case of intraperitoneal hæmatocele followed by some serous effusion; for I regarded it as impossible that it could be all blood. I tapped the abdomen, and removed about three quarts of fluid, which seemed like pure venous blood. This gave immediate relief; and when I left her, at 7 A.M. on March 3d, the pulse had fallen to 120 and the respirations to 32 in the minute. The relief of the tension also enabled me to discover floating doughy masses, which I regarded as blood-clot. She improved considerably after this, and was brought to Birmingham on March 30th. The journey was delayed as long as possible; but she became so urgent for something more to be done that it was impossible to appease her any longer. The exertion of removal was, however, evidently too much for her, as on the following day a passive œdema of the left thigh occurred, and this was followed by an increase in the size of the abdomen. This latter condition became so serious on April 2d that it was necessary to tap her again, and eight pints and a half of a fluid which seemed like equal parts of blood and water were removed. The breathing was greatly relieved by this for a few hours; but on the afternoon of the next day it became again very bad, and, as it then seemed to come from the chest, Dr. Heslop was called to see her in my absence. It was then discovered that the left pleura was full of fluid, and three pints and a half were immediately re-

moved from it by aspiration. This quite relieved her breathing. As the fluid was distinctly tinged with blood, Dr. Heslop suggested that there might be some malignant disease in the chest; but, on careful discussion of the whole aspects of the case, there was no sufficient data to arrive decisively at such an unfavorable conclusion, as it was thought possible that the pleural effusion might have been the result of mechanical pressure from the abdomen. On April 4th, 5th, and 6th she was very comfortable; and, after careful discussion, it was determined to open the abdomen to determine if anything could be done to arrest the hemorrhage. In cutting open the abdomen, a line of abnormal tissue was cut through which looked like malignant growth, and then a cavity was opened which contained a quantity of bloody fluid and thick layers of laminated fibrin. As no point of hemorrhage could be detected, and as it was felt that any disturbance of the structure might lead to hemorrhage which might not be controllable, the cavity was washed out with thymol solution, a drainage-tube inserted, and the wound closed. The nature of the case was not made absolutely clear by the section, as no accurate idea could be formed as to the nature of the posterior wall of the hæmatocele cavity. She died on the afternoon of April 9th. The post-mortem examination was made by Dr. Saundby, who found that the left pleural cavity contained about two quarts of blood-stained serum, the lung being quite collapsed, but healthy. There was a fungating ulcerated growth about the size of a walnut, covered with blood-clot, on the pleural surface of the diaphragm. The right pleural cavity contained about a pint and a half of similar fluid, with a similar growth on the diaphragm. Some of the mediastinal glands were as large as hens' eggs from cancerous infiltration. The abdomen was occupied by a large tumor matted down to the uterus, broad ligaments, and section, and only after careful dissection could it be made out that this mass was a cancerous tumor of the left ovary, that into its cavity the hemorrhage had occurred and the incision had been made. It was adherent over the whole of its anterior aspect to the abdominal wall. The microscopical appearances were those of encephaloid cancer.

Looking back on this case, I of course regret that I performed abdominal section; indeed, I did so against my own convictions and entirely at the patient's most urgent request. I had the advantage of the help of Dr. Marion Sims in the case, and therefore had as good security for avoidance of error as could be—yet we were all mistaken.

The term *colloid*, as applied to tumors of the ovary, must be held to refer only to the consistency of the fluid contained in them,

and in no way as a point for classification. I have never met with a description which has persuaded me that the so-called *colloid cancer*, as seen in the breast, intestines, and peritoneum, has ever been met with in the ovary. What we see of it is the myxoma already described, and which is always quite localized in the tumor, a mere incident, as it were, never forming the mass of the growth. In other organs it is practically a malignant disease, but whether it is so in the ovary I do not know. It is, as I have said, the reversion of the stroma of the ovary to its young form, and may therefore be suspected. The first time I saw it was in a tumor sent to me for examination by Mr. Spencer Wells, who asked the question, "Do you think this cancer?" In my reply I said I feared it was; the stroma was so young and immature as to resemble perfectly a myxomatous growth, or the canalicular structure of the umbilical cord. Indeed, if I had placed sections from these three structures under adjoining microscopes, I do not think that I have yet met with the histologist who could distinguish between them.

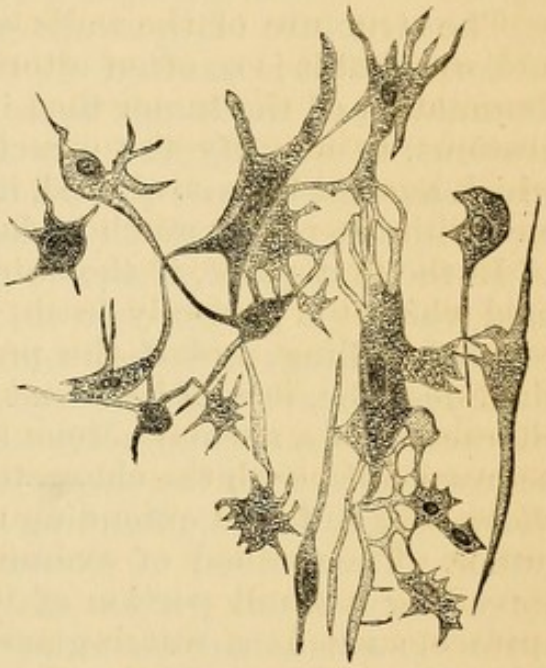


FIG. 27.—Myxomatous Growth of Ovary.

An example of this disease was exhibited before the Obstetrical Society of London, in June, 1878, and as it illustrates not only this disease, but the fact that diseased ovaries lead to intractable menorrhagia which kills the patient, I shall quote the report in full. If instead of using a sponge tent, which merely had the effect of killing the enfeebled patient by septic peritonitis, the surgeons had removed the patient's ovaries, they would have removed the cause of the hemorrhage, and probably have saved and cured their patient. The victim, a woman aged twenty-one, had suffered from almost constant hemorrhage since her marriage three years previously; and when admitted into Guy's Hospital she was so exhausted that transfusion was thought of. The hemorrhage, however, was checked by the use of a sponge tent and the subsequent injection of warm water, but the woman died ten days later of suppurative peritonitis. Both ovaries were found enlarged, but retaining their normal shape; and it was at first thought that the enlargement was due to acute inflammation.

Microscopical examination, however, showed that the histological characters of the growth were those of myxoma, though the harder portions exhibited the characters of sarcoma. The spleen was leukæmic. The uterine mucous membrane was disintegrated on its surface (as shown in one of the microscopical sections) and altered in structure, its round cells appearing separated, as if by fluid effused between them, and being surrounded by a fibrillar growth, reminding one of the state of things found in the ovaries. He would leave it, the reporter said, to the pathologists to decide whether there was any connection between the leukæmia, from which the patient suffered, and the myxomatous enlargement of the ovaries.

The structure of the walls of ovarian cystomata is tolerably uniform, but it is so often altered by protracted growth and inflammation of the tumor that it may be difficult, in many preparations, to identify the structures. If a tumor be examined which has not been so altered, it will be found that the structures met with are pretty much as follows:

In the first place, if the outer surface of the tumor be examined while it is perfectly fresh, before it has been damaged by rough handling, and if the preparation be made in the way I shall describe, it will be found that this membrane has all the characters of a normal serous surface. A mosaic of flat, polyhedrous cells, with the characteristic cement substance between them, will be found extending uniformly over the surface of the tumor. The method of examination which I pursue is that of spreading a small portion of the tumor over a slightly convex surface, such as a watch-glass, and placing it in my freezing microtome with distilled water only. I then screw it carefully up to the level of the cutting-slab, and take a thin slice off the outside surface of the tumor. This is immersed in a solution of .5 per cent. of lactate of silver, and is then mounted in glycerine jelly. This treatment brings out all the structures I have described in similar situations, and we see the stigmata and stomata, with the characteristic endothelium of the latter, just as we find them in all other serous pavement surfaces. From the endothelium of the stomata I have an absolute assurance that the malignant growths of which I have spoken, and of which I shall have to speak again, are derived. They constitute a variety of epithelioma, and are, in my estimation, analogous to the nesting of the epithelial cells which we see in skin cancer. These cells are produced by a rapid and immature proliferation of the endothelium of the stomata. In a favorable section they may be seen to be crowding out of a stoma, tearing asunder the relations of the epithelium to its subjacent structure; and thus it is

that they possess to the naked eye and to the touch the peculiar characteristics which have obtained for this disease its special names of papilloma and miliary cancer. It will be seen, therefore, from this description, that it differs in no way from any other form of epithelioma, and it is to be seen on the surface of an ovarian tumor as often as it is to be seen either on its inside or upon the peritoneum without the presence of an ovarian tumor at all.

The descriptions given by De Sinéty and Melassez of the external surface of an ovarian cystoma differ very materially in some points from my own, but I have only to point to their methods of examination as affording a complete solution of the discrepancy. I have already said—in a paper read before the Royal Society upon the "Anatomy of the Umbilical Cord"—that no correct descriptions can be given of any tissue from a microscopical examination made upon anything but perfectly fresh tissue, and this is most peculiarly true of an epithelial surface. To employ any hardening reagent, and then describe what is seen, is, therefore, not describing what may be seen in fresh tissue. When the French authorities say: "That by employing these two processes we have assured ourselves that the exterior investment of the walls in no way resembles the endothelium investment of the peritoneum," the difference between their description and mine is further to be explained by the different use of the term *endothelium*. They seem to employ it to mean a subepithelial layer of cells, while I use it, on the authority of Dr. Klein, exclusively to mean the cells within the stomata; and while there can be no doubt that the subepithelial arrangements are altogether different in the ovarian cystoma from those of the peritoneum, because the structures are in themselves altogether different, the subepithelial arrangements of an ovarian tumor are precisely those of the ovary.

Underneath the epithelial layer which I have just described there is a layer of fibrous stroma of varying thickness, and having a variety of structures in it. The stroma is stated by different authors to be capable of subdivision into a varying number of layers—from two to six—the only point of agreement between them being that the divisions are most marked at the point of implantation of the tumor. Thus, De Sinéty and Melassez say:

"In the neighborhood of the point of implantation may thus be obtained three principal layers: one external, in connection with the peritoneal cavity; one internal, in connection with the cystic cavity; and one medium, interposed to the two preceding. The external and internal layers have the aspect of fibrous membranes, while the medium layer has rather the aspect of loose

cellular tissue. It is in this latter layer that the large vessels of the pedicle spread themselves out. Departing from the base of the tumor, the medium layer becomes thinner, is no longer isolable, and the cystic wall then appears to be formed only of two fibrous layers. Still farther toward the summit of the tumor the cystic walls are thinner, and can no longer be dissociated into several layers, except by the aid of a most artificial dissection. They are no longer anything but a fibrous membrane, homogeneous in its whole thickness. These transformations are easily explained. They are due to the medium layer of loose cellular tissue becoming thinner and thinner, and finally disappearing, and to the internal and external fibrous layers adhering together and becoming intimately blended."

Concerning these various statements I would only say that a skilful dissector could manage still further to subdivide them, and the results obtained in this dissection would, of course, be very materially influenced by the number of diseased follicles in the neighborhood where he was working.

The microscopic structure of this middle layer varies very much with the age of the tumor. In an old example with thickened and hardened walls, more particularly if it has been frequently tapped, the elements are almost entirely fibrous, with here and there a few indications of nuclear arrangements of the proper ovarian stroma, the almond-shaped nuclei being often so altered and elongated that they are taken for the rod-shaped nuclei of unstriped muscular fibre. I have, however, never been able to satisfy myself in a single instance of the presence of muscular fibres in the walls of an ovarian cystoma; while, on the other hand, in the parovarian cyst it is an almost uniform experience to find muscular fibre largely entering into its constitution. Dr. Grailey Hewit has described an ovarian tumor with muscular envelope, but on reading the account I had no doubt whatever that what he found was a parovarian and not an ovarian cystoma. I have myself seen a mass of muscular fibre in an example of the former almost half an inch thick.

De Sinéty and Melassez, however, speak of having found a great abundance of unstriped muscular fibre in the walls of an ovarian cystoma, an observation the results of which are open to various explanations. They speak as a conclusion to be drawn from their researches, that the employment of ergot of rye may have the result of arresting the growth of ovarian cystoma by reason of its power over involuntary muscular fibre, and they actually quote a case in which hypodermic injections of ergotin are supposed to have cured one. I cannot, however, assent to anything which would involve the application of therapeutic re-

sults for the establishment of microscopical or pathological investigations. In the words of Dr. Mathews Duncan I may say that "we know of not one single case of cure of an ovarian cystoma by any other proceeding than that of the operation of Ephraim McDowell."

In the wall of a cyst of recent growth we constantly find the remains of normal Graafian follicle, to which there had not extended the mysterious influence which directs cystic development; but in an old tumor these are not readily found, for they have either already developed into cysts, or the advancing sclerosis has altogether destroyed their characters. At its base an ovarian tumor may easily be separated from its peritoneal capsule, just as a normal ovary may be, and upon this fact is based the ingenious treatment of sessile tumors by enucleation, originally suggested by Dr. Miner.

Among the conclusions made by De Sinéty and Melassez is one to the effect that a large number of ovarian cystomata have their origin in the tubes of Pflüger. Now, these tubules have been the cause of a great deal of discussion, and I, for one, am perfectly satisfied that they have received an amount of attention, and have been elevated into an importance they do not deserve. In Figures 7 and 9 very fair representations of these tubes are given, after Balfour. They are confined entirely to the hilum of the organ, being, as I have said, survivals of the Malpighian tubes, and therefore no part of the true structure of the ovary. I have never yet seen them lined with epithelium, and therefore I do not believe they are capable of undergoing cystic development, for without epithelium I do not think any such process could occur. In all probability they have some kind of epithelium in the early stages of their existence, but if they ever had any at all they lose it in more mature growth. It is possible that in occasional instances they may retain it, and then be developed, just as the tubules of the parovarium; but I have never seen any case in which I had reason to believe these tubes were the origin of the cysts. If they should give rise to a cystic tumor, one would suspect it ought to be of a unilocular character, and that its walls would not possess the features which are always characteristic of an adenoid cystoma.

In the *Archives für Gynekologie*, 1870, Waldeyer confirms this view in a remarkable sentence: "My researches have demonstrated to me that the opinion of Foster and Rindfleisch, to the effect that cystomata originate from the connective elements of ovarian stroma, is not admissible;" and with this opinion I entirely agree. It will be seen, therefore, that, with the exception of the possible occurrence of an occasional unilocular cyst

arising from the tubules of Pflüger, my belief is that all ovarian cystomata have their origin in follicular dropsy.

I have failed to find any description of a cartilaginous growth of the ovary apart from cystic alteration, but I have twice found plates of cartilage in the walls of ovarian cysts, and in neither of these tumors were there any other structures which might place them in the category of dermoid cysts. The cartilage was composed of large cells with very little fibrous matrix; in fact, it was hyaline cartilage, identical with what I have seen repeatedly in the testicle. There is, of course, no good reason why enchondromatous tumors should not be met with in the ovary, just as they are in the testicle; but in the latter organ they occur independently of cystic degeneration, while I am not aware that they ever have done so in the ovary.

Fibromatous tumors of the ovary must be very rare, for I have only met with three cases, and one which was clearly malignant. Growth of the fibrous stroma of the ovary, so as to form a large abdominal tumor requiring removal, has not yet been described, so far as I have been able to discover; and under any circumstances the condition is a rare one, for Peaslee has collected only seven cases, including two which he had seen himself, and Atlee describes another which probably was of this nature, though unfortunately no microscopic examination of it has been recorded. I think that if I had a similar opportunity now of examining such a preparation, I should be able to give a much better account of it, for I suspect that the reason of its malignancy would be capable of explanation by reversion to an immature form of growth of the cells of the ovarian stroma, analogous to that of the cystic epithelium.

The patient in whom occurred the first of the tumors I am about to describe was forty-four years of age, was very stout, had borne six children, and had been failing in health, owing to the increasing size of her abdomen, for about two years. She was sent to me by Dr. Vinrace in July, 1873, when I found the abdomen occupied by a large quantity of ascitic fluid, in which floated a large and perfectly solid tumor. The abdominal walls were also very œdematous. I tapped the abdomen and punctured the skin repeatedly with a lancet to get quit of the anasarca. This was repeated several times, until it was evident that only the removal of the tumor, which I had diagnosed to be solid ovarian, would permanently benefit the patient. When the abdomen was opened, it was found necessary to extend the incision eight centimetres above the umbilicus, in all nearly twenty-five centimetres, before the tumor could be removed. It had an adhesion to a coil of intestine, and a very extensive adhesion to the great omen-

tum, and it occupied exactly the relations of the left ovary, the other being perfectly healthy. Its pedicle was clamped, and the wound closed in the usual way. The patient died on the fifth day, as was usual when the clamp was employed. The tumor was round, smooth, and of a creamy white color, and it weighed almost nine pounds. When cut into, it had a glistening, white, and trabeculated structure; and it was perfectly solid throughout, there being no indication anywhere of cystic formation. A number of very thin sections were made, and these were treated by various processes, their uniform result being to show that the tumor really was the ovary, and that its overgrowth was limited to the fibrous stroma. The fibres were ranged in bands which crossed in all directions, and treatment by acetic acid showed that a few of these bands, or perhaps I should say a very few, were composed of muscular fibres, an observation which substantiates that of Sangali, quoted by Virchow, made in a similar but much smaller tumor. Looking at my sections of this tumor, made nearly ten years ago by the rough processes in use before I had introduced the method of cutting sections of fresh frozen tissue, I cannot make out very much more now, but I feel nearly satisfied that these fibres are but the result of immature fibre-cell growth running to riot. Throughout the tumor, but chiefly toward its surface, a number of minute cavities were observed, lined by epithelium, and having in one or two instances a large cell with a nucleus, presenting all the appearances of an ovum. The number of these cavities in a less pronounced condition was very large, and I have no doubt they were immature Graafian follicles. I have, within the last few days, removed an exactly similar tumor of smaller size. The pedicle was ligatured, and the patient recovered as usual without any difficulty.

A Microscopical Committee of the Philadelphia County Medical Society reported as follows on a tumor of a similar kind, submitted to them by Dr. Washington L. Atlee, and which he had successfully removed in 1876:

"Thin sections from both the fresh tumors and from hardened preparations exhibited a dense, fibrous-looking stroma, in which the spindle-cells apparently constituted but a small portion, the large majority having, it seemed, been developed into the fully formed fibrous tissue which gave its firm, dense character to the growth. The application of diluted acetic acid brought into view small oval nuclei, arranged with considerable regularity in the section, and which, even under a high power (1,250 diameters), displayed none of the double, triple, and multiple character commonly met with in neoplasmata of the more malignant type.

"Your committee, therefore, conclude that these two ovarian

tumors are the spindle-celled sarcomata of Wagner, Virchow, Rindfleisch, and other late German pathologists, and accurately correspond with those described by Rokitansky as 'Fibrous Cancer,' and by Paget under the name of 'hard cancer with fibrous structure.'

"According to Rokitansky, ovarian growths of this character occur very rarely, and Scanzoni states that these 'fibrous bodies' of the ovary had, to his knowledge, only been proved to exist in four cases, up to the time his work was revised, in 1858."

I have never met with that rare variety of fibroma of the ovary, of which only three instances have been described—two by Rokitansky and one by Klob—and in which small fibrous growths arise from the corpus luteum.

In the "Transactions of the Obstetrical Society of London," for 1874, Dr. Goodhart relates a case of fibroma of the ovary, probably of this kind, in a woman aged forty-two, who died from granular disease of the kidneys. She had several fibrous tumors in the uterine wall, etc., as well as one in the ovary, which was about one and one-half inch in diameter. The relations of the tumor were accurately determined. It grew directly from the free or epithelial surface of the organ, and had no connection with the broad ligament. On section it was shown to spring from the outer layer of the ovarian stroma—that part characterized as "tunica albuginea" by old writers, and as the condensed external layer of the proper ovarian stroma by Waldeyer.

The case of malignant fibroma was one which I saw in October, 1869, in consultation with Dr. Hollings, of Wakefield. She had a large oval tumor, solid, hard, and smooth, moving freely in the abdomen, centrally situated, with a similar smaller one to the right side. The larger tumor reached about two inches above the umbilicus, could be felt high in the pelvis, and had an attachment to the uterus. I diagnosed it as a case of solid cancer of the ovary, and declined to operate. The abdomen was free from ascites or other complication. I saw her a second time in a few weeks, and found that the tumors had both increased in size, and then, having made myself more familiar with the subject, I gave it as my opinion that it was an instance of the rarest of all forms of cancer—the fibroid. On December 5th I found her with symptoms of peritonitis, and a considerable effusion of ascitic fluid. I tapped her, to relieve the breathing, and found a large, soft, semi-fluctuating mass extending from near the xiphoid cartilage to within three inches of the pubis, masking the outlines of the tumors. This I recognized as possibly a fungoid growth of the omentum. I tapped her again on the 9th, and she died next day. Twenty-four hours after death I examined the body,

and, on opening the abdomen, I found adhesions everywhere to the large, deep-colored fungous mass which I had correctly regarded as growing from the omentum. It was adherent behind to the intestines and to the tumors. The larger of these latter was found to be perfectly loose, save from the omental mass and the right corner of the uterus, to which it was attached by a short, thick pedicle; in fact, it was the right ovary, as no other trace of the gland could be found. The smaller tumor was similarly the left ovary, and a still smaller tumor seemed to grow from the same pedicle. Scattered over the surface of the peritoneum were patches very similar to those previously described as papillary cancer of the peritoneum. These patches were also found on the surfaces of the tumors, and were stripped easily off with their investing epithelial coverings. In the right or largest tumor were a few cavities containing fluid, and also some curious cretification at the base and in the pedicle. It weighed probably twelve or thirteen pounds, so that I could not remove it conveniently for preservation. I removed and carefully examined the smaller tumors, some peritoneum, with specimens of the patches and a piece of the omental fungus.

In the piece of omentum nothing but blood-detritus, a few scant fibres, and numbers of irregular cells were found, with immense numbers of free nuclei, or what seemed to be such. The nodules on the peritoneum showed all the characters of cancerous tissue, being composed of large, irregularly shaped and irregularly sized cells, containing variously shaped nuclei in varying numbers. There seemed to be no fibrous tissue in them at all, and their elements readily separated by gentle pressure between the cover and the glass slide. A careful section showed the epithelium of the free surface of the peritoneal layer to be undergoing interesting changes. The cells of the upper layer were normal, but at two or three layers' depth they were seen to be larger, more irregular, and the number of nuclei increased, the latter fact being most clearly displayed on the addition of acetic acid.

The ovarian tumors removed were ovoid, smooth, and glistening, and here and there the surfaces were marked with the peculiar patches above described. Together, they weighed nearly three pounds. When cut into, no juice exuded from them, and the scant moisture scraped from the cut surface showed no cells. The tissue was pearly white and very tough. Teasing with needles did not give any satisfactory results, and a great many sections had to be made before one thin enough for examination was obtained. I then found that the texture was purely fibrous, there being nowhere, under the epithelial

layers, any cells discoverable. The fibres were extremely fine, closely and regularly packed, without any appearance of undulation or interweaving, but seemed to lie parallel, with only faint curvings in their general direction. They were readily stained by carmine. Acetic acid showed no nuclei, and did not influence the fibres beyond a slight clearing of the section. The tumors differed quite from those already described as fibroma, which do not seem to be malignant.

It will be found that this description differs in some important particulars from that given by Sir James Paget of similar structures, especially in the absence of nuclei; but the rarity of opportunities for the examination of such peculiar growths stands much in the way of their proper investigation; and now we have much better methods of examining such growths, so that future experience will give far more precise results.[1]

Among all the tumors I have examined I have never seen any of the so-called osteomata; indeed it is greatly to be doubted if any true osteoma has ever been found in the ovary, except as part of a dermoid cyst. All the others, of which I have seen descriptions, are evidently only instances of cretification.

This form of change has been very carefully studied by De Sinéty and Melassez, and they summarize their conclusions as follows:

It is presented in two forms, of which the first and more simple consists of a deposit of small calcareous grains in the connective substance. Occasionally these appear deposited in the interstices of two lamellæ or two connective fasciculi, in the place occupied by the connective cells; but that is only an appearance due to the calcification having commenced upon one of the faces of the lamella or connective fasciculus. These little grains of calcification are sometimes isolated, and sometimes united in a band. This form rarely exists alone; it is habitually met with associated with the second, of which it appears to be the origin.

The second form consists of more or less extensive patches, disposed parallel to the cystic surface. As we have already said, there usually exists a layer of connective tissue between them and the cystic cavity. Their internal or cystic surface is generally smooth, and forms, upon sections, a rectilinear line; while the external or deep face, and especially the extremities, are irregular, and present excrescences or depressions. The excrescences have almost always a rounded, semi-spherical form, as if due to the addition of little calcareous grains to the princi-

[1] Wilson Fox has noticed these degenerations of vegetations (loc. cit., p. 268).

pal mass. The depressions are in the form of cupolæ, rather like those presented by bones attacked with atrophic osteitis; they are sometimes situated in the interior of the calcareous patch, and there form very irregular anfractuosities. In the substance of the patch may be discovered, further, the disposition of the lamellæ and connective fasciculi, fine striæ indicating the place occupied by the connective cells now destroyed.

Besides the formation of cystoma of the ovary from simple distention of the Graafian follicle by an excess of its proper fluid, Rokitansky and Arthur Farre long since drew attention to the formation of ovarian cysts by hemorrhage into the cavity of the follicle. In his classic article on the ovary in the "Encyclopedia of Anatomy and Physiology," Dr. Farre figures and describes such a cyst as having "its cavity filled with loose flocculi, of a dark chocolate color, consisting of decomposed blood-clot mixed with patches of membrana granulosa. The walls of the follicle were not yellow, and contained no oil-globules, and they were slightly thicker than those of the healthy follicle." Their component tissues were precisely those he describes as characterizing the ovisac in its normal condition, and the main bulk of its structure was made up of granules and embryonic fibres, intermixed with a few developed fibres of ordinary white fibrous tissue. He considers it to constitute one of the early stages of these enormous growths, forming an ordinary cystoma of the ovary.

Rokitansky regards these cysts as being due to a cystic degeneration of the corpus luteum. He says they may exceed the size of a walnut, that their interior surface is wrinkled, and their exterior limit is easily recognized.

Cruveilhier speaks of ovarian hæmatic cysts as a consequence of an apoplexy of the ovary, and as a probable consequence of an exaggeration of the small clot of blood produced in the ovary at the rupture of an ovisac. These he says are often transformed into perfectly organized serous cysts, of which the origin is sometimes of very doubtful determination, though it may be revealed by fibrous concretions within them or by the orange yellow coloration of their walls. The largest he had seen was in the ovary of an old woman, in which the cyst was filled with a dark brown matter having the consistence and color of water chocolate. The walls of the cyst were greatly injected and were patched with red, being infiltrated with blood in their thickness, but otherwise they presented the usual appearances of the structure of the tunic of the ovisac.

De Sinéty and Melassez have made similar observations, and I can substantiate all that these observers have noticed in

connection with the production of small-sized cysts from what may be described as an apoplexy of the ovisac. I have no doubt at all that they are the result of excessive hemorrhage into the cavity, occurring either at the time of its rupture for the discharge of the ovum, or perhaps occurring when the ovum was not discharged as it ought to have been. In one case I removed a tumor of this kind on account of persistent and intractable uterine hemorrhage. Before the operation, which was performed in 1873, I regarded the patient as suffering from hemorrhage due to a uterine myoma, but when I opened the abdomen I found it was a true ovarian tumor, consisting of one large cyst, and at its base a few small cavities. The large cyst contained a dark purple material of the consistence of putty, which was evidently the remains of blood-clot, the serum of which had been absorbed; and this material dried into a brittle substance, exactly as blood-clot does. The removal of the tumor arrested the uterine hemorrhage completely, and the patient made an excellent and permanent recovery. Unfortunately at that time I was not so conversant with the facts of ovarian pathology as I now am, and the tumor was not properly examined, and the only note which I possess of its appearance, beyond what I have already described, is that on its inner surface there was a large patch having an appearance as if it were ulcerated, and that this spot probably was the source of the hemorrhage. Upon this ulcerated surface there was no appearance of an epithelial layer. Before any conclusion of value can be given from these cases some similar experiences would have to be more carefully investigated, but I am strongly of the belief that this tumor was an example of Rokitansky's hæmatic cyst, which had reached a size and an importance which those structures do not usually possess.

I think that Arthur Farre's view is probably correct, and that when follicular dropsy begins as an apoplexy this character is maintained for only a short time in its history, and that its after-course is that of an ordinary cystoma. It is quite likely, however, that an occasional instance will be met with where the distention is due to recurrent hemorrhage, and of this I think my case was probably an example. I cannot pretend, however, to explain why this should be accompanied by the terrible uterine hemorrhage from which my patient suffered; still less can I see why the removal of the tumor should completely arrest this symptom.

The tubules of which the parovarium is made up, frequently contain a perceptible amount of fluid, and I have repeatedly seen them accidentally in post-mortem examinations, distended to

the size of beans or filbert-nuts, and have disregarded them as "Wolffian sacs," of no pathological importance. Some years ago I had occasion to make a medico-legal examination of the body of a woman far advanced in life, and I found in her left broad ligament a cyst as large as an orange, filled with clear, limpid serum. It was pressing upward and backward out of the pelvis, the ovary being at its lower and anterior aspect, and the Fallopian tube arched over its anterior surface. On the side next the uterus two smaller cysts were lying close to it, and, nearer still, a very minute sac, which was evidently, from its mere shape, a distended parovarian tubule. The ovary was white, puckered, and shrivelled, and had not a continuous relation to any of the cysts, though it touched the largest at its hilum. The Fallopian tube was normal, and had no other relation to the tumors than slight connection by loose areolar tissue. There was in my mind no doubt that this was a pathological indication of value; for in an ovariotomy that I had performed not long before, I was struck by the fact that the ovary was perfectly healthy and separated from the tumor, as was also the tube, by a mesovarium of some extent; in fact, I did not do ovariotomy at all in the removal of the tumor; for, in passing the chain of the écraseur round its base, I did not include either the tube or the ovary, and they were both returned into the abdominal cavity. In the records of ovariotomies performed these cases have, up till now, always been stated as ovariotomies, and the ovary and tube associated with the tumor have been removed with it. Both the record and the removal of the ovary are mistaken. The operation is not an ovariotomy at all, and nine times out of ten both ovary and tube might easily be separated from the tube and left, and this practice I now always try to follow. It is very curious that those who are crying out most loudly against the unnecessary removal of ovaries have been in the habit of pursuing this practice in the case of parovarian tumors, without compunction.

The result of all my observations has been that in every truly unilocular tumor I have found the ovary unaffected, though on several occasions I have seen it stretched over the cyst-wall. I have three or four times observed the ovary separated from the cyst by a more or less distinct mesovarium, and on one occasion I found in that fold some unaffected parovarian tubules, in the case of a lady, a patient of Mr. Hall-Wright, from whom I removed a large unilocular cyst about six years ago. In another instance the healthy ovary was left at least an inch below the clamp; and in a third the ovary and tube were found glued on to the cyst, but forming no part of it. In this cyst the walls were extremely thick, and contained large quantities of involun-

tary muscular fibre—a fact which I do not think militates against my view that it was of parovarian origin; for nucleated muscular fibre-cells exist in the broad ligament to some considerable extent, and myomatous tumors are found occasionally within its folds.

The case to which I have alluded as presenting a tumor with many cysts, but which ought to be placed under the same category as the unilocular cysts, occurred in the person of a lady aged sixty-six. She was a widow, having been married forty-three years before the tumor appeared. The menses had ceased for nearly twenty years, and her youngest child was twenty-five years of age. There was every reason to believe, therefore, that the condition of the cell-growth of her ovaries would be one of very low activity. The tumor was first discovered about five years before I saw her, and had grown slowly for four years and a half, but with extreme rapidity for six months. The abdominal parietes were very thin, and the percussion-wave was communicated with extreme and uniform rapidity in every direction. I diagnosed, from my former experience, that it was a unilocular Wolffian cyst, and that the ovary would, in all probability, be found uninvolved. I was right about the ovary, for that was found, along with the tube, almost undisturbed, and not in any way involved in the tumor, the latter having apparently escaped from between them backward and upward. I had made a mistake, however, about the tumor being unilocular, for it was composed of five or six sacs. The walls of these were very peculiar, in being of uniform thickness, or rather thinness, for they were like tissue-paper, and had no thickening toward the base of the tumor, as is always the case in the multicystic adenoid or multifollicular tumor of the ovary. My belief is that this tumor was a specimen of dropsy of a number of the parovarian tubules; for, if one alone may become dropsical, there can be no reason why a number should not be so coincidently. My opinion has been greatly strengthened, however, by a re-examination of the tumor for the special investigation of one point drawn attention to by Dr. Bantock; that is, the possibility of separating the outer or peritoneal coat of the cyst. This can readily be done toward its base for a short distance up from the ovary, discovering the fact that the gland and its duct can be stripped off the tumor without damaging its wall. The rapid growth during the later periods of its existence, however, seems to have so stretched the walls, that, beyond two or three inches from its base, the peritoneal layer cannot be separated from the cyst-wall proper. I have quite satisfied myself that this case is really one of multilocular parovarian tumor; and I am confirmed in this view when I find

that Dr. Bantock refers to a case of Mr. Spencer Wells's, which was recognized as one of bilocular parovarian cyst.

Considering this, it is a point for investigation whether or not the curious little pedunculated cyst, representing the terminal bulb of the Wolffian tube, and generally known as the organ of Rosenmüller, may not sometimes form a unilocular tumor of morbid size, and be removed as an ovarian growth. In one case I have removed it during an ovariotomy on account of increase in its size. All these rudimental structures are lined with epithelium, and may, therefore, conduct themselves as other structures so provided are known to do.

The diagnosis of parovarian cysts is generally very easy to the practised hand, for they give a uniform and very rapid wave of fluctuation in every diameter of the tumor. Their shape is usually globular, but they do not project into the pelvis, as is very often the case with the minor cysts of an ovarian tumor. They very rarely give rise to symptoms of any kind, and still more rarely to any symptoms of urgency. They sometimes grow very rapidly. I removed a very large parovarian cyst some years ago from a patient under the care of Dr. Campbell, of Stourbridge, where the fact was fully ascertained that the tumor grew in less than six weeks. It may happen, however, that all the conditions of a parovarian cyst may be very closely imitated by an ovarian tumor, and they are absolutely mimicked by two rare forms of cyst to be afterward described, one of which is a development of the occluded tube of the urachus, and the other I believe to be developed from a wandering ovum. The fluid removed from these cysts is often limpid, of low specific gravity, containing little albumen. This, however, is by no means always the case, for I have removed many parovarian cysts which contained thick, gelatinous, grumous, or bloody fluid, which mere tapping would never have led us to suppose had been produced in any other cavity than that of an ovarian cystoma. At the meeting of the Medical Society of Strasburg, November 15, 1875, M. Kœberlé read a paper on the diagnosis between ovarian cysts, cysts of the broad ligament, and cysts of the Fallopian tube, based on the chemical examination of the fluid contained in them. He finds that the fluid of ovarian cysts contains some albumen, but a much larger proportion of the variety of albumen called paralbumen, the precipitate of which by nitric acid is soluble in acetic acid. The fluid found in cysts of the Fallopian tube, on the contrary, he says, contain albumen, but no paralbumen, so that the precipitate formed by nitric acid is rather increased by acetic acid. The fluid of cysts of the broad ligament is generally very limpid, containing salines, but no albumen.

Sometimes, however, it contains a small quantity of albumen, and the precipitate formed by nitric acid may be soluble in an excess of that acid. The researches of Schutzenberger with the tannin process for estimating the quantity and kind of albumen have, however, thrown great doubt on these conclusions, and by the same means I have quite satisfied myself that M. Kœberlé's conclusions are not to be accepted. At my request my friend Dr. McMunn, of Wolverhampton, undertook to investigate the possibility of determining the source of fluids by means of the spectroscope. I furnished him with a number of specimens of fluid, the sources of which were absolutely known, but the results of his researches were entirely negative. They are given in detail in his valuable work on "The Spectroscope in Medicine" (London, 1880). There is an impression abroad that these cysts are occasionally cured by tapping, but I am bound to say I have never met with an instance of it. I have tapped many of them, and I have seen them remain quiescent for a time—as long as three years—and then require to be tapped again. In my recent practice I have altogether discontinued tapping, and I invariably remove them, the operation for their removal being simple and easy, and in my hands it has been uniformly successful.

A further point of great importance in recognizing mere ascites from an ovarian or parovarian tumor, is that in the former condition there is generally an appearance in the patient's face of suffering from serious functional disturbance, whereas in the latter the patient often looks in perfect health. Sometimes we find the walls of a parovarian tumor very thin and flaccid, in this way closely resembling the appearance of ascites.

This class of tumors it is which has given rise to a great many different beliefs in connection with the history and treatment of ovarian tumors which Dr. Mathews Duncan has very properly designated delusions. Among these was the belief, originated by M. Boinet, that ovarian tumors had been cured by tapping, by injection with iodine, by what Mr. Baker Brown called a formation of a false oviduct by the insertion of setons, and by a variety of other more or less barbarous and unscientific proceedings.

The walls of these cysts are nearly always very thin, consisting of little more than a thin basement-membrane and a lining of columnar epithelium. This epithelium undoubtedly undergoes alterations such as I have described as occurring in ovarian cysts, for I have seen all the appearances on the lining of a parovarian cyst that I have seen in an ovarian tumor. They undergo malignant degeneration, they suppurate and become gangrenous just as ovarian tumors do. Sometimes the basement-

membrane of their walls, which always contains some muscular fibre, becomes enormously thickened, and I have removed a parovarian cyst with walls more than half an inch thick, the greater part of which was composed of fusiform muscular cells.

They are therefore not matters to trifle with. Their early removal is always simple and safe. They should never be tampered with by tapping, but ought to be removed by abdominal section in their early stages, just as should ovarian tumors. Sometimes they burst and seem to disappear spontaneously, and this again has given rise to the statement that this result is obtained occasionally for ovarian tumors. When this fortunate accident takes place early in their history it will probably do no harm, but if it occurs during the advanced stages it is just as likely to result in cancerous implantation of the peritoneum as if the cyst had been ovarian. A few months ago I removed a large parovarian cyst which had several times been tapped, and which had ruptured into the abdominal cavity. On removing it I found the peritoneal surface studded with papilloma, of which the patient has since died.

I have now to speak of that variety of cystoma to which I have frequently referred as "Rokitansky's tumor," or the multiple cystoma. I am quite aware that both of these names are open to objection, but I have failed to find any other more appropriate or descriptive. To Rokitansky is clearly due the credit of having first described the tumor as a special variety of ovarian cystoma, and to Ritchie must be accorded the priority of discovering ova in its cysts; though, as I have already shown, the observation led him into a too hasty generalization. I think that I may claim for myself the position of having first arranged the various contributions into their proper positions, and from two specimens I can now confirm and extend the observations of the two authors I have cited.

These tumors are always double, no case having yet been described as having occurred on one side only. They are always of very slow growth; their cysts are uniformly small, rarely reaching the size of an orange, and generally being little bigger than grapes. The tumors are never large, and it is only the fact that both ovaries are always affected that makes them objects of surgical interference. The contents of the cysts are invariably limpid, and the ovum may nearly always be found, and in these two respects, as well in the immense number of the cysts, the tumors differ absolutely from ordinary cystoma.

The first case occurred in the person of a hospital patient from whom I removed both ovaries. Both tumors were multilocular, and had one or two major with innumerable minor cysts, gradu-

ating down to the most minute size. The fluid contents of all were limpid, and what was evacuated from three or four cysts at the time of the operation, together with the solid masses of both tumors, did not weigh quite ten pounds. The right tumor seemed to be about one-fourth larger than the left, so that they were probably four and six pounds in weight respectively—small-sized tumors. Both pedicles were included in one clamp, and the patient made an uninterrupted recovery.

After removal, the most careful examination of the tumors failed to discover any remnant of the ovaries outside them, nor did I find any trace of either of the Fallopian tubes, as I had not removed them. The tumors were pearly white and glistening; but the thin parts of the major cysts had a peculiar transparency that I had never noticed in any other tumors before, and columnar bands stood out here and there in relief on the walls. I may say that the tumors had been of extremely slow growth; for I had had the patient under notice for nearly a year before the operation, and had not discovered any increase in the size of the tumors, though they had been in existence probably five or six years.

The interiors of the large cysts were lined with regular columnar epithelium, and the wall seemed composed of fibrous tissue with some nucleated almond-shaped cells. The smaller cysts were densely packed together, and at some places, where they were of a uniform size, the tumors had much the appearance of huge white raspberries. I was struck with the resemblance the tumors presented to what I recollected of those in which Rokitansky and Ritchie had found ova, and I at once turned to Dr. Ritchie's admirable monograph, and found that the tumors answered the descriptions completely. I therefore examined the contents of as large a number of the cysts as I could, and in every one—I think without exception—I found more or less distinct evidence of an ovum.

It will serve my purpose best to quote at length from Dr. Ritchie's book—for our experiences are almost identical—and he also gives the observations of Rokitansky.

"In the first volume of the *Wochenblatt der Zeitschrift der K. K. Gesellschaft der Aerzte zu Wien*, Rokitansky describes the appearances observed in the post-mortem examination of a woman, twenty-six years of age, who died with diseased ovaries. Both ovaries were affected. The tumor on the right side was as large as a child's head, that on the left as large as a man's fist. Both ovaries were composed of a number of cysts as large as a cherry, which for the most part lay closely packed together, here and there had become flattened by mutual compression, and oc-

casionally even projected into each other. The surfaces of the tumors were thus slightly lobulated, and between the protuberances were seen, at intervals, cysts as large as a barleycorn, a pea, or a bean. These latter cysts, on being punctured, gave exit to a greenish-colored fluid containing membranous flocculi, and in all of them the ovum was found. In each of them, however, the ovum was softened, very dull-colored, easily disintegrated. The zona pellucida had for the most part lost its sharp contour, and, except in one case, no germinal vesicle was discoverable.

"As far as I am aware, this observation of Rokitansky was never publicly confirmed until July, 1864, when the reporter to the *Medical Times and Gazette* of four cases of ovariotomy, performed by Mr. Spencer Wells in the Samaritan Hospital, mentioned that, in two of the tumors removed, Dr. Webb and myself had been fortunate enough to discover many ova.

"The patient from whom the tumors in question were removed was fifty-four years of age, and had been for some time suffering from double ovarian disease. The tumors were easily extirpated, and the patient recovered. Each tumor was of the size of the head of a child four years of age. Each contained several large central cavities, and a number of smaller ones in the wall of the central cavity, the wall itself never exceeding one inch in thickness. In the *Medical Times and Gazette* for August 6, 1864, Mr. Spencer Wells wrote as follows:

"'The two tumors in question were examined directly after their removal by Dr. Ritchie, who pointed out to me, in each of them, a number of small cysts, which were evidently enlarged Graafian follicles. Knowing the great and long familiarity which Dr. Woodham Webb has had with the ova of various species of animals since his researches in conjunction with Barry, I asked him to examine some of the cysts, in order to ascertain whether they did or did not contain ova, knowing that on this point no higher authority could be appealed to.

"'As one friend has suggested that we may have mistaken a blood-corpuscle for an ovum, there was evidently some reason for my caution; but I trust that the following note from Dr. Webb will set all such doubts at rest:

"' "Both the tumors you sent me, after their removal from a woman fifty-four years old, were growths in excess of true ovarian structure. The multilocular character was produced by clusters of ovisacs of various sizes. Ova, with the other natural contents, were to be found in all the small sacs. The fibrous coats of the larger sacs were thickened, and had many other secondary sacs developed in them. The interior was lined with

epithelium, which in some instances had, by parthenogenetic enlargement and successive budding of the cells, given rise to bunches of grape-like growths, repeated generations of imperfect ova."

" 'The whole, then, was nothing more than a reproduction in the human subject of conditions which are natural in some of the lower creatures. I suppose the description, in your orthodox pathological terms, would be, "hypertrophy of the ovaries, with arrested development of the contents." '

"This letter, coming from a gentleman of Dr. Webb's known experience, is of great interest. Dr. Webb evidently inclines to the belief that the ovum is only an altered epithelium-cell. He also seems to believe that the grape-like growths—those described farther down as dendritic growths—are repeated generations of imperfect ova."

Dr. Ritchie's unfortunate death, and my want of acquaintance at that time with Dr. Webb, hindered me from becoming acquainted with their method of manipulation; but that which I devised for myself answered my purpose completely. It consists in slitting open the cyst freely with a cataract-knife over a conical glass, collecting the whole contents, and afterward syringing out the cavity of the cyst gently with a solution of sulphate of magnesia in distilled water of a density something near that of the cyst contents. The fluid with which the cyst is syringed out is allowed also to fall into the vessel, and the whole to stand for a few hours, at the end of which time a little flocculent sediment will have collected at the bottom of the vessel. This is to be carefully lifted by a pipette, deposited in a clean watch-glass, and the ovum searched for under the microscope. What I found in every case I could not, of course, assert was an ovum; but having found one or two specimens about which there could be no doubt, and in every case something that was more or less like one, I am perfectly satisfied that in those tumors every cyst was a dilated ovisac. The smaller the sac, the more perfect the ovum seemed to be, and consequently I assumed that these were the more recent growths. Not only were these dilated ovisacs in the periphery of the tumors, but they were found throughout its substance. In fact, it seemed to me as if, for a long period of her ovarian history, the ova had been garnered up in cysts instead of being shed in the usual manner. Her youngest child was six years old, and if we were to assume that an ovum is shed from each ovary monthly, then we should get the number of ova so retained as about one hundred and fifty. The number of cysts was, however, much greater, probably two or three times as many; so that we may choose between two explanations—either

that the tumors had existed before her last pregnancy, or that more than two ova are shed in the month. My own belief is, that both these suppositions are correct; for, when speaking of ovulation at the beginning of the essay, I gave reasons for my belief that ovulation and menstruation had only a connection of concurrence. I am of opinion that ovulation takes place far more frequently than menstruation does.

It is a somewhat singular fact that the observation of Rokitansky has not been confirmed, as far as I have yet seen, by any others than Ritchie and Webb, and by my own cases. Still more curious is it that all four cases are almost identical, presenting small multilocular tumors of slow growth, and that in all four cases both ovaries were affected. This inclines me to believe that in these cases we have to deal with a special kind of ovarian tumor, occurring rarely and differing from the ordinary adenoid growth. Whether this be so or not, further experience alone can show. Dr. Ritchie says, immediately following the quotation I have given above, that he subsequently succeeded in finding ova in some of the loculi of a large number of ovarian cysts, but never in a loculus larger than a cherry, nor in one that had jelly-like contents. This observation I cannot confirm; for, though I have made many searches in the endogenous and subjacent secondary cysts of ordinary polycystic tumors, I have never found an ovum or anything resembling one. Perhaps my method is defective. Dr. Ritchie further says that, when no ovum is to be found, a single minor cyst is to be seen, embedded in one part of the wall, and he has thought himself justified in concluding that this appearance represents dropsy of the blastodermic vesicle. I have certainly seen such vesicles in the walls of small cysts, but I have as often found them multiple as single, and I have never seen reason to interpret them as Dr. Ritchie has done.

The second case of the occurrence of this peculiar form of tumor was that of a patient sent to me by Dr. McVeagh of Coventry in December last. The tumor had been noticed for two years, and had been increasing somewhat rapidly for seven months. I had no doubt as to the diagnosis of an ovarian tumor, but there was something in the examination that made me fear it was malignant. The patient was thin and looked very ill, but yet had no particular symptoms, and the distention of the abdomen was not great—that is, the tumor was not very large. I operated on December 7th, and when I opened the abdomen I found a large mass of glistening cysts of a pearly white lustre, varying in size from a pea to a small orange. The omentum was inextricably mixed up with them, so that at first sight

I was under the impression I had made a mistake and had to deal with a case of hydatids of the peritoneum. I found, however, that the mass was removable and that the cysts were pediculated upon a common stem. To get the mass out of the abdomen was a matter of no small difficulty, and to accomplish it I had to make an unusually large incision, nearly seven inches long. Then I found that the stalk of the tumor reached toward the left cornu of the uterus, and that as the left ovary could nowhere be found the tumor must represent it. I tied the pedicle and removed the mass; I then looked for the right ovary, but could nowhere find it, but loose in the abdomen I found a smaller mass of cysts which I think must have been the missing gland. How it came to be separated from its stalk I do not know. During the operation several of the cysts had become detached from the common stem by rupture of the slender petioles, and I had to hunt carefully about among the intestines to make sure that I left none of them. The patient made an excellent recovery, and is now in perfect health.

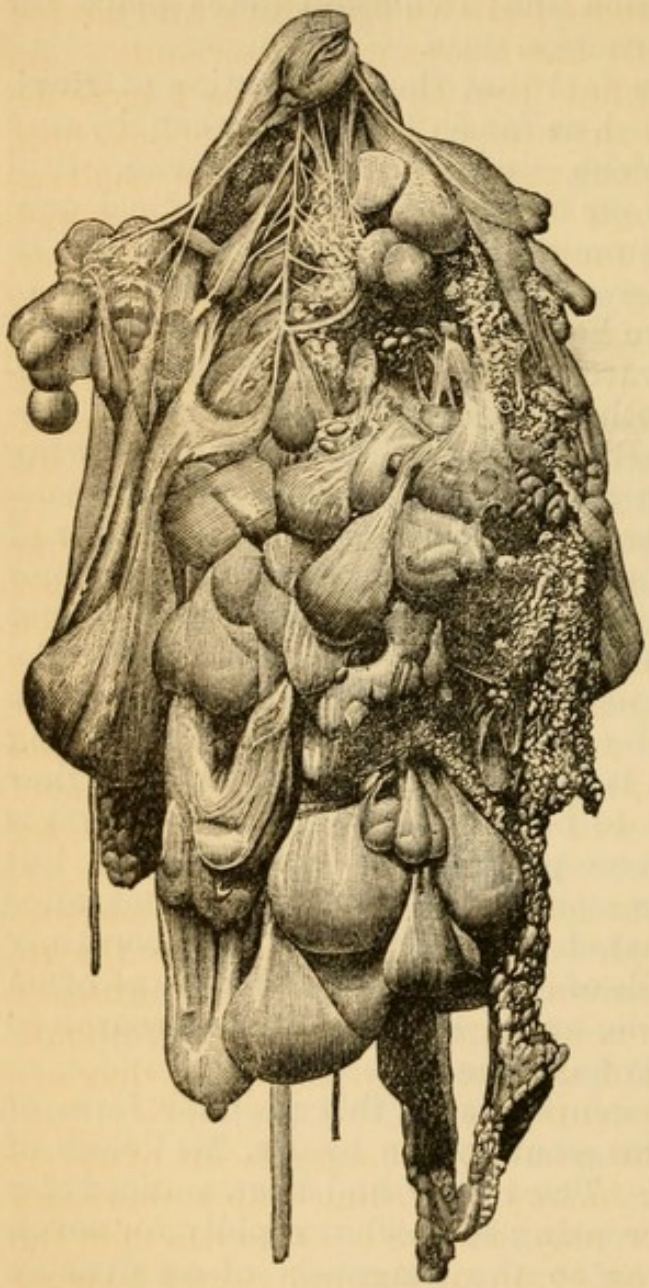


FIG. 28.—Rokitansky's Tumor, each cyst containing an ovum; from a photograph by Thrupp, about one-third of actual size. Preparation in Hunterian Museum of Royal College of Surgeons.

The adjoining illustration, which is taken from a photograph, will give a better idea of the appearance of the tumor than any description. On the right side and hanging down from the mass may be seen the omentum, nearly the whole of which was removed with the tumor, and through the meshes of which many of the cysts seem to have grown. It is adherent

here and there, but its association with the tumor seems to be more that it is involved with the growth in a sort of mesh-work, than as having the ordinary form of adhesion. Everywhere the curious small cysts with their slender pedicles may be seen hanging like grapes upon their stalks, and at the upper part of the tumor may be seen some of these pedicles branching and having cysts attached to them exactly like leaves upon their stalks. In this respect it differs somewhat from the first case I have described, but at other parts of the tumor the resemblance is close, and microscopical investigations gave exactly the same results as those above described. Save in the very largest cavities which had been emptied in order to facilitate the removal of the tumor, I found a normally columnar epithelium, and normal or nearly normal ova. In the small cysts the appearances were precisely those seen in Graafian follicles. The tumor is now in the museum of the College of Surgeons, and I trust that any one meeting with a similar example will secure for it a most careful examination.

Partaking of the nature of the cysts last described, to some extent and in some way yet inexplicable, are the mysterious productions known as dermoid cysts. The term "dermoid" is not a good one, for it by no means gives expression to a constant character of these tumors. Sometimes there is no trace of any epithelial products at all; while we find bones, muscle, and brain-substance even, according to Beneke, in some of them. The name, however, is hallowed by tradition, and it is not easy to coin a better one.

I have already explained at some length, and I hope successfully, my view that Rokitansky's tumor is produced by the retention of the ova in the Graafian follicles and the distention of their cavities by a continuous secretion of the liquor folliculi. My theory of the production of dermoid tumors is that they are the result of a growth of the ovum itself.

That these tumors are the result of change in an ovum is about the only part of their history regarding which there can be any certainty. But there may be doubt as to whether the abnormality takes origin in an ovum of the individual bearing the tumor, or in the ovum from which she herself was developed; in other words, whether the tumors are abnormally developed ova or are due to inclusion. That they have any origin in impregnation, we may at once dismiss as excluded from serious consideration, since they have been frequently found in newly born children, and their most common seat is in the ovaries of young women, chiefly, according to Mr. Spencer Wells, of fair complexion.

The question of their origin, then, lies between the hypothesis of an effort on the part of some over-active ovum in the direction of parthenogenesis, which has been based by Dr. Ritchie on Blumenbach's less scientific and more scholastic expression of "excess of formative nisus," and the equally hypothetical process of inclusion. As far as we know anything about inclusion, it follows the usual law of teratology, that any attached individual, whether developed or blighted, is symmetrically connected. Thus the Siamese Twins and the Millie-Christie monstrosity have the attachment in similar and identical structures, the one to the other (see Vrolik, Von Baer, etc.). I have seen nowhere on record that any fetal remains have been found attached to an ovary, or situated in an ovary in any way which could find them a classification under this law. The tissues met with are always rudimental, and such as, while they are the product of the ovum after conception, have no anatomical analogy whatever to the tissues of the ovary. I am disposed, therefore, to set aside entirely the view of their origin by inclusion, as the ovary is about the most unlikely structure in the embryo for such a process; and, if they had their origin in such a way, we ought to find dermoid cysts in the testicles of the male quite as often as in the ovaries of women.

There is only left, then, the explanation that dermoid cysts are the result of an altered nutrition of one or more ova; and, if I may lay down a dogma from my own dissections, I should say, of one ovum only. Dermoid cysts are generally unilocular, and, when they are not so, it is not difficult to show, as has been done by Dr. Ritchie, and has been evident in one or two specimens that I have examined, that the secondary cysts are formed by the mother cyst being partitioned off by the growth of ridge-like walls on the inside of the cyst.

The occurrence of cysts having a structure somewhat resembling the dermoid cysts of the ovary in other parts of the body, especially in the neighborhood of the orbit, has led to confusion in the discussion of the origin of the ovarian cysts. In the orbital cysts we have only aberrations of the normal process of the involution of epithelium from which the structures are developed, and there is no mention, as far as I can find, of these extremely small congenital cysts, which never enlarge in after life, having been found to contain anything but purely epithelial products, such as hair, dead epithelial cells, and fat. In dermoid cysts of the ovary, however, the variety of products is so great, as to put all analogy between them and inclusive cysts out of the possibilities. Thus, in one ovarian cyst which I examined under the direction of my friend and teacher, Dr. Grainger

Stewart, many years ago, in the substance of a wall between two loculi, were spread out flat bones which were undoubtedly some of the bones of the skull, and near them could be felt the representatives of the bones of a limb arranged in order. True bone is frequently found in ovarian cysts, and often in those that have no dermoid structures at all.

Sir James Paget refers to a remarkable specimen in the museum of St. George's Hospital, which exhibits a mass of fatty matter and a lock of dark hair, one and a half or two inches long, attached to the inner surface of the dura mater at the torcular Herophili, found in a child two and a half years old, in whom it appeared to be congenital. He adds, in a foot-note, that Dr. John Ogle, who had carefully examined the specimen and described it to the Pathological Society, was of opinion that the cyst was originally of extracranial formation, but that, at an early period of fetal life, before ossification of the occipital bone had taken place, the cerebral membranes and scalp had become adherent, and that, as the development of the bone went on, the outer integument was drawn in by retirement of the cerebral membranes. In this way some of the cutaneous structures had become included within the cranium. He considers that the cyst possesses characters which warrant the above supposition, and he adds that, in a similar manner, cysts within the orbit may extend into the cranial cavity. No such explanation could include the phenomena of the ovarian tumors which contain such structures as teeth, bone, cartilage, striped muscular fibre, brain and nerve tissue, etc. The true solution can be found only in a hypererchetic development of an ovum, a cell which has in it the power of formative origin for all these structures. The process of growth of the ovum after impregnation can be followed only after the assumption, either expressed or unconsciously accepted, of such a hypothesis as is contained in Mr. Darwin's "Pangenesis." The germ contributed by the male contains, we knew only too well from pathological experience, gemmules having certain powers and functions; and we may therefore assume, as indeed we also know, that the female germ contains also such gemmules. It may be that the ovum has in it the origin-buds of certain tissues, and that, under exceptional hypererchetic action, they may go on to the rudimental formation of these tissues without a fusion with the male germ. More careful and accurate description of what is found in dermoid cysts may help to solve this riddle; still better, perhaps, a careful consideration of what tissues are not found in them.

This doctrine of hypererchesis is supported by many facts which have been observed in cases where the changes in the

ovum can be watched outside the body of the parent. Thus Bischoff and Leuckhart have both described partial development of ovules which have been placed beyond the possibility of impregnation. Dr. Moquin-Tandon has, more recently (1875), described analogous facts before the Academie des Sciences, among which he gives details of the process of segmentation in the ova of a frog which had been kept in confinement for four months.

There were first noticed two large vertical fissures in the ovule, followed soon by similar horizontal segmentation, and this process of division proceeded further, but in a less regular manner than usual, the yolk spheres multiplying irregularly, and becoming of unequal size, and it was more rapid than in fecundated eggs which were allowed to develop at the same temperature. Only a small number of the ova presented this evidence of commencing development, for the majority died without any sign of segmentation. Sometimes death occurred after the division into two or four segments, sometimes at a more advanced period, but the ovule never assumed the mulberry appearance. The author considers that this incontestably proves that the ova of vertebrata, not impregnated by spermatozoa, may pass through the earliest stage of development in certain conditions, the exact nature of which is at present unknown. It seems to me that we may take this process to be exactly what occurs in the development of the spore of the fern into the prothallus, and the tendency which these unfertilized ovules have to this primitive and ineffectual development is derived from the continuity of descent. In the insecta the process is carried much further, for Balbiani exhibited some eggs of the silkworm moth to the Société de Biologie (1873), which had been deposited before fecundation could have been effected. A certain number of these eggs remained sterile, but others showed signs of development, though in no instance had the larva escaped from the egg. The number of these developing eggs varied extremely according to the species of moth by which they were deposited. The largest number was met with in those which produce several generations per annum. Among 9,000 eggs of a polyvoltine race, 513 developed spontaneously; while of 50,000 of an annual race, 29 only were fertile. M. Balbiani thinks that this enormous difference is probably due to the feeble vitality of the egg in the annual races, a suggestion which cannot be considered in any way as an explanation; neither can his idea be entertained that the parthenogenetic development is to be accepted as proving the hermaphroditism of the egg, for there is no evidence whatever of such a condition. What has been observed by Balbiani is indeed only an attempt to fulfil the

conditions seen in the aphides, where the cell multiplication in the pseudovaria produce a new individual without any sexual congress. Putting these facts along with others observed by Agassiz and Burnette in fish, by Hensen in the rabbit, by Bischoff in the sow, and more especially the remarkable observation of Oellacher, that segmentation occurs in the eggs of fowls kept from the cold, while the eggs are still in the oviduct, I do not think that there is any difficulty in believing that the hypererchetic efforts of the human ovum which result in the formation of those so-called dermoid cysts, are parthenogenetic, and have originated in the early phases of our ancestry. In the human ovary these processes are carried further by an abundant blood-supply.

Sir James Paget has, it seems to me, struck the key-note of the pathology of dermoid cysts when he wrote, "It is, perhaps, only during the vigor of the formative forces in the fetal or earliest extra-uterine periods of life that cysts thus highly organized and productive are ever formed." A most important point in the pathology hangs on this sentence, and can be decided only by a determination of the age at which such tumors are most frequently found. It is, of course, evident that the ages at which these tumors are removed by the operating surgeon cannot be taken into account, as they are of slow growth, and have often been recognized as being present for many years without perceptible increase. They are quite unlike, in this respect, the ordinary adenoid tumors. Their contents even show that their existence must often have been contemporaneous with the life of their bearers; for we find large balls of hair, the result of the epithelial growth and shedding of a nipple-like process not bigger than the tip of one's finger; and in one sac over three hundred teeth have been found, resembling, in many respects, milk-teeth; so that we may reasonably suppose that they were the repeated products of a limited dentigenous area. In one of Mr. Spencer Wells's cases, the preparation of which is in the Hunterian Museum, a piece of bone was found resembling greatly a part of the upper maxilla and sphenoid bones, and containing mature molar teeth. In fact, inspection of the specimen almost carries conviction to the mind that the bone and tooth-sacs were produced at an early, perhaps intra-uterine, period of the life of the patient, and that they grew and matured as she did till the tumor was removed, at the age of thirty-nine.

Dermoid and dentigenous cysts have been so frequently found in children, that it may be suspected that if the histories of all such as are removed by operation could be traced, they would be found to be, as Paget suggests, either congenital or

originating very early in life. Indeed, it seems to me to be impossible that it can be otherwise when we remember how soon after birth all processes of development must cease, and those of growth alone are continued; impossible that new tissues, so strange and displaced, should be developed after the formative powers have ceased to produce new tissues in normal positions. The more we know of pathology, the more we find its processes resemble those of physiology; and it seems to me far more simple to explain the occurrence of dermoid cysts in the ovary by hypererchetic action of an ovum at the time of life when such processes are in vogue in the economy, than at some other time when they have entirely ceased everywhere else. I have already shown that the formation and destruction of ovarian cells goes on from the earliest to the latest times of existence, the degree of their maturity varying with the periods of life. Fully dilated Graafian follicles are often seen in the ovaries of newly born children, containing ova which are minute, transparent, and structureless cells. But let us suppose that, during the developmental period of life, some stimulus be given to one Graafian follicle and its contained ovum, which for want of better knowledge we may call accidental, and that this should lead to the premature maturation of the ovum, so that, were the rest of the organism ready for the process, it might be carried into the uterus and there be impregnated. Let us further suppose that, instead of being destroyed by rupture of the ovisac, it should remain in the ovisac and share alike with the rest of the economy in developmental activity, there could be only one result, and that would be the formation, to an incomplete degree, of those structures which it would evolve in perfection under more favorable conditions.

In support of my supposition, I may draw attention to a description further on of a dermoid cyst which I found in the peritoneum with attachments to its surface, but without connection to either of the ovaries. This tumor was so intimately and so extensively adherent to the peritoneum that I had to leave it, and I had reason to believe it had no ovarian connection. Might not such be developed from an ovum which had escaped from the ovaries in early life, and become attached to the peritoneum, as we know they do in after life, and there have carried on its attempt at parthenogenesis ?

The logical conclusion of this view is, that if such an ovum could get into the uterus after its escape, it would develop into a perfect instance of parthenogenesis—a speculation, of course, but no wilder than some of the facts of embryology seemed to us before we understood them. It is in fact quite analogous to the

production of the aphis by a virgin and sexless larviparous mother.

Whatever may be the value of the suggestions I have thrown out, they are certainly consistent with my own clinical experience; for in one case where I removed a dermoid cyst from a young woman, there were many reasons for believing that it had existed long before puberty. The oldest patient from whom I know that a dermoid cyst has been removed is a case of my own, the woman being in her forty-fifth year. The tumor weighed only six and a half ounces, and was full of hair, which had grown and been shed from one little spot of skin not bigger than the tip of my little finger. The amount of hair in the sac, had it grown from a similarly sized area of scalp, would have taken almost a lifetime to grow and be shed. In Mr. Wells's oldest case (38), the tumor had been recognized for eighteen years; and in a case (37) not operated upon, but examined after death, the tumor had been known to be in existence for at least twelve years. The usual age for dermoid cysts to come under the notice of the surgeon is from seventeen to twenty years, and then it is generally certain that they have been long in existence. After puberty, the recurrent congestion of the whole sexual apparatus must stimulate into growth what is in readiness for it after having been developed long previously, as I have suggested in my hypothesis. The results of that development may remain of minute or even microscopic size, until the stimulus of the menstrual hyperæmia so increases them as to make them of surgical importance; just as Hunter's celebrated experiment of the transplantation of the spur of the cock into his comb resulted in an extraordinary increase in length and size of the spur by the altered character of its hæmic nutrition. Occasionally, however, they grow to a large size before puberty.

At a meeting of the Pathological Society of London, on Tuesday, May 5, 1874, Dr. Dickinson showed an ovarian tumor removed from a child, aged ten, at the post-mortem examination. About twelve months previously the child complained of pain in the right side, and lower part of the abdomen, and then a small swelling the size of an egg was noticed. In six months' time this was as large as a small apple. It then rapidly increased. When first seen the umbilical girth was twenty-five inches, and she was in much pain. The diagnosis was difficult, and it was at first supposed to be a malignant growth of the kidney. She was removed from the hospital, and on her return, some months after, the umbilical girth was twenty-seven and one-half inches. She died soon after of peritonitis. A tumor of the right ovary was found, weighing five and one-half pounds. It contained

hair, bones, etc. All the other organs of the body were healthy. The child had never menstruated. An exploratory incision would probably have saved this child's life.

Briefly, then, I believe dermoid cysts to be the result of hyphererchetic development of an ovum in fetal or infantile life, growing into a tumor during and subsequently to puberty. They are always invested by the ordinary peritoneal covering of the ovary, beneath which is a more or less thick layer of the nucleated and banded fibrous tissue, which forms the basis of all ovarian cysts. I have seen this layer as thin as tissue paper, and in one old-standing dermoid cyst it was more than an inch thick, and occupied by large plates of calcification. In it are to be found the same almond-shaped nuclei which characterize the stroma of the ovary, only they are sparsely distributed, as I have already said they always are in old tumors. Within this layer the peculiar structures met with in dermoid cysts occur, an arrangement strongly indicative of the method of origin which I have suggested for them. I do not know of any tissue in the body which may not find its representative in them, for Beneke has even found brain-substance. Usually, however, they have an epithelial character, and in some instances show great advance both in development and growth. When skin or mucous membrane is present, all the details of their structure may be made out; and as there is no vasomotor check on the vascular supply, the materials which they secrete normally are often found in vast quantities, as, for instance, hundreds of teeth, and pounds of sebaceous matter.

Another kind of tumor, undoubtedly of ovarian origin, though without the appearance of any relation to the ovary save that of contiguity, has several times come under my observation, and as I have met with no description of a precisely similar case, I am induced to place on record a full description of it. The first case was in a patient, thirty-seven years of age, who was placed under my care by Dr. Blackwood, of Wednesbury, who had attended her in three confinements, the first of which was natural, the second had to be completed by the use of forceps, and in the third version had to be performed on account of obstruction. The last labor occurred in 1869, and after that till the time I saw her, April, 1873, menstruation occurred normally. During that time a protrusion from the vulva gradually formed, and when first seen by Dr. Blackwood it had reached an enormous size, and included the uterus, bladder, and rectum, and it had become perfectly irreducible. Dr. Blackwood also discovered a large abdominal tumor, which seemed to be the cause of the protrusion. I found that this tumor extended to about four inches

above the umbilicus, that it filled the pelvis, and the character of the fluctuation made it apparent that it was a unilocular cyst. It was very much fixed in the pelvis, so that I gave the opinion that it was adherent, and that probably much difficulty would be encountered in its removal, but as its growth had been rapid I advised an exploratory incision. This I made in the usual way on April 27th, but could find no line of demarcation between the peritoneum and the cyst-wall. The latter was very much thickened, and on being cut through it was seen to contain a large number of hairs, not growing into the cyst, as is usually the case, but growing merely in the wall, for not a hair was to be found free on the inner surface of the cyst. The contents consisted of clear serous fluid, in which floated long processes of translucent membrane, exactly resembling the omentum of a fœtus; and there was also one long finger-like process of pure fat, encapsuled in serous membrane. The sac was emptied of everything, and attempts were made on every side to discover a division between it and the peritoneal cavity. Above, I dissected till I found that its union with the intestines was so intimate as to render its removal impossible. On each side it seemed to be entirely continuous with the abdominal walls as far as the brim of the pelvis. Below it, the uterus and ovaries were felt to be quite free, so that it was made certain that the tumor was not ovarian, and behind it the intestines could be felt in a cavity which probably extended down to Douglas's pouch. When these details had been made out, it became quite evident that the proper treatment for this anomalous case was to close the wound, save at its lower angle, where a drainage-tube was placed; but before I did so, I removed a fragment of the wall of the cyst in which I had noticed the hairs. I need not give a detailed account of her progress toward recovery, more than to say that the cyst suppurated freely, and that the suppuration slowly diminished, so that in July I removed the drainage-tube. In October there still remained a slight discharge from the site of the drainage-tube, all tendency to protrusion from the vulva had ceased, and it could be felt that the roof of the pelvis was somewhat fixed and the uterus retroverted. The wound also was slightly drawn inward, but there was no other trace of the tumor. The patient now (1880) enjoys robust health, and still menstruates regularly.

Examination of the fragment of the cyst-wall which I removed showed that hairs were growing in it, or at least existed in it, in large numbers, and that they all lay in a direction parallel to the cyst-wall. There were also traces of rudimentary skin structures, as papillæ, fat loculi, and something like glands,

quite sufficient to place this remarkable tumor within the category of dermoid cysts; and in this direction also the serous membrane found within it pointed. What, then, was its origin? In answer to this, only two suppositions can be entertained, the first and least likely of which is that it was an inclusion cyst, similar to that already referred to as situated at the Torcular Herophyli. The other, and I think that which must be accepted, is that it had grown out of a wandering ovum, which, after its escape from its Graafian follicle, had failed to be extruded in the ordinary way, had not died, but had gone on to a hypererchetic development.

We know that ova are sometimes matured in infantile, even in embryonic life, and also that they sometimes undergo this hypererchetic development in the ovary. In adult life, we also know that all the ova which escape from the follicles do not reach the uterus, and it is more than probable that a large number of them escape into the peritoneal cavity, and there wander till they die. As the ovum when impregnated fixes itself at once to the surface with which it is at the time in contact, and there develops, so it is not impossible that one of these hypererchetically inclined ova, having escaped into the peritoneal cavity, there becomes adherent, and grows into such a dermoid cyst as I found in Dr. Blackwood's case. All the circumstances necessary for this coincidence being rare, of necessity its results will be rare; but as our surgical experience of such matters is just, as it were, beginning, such a case as the one I have narrated may not be without a parallel. Certain it is that I have met with no description of an exactly similar instance, though the development of wandering ova into cysts is a possibility recognized by several authors, especially by Boinet. Dr. Lloyd Roberts, of Manchester, has described a similar cystic tumor which he removed successfully, which had no connection with the uterus or ovaries, and which he regarded as a non-fecundated ovule which had dropped into the peritoneal cavity, and there become enormously developed.

I have met with quite a series of cases to which I think no other explanation can be given than such as I have advanced for the case described above. In none of them were there anything like dermoid structures, though in other respects they closely coincided with its appearances, and they had features perfectly in common, more particularly in the matter of their relations to the pelvic and abdominal organs. The series includes six cases, of which I need only describe one, as there was nothing different in any of them, either in appearance, treatment, or result. They were all young women, from fifteen to

twenty-six years of age, and they had all the physical characters of parovarian cysts. When I came to operate, however, I found that between the cyst and the peritoneum there was an absolute adhesion, and when I opened the cyst I evacuated a large quantity of perfectly limpid fluid. The cysts were lined with epithelium, and had a perfectly smooth, glistening surface. They were intimately adherent to the whole of the pelvic surface and also to the posterior abdominal wall. For some two or three inches above the pelvic brim this adhesion extended in a sloping line forward, upward, and outward to a level an inch or two above the umbilicus. From this line of attachment the cysts seemed to be free, and when emptied the intestines pushed down the upper wall into the cyst cavity like a huge pouch. In every one of the cases the uterus and ovaries could be felt, in their normal situation, through the cyst-wall, and, as far as could be determined, perfectly healthy, and independent of the cyst.

They were therefore not ovarian tumors, and they certainly were not of parovarian origin ; and the uniformity of their relations makes it, I think, certain that they form a specific class of pathological cysts. In the first case I met with, which was sent to me by Dr. Eshelby, of Stonehouse, some years ago, I tried very hard to remove the cyst, but failing to do so, I inserted a large drainage-tube, keeping it in for some weeks, and in this way I cured the cyst, and the girl now remains perfectly well.

The same history is to be given of the other five cases, and in the last four I have been able to recognize at once the nature of the case, and have therefore made no attempt to enucleate the tumor, but have been content with draining it in the method described.

My impression about these cases is, as I have said, that they are cysts formed by a dropsical distention of an ovule which had not been impregnated and which dropped into the peritoneal cavity and had there become attached and developed.

There is a class of tumors which closely simulate cystic tumors of the ovary which I have seen occasionally referred to in published accounts of operations as extra-peritoneal cysts, and in one case, at least, the description is such as makes it clear that the writer regards the tumor as having been a true ovarian cystoma developed outside the peritoneal cavity. It is, of course, absolutely impossible to accept any such explanation for these tumors, for one cannot see how a tumor of the ovary, or a tumor developed from a wandering ovum, could by any possibility be developed on the outer side of the membrane of the peritoneum. In my own practice I have only seen two cases of extra-peritoneal cysts, and in both of them the operation unfortu-

nately proved fatal. As no *post-mortem* examination could be obtained in either instance, it remains quite uncertain what the exact nature of the tumor really was, though I think I have a satisfactory explanation to give of them.

The first case that I met with was in a lady, aged fifty-six, under the care of Dr. Lamb, of Albrighton, who for twelve months before had complained of abdominal pain and tenderness, and in October, 1880, began to suffer from somewhat serious symptoms, more particularly frequent vomiting and disinclination to take solid food. Some swelling in the lower part of the abdomen was noticed about the same time, this being then regarded as ascitic. The symptoms slowly increased in severity until February 11, 1881, when a consultation was held between Drs. Lamb, Heslop, and Saundby. As a result of this consultation she was tapped and ten pints of fluid were removed, though this was by no means the quantity of fluid in the cavity, because large masses of flocculi obstructed the tube of the trocar, and prevented the complete emptying of the cyst. Some of this fluid was submitted to me for an opinion, and from the facts that it was brown and thick and gave an abundant flaky, yellow deposit which consisted chiefly of pus, I unhesitatingly gave the opinion that it was not ascitic, but a fluid that must have been contained in some cyst cavity, probably a cyst of the parovarium. I saw her on February 13th, when we found that the abdomen was quite as much distended as before the tapping. I therefore proposed an exploratory incision for the removal of the tumor, if it were possible to remove it, although the extremely exhausted condition of the patient gave no very great prospect of success. It was perfectly clear, however, that if left alone nothing but death could be the result, and therefore an operation was accepted by her attendants and relatives.

I opened the abdomen at the usual site, and after cutting through all the layers except the peritoneum I came upon the cyst wall. I opened the cyst and removed about thirty pints of fluid exactly the same as that which had been removed at the tapping, and mixed up with it I found large masses of the fibrinous deposit, which accounted for the failure of the tapping to remove the whole of the fluid. I then proceeded to remove the enormous cyst, which was uniformly attached to the parietal wall on its outer aspect and to the outer surface of the thickened peritoneum on its posterior aspect. The cyst did not dip into the pelvis at all, and the anterior parietal peritoneum did not reach the wall lower than the ensiform cartilage. The intestines and the pelvic organs could be felt through the anterior peritoneal fold, non-adherent and, as far as could be determined, perfectly

healthy. The cyst lay, therefore, entirely between the transversalis fascia on the outer side and the parietal peritoneum on the inner, the peritoneal cavity having been nowhere opened during the protracted and severe operation. The cyst was removed in its entirety, and its inner surface consisted of broken-down mucoid epithelium, infiltrated everywhere with pus, lying upon the basement-membrane, which consisted almost entirely of muscular fibre.

The conclusion concerning the nature of this cyst at which I have arrived is that it was developed from the urachus, a part of which had been occluded at both ends, but during the developmental changes of embryonic and infantile existence had not become obliterated. I entirely fail to see any other possible origin for it, and if my explanation be correct it is very marvellous that this structure should have remained quiescent for fifty-six years and then should suddenly undergo an inflammatory change which developed it into this enormous cyst. The patient went on very well for about three days and then rapidly sank from exhaustion. No post-mortem examination was allowed, and therefore I can shed no further light upon it, and as far as I know the observation is unique, although it is perfectly well known, as I myself have repeatedly had occasion to observe, that small cysts of the urachus are opened in abdominal section. I do not know that any such cyst has previously been met with sufficiently large to be of pathological importance.

The second case of extra-peritoneal cyst was sent to me by Dr. Craig, of Stoke-upon-Trent. Here again the patient was almost hopelessly beyond the reach of surgical interference before I saw her. The tumor had been recognized by Dr. Craig in 1878, and then he had recommended her to put herself under my care for its removal. She, however, declined to do so until the middle of last May, and when she arrived in Birmingham she was practically moribund. I happened to be from home when she reached my house, and my servants were under the impression she would never leave the house alive. She was placed in lodgings close by, and I operated upon her immediately on my return, three days afterward. The cyst I found gangrenous and full of pus, with a large mass of broken-down lymph. It was situated entirely outside the peritoneum, which was never opened at all, and it dipped down into the pelvis on the right side only. As far as I could make out, its structure was very much like the other, save that it extended almost entirely on the right side, the peritoneum seeming to be pushed altogether toward the left. Its characters were very much like those of Dr. Lamb's case just described.

She rallied from the operation very well, and for eight days seemed likely to recover, but as soon as the stitches were removed from the wound it reopened and continued to discharge a large quantity of unhealthy brown purulent fluid until her death, seventeen days after the operation. Here again, unfortunately, no post-mortem examination was obtained, and therefore I can say nothing with absolute certainty as to the origin of the tumor, but my belief is that this also was a cyst of the urachus.

In both of these cases I inserted drainage-tubes into the cavity left by the cysts, and I am under the impression these tubes had something to do with the fatal results, though this may not be the case. The explanation of the deaths is of course first of all to be found in the advanced condition of exhaustion in which both patients were at the time of operation, and I think it very likely that a fortunate result would have been obtained in both of them if the operations had been performed earlier in the histories of the cases. Perhaps the immediate cause of death was the destruction of the vitality of the peritoneum, which was associated with the inner wall of the cyst. In both cases the extent of peritoneum denuded from the cyst-tissue, to which it doubtless owed its blood-supply, was very great, and if this important structure died from loss of its blood-supply, it would of course be quite sufficient to account for the deaths of the patients. I think if I should ever have similar cases I should feel inclined to remove a large portion of this denuded peritoneum and trust to a careful arrangement by sutures of the portions left rather than run the risk of what I think may have been the cause of death, for I often find I have to remove in cases of adherent cyst a very large piece of parietal peritoneum, and this is done without interfering in any way with the recovery of the patient.

These cases illustrate very well the unexpected and great difficulties which arise in the practice of abdominal surgery, and how much we have yet to learn in this important branch of our art. They also illustrate the abundant causes we have for regretting that abdominal tumors are often allowed to go so long as to remove any reasonable prospect of success in dealing with them.

Before dealing with the very numerous conditions which simulate ovarian tumors it will be convenient if I now discuss the signs and symptoms by which an ovarian tumor may be recognized; and I may here say at once that the conditions which mimic these tumors are so numerous, and there are so few facts in connection with them upon which implicit reliance can

be placed, that safety is to be found only in the process of reasoning by exclusion; that is, for a proper diagnosis in the case of an ovarian tumor it will be found the best plan, first of all, to make a mental list of all the conditions that it may be, and exclude them one after another until no alternative is left. Any one who habitually follows a converse plan will sooner or later be led into some fatal blunder. Our anxiety should always be, not to prove that a given tumor is ovarian, but to show that it cannot by any possibility be anything else.

It may be said with perfect certainty that from the history alone no ovarian tumor could be diagnosed, so various are the stories told by the patients about their cases. Thus one patient will present herself totally unaware of the fact that there is any tumor, her only sensation being one of discomfort from the swelling, while another may have known for many years of the presence of a small lump which had long remained quiescent, and had taken to enlarging only for a few weeks or months. The rate of increase gives no guide, either in unilocular or in multilocular tumors; for I have removed multilocular tumors which had been in progress for a great many years, and I have removed one of great size from a patient aged sixty-six, which had grown in four months. I have removed, on the other hand, a large unilocular parovarian tumor which had been in existence for more than ten years, and the structure of which showed that it always had been unilocular; and I have removed two unilocular tumors, one of which grew so as to completely distend the abdomen in seven weeks, and another, almost as large, which had not been noticed for more than five weeks.

The details given by the patients as to the region in which the tumors were first observed are often very misleading, and no dependence whatever can be placed on some. One patient, in whom there existed an undoubted fibroid tumor of the uterus, asserted that it originally grew somewhere in the neighborhood of the spleen, and gradually descended to its present uterine situation. Tumors of one ovary are often stated by their bearers to have originated on the side opposite to that from which they are found to grow. One condition which on rare occasions comes under our notice, hydatids of the peritoneum, beginning as it does generally by rupture of an acephalocyst of the liver, presents usually a history of origin at the upper part of the abdomen; so that, when such a story is given with subsequent general enlargement of the abdomen, caution is necessary before excluding hydatids from the possibilities. A tumor which began centrally and remains so is of course likely to be uterine; but this is far from being constantly the rule. I have heard a pa-

tient state that an ovarian tumor of considerable size had appeared suddenly; and this might have really happened, for its escape from the pelvis might have been sudden. I have often pushed an ovarian tumor out of the pelvis that had been impacted there, and the same thing may be experienced with uterine myomata.

The menstrual histories given by patients with ovarian tumors have been so various, in my experience, as to lead me almost entirely to disregard them in the diagnosis. Dubois asserted that he had not known an ovarian cystic tumor accompanied by hemorrhage, but this has been repeatedly noticed in my practice; and the explanation of the apparent discrepancy is that, when the great obstetrician wrote, the diagnosis of pelvic tumors had not arrived at its present state. In some instances, two of which I have already detailed, ovarian tumors gave rise to uncontrollable menorrhagia, and I have pointed out that there seems to be a close association with small cystic ovaries and this serious symptom.

I have frequently known complete arrest of menstruation to be associated with rapidly growing tumors both of the ovary and parovarium. Such a fact in the history of any case ought to make us especially careful to eliminate pregnancy, more especially the condition of hydramnios, which I have known to be treated fatally on two occasions by tapping, once as an ovarian tumor and once as ascites. The uterus, in the early months of normal pregnancy, is not unfrequently displaced to one or other side, and has been often mistaken for an ovarian cyst; in one case, by myself, for an abscess in the broad ligament. In this latter case, I was led astray by the general symptoms of hectic from which the patient suffered. It was to me a lesson to trust to no one symptom, nor to any group of symptoms, in a pelvic diagnosis; fortunately the patient recovered completely after a miscarriage.

A large number of cases of ovarian tumor are met with near the climacteric period of life, and it is not unusual for their appearance to be ushered in by a premature arrest of menstruation; so that during the first few months of the growth of the tumor the patient takes it for granted that she is pregnant. It is somewhat curious that I have had at the same time under my care two cases upon whom I performed ovariotomy, in both of whom pregnancy was believed to exist for many months until the lapse of time made an investigation advisable. Arrest of menstruation occurred in both before the tumor was observed, so that the abdominal enlargement was of course taken for pregnancy. In one case the abdominal parietes were so dropsical that it was a

matter of great difficulty to be certain that there was not pregnancy as well as an ovarian tumor, the difficulty being overcome by the use of the sound after some hesitation.

For the diagnosis of ovarian tumors, either subjective or differential, there are varieties of symptoms, almost numberless, the great majority being of little or no consequence for accuracy, and none of them alone being trustworthy. The symptoms vary in their character and intensity very much according to the size of the tumor, though this is far from being the rule. Thus the largest ovarian tumor which I have removed, somewhere over one hundred pounds in weight, gave rise to no other symptoms than the inability of the patient to get about from its immense weight; while the smallest, only six and a half ounces, was the source of agonizing pain and a great variety of reflex symptoms, including aphonia; and it had completely disabled the patient for some years. In the early growth of a simple cyst, symptoms of any kind are seldom met with until the tumor is sufficiently large to be impacted in the pelvis. The growth of dermoid cysts, on the contrary, is often accompanied by pain of a most intense kind, for which no explanation can be advanced.

In one case, already referred to, I had to remove a very small dermoid cyst on account of the agonizing pain in it. Though this has been relieved completely, a variety of nervous symptoms have supervened, very mysterious in their nature, for which no remedy has been found, and which, among other results, have induced a contraction in the hamstring muscles, and an absolute rigidity of the knees, so that the patient has never been able to walk since the operation, now nearly ten years.

As a rule, pain is not met with until cystic tumors are large enough, if out of the pelvis, to press on important viscera; or unless the surface of the tumor undergoes inflammatory change. In the latter case, pain and increase of pulse and temperature are the indications, though it is surprising to what an extent a tumor may be found to be adherent, and yet, throughout its history, no indications of inflammatory attacks have been given. Until the tumor is sufficiently large to interfere with nutrition, and if it be not of a cancerous character, there are rarely any symptoms of constitutional disturbance; though sometimes I have seen a small tumor very loose in the cavity of the abdomen give rise to great pain and discomfort. Such tumors also occasionally give rise to symptoms of intestinal obstruction, as was the case in the instance of a large uterine myoma which I successfully removed by abdominal section.

During the growth of an ovarian tumor, the appetite is usually not interfered with until the case is far advanced; nor is sleep,

though it is often found that the patient can lie only on one side; nor do we find that either the temperature or the pulse is affected to any appreciable extent. Hysteria is sometimes found in connection with ovarian tumors, and dependent directly upon them. In one of my cases this was markedly the fact, for the hysteria disappeared entirely after recovery from ovariotomy. Hysterical symptoms are in constant association with phantom tumors, and these cases, in the days of the early ovariotomists, were in several instances operated upon by mistake.

The enlargement of the veins often seen in the skin of the abdomen in cases of ovarian tumor is of no great assistance as a diagnostic sign, for it is present in almost every other disease simulating ovarian dropsy. Any very marked enlargement of the veins may, however, be a reason for suspecting malignant disease if the other indications be negative. In one or two instances I have seen this enlargement the only indication of the cancer found on opening the abdomen.

As the tumor enlarges, the symptoms become more numerous and various; thus in the pelvis, by pressure on the rectum, bladder, and nerves, it may give rise to dysuria or incontinence, to constipation or diarrhœa, and to various neuralgiæ. In the abdominal cavity, by pressure on the stomach, liver, and diaphragm, it produces very frequently nausea and vomiting, and distaste for food; in one case in my own experience it caused jaundice; and very often difficulty of breathing, amounting in the latter stages to orthopnœa, is induced. Coincidently with the production of these visceral symptoms, indications of great systemic alterations come on gradually, due partly to direct interference with nutrition and partly to its perversion. Thus the patient becomes thinner, and the skin dry and often hot, the eyes sink, and the features become pinched, and then comes on the peculiar expression of face which has been named the "facies ovariana." The legs at this stage generally become œdematous, from the mechanical obstruction to the return of the blood from the limbs, and the œdema extends to the vulva and over the lower and central walls of the abdomen. When the tumor has reached such extreme size as is indicated by these symptoms, if then seen for the first time, its diagnosis becomes a matter of some difficulty, even by the careful consideration of its signs; for it is in the very small and in the very large ovarian tumors that the diagnosis is most difficult. In those of medium size the task is much more easy.

The physical signs which indicate the presence of an ovarian tumor come under the notice of the surgeon, as a rule, only when the tumor has reached a size sufficient to have obliged it

to rise out of the pelvis, and appear as an abdominal enlargement. It is often, however, necessary to determine the nature of a small pelvic tumor, and, as I have already said, to remove it. Such a diagnosis is a matter of no great difficulty to any one accustomed to make the bimanual examination, more especially if it be conducted while the patient is under the influence of an anæsthetic. An ovarian tumor will be found to be almost invariably behind the uterus, that viscus being pressed forward close to the pubic bone; and its fundus may, save in exceptionally obese patients, be felt just above the pubes. Usually the uterus can be fixed between the two hands, and then no doubt can be entertained as to what it is. Behind it is the tumor, and if the uterus can be moved independently of it, and if the tumor can also be raised out of the pelvis independently of the uterus, no doubt need be felt that it is a tumor of the ovary or of the broad ligament. An absolute determination between these two is not a matter of much importance, but it may be made by the practised fingers being able to determine a uniform intensity of the fluctuation wave in different diameters of the tumor, and by this being also distinguished between the two forefingers in bimanual examination.

As the tumor increases in size and rises out of the pelvis it becomes somewhat more difficult to determine that it is not intimately associated with the uterus. It may be necessary to introduce the sound in order to determine this point; but this, as a rule to which I think there can scarcely be an exception, ought never to be done at the first examination. I have known a miscarriage, in more than one instance, brought on by neglect of this rule by very competent surgeons. It not unfrequently happens that menstruation, or some loss resembling it, goes on for the first few months of pregnancy; and to assert the diagnosis between early pregnancy and an ovarian tumor just rising out of the pelvis, at a first examination, is a task which only the rash or the greatly experienced will undertake. If, with the patient on her back, one forefinger on the os uteri and the other on the fundus of the tumor, the two be found to embrace something which moves *en masse*, then it is, of course, certainly uterine. But if the two fingers seem to be in relation with different structures, then the outside finger must search for the fundus uteri, and after it has been found, and after it has been ascertained that the uterus is not enlarged, *and then only*, the sound may be introduced into the uterus, and its relation to the tumor readily ascertained. The first matter, then, is to be certain that the tumor is not uterine. If it be not, and it be rounded, elastic, and capable of being raised to some extent out of the pel-

vis, then it is almost certainly ovarian. It still may be ovarian, even if fixed to the pelvis, though it is rare that ovarian tumors contract adhesions at such an early stage of their growth. If fixed, then, it may be a hæmatocele, or an abscess, or a soft tumor growing from bone; but the diagnosis of all these may be greatly assisted by the previous history and the general symptoms.

Examination by the rectum will often yield valuable additions to the information obtained by vaginal examination as to the relations of a pelvic tumor, and it may be carried out according to Simon's plan, by the introduction of the whole hand into the rectum. This should only be done, however, under exceptional circumstances, when other means have failed to satisfy the mind of the examiner, and surgeons having large hands should not attempt it. Personally, I have never employed this method, and I do not think it has met with very general acceptance. I saw Dr. Simon perform it several times at Heidelberg, and he informed me that he never found any ill-effects from it. I have since heard, however, of many disasters from its use in this country, none of which have I seen published, and I think it had better be dismissed.

When an ovarian tumor has risen out of the pelvis, and has as yet met with none of the accidents to which they are liable, and which lead to complications, its diagnosis is a matter of ease. First of all, palpation will discover that it is a tumor by its resistance, and firm pressure on it with the fingers of one hand, and percussion on them with the fingers of the other yielding a dull note, will exclude the possibility of the case being one of phantom tumor; and, as the tumor pushes the intestines before it upward and to each side, in these regions a tympanic note will give the indications by percussion peculiar to uterine and ovarian tumors, what I have termed the "tympanic corona." It must be remembered, however, that some intestine may have slipped down in front of an ovarian or uterine tumor, or some may be adherent there, so as to produce a note of clear resonance. This condition is, however, very exceptional, and if a resonant note be found in front of the tumor the chances are greatly against it being ovarian.

Mr. Spencer Wells has pointed out one very exceptional condition which may completely baffle us in drawing conclusions from the percussion sign. I have never seen it, am very doubtful about it, and therefore I give at length an extract from his lectures on the subject:

"Another point of doubt and difficulty may arise from the air having entered an ovarian cyst; for, as an ovarian cyst contains

a certain amount of fluid, it may also contain gas, and either the fluid is decomposed after tapping, or there may have been some possible communication set up between the intestine and the interior of the cyst. Sir Thomas Watson records a case in which a patient had a cyst filled alternately with fluid and with air; when the fluid collected to a certain quantity it seemed to open some valve-like communication with the intestine, emptied itself, and became filled with air. As the fluid gradually reformed, the air was displaced, and the same series of changes went through again. I have known a case in which air distinctly entered."

To exclude the possibility of the tumor being uterine, some care is necessary; but it is not difficult when the educated touch has determined that the tumor fluctuates, and that, throughout its extent, the peculiar wave passes which is found on gently striking any part of a bag of fluid while the hand rests on some other part of its circumference. A knowledge of what fluctuation is, and what this peculiar thrill is, is not easily communicated by description, and it requires long practice to be able to recognize it accurately.

If the wave be equally distributed in every direction all over a tumor, then, in all probability, it is unilocular. A multilocular tumor, or one composed of two or three large cysts, may often be recognized by the practised fingers detecting a difference in intensity of the wave along different diameters of the tumor. There are two conditions, however, which must be carefully excluded from the possibilities, and, just because they are both very uncommon, their probabilities are every now and then overlooked. They are cystic diseases of the uterus and hydramnios. In the former, the tumor will be found associated with the uterus, the latter moving along with the tumor when it is moved, and being dragged upward by it to an extent that ought always to make us cautious, and warn us to wait and watch.

Finally, there is that great aid to diagnosis, the employment of an anæsthetic, without which, in every case of doubt, no positive opinion should be arrived at. When the muscles are completely relaxed a great deal may be determined which is not possible otherwise, especially in the pelvis.

I need not say that in all cases of abdominal tumor vaginal examination of the pelvis is of the utmost importance. As far as ovarian or parovarian tumors are concerned, the most important indication is when a vaginal examination gives entirely negative results, that is, when there is nothing felt in the pelvis save the uterus in its normal position and freely movable. We may then take it for granted that the pelvic relations of the tu-

mor are of the most favorable kind and that the pedicle is of reasonable length. If the tumor is felt in the pelvis it will generally be behind the uterus, but this is by no means a uniform condition. The uterus may be behind the tumor, and in that case the chances are that it is sessile and a good deal of trouble will be experienced in dealing with the pedicle. If the cervix is found to widen out and be associated with the tumor in every direction, we have a clear indication that the tumor is uterine, that is, it may be pregnancy or a myoma. If the uterus is drawn up very much in front and the posterior lip seems to be lost upon the tumor, then we may expect a tubal pregnancy. But on the other hand it is not to be assumed that a tumor felt in the pelvis must necessarily be either ovarian or uterine, for I have felt in the pelvis tumors of the kidney, spleen, and liver; and then we may also have exceptional growths, such as malignant tumors of the bones of the pelvis or of the omentum, and hydatids of the peritoneum, etc., etc.

Auscultation of ovarian tumors gives chiefly negative signs, but these are often of value, as in the case of perfect absence of intestinal gurgling over the tumor. A loud friction-sound is often heard, but this is only an indication of a dryness of the peritoneal surfaces where it is heard, and there is sure to be an absence of adhesions at the spot. The hydatid fremitus, as described by Mr. Wells, I have never been fortunate enough to meet with, though I have operated on a large number of cases of this disease.

Dr. Le Double, of Tours, read a memoir at the meeting of the Scientific Congress at Havre on this subject, but I have not gathered from it any new facts of importance.

The best way to learn to recognize fluctuation is to practise upon a large bladder having a nozzle inserted into it so that its tension may be varied; for it will be found that there is a considerable difference in fluctuation according as the cyst is tightly filled or not. The sensation will also vary very much according to the weight with which the fingers of the examiner are pressed upon the cyst; and the first instruction I have always to give any one who is observing abdominal fluctuation for the first time is to press as lightly as possible upon the skin. Placing the finger-tips of one hand gently upon the surface and retaining them there immovably, the fingers of the other hand must be tapped with exceeding gentleness on the skin at some little distance. I lay particular stress upon the immobility of the first hand because it is not unusual to see persons endeavoring to ascertain the presence of fluid in the abdomen by a simultaneous to-and-fro movement of both hands, which can only

result in wobbling the contents without conveying any real impression to the observer's mind.

The first thing to be learned is that in the subcutaneous layer of any patient not extremely emaciated there may be communicated between the two hands a wave something like that produced by the presence of fluid, but having become familiar with this by practising upon a healthy abdomen, especially that of a stout person, this difficulty will speedily be eliminated.

A further test in cases of doubt may be applied by the method first suggested by Sir James Paget of palpating first in one diameter and then at right angles to it, and this test will be perfectly safe if the umbilicus is included in the second diameter; for pseudo-fluctuation may be perceived in the first instance but will be corrected in the second. As Sir James Paget has pointed out, this phenomenon is due principally to the muscular tissue, which gives a thrill closely resembling the fluctuation of fluid across its length but not in the direction of the fibres. A very convenient position for studying this fact is the calf of the leg. Having made himself familiar with this superficial subcutaneous wave the pupil will be less liable to mistake it for the superficial wave of ascitic fluid, and still less liable to mistake it for the deeper wave of the fluid of an ovarian or other cyst.

Ascitic fluid may be generally recognized by the fact that it is associated with the uniform occurrence of a tympanic note on percussion; but when there is ascitic fluid as well as fluid contained within a cyst, we have a double wave of fluctuation extremely confusing but easily recognized by practised hands. There is a very simple and neat way of confirming the value of the sign of percussion in such a case which I have had occasion to practise, and which will almost always decide between ascites and ovarian dropsy in such exceptional cases. It consists in mapping out the marginal area of clear percussion note by a pen-and-ink line, and then ascertaining whether a clear note, obtained by percussing on a finger laid gently on the skin immediately outside that line, can be altered to a dull note by increasing the pressure. If this alteration takes place generally round the line or throughout its greater part, it may be taken for certain that an ovarian or parovarian tumor is present. On the other hand, if there be a clear note somewhere over the area of the swelling which is not removed by firm pressure, but is rather extended or intensified, still more if pressure bring out a clear note where dulness existed without it, then it will be evident that ascites is present, and not an ovarian cyst. The explanation of these signs is, that pressure round the margin of an ovarian tumor will bring it into more extensive relation with the

abdominal wall by displacing the intestines, and this is most easily accomplished in the epigastrium. In the converse condition, when a clear note is produced by pressure in ascites, the abdominal wall is brought into contact with floating intestine, the mesentery of which is so short, and the quantity of fluid so great, as to keep the structures apart in the absence of the pressure. We have, further, a difference between the clear percussion notes of ascites and ovarian dropsy, in that the former readily alters its position, always appearing at the part of the tumor highest in relation to the patient's position. Thus, in a doubtful case, if there be a corona of clearness above the supposed tumor, extending from the hepatic to the splenic regions, and any alteration of position, such as lowering the shoulders and raising the pelvis, should alter the position of the area of clearness to the region of the umbilicus, then the case is almost certainly one of peritoneal dropsy.

More than once I have met with a case where even this sign failed me, and where I have opened the abdomen to remove an ovarian tumor and found only masses of peritoneal cancer. The reason of such a mistake was, that the intestines were all matted together by growths in the great omentum, and were drawn up into an arch under the diaphragm. The exploratory incision, however, did no harm, the patients recovering from it and dying some time after from the extension of the disease. Additional difficulty was created in one such case by the fact that the patient had been twice tapped before the operation, and no doubt was entertained that a cyst had been emptied, and that the masses felt were smaller cysts. I have also operated on a case in which no intestinal note could be obtained anywhere. The patient had suffered from recurrent peritonitis, and it was evident that the intestines were all behind the tumor. At the operation this was found to be the case, and the adhesions were of the most formidable character. Yet the patient recovered without a bad symptom.

The rapidity with which the waves travel is governed by three conditions chiefly. First, the thickness of the abdominal walls, and this will also influence the sharpness with which the wave is felt. The tension of the cyst will also materially affect the wave transit, for in the tightly filled cyst the wave rushes along with great rapidity, while in the flaccid cyst it is transmitted much more slowly and is so much less easily perceived that the unpractised observer may altogether miss it. If the contents of the cyst are thick and gluey it may be altogether impossible to obtain anything like a distinct wave of fluctuation, and there are many cases of really solid tumors, more particu-

larly the œdematous variety of uterine myoma, which are sufficiently soft to give a wave of fluctuation which makes it impossible to discriminate them from ovarian cystomata with gluey contents. In fact I do not know anything more difficult to teach than the many facts about this sign of fluctuation. I do not know anything requiring longer practice and greater variety of experience for being perfectly learned; but I also know nothing giving greater diagnostic power when the fingers have been sufficiently trained to perceive its differences. Thus, given a case of ovarian tumor in a young woman and another case of advanced pregnancy, the practised fingers alone, without any question being asked, without a vaginal examination being even proposed, and without the use of the stethoscope, will in the majority of cases at once be able to make an exact diagnosis; while in the case of a large uterine myoma, the mere sense of resistance upon the first impact of the hand is often enough to determine the nature of the case. This "sense of resistance" is a thing quite impossible to teach. I saw it first practised by Dr. Warburton Begbie, and have seen him diagnose accurately and by it alone between an effusion of fluid in the pleura and consolidation of the lung. Similarly in the abdomen this sign is nearly always enough to enable me to discriminate between pregnancy, an ovarian tumor and a solid mass, though of course I should never dream of trusting to it alone.

Of a similar character is the sign to which French writers have given the term *ballottement*. This is not confined in its usefulness by any means to the determination of pregnancy. In a case where there is an abdominal tumor surrounded by ascitic fluid, the latter may be easily recognized by the sign of fluctuation, but the tumor may escape observation if an effort be not made to discover its *ballottement*. It is my uniform practice, therefore, in examining an abdomen in which I am satisfied of the presence of ascitic fluid, to place the fingers of one hand upon the skin very lightly, and then with a steady and somewhat rapid movement downward the ascitic fluid is made to move away, and if there be a tumor the fingers come promptly in contact with it and convey a sensation which cannot be mistaken. In cases of doubtful pregnancy this sign is well enough known, and there, of course, it is the liquor amnii which is displaced by the sudden movement of the fœtus which is felt. This ballottement may be determined through the abdominal walls with quite as good effect as through the vaginal cul-de-sac. If the fluid outside the cyst be small in quantity, its diagnosis is of no great consequence; but if large, its non-recognition may lead to serious mistakes. For instance, in one of my cases I satisfied myself

that there was an ovarian tumor from the signs given by percussion, and that there was evidently some ascites from the double wave of fluctuation. The patient was of an enormous size, and the growth had not existed for more than six months. It was a grave question whether I had to deal with a multilocular tumor having one or two very large cysts and a small quantity of ascitic fluid, or with a small tumor and a large quantity of ascitic fluid. The only method of deciding the question would have been to tap the abdomen above the tumor by my blunt trocar, and to have evacuated the ascitic fluid only; but to this the patient would not accede, and I had to begin the operation in serious doubt. The result showed that the plan referred to would have been a wise one, for it proved to be a comparatively small tumor with an enormous ascitic collection, all the intestines having been pushed above the tumor. There are some minor signs which often serve to indicate the presence of ascites to any marked extent, such as the protrusion of fluid through the omphalic ring, carrying in front of it a layer of peritoneum like the finger of a glove. The uniformity of the enlargement by ascitic fluid is greater than that produced by ovarian dropsy, though in the case just referred to this indication failed me; for it was the want of symmetry in the measurement which suggested that the chief cause of the enlargement was cystic. The readiness of alteration of the form usual to an abdomen distended by peritoneal dropsy was also absent; for, in whatever position the patient lay, the same outlines were preserved; and the greatest proportional increment of measurement had occurred between the umbilicus and the pubes. This peculiarity is usually an indication of ovarian cysts or of uterine tumors.

I have already uttered a warning, which I think it necessary to repeat, concerning the use of the sound; and here I may introduce the account of a very singular experience in which the use of the sound, instead of being an assistance, might have led me into a very serious mistake, had I not been familiar with the fact that every now and then a case occurs in which the fundus uteri is perforated by the sound while it is being used in an ordinary way by one quite accustomed to its use and without any undue force. Such perforations never do any harm, and I used to see them often when I used the sound a good deal, but now that I use it hardly at all I have not seen one for a very long time.

Some years ago I drew attention to cases of persistent metro-peritoneal fistula in which the condition has been one of interest only on account of its curiosity, but in the following case it presented features of great importance in the diagnosis of an

ovarian tumor. S. W——, aged forty-nine, and unmarried, was sent to me from a distance in September, 1874, on account of an abdominal enlargement which had been in progress for three months, and had reached a considerable size. The parietes were extremely thin, and the wave of fluctuation was everywhere extremely vivid. Behind the uterus was a round nodular mass, freely movable, and the sound readily entered the uterus for three inches. I diagnosed ovarian dropsy, and admitted her into the hospital for the purpose of removing the tumor.

At a consultation, held on October 8th, some doubt was entertained by some of my colleagues that it might be a case of peritoneal dropsy, and to assist the diagnosis the sound was introduced by Mr. Ross Jordan. It readily passed in seven inches toward the left side, though there was absolutely no force used in its introduction, the instrument seeming to slip in most easily. I at once expressed my opinion that the uterus had been perforated, and with the consent of my colleagues I proceeded with the operation. The tumor was removed without difficulty, and the nodular mass behind the uterus was found to be a small fibroid growth in the fundus. It was the right ovary which was removed, the left being quite healthy; and as the pedicle was very short, and the uterus somewhat dragged upon, I did not think myself justified in endeavoring to gratify my curiosity by looking for the point of perforation. From the position of the fibroid, however, I am certain that the aperture must have been situated in the anterior wall, for the fundus was completely retroflexed, both Fallopian tubes being carried with it. It is probable that the anterior wall had become very attenuated, or even completely perforated, from being stretched over the fibroid by the retroflection, this latter being evidently due mainly to the pressure of the tumor from above.

Whether Mr. Jordan made the perforation or not, it made no difference to the progress of the case, for she got well without a bad symptom, and went home on the twentieth day. She called on me eight weeks after the operation with the wound almost healed. I passed the sound very cautiously into the uterus, and found that it readily entered three inches, and then met with the usual obstruction. I happened, however, to move its point about a little, and found that toward the left side of the cavity it slipped through a hole, and made itself immediately perceptible under the integuments to the left of the cicatrix. The right cornu of the uterus was tilted upward toward the wound, on account of the adhesion of the pedicle, and I know that the sound could not have passed through the Fallopian tube on that side, for it had been embraced by the clamp; while the thinness of

the abdominal walls and the fixidity of the uterus enabled me to determine that the sound had clearly passed through the anterior wall somewhere to the left of the middle line of the organ.

In addition, therefore, to the interesting fact which my former cases clearly established, that we may have permanent communications formed between the peritoneal cavity and the cavity of the uterus other than those of the oviducts, and without any deleterious results, we have in this case the further fact of great clinical importance, that these abnormal apertures may be the cause of confusion in diagnosis. If I had not been firmly convinced, from the physical signs, that my patient really suffered from an ovarian tumor, the passage of the sound inward for seven inches, in the hands of one so careful and so skilled as Mr. Jordan, would have so staggered me that I should have fallen into error.

Familiarity with the somewhat common occurrence of such apertures, however, enabled me to have the courage of my opinions.

In my earlier practice I placed considerable reliance upon tapping as a means of diagnosis in abdominal tumors, but further experience has led me to distrust it. Some of my reasons for distrusting tapping will be given at greater length in another chapter and others I have already discussed.

In cases where there is ascitic fluid its removal may indeed help us to map out the position and size of a tumor with greater accuracy, or the removal of the fluid from a cyst may help us to determine that it is parovarian, or that it is ovarian, from the fact that other cysts or solid matter were left behind; but beyond this tapping helps us but little. It serves in no way to clear up the nature of a doubtful tumor, nor does it reveal what the intimate relations of that tumor may be. The operation has its own special risks, and our more recent experience shows that these risks are greater than, or at least as great as, those of a simple exploratory incision. I therefore prefer the latter in all cases, for if we can do no more than relieve the patient of a quantity of ascitic fluid, or of the contents of one or more major cysts, we can do this far more effectually by a small exploratory incision than by the wound of a trocar, and with quite as little risk.

Of the occasional misfortunes which follow tapping, I had a very notable instance in the case of a patient sent to me some years ago by Dr. Laidler, of Stockton-on-Tees. She was of enormous size, so that it was advisable to tap her before removing the tumor. Unfortunately, the cyst-walls were permeated by large venous sinuses, and one of these was injured by the trocar,

so that several pounds of blood poured into the cavity of the emptied cyst, and the result was unfavorable to the success of the subsequent ovariotomy. Besides such an exceptional risk as this, there is the possibility of suppuration of the cyst after tapping, and the infection of the peritoneum by its septic contents.

Finally, by means of an exploratory incision, which need not as a rule be more than an inch and a half or two inches long, we can ascertain absolutely the nature of the tumor and very many of its relations, and we may generally obtain information concerning it altogether beyond the reach of a tapping. This latter operation, therefore, I have almost entirely excluded from my practice for any purpose of diagnosis, and it is now only used for relief in those cases where removal of the tumor is impossible. When it is absolutely necessary to tap, the operation is best performed by a trocar which I have devised, having a steel point with a chisel edge, which is almost blunt. The patient having been placed in a convenient position, a puncture is made by an

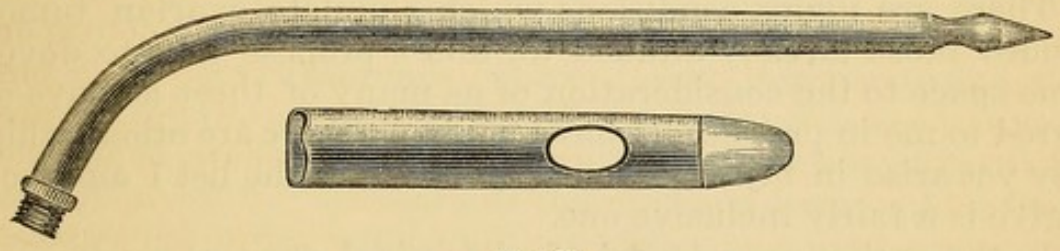


FIG. 29.

ordinary lancet into the cyst, and the trocar is made to follow the track of the lancet. The trocar is so simple that it never can be out of order; it forms a solid rod, which is extremely useful as a probe, and its point is sharp enough to penetrate an inner cyst, and yet so blunt as to be incapable of mischief save in the hands of the clumsy or the careless. Great care should always be taken to empty completely the cyst which is tapped, and to prevent the admission of air, and for this latter point the perfect solidity of my trocar is the most absolute guarantee.

Tapping by the vagina was an operation a good deal in vogue about eight or ten years ago, and I have had two cases in which cysts of some kind were permanently cured by this means; but as it is by no means always attended with good results, I have almost discontinued it. I have known death occur three times after it, and I may state as my general conclusion that I have a growing distrust of tapping of any kind, either by the aspirator or the trocar, and in all cases where treatment is or may be an object, as well as diagnosis, I infinitely prefer an abdominal section.

The diagnosis of the variety of tumor in each case is of im-

portance in guiding us to its treatment; therefore it is not for the mere exercise of ingenuity that I recommend every practitioner dealing with a case of ovarian tumor to exhaust every point on which I have dwelt, together with many others to which I have not alluded, but which the individual peculiarities of each case and his own personal shrewdness may suggest. Above all, let me again urge the necessity of reasoning by exclusion, and of making repeated examinations at intervals before any certainty of diagnosis is felt. Three times it has occurred to me to remove ovarian tumors which had hastily been set down at an early period of their history as floating kidneys, that diagnosis having been made in all three cases because the wish was father to the thought, and because the practitioners who made it had not learned the value of patience. It is this want of patience that is to blame for those melancholy instances of blunders, altogether unpardonable, where the abdomen has been opened in cases of normal pregnancy mistaken for ovarian tumors.

There are many conditions which simulate ovarian tumors besides those already alluded to, and I propose now to devote some space to the consideration of as many of these as have occurred to me in practice. I have no doubt there are others which may yet arise in my experience, but I think the list I am about to give is a fairly inclusive one.

In the earlier experiences of abdominal surgery many cases occurred in which an operation was performed for the removal of an ovarian tumor and no tumor found; that is to say, where there was no condition whatever to justify the mistake. During the last twenty or twenty-five years, however, I have not heard of any instances of this kind, indeed the introduction of anæsthetics has rendered such a misfortune all but impossible. We can easily understand how such mistakes as those so candidly narrated by Mr. Lizars could occur. We can only admire the frankness with which he published them, and acknowledge the great service the publication rendered to his successors. It is a trite saying, but one worthy of frequent repetition, that we learn a great deal more from our blunders than we do from our successes.

Before the introduction of anæsthetics the most likely condition to give rise to such a mistake was that singular disease known as phantom tumor or phantom pregnancy, to which the name of pseudocyesis has been given by Goode. This singular disease is certainly an affection of the nervous system, and lies in that border-land which exists between hysteria and insanity, and where everything in the way of explanation consists merely of guesswork. There can be no doubt that whatever may be the

mechanism of this disease, its immediate exciting cause is intimately associated with the ovaries, as indeed is the whole group of hysterical diseases. We know, however, that these affections are by no means unknown in the male sex, and I have seen a marked case of phantom tumor in a male, and singularly enough a medical man was the victim of the fancy. Mr. Spencer Wells (*British Medical Journal*, June, 1878) says he saw a phantom tumor in a soldier who came down from the Crimea to Smyrna with an abdominal enlargement which entirely disappeared when the man was narcotized with chloroform. Perhaps this was a case of malingering.

Simpson, quoting Harvey and giving also his own experience, tells us that symptoms such as we find in these cases are to be observed in cows and bitches; and doubtless, if accurate observations were to be made on these animals so affected, some kind of explanation would be arrived at. That the symptoms are due to a perverted intelligence, or a mere desire to defraud, is not an explanation which would apply to many cases which have come under my own observation, and it could scarcely be urged in the case of animals. I have failed, however, to find that in cows or bitches any of the imitative symptoms have often been observed; that is to say, such symptoms as distention of the abdomen seldom occur. The signs of the spurious pregnancy in them consist in the reflex phenomena which accompany true pregnancy, and this points conclusively to some false start given to the reflex mechanism which connects the ovaries and uterus with their subsidiary organs and the system generally.

Dr. S. Haughton, F.R.S., made a communication to the Dublin Obstetrical Society on February 7, 1880, on a case of phantom tumor in an ass. Having purchased a fine specimen of a rare variety of zebra, he was anxious to provide a suitable partner for him; he therefore obtained a healthy three-year-old virgin ass. It was necessary to have a virgin, as it was known that the first intercourse gave a stamp to the subsequent progeny. Frequent and apparently satisfactory intercourse took place between the two. The ass came into season at intervals of five weeks, and remained so from ten to fourteen days, and its period of utero-gestation was eleven months. It was therefore easy to discern when the animal was in foal. After six weeks, the ass began to enlarge visibly, and a man much accustomed to the breeding of horses declared that he could "feel the foal inside her." The eleven months expired, and the ass came into season again without having given birth to a foal. After a lapse of four months she was again given the zebra, and again she swelled, continued so for eleven months, and again gave birth to noth-

ing. In this case he considered that the mental element might be disregarded, for the ass could have no object in deceiving, and her illusion of being in foal influenced her physiological condition, for her mammary glands were enlarged, and when the supposed pregnancy was over they subsided with the abdominal enlargement.

The great majority of cases of phantom tumor are really instances of spurious pregnancy incompletely developed; indeed, I am not quite sure but that every case really is so; those about which I have doubt being of a class which are certainly hysterical, which have no other sign than abdominal distention, and which seem to me perfectly analogous to crib-biting among horses. I have seen a sufficient number of these to be able generally to distinguish them as they enter the consulting-room, by the one sign which can always be heard, loud intestinal gurglings. These gurglings are due to the swallowing of air which the patient indulges in for a few minutes before she visits the surgeon, and she generally begins the consultation by attracting attention to them and to her large size. The increase in size is partly due to the spurious flatulence and partly to the peculiar muscular rigidities which these patients indulge in. If the patient is kept engaged in conversation for fifteen or twenty minutes, without an opportunity of renewing her air supply, the gurglings will entirely cease and she will markedly diminish in size. Physical examination, especially under an anæsthetic, at once confirms the spurious nature of the abdominal distention. In these cases there is usually no attempt on the part of the patient to induce the surgeon to believe that she is pregnant, or even that she has a tumor, and the belief that a deliberate and voluntary fraud is intended is, I think, to be justified only in exceptional cases. The object seems really to be that of gratifying that insatiable love of attracting attention so deeply rooted in the female mind, a weakness which is at the bottom of ninety-nine cases of hysteria out of every hundred; and it must be borne in mind that this desire is characteristic of many forms of insanity in men as well as in women. The majority of cases of eccentric hysteria occur in women to whom nature has denied the external attractions of beauty, or in whom there is not the compensation of a refined and cultured intellect. It is therefore in neglected and ill-educated women that these objectionable forms of hysteria are chiefly to be met with. I have seen the kind I am now speaking of imitated by crib-biting mares and geldings very closely, the best instance having occurred in a mare. She was generally required to go out at a particular time of day, and as that hour approached, if she could succeed in getting a hold of

any fixed object with her teeth she would secure a quiet day in the stable by rendering herself quite unfit for work for many hours. She would swallow large gulps of air, so that the distension looked, to those unaccustomed to it, almost like the last stage of a peritoneal dropsy, and the intestinal gurglings could be heard at many yards distant. In a few hours she would be well again and ready for work; but nothing could prevent her succeeding in her trick unless care was taken to have nothing about upon which she could fix her teeth.

Women who indulge in this objectionable habit are nearly always sterile, though I can call to mind two cases in mothers of large families. It is by no means confined to any period of life, as I have seen it in very young and in very old women.

Between this group and those in which there is always present a distinct conviction that they have a tumor, there is no defined line, but they are generally women of the same type. They do not usually, however, have the gurglings, the distension being produced entirely by some peculiar muscular fixation, in which probably the diaphragm is the chief factor. In order to distend the walls of the abdomen, the first step is to fix the diaphragm at as low a level as possible; and after this is done, breathing can be carried on by the ribs alone. The time through which this kind of respiration can be employed is very brief in men, but is practically unlimited in women, owing to the peculiar superior costal method of breathing which exists in them. After fixing her muscles in this way, a woman has only to throw her shoulders back and her pelvis forward, and if her clothes are loose she at once presents the appearance of pregnancy; and in these cases, if the confidence of the patient be sufficiently reached, it will always be found that there is either a hidden desire or a concealed dread of pregnancy. Generally, there is some little sign, or a group of symptoms, which gives coloring to the suspicion; such as morning sickness, pain in the breasts, flow of milk, or arrest of the menses; but in those cases where there is no expressed belief in the existence of pregnancy, the history of the symptoms given seldom leads up to that supposition, and the patients are generally very reticent in giving their own impression. Between this second group of cases and the third, in which I class those in which the belief in pregnancy is expressed, and both its symptoms and signs given with more or less completeness and without hesitation, there is no well-defined distinction; for cases present themselves in which the condition is not sufficiently complete to place them under the heading of spurious pregnancy, and yet where there is evidently a belief on the part of the patient that there is something more than a swelling.

A well-marked case of spurious pregnancy, with its train of imaginary symptoms well described, and its reflex phenomena well developed, is one of the most singular experiences any one can have, and is most bewildering to those unaccustomed to physical examination of the pelvis. It is by no means confined to women at the climacteric, as many authors seem to have taken for granted without having properly analyzed the facts; and it is not even confined to married women, or such as have engaged in sexual functions without being married; for I have seen a very well-marked case in a young woman, twenty-two years of age, who presented all the usual and most trustworthy features of virginity.

There is some peculiar nervous machinery put in action the moment a fertilized ovum becomes attached to the uterine or tubal mucous surface, and that machinery sometimes gets a false start. How this occurs, we do not know, but the result is the appearance of all the symptoms without the reality of pregnancy.

One of the most perfect cases of phantom pregnancy or pseudocyesis which I have ever met with, was one I saw in consultation with Dr. Charles Warden and Mr. Machin, of Erdington. The patient was thirty-two years of age, had been married eleven years, and had menstruated with perfect regularity until June, 1872. Menstruation was then suddenly and entirely arrested, she slowly increased in size, and had morning sickness and many other symptoms of pregnancy. The breasts enlarged, she described the sensations of quickening, and she engaged Mr. Machin to attend her in the confinement she expected in March. Nothing, however, came of it. When I saw her in the following May she presented all the appearances of being pregnant at the full time, the breasts containing quite an abundant supply of milk, and the question to be considered was, had she an extra-uterine pregnancy? As the uterus was perfectly normal, having no tumor of any kind in association with it, this suspicion was at once dispelled; and on placing her completely under the influence of ether it became at once apparent that the pregnancy was a phantom, which ultimate test for pseudocyesis is one of the many triumphs of gynæcology due to the genius of Simpson. This patient was in the same condition in 1879.

Another condition which frequently gives rise to the suspicion that the patient is suffering from a tumor, and therefore, of course, most probably an ovarian tumor, is the curiously rapid growth of omental fat which many women put on at the climacteric period. Only a few days ago a remarkable instance of this occurred in my experience, the description of which will serve for all I have to say upon this subject.

A lady was brought to me from a distance by her medical attendant, who was a gentleman of exceptional experience and ability, but, of course, like other men engaged in general practice in the country, he had but very occasional opportunities for obtaining experience in the diagnosis of abdominal tumors; and, as he frankly told me, he knew very little about them. He brought his patient to me because he was in doubt, and there was no discredit due to him by reason of his difficulty. The patient was forty-seven years of age, and for about a year her menstruation had become irregular, her abdomen had increased very much in size, and she had lost flesh in the face and limbs. Her arms bore distinct evidence of this, for the subcutaneous fat had disappeared from them and the skin was wrinkled and flabby. Her abdomen was large, and, as she said, dresses she could wear only a few months before, she was now wholly unable to put on. As she lay on the couch the appearance was certainly that of an abdominal tumor, but the moment I touched the abdomen and felt the tight condition of the skin, I suspected what I had to deal with. The layer of subcutaneous fat there was extremely thick, for on taking up a handful of the skin it was found to include considerably more than two inches of fat. All over the abdomen a clear resonant note could be determined, no traces of fluctuation could be detected, the cavity of the pelvis was perfectly normal, her functions were healthy, and nothing distressed her but the size of her abdomen. But for my previous experience in such cases I might have hesitated to give the opinion I did, that the case was nothing but a climacteric accumulation of fat in the abdomen. I comforted both the patient and her doctor with the assurance that after the climacteric period was over a redistribution of fat would probably occur, that it would become more equalized over the body and less pronounced in the abdomen, and this I have seen occur so many times that I have little doubt that in the case I am describing I shall find in about two years that my prognosis will be verified.

This of course was a somewhat extreme case, because it is much more usual to find a general increase in the adipose tissue of the body than that it should be deposited in the abdomen at the expense of the other regions. I can, however, recall to mind a sufficient number of mistakes which I have made in such cases as this to be able to give a warning to others to be cautious in expressing an opinion concerning the existence of a tumor merely from an increase in the size of the abdomen of a woman at the climacteric period.

Another of the conditions which simulate ovarian tumors, and one about which we must be more cautious than any other,

is pregnancy. I have already said that during the whole period of gestation menstruation may occur with normal regularity and in normal quantity, and also that a rapidly growing ovarian cystoma may arrest the menstrual flow. It has been my experience, as it must have been the experience of every special practitioner, to have patients brought to me as suffering from tumor when their real state was that of pregnancy, and I need hardly say that this occurs chiefly in unmarried women. I have, however, had it occur more than once in a married woman, and I can call to mind an instance of this of a somewhat dramatic kind.

A lady forty-two years of age who had been married twice and whose married life extended over eighteen years was brought to me by her doctor as a case of ovarian tumor. With very great difficulty indeed I persuaded him that it was a case of pregnancy, and that she must be within a few days of her confinement. My opinion, however, was entirely disbelieved by the patient, and it was only when the pains of labor came upon her that she accepted the accuracy of my opinion. She was confined of a dead child and her labor very nearly cost her her life. Only a few days ago a hospital patient was sent to me as being a case of ovarian tumor. She was of very large size, the feet and legs were much swollen, nothing could be felt in the pelvis, and she had seen no appearance of menstruation for sixteen months. Yet a careful stethoscopic examination revealed the sounds of the fetal heart, and by getting my hand into the vagina I found the cervix normal, though very high up. It turned out to be a case of pregnancy with cardiac dropsy.

In the event of a young unmarried woman presenting herself with an abdominal tumor the utmost caution must be observed, for there is no limit to the persistency of the denial such patients will make as to their condition. Medical experience is full of illustrations of this, and I have heard very many anecdotes from my professional brethren illustrating it. Some of their patients even went so far as to deny the possibility of their being pregnant when labor was in the third stage. A few weeks ago a patient came to the hospital to consult me concerning an abdominal tumor. It was manifestly a case of advanced pregnancy. Yet when it was delicately suggested what the possibility of her state was, she indignantly denied it; but when I proceeded with the requisite examination I found not only that she was pregnant but that the recto-vaginal septum had been completely destroyed in a previous labor. No amount of scepticism in these cases will therefore be too great, but the practitioner will be wise who keeps that scepticism to himself. If

the tumor is of small size and the patient is not suffering, only two questions need be asked—is her menstruation arrested, and, if so, was it regular previous to its recent stoppage? If these two questions are answered in the affirmative, I would advise that an examination should not be made at the first visit but that some slight *placebo* be given and the patient be asked to repeat the visit at an interval of seven or eight weeks; and at the same time some gentle hint may be dropped that the case is probably one of a nature that will not require operation. In the majority of cases I find this is quite enough, and that the patients very soon realize their position and do not trouble me again. If, however, they should come back, upon the second or third visit I advise that an investigation should be made. I need not here describe the evidences upon which we base a diagnosis of pregnancy, and shall allude to one only in detail, because it is one but little known as yet, and it is a sign more valuable perhaps even than that derived from auscultation, in that it can always be observed, whereas the fetal heart cannot always be heard. I mean the rhythmical contraction of the uterus. If the hands be placed on the abdomen of a case of suspected pregnancy and a fluctuating tumor be felt, that tumor will become quite tense and like a myoma if the examination be prolonged for a few minutes. Then again it will become flaccid and fluctuating, and this alternation will go on rhythmically at varying intervals. Once this sign has been felt and recognized, I think it will be impossible for the observer ever again to be deceived by a pregnant uterus. Let me again impress upon every one the necessity of caution in giving an opinion to the effect that a patient is pregnant.

A most disastrous case of this kind occurred some years ago in my practice, where a young and very attractive girl, who suffered from an ovarian tumor which I subsequently removed, was examined by four different practitioners, all of whom were men of experience and two of whom saw the patient together in consultation. All four of these men asserted, I am told, that they heard the fetal heart, a sign which is regarded as conclusive of the existence of pregnancy. The girl when seen by me communicated none of these facts; she was brought to me by her parents and I knew nothing of her previous history. I had no hesitation at all in pronouncing it to be a case of ovarian tumor, and in the course of a few days I removed it. One of the practitioners under whose care she had been, was foolish enough to continue his statement that the girl had been pregnant and that my statement that I had removed an ovarian tumor was a falsehood. For this extraordinary conduct he was very properly

called to account by the parents of the patient, and it was only by his tendering a most ample apology that he was saved from an action for damages. Fortunately for me it happened that the operation was performed in the presence of two gentlemen who knew both the patient and the practitioner, so that it was a very simple matter for me to prove the accuracy of my statement.

The interest of the case centres upon this: How was it that all these four men declared that they heard the fetal heart? I am bound to say that upon this point I can offer no possible explanation, unless it be that some curious intestinal sound of a rhythmical character was given out during their examinations. But the case is such a striking one, and has, I believe, been so disastrous to the practitioner to whom I have alluded, that I quote it as a warning to every one to exercise the utmost caution in pronouncing an unmarried woman pregnant. Nevertheless, when such a patient presents herself, the suspicion of pregnancy must be ever present in the mind, for it is by far the most likely condition, and it must only be by repeated examination and the concurrent testimony of physical signs that any positive opinion should be given. Far better at any rate to defer an opinion for some time than to run into such a mistake as that I have instanced.

One other caution I shall give in connection with such cases, and that is never under any circumstances employ a sound where there is a possibility of pregnancy.

Sometimes pregnancy may coexist with an ovarian or par-ovarian tumor, and this might of course happen in an unmarried woman, though I have never seen such a case. But it is by no means infrequent in married women, and then the diagnosis is a matter of very great difficulty. We are, however, removed from the risk somewhat by the fact that married women are much more likely to assume they are pregnant than that they are not, and attention will be drawn to their condition more by unusual size than by any other feature, and they have no inducement to conceal the possibility of pregnancy.

Here I may mention one of the abnormal conditions of pregnancy which every now and then leads to a terrible disaster by being mistaken for a peritoneal dropsy or an ovarian tumor. In this disease, hydramnios, we have of course an arrest of menstruation for some months, generally four or five, and this ought to lead to a suspicion of what is the matter; but on the other hand, being one of the diseases of the primiparous woman, we have it unfortunately more frequently, or at least quite as frequently, in unmarried women as in married women. It is always associated with albuminuria, and very often in its course we have

the characteristic convulsions of that disease when it is associated with pregnancy. I have seen eight cases of hydramnios, and I am very pleased to be able to say that I have not been led into the blunder of tapping any of them. Seven out of these eight cases have been associated with twin pregnancies, and this is too great a proportion to be a matter of accident. They have all occurred in primiparous women, and the most advanced case had reached only the sixth month of pregnancy. The distention of the uterus in all of them had taken place with amazing rapidity; in one case the limit of time was probably less than a fortnight. I would therefore advise that any young woman whose abdomen was found to be large, and inwhom the distention had occurred with great rapidity, the first thing to be examined should be the urine, and if this be found to be albuminous, let every proceeding be taken with the utmost caution. Of course no intestinal note of resonance will be found save at the usual seat of the corona, and on pelvic examination the distention of the uterus will be made clear. This is easily determined by getting the patient into the erect position and then making a vaginal examination. The child or children will then be found settling down on the point of the finger, and can easily be felt through the thinned uterus. A slight push will send the mass floating up toward the fundus, whence it will sink in a few seconds. In this way I have readily made a differential diagnosis between a unilocular ovarian cyst and a distended uterus.

The diagnosis will then be complete, and the treatment, which is to empty the uterus as rapidly as possible, to administer chloroform freely in the event of convulsions, and to give such remedies as are appropriate for puerperal albuminuria, may at once be proceeded with.

I have known three practitioners, all men of ability and extended experience, who have been unfortunate enough to tap a patient suffering from this disease, and who were immensely surprised to find the patients miscarry in a few hours and die shortly afterward. When conversing with these men I found that none of them had ever heard of this unusual disease; and yet, from my own experience, I can hardly regard it as extremely rare. It is, however, but slightly alluded to in the text-books upon these diseases.

There is another disease in which fluid is collected in the cavity of the uterus, owing to occlusion of the cervix, and to which the name of hydrometra has been given. This condition is probably very rare; I have only seen one case. It is rather difficult to understand how it is arrived at, more particularly in such a case as that which came under my care, in which the patient

was nineteen years of age and had menstruated for about three years in the normal way. Before I saw her, menstruation had ceased for about two years, and her abdomen had steadily increased in size. The physical signs were all those of a parovarian cyst, and I did not examine the condition of the pelvis because the patient was a virgin. I proceeded to operate in the ordinary way, and found there was no appearance of the layers of peritoneum. After passing through what was clearly a thin layer of muscular tissue, I opened the sac and removed nine pints of clear limpid fluid. The inside of the cyst was rugose, and the cyst itself rapidly contracted after being emptied. Passing the forefinger of my right hand into the vagina and having my left forefinger in the pelvis, I made out clearly from the relations of the cervix that the cyst was truly the cavity of the uterus. I fastened in a drainage-tube, and kept it there for about three weeks, and after its removal the wound speedily healed. The patient has since remained in perfect health, but has never menstruated. I doubt very much if I should have diagnosed the case any more completely if I had examined the pelvis previous to the operation, and the mistake I made fortunately turned out to be of no importance. Had I been able to make a correct diagnosis of the case it would have been an easy matter to have passed a drainage-tube through the cervix without performing abdominal section. This disease is only alluded to by authors as a possibility, and there are very few descriptions of cases, such as there are being described as having occurred in old women after the climacteric. There is one exception, given by M. Richard as a case of hydrosalpinx in which, when he applied pressure, he was able to force the fluid into the uterus and out of it. His diagnosis may of course be correct, but I think that it much more likely was a case of hydrometra.

The retention of menstrual fluid within the cavity of the uterus from occlusion of the cervix, or more frequently from atresia of the vagina, is a much more common occurrence. I have seen some seven or eight cases of it, but in none of them did there seem to be much possibility of it being mistaken for an ovarian tumor. In the first place the patient is nearly always brought under notice on account of the intense menstrual pain due to the monthly additions to the contents of the uterus. The history was given in all of them that no external appearance of menstruation had occurred, and these two circumstances alone are sufficient to suggest the true nature of the case. Examination of the patient will reveal two things which are decisive. The first is the presence of a tumor of comparatively small size, smooth, ovoid, central in position, and very tender to the touch;

and, secondly, there will be found some form of malformation of the genital canal. If there be doubt, an examination under ether will easily reveal the fact that the tumor is uterine. Some cases of this kind are described as occurring in women who have menstruated normally and who have even borne children, the occlusion having been due to the union of the uterine lips or of the walls of the vaginal canal after some injury or ulceration. I have not seen such a case, but the symptoms would be the same, and the history would probably give a significant clue to the diagnosis.

There are two remaining conditions of the uterus which may require to be differentiated from ovarian tumors, one of which is much more common than the diseases I have just spoken of. They are fibro-cystic tumors and myomata. The fibro-cystic tumor of the uterus is an extremely rare affection, so rare that until four months ago I had never seen a case, and in that case it was absolutely impossible to distinguish the growth from a parovarian cyst. She was sent to me by Dr. Leacroft, of Feckenham, and was seen by me originally about five years ago, when I diagnosed a parovarian cyst and advised its removal. This she declined, and in the interval she was tapped several times by Dr. Leacroft, at each tapping the tumor being apparently emptied and no solid matter being left. She had never been pregnant, was fifty-three years of age at the time of the operation, and had menstruated all her life with perfect regularity, until the age of fifty. After the climacteric period the tumor grew with great rapidity, and it was after the change that she was first tapped. When examined the uterus appeared to be quite free from the tumor, and the physical signs were those of a unilocular and therefore probably parovarian cyst. At the operation I found the tumor to be densely adherent all over its front aspect, and I had not been engaged in its separation long before I recognized the familiar appearance of uterine tissue. It was quite impossible to remove the whole of the tumor, which consisted entirely of one cyst, and I left about one-sixth of it in the pelvis, cutting it off as low down as I could by means of the cautery and tying several large vessels. Subsequent examination gave conclusive evidence that the walls of the tumor were composed of uterine tissue. The operation was an extremely severe one, for numerous coils of intestine had to be separated from the posterior wall of the tumor and both ureters were laid bare. She had no bad symptoms for four days, but upon the fifth she began to sink and died upon the seventh. A *post-mortem* examination was made by Dr. Saundby, who found that the space occupying the lower third of the abdomen contained a considerable quantity of bloody fluid,

on removing which a suppurating area was discovered. The lower part of this consisted of the remains of the uterine mass. Dr. Saundby notes that a loop of intestine had its mesentery entirely torn away, but was living and uninjured. The left ureter was compressed by old inflammatory adhesions, and the left kidney was atrophied and cystic. Death in this case might, I think, possibly have been avoided if I had adopted Dr. Keith's plan of drainage. It is very likely indeed that a fatal issue would have occurred even if the drainage-tube had been used, but if I had another such case I should certainly use it. Had I operated when I first saw the patient, before she was tapped, I think it more than likely she would have recovered; and I also think it might have been better to have clamped the cyst outside than to have dealt with it by the intraperitoneal method. It is in such exceptional cases that the clamp may be of service.

The growths which have usually been described as fibro-cystic tumors of the uterus have been multi-cystic, and what their origin is I really cannot say. There seems to be no tissue in the uterus from which one would expect cysts to grow, yet there is no doubt they are produced in the organ. Their diagnosis as ovarian tumors would be very much the same as the diagnosis of myomata, with the addition that the discovery of fluctuation would lead to the suspicion of cystic disease. I am under the belief, however, that without experience a differential diagnosis of fibro-cystic tumors would be a very difficult thing, and that it is possible only in the hands of a surgeon who had made two or three previous mistakes. In such a case as I have described of a unilocular cystic tumor of the uterus a correct diagnosis was absolutely impossible.

Concerning uterine myoma I shall only speak so far as this disease simulates ovarian dropsy, for with the many other phases of it I am not at present concerned, though when speaking of ovariotomy I intend dealing more fully with it. There is one most constant clinical feature characterizing myoma which is seldom met with in ovarian cystoma, which alone will often decide the nature of the case: I refer to menorrhagia. I have already said that there are conditions of the ovary which produce intractable uterine hemorrhage, but this is quite exceptional. On the other hand we see many cases of uterine myoma in which hemorrhage never appears as a leading feature; still, given an abdominal or pelvic tumor, constantly recurring menorrhagia and a distinctly anæmic appearance of the patient, the surgeon may take it for granted that the chances are immensely in favor of the disease being a uterine myoma. On examining the abdomen of a patient suffering from the disease a

variety of conditions may be discovered. Thus there may be a large ovoid smooth tumor occupying a perfectly central position and which may give rise to a feeling so closely resembling fluctuation as to deceive the most experienced hands.

Such a case as this I operated upon three years ago, where a very distinguished surgeon had previously performed dry tapping, that is to say, he plunged a trocar deeply into the tumor and got nothing out, the result of this experiment being that the tumor grew with increased rapidity. When I first saw her I also was under the impression that the tumor was ovarian because there was nothing in the pelvis to contraindicate that view, and the tumor was uniformly smooth and ovoid. It was only after repeated examinations that I began to entertain a suspicion that the tumor was really uterine and was probably one of the variety to which I have given the name of "œdematous myoma." I advised the removal of the tumor, and when I came to operate I found that this suspicion was correct. The tumor grew from the fundus and was encapsulated by a thick layer of uterine tissue. The body of the uterus formed an excellent pedicle around which I placed a clamp. The patient recovered perfectly well and is still alive, enjoys excellent health and continues her occupation of monthly nursing. The tumor on removal weighed thirty-seven pounds, and consisted entirely of uterine muscle-cells distended into a meshwork by a large quantity of serum which drained away, leaving a solid mass of only about twenty pounds. The sexual functions of this patient are wholly unaltered.

It is very much more common to find these myomatous tumors perfectly solid, so that they give no sensation of fluctuation at all, and instead of being smooth and uniform in contour they are far more often nodulated into eccentric forms. Sometimes their position is not at all central. I have seen a large myoma shaped like a cocked hat running up altogether on the right side and having no position at all to the left of the middle line. In such a case there can hardly be room for doubt in the diagnosis, for we never see ovarian tumors having such characters, at least none such has ever presented itself in my experience, although sometimes small outlying cysts of an ovarian tumor may somewhat resemble through the skin the nodules of a uterine myoma. But it is when a pelvic examination is made that little room is left for doubt. Nearly always the tumor may be at once determined as having a most intimate relation with the uterus; and if the forefinger of one hand be kept firmly pressed upon the cervix, while with the other hand the tumor is made to move freely from above, the case will be easily and

clearly determined, though here again sometimes an ovarian tumor, solid and with a very short pedicle, may very closely resemble in its pelvic conditions those of a uterine myoma. Finally, the sound may be employed to assist in the diagnosis. In an ovarian tumor the uterus is rarely elongated, while in a myoma it nearly always is; but, as I have elsewhere said, with increased experience the surgeon will find the sound to be an instrument becoming less and less useful to him, and he will find the cautions I have already given regarding its employment becoming more and more deserving of respect.

Solid uterine tumors, besides the absence of fluctuation, often have in addition two vascular signs which I have never met with in ovarian tumors; namely, an aortic impulse, which may be seen and felt, and an enlargement of the uterine arteries to be felt in the vagina. In one case I satisfied myself that the tumor was uterine, mainly because at the flexure of the vagina on one side I felt an artery as large as the radial. There is also a uterine souffle to be heard in most of the growths, and it is best heard in the vagina.

If the tumor be found to be solid but not uterine, yet attached to the uterus and moving it to an extent which may lead to the belief that it is ovarian, then we have a choice between a dermoid cyst, a fibroid tumor of the ovary, cancer of the ovary, or a pedunculated myoma of the uterus. A dermoid cyst is rarely so constituted that it will not give fluctuation at some part or other; and its peculiar nodulated character, with here and there spots of bony hardness, will often betray it. Fibroid tumors of the ovary are very rare, and cancer of the ovary alone occurs in only one form, the fibroid, which is of extreme rarity.

Mere dropsical effusion into the cavity of the peritoneum does not usually offer any difficulty in its recognition, but every now and then a case will be met with in which, from exceptional causes, some difficulty will occur in recognizing such a case. Thus I have more than once opened an abdomen under the complete belief that I should find an ovarian tumor, but have instead found only masses of cancer with an abundant ascitic effusion. This is due to the fact that in such a case the intestines have become adherent or have been wedged backward by a large mass of fungus in the omentum, so that no resonant note could be obtained in front, whilst it was readily given in the flanks. In such a case, of course, the mistake is of no great importance, for all that happens is that the patient is tapped by an exploratory incision instead of by a trocar, and there is the advantage that an absolute certainty of the diagnosis is arrived at.

One very curious case came under my notice about two years ago in a girl aged eighteen, who had an enormous ascitic effusion. When she was admitted under my care at the Women's Hospital there was no difficulty at all in recognizing the condition, so that I tapped her for it more than once and was absolutely certain that my diagnosis was correct. The girl appeared to be in all respects save that of the dropsy perfectly healthy, and no kind of lesion which could account for the dropsy could be discovered. In order to clear up the case I transferred her to the care of a friend who was a physician attached to another institution. There she was kept under observation for several months without any additional information being obtained, save that there appeared to be a small amount of effusion in both pleuræ; still no distinct clue could be obtained of the cause of her singular condition. By a mischance she fell into the hands of another practitioner, who unwisely expressed the opinion that it was undoubtedly a case of ovarian disease, and he proceeded to treat it on that belief. When the abdomen was opened, however, my diagnosis was completely confirmed, but unfortunately the operator did not take advantage of the opportunity to discover the cause of the dropsy, so she left the institution in which the operation was performed without any additional light being thrown upon the case. She was tapped repeatedly until she died, and then again an opportunity was lost of obtaining information upon one of the most remarkable cases which has ever been under my care.

Quite recently I had another singular experience of dropsy of the peritoneum imitating, still more closely, cystic disease. I was called by Mr. Whitcombe, the superintendent of the Birmingham Lunatic Asylum, to see a girl in whom the abdomen had increased with amazing rapidity. I diagnosed a parovarian cyst, and in a few days I opened the abdomen to remove it. I found, however, that it was not a cyst of the broad ligament, but a dropsical distention of the lesser cavity of the peritoneum, due to occlusion of the communicating cavity by peritonitis. The inflammation was general, and in spite of drainage she died of the disease in a few days. At the post-mortem it was found that the whole mischief was due to a common seamstress' sewing needle lying in the great omentum just over the foramen of Winslow. The patient had probably swallowed it, and from the stomach it had passed out into the position where it was found.

Such an occurrence is of course of the most unusual kind, but of great interest as showing how difficult exact diagnosis is in abdominal diseases. The case is also of value as an illustration

of the advantage of an exploratory incision over tapping. If I had tapped in this case at the usual point, I should have gone straight through the large intestine which lay immediately in the way, displaced by the distention of the lesser cavity of the peritoneum. Death must have resulted from such a mistake. My third and fourth cases of operations for peritoneal hydatids are of sufficient importance to be given in full detail.

In this country hydatid disease of the peritoneum is not very common, and what we know of it is chiefly derived from the experience of practitioners in hot countries, more particularly in Australia, where this form of parasitic disease seems to be peculiarly rife. I have seen four cases, and in but one of them was I able to make a diagnosis previous to operative proceedings. They all presented the ordinary appearance of abdominal tumors of a cystic character, and they certainly did not yield the special sign upon which so much reliance is placed by some authors—the so-called hydatid fremitus. I understand, however, that our great English authority upon this disease, Sir William Jenner, has only noticed this sign once in his large experience. The invasion of the peritoneum by these parasites nearly always occurs by the rupture of a sac in the liver, from which the foreign organisms are set free to attach themselves and begin another phase of growth in the peritoneum.

The first case which I saw occurred in the practice of Mr. Langley Brown, in the person of a lady who had resided in Australia and who had apparently acquired the disease there. She had several distinct and separate tumors in her abdomen, and the pelvis was occupied by a fluctuating tumor which we believed to be a mass of hydatids. A complete record of the case is given by Mr. Brown in the *Birmingham Medical Review* for July, 1876, to which I am indebted for the following *résumé*. There was a fluctuating tumor in the recto-uterine cul-de-sac, which we believed to be a mass of hydatids, because in the abdomen was another fluctuating mass attached apparently to the omentum. I tapped the pelvis and drew off the contents of the mass felt there, and from this operation a sharp attack of peritonitis followed, which seemed to result in the death of a number of other colonies, some of which at least found their way in some mysterious fashion through the walls of the bladder and were extruded by the urethra. A large jarful of cysts were so passed, much more than the bladder could have contained, and there was no kidney tumor to account for them; nor is there reason to believe they grew in the bladder. The explanation may be that they were really extra-peritoneal, or that the pelvic mass may have been a ureter. The scolices found in them were pro-

nounced by Dr. Cobbold to be those of *echinococcus hominis*, and the patient made a perfect recovery.

The second case occurred in the person of a hospital patient who had spent all her life in the neighborhood of Birmingham, and who had nothing in her history or in her physical signs to lead me to believe the disease from which she suffered was other than a cystic tumor of the ovary. Even after I had opened the abdomen I found the organs so matted together with cysts of varying size, having all the appearances of ovarian cysts so intimately associated with all the structures, that it was some time before I recognized what the extraordinary condition really was. I removed a few of the cysts, and a microscopical examination completely revealed the nature of the case. The tentative operation had a fatal result, and upon *post-mortem* examination the hydatids were found to occupy the whole abdominal cavity—to have insinuated themselves into the muscles of the walls and into various organs besides the liver, so that the operation could not have been completed by any possibility.

R. P., aged eighteen, was seen by me in consultation with Dr. Hickinbotham and Mr. Pugh, of Ashted, on December 23, 1881, on account of a pelvic tumor, with which were associated very severe symptoms. There was a high temperature, and the pulse was extremely rapid. The patient was constantly sick and presented all the appearance of being extremely ill. In fact Mr. Pugh had called in Dr. Hickinbotham under the impression that she was dying, and they asked me to see her on account of a large mass in the pelvis, which they regarded as an abscess. When I saw the patient I concurred in their view, and the patient was sent at once to the Hospital for Women for the purpose of an abdominal section. This I performed next morning, in the presence of Dr. Hickinbotham, Dr. Savage, and Mr. Pugh, and assisted by Mr. J. Raffles Harmar. On opening the peritoneal cavity I found that all the structures were matted together, and that it was extremely difficult to identify any of them. The adhesions were those of recent peritonitis and also the peculiar agglutination caused by the presence of hydatids in the peritoneum. Deep down in the pelvis, and attached to the back of the right broad ligament, I found a bunch of hydatid cysts, some of which I removed and others of which I ruptured. On the left side was a much larger mass, which I could not touch on account of the dense adhesion of layers of intestines. I therefore inserted a glass drainage-tube and closed the wound over it. The tube was removed on the fourth day, but the pulse and temperature did not fall perceptibly till the eighth day. The sickness, however, ceased almost immediately after the operation, and the

patient complained of no pain after the third day. On the fourteenth day the mass on the left side had diminished to less than one-half of the size which it had at the time of the operation. She left the hospital on the twenty-fourth day, at which time the pelvic mass had almost disappeared, and the patient's symptoms were all gone. She had gained immensely in health and strength, and Mr. Pugh reports that she is now quite well and going about.

There are two points of note in this case, in that it illustrates that remarkable fact, of which I have published many illustrations, that a judiciously performed, yet incomplete, operation may often effect a complete cure, and that the chief difficulty in abdominal surgery is to know when to stop. The latter lesson is to be learned only in the school of painful experience.

The fourth case occurred in a woman aged twenty-six, placed under my care by Dr. Blackwood, of Wednesbury, in whom a pelvic tumor had been growing for two years. Until last Christmas she had suffered very little inconvenience from it, but at that time it became very painful, increased greatly in size, and obliged her to remain in bed.

I saw her on January 16, 1882, with Dr. Blackwood, and found a large fluctuating tumor rising almost as high as the umbilicus, quite fixed and extremely tender. The patient had a high temperature and rapid pulse, and was quite unable to straighten herself in bed. I was of opinion that she was suffering from a large abscess, though I could not make out much in the pelvis. I admitted her to the hospital for the purpose of performing abdominal section, and this I did on January 23d.

I found the tumor to be a large sac full of hydatids. I emptied it and fastened in a large glass drainage-tube, and the patient is already nearly quite recovered. The cyst is quite small, and the secretion from it is perfectly healthy pus. All her symptoms have disappeared.

I do not know what the relations of the cyst were, more than it was in the peritoneum and had intestines adherent to it. In this case, as in the other, tapping would have been quite useless.

At the present time (July 15, 1882) these cases remain perfectly well.

An equally rare condition, which it is perfectly impossible to diagnose previous to operation, is one of which I have been able to find only one case recorded besides the case which has occurred in my own experience. In the *American Journal of Medical Science* for October, 1852, Dr. Buckner relates a case which he diagnosed as an ovarian tumor, and upon which he

decided to operate under that belief. When he came to operate, however, the tumor was found to be not ovarian at all, but a cyst situated in the mesentery between the layers of the peritoneum and surrounded by small intestines. He proceeded with the operation and dissected the tumor out, dividing in his proceeding the superior and middle mesenteric arteries, which he had to tie. The patient recovered, and, in spite of the great separation of the mesentery from the intestine, no apparent bad consequences of any kind ensued.

My own case occurred in February, 1878, and was of a precisely similar nature. My diagnosis before the operation was that of a parovarian cyst, and if I had tapped her instead of opening the abdomen I should in all probability have killed her, for just at the point of election for tapping I found, as soon as I had opened the peritoneum, a loop of intestine running at right angles to my incision. I did not do as Dr. Buckner did—dissect out the cyst; I contented myself with the much less severe operation of tapping it and fastening in a drainage-tube. In this way I completely cured my patient and she remains to this day perfectly well.

At first sight it would hardly be thought likely that tumors of the liver, gall-bladder, kidney, and spleen would often be mistaken for ovarian tumors, but I have had a large number of cases of these conditions sent to me as ovarian tumors, and I have on several occasions opened the abdomen under the impression that I was about to remove an ovarian tumor and found the case to be one of tumor of the kidney. On the other hand I have several times removed ovarian tumors which in the earlier stage of their existence have been diagnosed as floating kidneys. In fact there is one physician of my acquaintance who seems to have a wonderful power of finding cases of this abnormality, and I think that no less than three of my ovariotomies have been diagnosed by him as cases of floating kidney before they came under my care. I cannot do better perhaps than quote what is said concerning this by Mr. Spencer Wells in his lectures to the College of Surgeons :

"I do not think there is a specimen here, I doubt whether there is in the Museum, of what is called 'movable kidney.' I believe there have been only one or two such specimens shown after death at the Pathological Society, but I have occasionally seen tumors in the abdomen which appeared, as far as one could tell, to be instances of movable kidneys, either enlarged or of their natural size. One case is such a remarkable one that I will detain you a moment to tell you about it. A lady came to me believing she had, or having been told she had, a movable right

kidney. I saw her with Dr. Wilson Fox, and we both agreed that was the nature of the disease. For some years it went on, and I saw her occasionally under the impression that she was suffering from a movable kidney. Then she became pregnant, and pregnancy went on to its natural termination and a healthy child was born. Soon afterward she began to suffer from an ovarian cyst on the left side, the movable kidney still being on the right side; and the ovarian cyst increased so much that it was decided I should perform ovariotomy, which I did. I said at the time, 'I will see what that movable kidney is now;' and after I had removed the ovarian cyst on the left side I felt what the supposed kidney was and took it out, and then I found it was the right ovary, but it was attached by a pedicle fully a foot long. It had been held up under the right false ribs by the merest little film of adhesion. Quite a small patch of adhesion kept this right ovary, which was about the size of my fist and very much the shape of a kidney, just in the position of a movable kidney. The patient recovered and remained in good health."

Tumors of the kidney sometimes extend so low down that they can be felt in the pelvis, and they are often so movable, especially from side to side, that it is quite impossible to say whether they are or are not ovarian tumors. One such case was placed under my care by my colleague, Dr. Hickinbotham, and I opened the abdomen perfectly believing that I was about to deal with an ovarian cyst. I found, however, that it was a large cyst of the kidney which I could not remove, but I dealt with it by opening it, emptying it, and stitching the wound in its wall to the wound in the wall of the abdomen and fastening in a wide glass drainage-tube. The cyst suppurated freely. The patient was completely cured in about two months.

Now that abdominal surgery has made the great advances recorded in the last three or four years, I have little doubt that tumors of the kidney will by and by be dealt with as successfully as uterine tumors certainly, and perhaps even as successfully as ovarian tumors. The deliberate removal of the kidney by Simon, of Heidelberg, which was successful, and the removal of a large cystic kidney by Dr. Campbell, of Dundee, in mistake for an ovarian tumor, with an equally fortunate result, will probably in time lead the way to further triumphs. As far as my own experience goes I have only once removed an enlarged kidney, and in that case the success was complete, and I am glad to say that all my patients have recovered from the incomplete operations.

Enlargements of the spleen have been mistaken for ovarian tumors, but this is hardly so pardonable a mistake. The posi-

tion of the spleen when enlarged, the history of its growth, and the appearance of the patient in most cases are sufficient, I think, to prevent the mistake being made. Besides this there is one perfectly decisive sign in the case of a spleen which ought always be looked for and will always be found. It is that on the right side of the tumor may be felt a sharp edge under which the fingers may be passed, especially if the patient is asleep, and the tumor may then be tilted over on its own axis upward and toward the left. This edge, moreover, has a peculiar arrangement of festoons along its margin, the notches of which are from three to five inches apart, and when these notches are discovered there can be no doubt whatever of the tumor being the spleen; and finally, if there should still be doubt a microscopical examination of the blood will usually give abundant proof of diseased spleen by the excess of white blood corpuscles. I have opened the abdomen three times in the hope of being able to remove a splenic tumor, but have never yet been able to accomplish it. These three patients recovered from the operation and two of them have since been completely cured. Whether that was a result of the exploratory incision or not I cannot say, but the patients were subjected to no other treatment for months after the operation than they had experienced for some months before it. The third case is yet more remarkable and is still under observation. I opened the abdomen and failed to remove the spleen. Three weeks afterward a large abscess formed in the spleen; this I laid open, without any Listerian precautions, and inserted a drainage-tube. The amount of pus in this huge abscess was two and a half pints. The patient is slowly recovering and the spleen is shrinking. I know of two cases of removal of the spleen in this neighborhood both of which proved fatal; in one of them the organ was removed in mistake for an ovarian tumor.

Perhaps the least likely of all the conditions to which I have alluded as being mistaken for an ovarian tumor would be a tumor of the liver, yet I have to confess that in one instance I committed this curious error. The patient was sent to me from Leicester with all the signs of a large cystic tumor of the ovary, but when I opened the abdomen I found a large collection of ascitic fluid, due to a huge mass of soft cancer which grew by a narrow pedicle from the liver. The abdomen was closed and the patient was enabled to return home, where she died shortly afterward. I believe it would be quite possible in some extreme cases to mistake a hydatid tumor of the liver for an ovarian tumor. A year ago I operated upon a lady in whom the diagnosis of hydatids of the liver was made by Sir William Jenner, and I think one

less experienced than that distinguished physician might have taken the tumor for an ovarian cyst. It occupied the whole of the abdomen and distended it enormously, and the edge of the tumor could be felt in the pelvis. I opened the abdomen and then opened the liver, emptied between two and three gallons of hydatids, fastened the two wounds together by continuous suture, secured a drainage-tube well into the cavity, and succeeded in curing the patient. The case is published in the volume of the "Transactions of the Royal Medical and Chirurgical Society" for 1880, and since that time I have operated upon eight other cases of a similar character with perfect success in all. I have also opened the distended gall-bladder and removed gall-stones in three cases by a similar operation, and have in these cases also had perfectly successful results, so that we have a prospect of a remarkable extension of abdominal surgery in unexpected directions.

There is one condition of the ovary concerning which a very great deal of discussion has taken place, and upon which I am yet by no means clear that a satisfactory conclusion has been arrived at. I refer to the displacement which has been described as ovarian pregnancy. It is a question which has a much greater interest from a pathological than from a clinical point of view, because even if there be such a condition as ovarian pregnancy I do not see that the treatment required for it would be in any way different from that required in Fallopian pregnancy, which I have fully described in another chapter. Until the appearance of Velpeau's article in the "Dictionnaire de Médecine," most of the cases of extra-uterine pregnancy seem to have been regarded as instances of the actual impregnation and development of the ovum within the structure of the ovary itself, and it is very evident from the description of a number of cases of dermoid tumor of the ovary that these singular structures were also regarded as having this origin. In fact, I think I may say that between the incomplete development of a fœtus in the dermoid cyst to its complete development in the extra-uterine pregnancy, there does not seem to have been any clear distinction until about 1850. The assertion, therefore, that ovarian pregnancy does really occur is made by the great majority of authors without any really critical examination of the facts of the case.

I have spent a great deal of time in the investigation of the literature of the subject, and I am bound to say that I am sceptical as to the reality of the descriptions of any of the instances which have yet been given of this phenomenon. Spiegelberg, according to Schmiet, has established the authenticity of nine cases and has added ten more; but Dr. Parry contents himself

with merely saying that the weight of authority is in favor of the possibility of ovarian pregnancy. Further than this I certainly am not inclined to go, for if we consider for a moment the chain of circumstances which alone could lead to such an incident, we can readily understand, in the first place, how extremely rare its occurrence must be; and, in the second place, how difficult it is to prove what is absolutely necessary—that the ovum was developed within the follicle which it had never left. We must imagine, in the first place, that the spermatozoa had penetrated the whole length of the Fallopian tube, a circumstance which must be of extreme rarity if the views of the physiology of the oviduct which I have advanced be correct. We must then further see that this incident must have occurred at the time when the pavilion has embraced the ovary, and the embrace must have occurred at the spot where the follicle was about to rupture. After all this the most extraordinary incidents of the process must occur: the ovum, instead of leaving the follicle when the rupture takes place, must remain within it; the spermatozoa must enter through the rupture; the rupture must then heal; the ovum must become reattached to the epithelial lining of the follicle; the rupture heals, and then the development of the ovum must take place within the walls of its original home. Supposing the whole train of these extraordinary circumstances to have occurred, it would not be impossible to imagine that the follicle would distend as it does in the case of cystic growth, and that we should have a case of true ovarian pregnancy.

During the progress and development, and in many of the troublesome accidents through which all cases of extra-uterine pregnancy must pass, there would inevitably occur such adhesions and displacements as would make it most difficult to prove that the actual seat of the fœtus was within the tissue of the ovary. Among many other conditions which would have to be fulfilled the following are some of the most important. In the first place, of course it would be quite impossible to admit any case as one of ovarian pregnancy in which a most careful *post-mortem* examination had not been made, by a thoroughly competent observer. Then we should have to find that the uterus and both tubes were absolutely intact; that one ovary was present and that the other could not be accounted for save by its existence as the cyst of the pregnancy; and in the cyst wall of such a case there would have to be found microscopic evidence of ovarian tissue. In several cases of tubal pregnancy which I have dissected it was a matter of the utmost difficulty to find the corresponding ovary, even when it was perfectly clear that

the seat of the pregnancy was one of the Fallopian tubes. In one of my dissections I could not find the ovary, and yet that case was, with perfect certainty, one of tubal pregnancy. In Spiegelberg's paper there is only one case cited to which these tests apply with any degree of satisfaction, and therefore I give its details in full.

An abdominal section was performed under circumstances of great difficulty, and after peritonitis had been some time in existence the sac had become closely adherent to the great intestine and to the right wall of the pelvis. On both sides the tubes were normally distributed, but the left one, after a course of 7 ctm., disappeared in the walls of its broad ligament. The right tube extended 10 ctm. along the upper edge of the thickened, broad ligament toward a sac which was united by the *ligamentum ovarii* to the *ala vespertilionis* of the uterus; it had a diameter of 10 ctm. and was in a collapsed condition. After the tube had reached the sac it could be traced along its surface for a distance of 22 ctm. and was permeable for a distance of 12 ctm., and in the remaining 10 ctm. of its length it disappeared as a narrow, smooth band on the outer surface of the sac. There was in this neighborhood a small dermoid cyst in the wall of the sac without any distinct boundary. The sac itself had two layers, the outer of which was thick and firm, and the inner one fine and delicate, these two being capable of easy separation. The inner layer was clearly the chorion, for over its greatest part it had the structure of placenta, which was thickest at the bottom of the sac and thin at the upper part.

Spiegelberg therefore concludes that the right ovary was the bag containing the child. He could find no ovary on the right side, but he found distinct ovarian elements in the outer wall of the sac. It must here be pointed out that, in the first place, the *post-mortem* examination is admitted to have been not very efficiently performed, and the description given of the tube makes it, I think, quite as likely that it was a case of pregnancy in the broad ligament which resulted in the rupture of the tube on its lower aspect—that being the most common variety of the tubal pregnancies which are not fatal in their early rupture—as that it was a case of ovarian pregnancy. The fact that there was present an ovarian tumor is proved by the existence of a dermoid cyst. This would account for the somewhat wide distribution of ovarian elements in the wall of the sac, and as Spiegelberg does not claim to have found ovarian elements all over the wall of the sac, I think we may be quite justified in being somewhat sceptical even about this case; though I frankly admit that the eminence of the observer and the manifest care with which all

his records are given make it quite possible that his conclusions are correct. Since this was written, I regret to say, Prof. Spiegelberg has passed over to the majority.

In a paper published by M. Puech upon this subject he describes a case in which the left Fallopian tube, like the right, was fixed behind the ovary by adhesion, but had remained permeable. Its pavilion was closed in great measure, but not completely, and admitted a probe. The left ovary measured 46 mms. long, 26 mms. broad, and 18 mms. thick. It contained Graafian follicles of various degrees of development, the largest being 8 mms. in diameter. On its outer extremity was a rounded body about the size of a large cherry, its largest diameter being 20 mms., while its smallest was 12 mms. Its envelope was transparent and furnished with well-marked reticulated vessels. At one spot a deep violet coloration was seen over a space about the size of a lentil, and around this the envelope was thickened. Over most of the rest of the surface a yellowish substance could be seen through the translucent envelope. On opening the cyst with scissors a prominence with a villous surface was found attached at the area of coloration, while over the rest of the surface a layer ½ mm. thick could be easily separated from the cyst wall. The villous prominence was furnished with large vessels, and formed a semi-ellipsoid measuring 11 mms. by 10 mms. On incising this with cataract scissors it was found to contain a cavity distended by a clear fluid, and in the fluid floated an embryo in the form of a vermiform body 1 m. long, curved in the middle and swollen at one extremity. It was enveloped in an excessively delicate membrane by which it was fixed to the presumed chorion.

Now of course the whole conclusion in this case depends upon the assumption that this vermiform body, only 1 m. long, was an embryo. It may have been one, but certainly there is no proof advanced in favor of this view; and although I am by no means prepared to deny its accuracy, I am certainly very doubtful about it. If it was an embryo it could only have been one of a few hours' existence, and one could hardly expect to find the machinery of the whole process so defective that the pavilion of the tube—the most important part of the whole machinery—was so damaged as to be, according to M. Puech's description, almost closed and fixed behind the ovary by adhesion. One would have at least expected this adhesion to have been over the seat of the rupture, and yet it is distinctly stated not to have been so. I have seen so many queer looking things in ovarian cysts and follicles that I am not inclined to admit that this vermiform body has been shown conclusively to have been an embryo.

In another case, recorded by Walter in the *Monatschrift für Geburtshulfe* (vol. xviii.), there is a description of a cyst which had ruptured and allowed the fœtus to pass into the abdomen. The cyst is stated to have been an ovary, but the conditions of the description are not sufficiently exact for this to be admitted, though it is stated that the cyst was free from adhesions to surrounding parts and was free from the pavilion of the corresponding tube. On the whole, I am inclined to regard the evidence advanced so far concerning the alleged occurrence of gestation within the proper tissue of the ovary as by no means complete; and while I am not prepared categorically to deny its occasional occurrence, as I did in a previous edition of this book, yet I cannot admit that it has been proved. Even if it does occur it would, as I have said, possess little more clinical importance than a case of tubal pregnancy, a differential diagnosis of it could not be made, and it would have to be treated on the same principles as the other varieties of extra-uterine pregnancy.

An extremely interesting contribution to the comparative pathology of the ovary has been made by Mr. Henry H. Slater, in the *Journal of Anatomy and Physiology* (vol. xiii.), concerning an ovarian tumor found in a hen pheasant. It measured when fresh 2.3 inches in greatest length, 1.1 in breadth, and the same in depth. It was very irregular in form, and was roughly divided into three principal lobes, which were united at their bases, and were subdivided into many smaller lobes; the whole presented the general sulcated appearance of a human brain on a smaller scale.

The ovary seemed to be almost entirely absorbed; nothing resembling its usual granular appearance was visible; and the tumor rested consequently directly upon the kidneys, a fold of peritoneum alone intervening. Though no ovary was visible, the left oviduct was convoluted as much as it would be in the earlier part of the breeding season, but this, of course, was not healthy excitement, but due to the inflammation of the organs. The right oviduct presented its usual aborted appearance.

On making a transverse section of the tumor, the interior was seen to be quite solid, and in no degree cystic or alveolar; nor was there, as might have been thought likely, any tendency to a concentric growth, but, on the contrary, bundles of fibres were seen faintly to radiate from the point of attachment of the tumor.

The minute anatomy was difficult to determine, owing to the necessity of employing a high power. There were visible fat-cells (by far the largest), granule-cells, and nucleated cells,

which I regard as resembling those found in tubercle—the last named being very numerous and irregular in shape and size. Interspersed were minute fibres, but these were rare; and lastly, diverging from the base of attachment of the tumor, were bundles of fibres, very irregular in shape. To these is due the radiated appearance of the section. They are unstriated muscular fibres, and seem to be the only remains of the original ovary.

This diseased ovary is only the most conspicuous part of a general tubercular affection which pervaded the whole of the viscera—the liver, pancreas, omentum, and intestines being all distinctly more or less involved.

I was under the impression that the tumor was cancerous, from the great resemblance under the microscope to medullary cancer of the human ovary; and my thanks are due to Professor Turner, who was kind enough to suggest that the tumor was probably tubercular, the correctness of which opinion was at once verified by means of dilute acetic acid.

CHAPTER V.

OVARIOTOMY.

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It is characteristic of every advance which has been made in surgery, as in all other human affairs, that the early phases are developed with extreme slowness, so that in perusing the history of any surgical progress it is almost certain that we should have to go a long way back to see the first germs of the movement. I have, I believe, examined very exhaustively the literary history of ovariotomy, but I can find no record of any deliberate attempt to relieve a patient from ovarian dropsy by opening the abdomen earlier than the year 1701. It is a singular and striking illustration of the slow progress made by any new idea, that, so late as 1861, an operation which was successfully performed by Robert Houstoun, of Glasgow, one hundred and sixty years before, should have been characterized by one of the leading surgeons of his time as an operation which ought to subject its performer to a criminal indictment for manslaughter. I can, however, well remember, in the days when I was a student at Edinburgh, one of the favorite subjects for discussion in the students' medical societies was the doubtful point of ovariotomy being a justifiable operation, and now, when I find my mortality varying from three to five per cent., it sometimes seems to me as if these discussions were but ill-remembered dreams; for it must be admitted there is no important operation which has received the same hostile criticism, the same searching scrutiny, or finally has achieved the same triumphant admittance as ovariotomy. Its critics have subjected it to a statistical ordeal through which no other surgical operation has passed, and it is the only one which has achieved a legitimate and complete establishment.

The merit of being the first to utter an opinion in favor of the radical cure of ovarian dropsy has usually been ascribed to William Hunter, but Houstoun's operation was performed seventeen years before Hunter's birth, and so far as the history of this question has been unearthed, I think the whole merit must be

given to the Glasgow surgeon. So important do I regard this as being the first case, that I give at length what information I have been able to obtain concerning this most interesting man and the details of his operation. For much of this I am obliged to Mr. Alexander Duncan, of Glasgow.

Robert Houstoun was the son of a Glasgow surgeon of the same name, who held the office of Visitor of the Faculty of Physicians and Surgeons of Glasgow in 1669, and again in 1677, this office being virtually that of President of the Surgeons. Robert Houstoun the younger was regularly apprenticed to his father in 1665, this being before the days of the existence of any medical schools in Scotland, and it is probable he had few other opportunities of making himself acquainted with the surgical art than those afforded him by his seven years' apprenticeship to his father. At its expiration he became a member of the Faculty and began to practise in Glasgow as a "surgeon-apothecary," or general practitioner.

In 1691 he was himself elected Visitor of the Faculty, an honor which was more than once renewed, so that it becomes perfectly clear that in his native city he was at a very young age a person of distinction, and probably an operating surgeon of large experience. In 1697 he took an active part in forming a collection of medical works, which was the nucleus of the present library of the Glasgow Faculty. The "list of such worthie personnes" as contributed to this collection, with the names of the works gifted by each, has been preserved, and under Houstoun's name stands a considerable list of donations. Of these some are no longer in the library, but others of them, bearing his autograph, are still to be found on its shelves.

In 1711 he appears to have contemplated the somewhat risky step of retiring from general practice and limiting himself to the work of a physician. Houstoun's is the first case in Glasgow of such a practice, and his example was followed by only some three or four others in the course of the eighteenth century, of men withdrawing into the quiet of purely consulting practice after having made their positions as general practitioners. To accomplish this object of his ambition Houstoun required to obtain the degree of Doctor of Medicine, a distinction which had been acquired by three or four of his townsmen by residence abroad, usually at Utrecht or Leyden. Houstoun, however, resolved to gain his degree from the University of his native city, which had the power of granting such degrees even though it had no medical school, and was entirely destitute of a medical faculty. In 1711 he applied to be admitted to examination for the degree, but it does not appear that the University authorities cared to com-

ply with his request, for we find in the Archives of the University a minute dated 31st December of that year, that "Mr. Robert Houstoun, surgeon, who sometime agoe, applyed for the Doctorat in Medicin did still insist that he might be examined in order to his graduation. The Faculty considering that they might still want Professors of Medicin doe appoint some of the Physicians in the city to assist at the examination." Before this extra-academical board Houstoun passed his trials successfully on January 3, 1712, and Mr. Duncan has favored me with an extremely interesting extract from the records of the examination of this remarkable candidate.

Shortly after graduating, and probably between the years 1713 and 1715, Houstoun seems to have found the field offered for his ambition in Glasgow too restricted, and he was induced to attempt the larger field of the English metropolis. In London it is probable that he practised as a general practitioner, for in the thirty-third volume of the "Philosophical Transactions," page 388, he mentions that he was engaged in the practice of midwifery, and of course no London physician would be so engaged at that time. All his published works tend, however, to show that the bent of his mind still lay in the direction of surgery, and chiefly of abdominal surgery. In 1720 he published a controversial pamphlet entitled "Animadversions on a late Pamphlet entitled Lithotomia Douglasiana," and in 1726 appeared his "History of Ruptures and Rupture Cures."

In 1722 he contributed to the Royal Society "An Account of a Case of Extra-Uterine Fœtus, taken out of a Woman after Death." In this case it is extremely noteworthy that he had proposed to operate during the life of the patient, and his offer had been declined; had it been accepted, this remarkable man would have ranked as the first operator in such a case, and it is possible he might have been the first successfully to interfere in this displacement. In 1724 he read before the Royal Society the case upon which his reputation was chiefly founded, and which formed the first case of ovariotomy. For this paper he was elected a Fellow of the Royal Society in the following year. He died in London, on May 15, 1734, about the age of seventy, so we may estimate that his birth took place about the year 1654. Unfortunately, I have not been able so far to obtain any account of his life and doings while in London, save that he practised somewhere about what was then the West End. His case of ovariotomy is entitled "An Account of a Dropsy of the Left Ovary of a Woman aged Fifty-eight, Cured by a Large Incision made in the Side of the Abdomen by Dr. Robert Houstoun," and the following is the gist of his description of his operation:

"I found this tumor grown to so monstrous a bulk, that it engrossed the whole left side from the umbilicus to the pubes, and stretched the abdominal muscles to a great degree. It drew toward a point. From being obliged to lie continually on her back, she was grievously excoriated, which added much to her sufferings, which, together with a want of rest and appetite, had greatly emaciated her.

"The operation of puncturing the abdomen being proposed, she consented: accordingly, with an imposthume lancet I laid open about an inch, but finding nothing issue I enlarged it two inches, and even then nothing came forth but a little thin, yellowish serum, so I ventured to lay it open about two inches more. I was not a little startled after so large an aperture to find only a glutinous substance bung up this orifice. The difficulty was how to remove it; I tried my probe and endeavored with my fingers, but it was all in vain, it was so slippery that it eluded every touch and the strongest hold I could take.

"I wanted in this place almost everything necessary, but bethought of a very odd instrument, yet as good as the best in its consequence, because it answered the end proposed. I took a strong fir-splinter, such as the poor in that country use to burn instead of candles; I wrapped about the end of this splinter some loose lint, and thrust it into the wound, and by turning and winding it I drew out above two yards in length of a substance thicker than any jelly, or rather like glue fresh made and hung out to dry; its breadth was above ten inches; this was followed by nine full quarts of such matter as is met with in steatomatous and atheromatous tumors, with several hydatides, of various sizes, containing a yellowish serum, the least of them larger than an orange, with several large pieces of membrane, which seemed to be parts of the distended ovary. I then squeezed out all I could and stitched up the wound in three places, almost equidistant; I was obliged to make use of Lucatellus' balsam, to cover a pledget, the whole length of the wound, and over that laid several compresses, dipped in warm French brandy; and because I judged that the parts might have lost their spring by so vast and so long a distention, I dipped in the same a napkin four times folded and applied it over all the dressings, and with a couple of strong towels which were also dipped, I swathed her round the body, and then gave her an opiate medicine, which was ordered to be repeated at intervals.

"She afterward mended apace, to the admiration of every one, and lived in perfect health from that time, which was in August, 1701, till October, 1714, when she died in ten days' sickness."

It may be said that Houstoun began this operation with the

intention of doing little more than relieving the patient by a tapping, but I presume that the first man who put a kettle on the fire had no intention of evolving a steam-engine from the simple process of boiling water; yet the discovery of how to boil water was by far the most important of all the phases through which the invention of the steam-engine has passed. There can be no question from Houstoun's description that he had diagnosed a dropsy of the ovary and that he had to deal with a condition which is often one of the most difficult that can be met with in the performance of ovariotomy, and he completed his operation by removing the cyst. Although he does not describe his division of the pedicle, or his having tied it, it is almost certain that he did both. He certainly must have seen and divided the pedicle, for he describes the disease as being of the left ovary, therefore he saw the pedicle. Perhaps he tore it and it did not need tying. That he performed a complete ovariotomy is certain, from his having noticed secondary cysts as well as from the recovery of his patient and the fact that she lived for thirteen years afterward, in perfect health.

William Hunter and his brother John Hunter must have known of Houstoun's case, for they were born and brought up in the district in which it occurred, and William Hunter lived for years in the city near which it was performed. They both advocated the theory of the performance of the operation, and John Hunter is reported to have said: "I cannot see any reason why, when the disease can be ascertained in an early stage, we should not make an opening into the abdomen and extract the cyst. Why should not a woman suffer spaying as other animals do? The merely making an opening into the abdomen would never be followed by death in consequence of it?"

Their friend John Bell, who practised in Edinburgh from 1790 till 1816, also pronounced in favor of its performance, but he is not known to have done anything toward trying it himself, and it is to a young Scotchman,[1] who was a pupil of John Bell's in 1793, that we owe the revival of the operation and its performance upon a scale which amounted to that of a legitimate experiment. Ephraim McDowell has been honored by the medical profession in America as the "Father of Ovariotomy," and whether we admit the accuracy of the title or not, there can be no doubt that it was in the backwoods of Kentucky that abdominal surgery received one of its greatest impulses. In 1809 the

[1] My American readers may object that McDowell was not born in Scotland. Of this, however, we are not yet clear. At any rate, his father and mother were Scotch, and at the time of his birth, 1771, the States did not exist.

second ovariotomy was performed successfully and the patient survived it thirty-two years. In 1817 Dr. McDowell published an account of these and of two other cases he had performed, and, as might be expected, his statements were received with general incredulity. The editor of the *British and Foreign Medical and Chirurgical Review* was bold enough to distinctly deny the credibility of McDowell's statements, but in 1827, when the authenticity of the accounts had been established beyond doubt, he had the manliness to "beg pardon of God and of Dr. McDowell of Danville" for his hardiness. The operation was performed twelve times in all by Dr. McDowell, with a mortality of thirty-three per cent., and in one other case he failed to complete the operation.

Lizars tells us that about 1816 Dr. McDowell sent his manuscript to John Bell, at Edinburgh, for his perusal. At that time the great surgeon had gone to Rome, suffering from his fatal illness, and the manuscript was read by John Lizars, who was doing John Bell's work. This accounts for Lizars being the first to follow in Houstoun's footsteps in Scotland, and it affords a curiously sequent history of the early phases of this notable surgical success.

In 1822, Nathan Smith, of New Haven, performed an operation successfully, and in 1823 Lizars made his first attempt, but unfortunately he had made a mistake and there was no tumor. Nathan Smith's case was undoubtedly one of parovarian cyst, and the operation was therefore not an ovariotomy at all. It is notable, however, for the facts that he used the short incision and the short ligature. The curious fact that so many of these early successful cases were parovarian tumors makes me believe that a great many more true ovariotomies were done, of which there are no records, for they probably all died. Parovarian cysts now are removed without any risk at all. I have never lost a single case. It is therefore very likely that they formed the early successes. On February 27, 1825, Mr. Lizars removed an ovarian tumor successfully, using the long ligature. On March 22d of the same year he removed another, using the short ligature, but his patient died. In his fourth case he could not remove the tumor, but the patient recovered from the incomplete operation. He concludes his account of these cases with the remarkable sentence: "From these cases, it appears that there is little danger to apprehend in laying open the abdominal cavity; and that in diseased ovarium, extra-uterine conceptions, *fœtus in utero*, with deformity of the pelvis preventing embryulcia, aneurism of the common or internal iliac arteries, or of the aorta, volvulus, internal hernia, and foreign bodies in the

stomach threatening death, we should have recourse early to gastrotomy. The delay in such cases is more dangerous than the operation." It has taken fifty years to establish the justice of this opinion.

Dr. Granville, of London, operated twice in 1827, and it is generally stated that both of his cases were unsuccessful, but in a volume of notes made by the late Dr. T. H. Tanner, now in my possession, and written in his own remarkably neat writing, I have found a note to the effect that one of Dr. Granville's cases, performed March 21, 1827, was successful, but I do not know upon what authority this note is made.

For some ten or twelve years after the death of McDowell, and after the failures of Lizars, ovariotomy seems by common consent to have been discontinued. In March, 1836, Dr. Jeaffreson, of Framlingham, removed a parovarian tumor successfully through an incision only an inch and a half long ("Transactions Provincial Medical Association, 1837"), and it is an interesting fact that Mr. R. C. King, of Saxmundham, assisted at this operation, for he shortly afterward described two cases of successful removal of similar tumors. In 1838, Mr. Crisp, of Harleston, and Mr. West, of Tunbridge (*Lancet*, 1837–8), also had successful cases, but they were clearly all parovarian, and not ovarian tumors. On November 6, 1842, Mr. D. Henry Walne performed three operations, all of which were successful, and published them as ovariotomies; but, singularly enough, in not one of these cases was the tumor removed an ovarian cystoma. Mr. Walne gives a figure of the first of his cases, and he so thoroughly describes the appearances of the other two that no doubt can exist that they were parovarian cysts, and it is not clear whether he did or did not remove the ovaries with them. On October 19, 1843, he removed a tumor which was undoubtedly of ovarian origin, but unfortunately the patient died. His method of operating was very curious, and to us now would seem very ghastly, but still he deserves the credit of a pioneer. He tells us his incisions were fourteen or fifteen inches long, and that they were extended bit by bit until the tumor slipped out of them; that is to say, a parovarian cyst, which might have been removed by a two-inch incision after being tapped, was allowed to deliver itself, with its wall unbroken, through an incision which was made large enough to accommodate it.

On September 27, 1842, Dr. Charles Clay, of Manchester, who may in all truth be regarded as the "Father of Ovariotomy" as far as Europe is concerned, performed his first operation for the removal of a diseased ovary. He had, on the twelfth of the same month, performed another operation, but here again there is

abundant evidence to show that a parovarian was mistaken for an ovarian tumor. Previous to September, 1842, we have therefore records of only two ovariotomies, properly so called, in this country, those of Houstoun and Lizars.

In 1843, Mr. Aston Key removed both ovaries, and Mr. Bransby Cooper also tried the operation in that year, but it was not till 1844 that there was a successful case in London, operated on by Dr. Frederick Bird, followed by one in the practice of Mr. Lane. In the provinces, however, many successful cases had been done, and the metropolis was, not for the only time, behindhand. Dr. Clay continued to operate with very remarkable success for many years until he had performed three hundred and ninety-five operations with one hundred and one deaths, his total mortality being therefore about twenty-five per cent. His operations were witnessed and alluded to by some of the most distinguished practitioners of the time, and yet Clay has received an amount of adverse criticism, and his statements have been received with an incredulity which is as undeserved as it is unworthy of those from whom it came, and which arose solely from the fact of his being a provincial surgeon. Looking back upon the work of a generation now almost passed, from a standpoint altogether free from personal bias, I have no hesitation whatever in ascribing to Dr. Clay by far the larger share of the credit which arises from the enormous advances made in abdominal surgery during the last forty years. It is quite true that McDowell was the first to do a number of ovariotomies, and it is equally true that Houstoun was the first successfully to remove a diseased ovary, but it was Clay, of Manchester, who first showed that ovariotomy could be made an operation more justifiable by its results than any of the major operations of surgery. His methods were imperfect, as are the methods of all pioneers, but it was upon his work that the foundation was laid for all those brilliant results we now attain. I say this with all the more readiness now that Dr. Clay is far advanced in life, and that but lately a most unfair and ungenerous attempt has been made to deprive him of his just merit, though perhaps I might have left it unsaid, as Dr. Clay has shown himself quite capable of his own defence. In Sir J. Y. Simpson's Lectures, published in the *Medical Times and Gazette*, 1859–61, there is the following evidence on behalf of Dr. Clay: "Indeed, the revival of it is principally due to the exertions and example of Dr. Clay, of Manchester, who has himself operated now in ninety-three cases." Bryant, in his book on "Ovariotomy" (1867), entitles Dr. Clay "the first great apostle of ovariotomy in this country." Peaslee remarks in his work on "Ovarian Tumors," "to him, more than to all other

operators the credit belongs of having placed the operations of ovariotomy, on a sure foundation."

One of the most conspicuous defects of his method of proceeding was the employment of what is called the long ligature—that is to say, he tied the pedicle, returned it into the abdomen, and left the ligatures hanging out of the wound, as was done by McDowell and Walne. Had he cut these ligatures short, and completely closed the wound, I have no doubt that instead of having a mortality of twenty-five per cent. it would speedily have fallen to six or eight per cent.

The great improvement which was effected upon Clay's method was that which, curiously enough, had been employed twenty years before by Nathan Smith, but which was neglected until reintroduced and firmly established by the late Mr. Baker Brown, and to this most able but most unfortunate surgeon I unhesitatingly award the position of having achieved the second great advance in abdominal surgery. He began to operate in 1851, and his career ended in 1867. He established the short incision, the intraperitoneal method of dealing with the pedicle, the use of the actual cautery for its treatment, and the complete closure of the abdominal wound. Between May, 1865, and September, 1867 (that is, during the time when he employed the cautery), he performed forty operations upon these principles, with four deaths, or a mortality of ten per cent. During the same period Mr. Spencer Wells operated one hundred and one times with twenty-six deaths; or a mortality slightly over twenty-five per cent. Verily ovariotomy would have had a very different history during the last fourteen years if Mr. Baker Brown had not fallen a victim to his own folly, or professional jealousy—for opinions differ very much as to the cause of his death. His methods, again, were in many respects faulty, and have since been immensely improved upon; but I give these details concerning the result of his practice because here again a most unfair attempt has been made to deprive a man of the credit he has deserved; and however much Mr. Baker Brown's actions in other respects are to be deplored, and however much he may or may not have deserved his sudden and disastrous downfall, he does deserve to rank second in order of English ovariotomists.

The following letter from Dr. T. Keith (*British Medical Journal*, July 31, 1880) gives very important evidence upon this point in the history of ovariotomy:

"Simple experience with the clamp alone did little to diminish this mortality; for, in Mr. Wells' published eight hundred cases, the death-rate in the last three hundred was greater than in the preceding three hundred. His results by the dropped liga-

ture were even worse—thirty-eight per cent. All over the frightful mortality of one in four continued !

"For some time past it has seemed to me that, had Baker Brown lived, the history of this operation since 1864 would have been different. His own method of dealing with the pedicle by the cautery at once lowered the mortality to one-half of that with the clamp, and it was becoming practised in London when illness came to him, and death. The man and his method were quickly forgotten; no one would have the lesson his work gave. All were strangely blind in those days to its value. Should I not rather say, we were all strongly prejudiced ? In truth, there is no more startling page in surgical history than that in which his latest results are given. On one page we have almost nothing but failures; on the other, by a simple change in the method of operating, an almost uninterrupted line of success. During the whole of his professional life he seems to have tried hard to cure ovarian disease. From 1851 to 1864 he made many efforts and tried many ways, all in vain, till he adopted the cautery. His published results show a mortality of less than one in ten in completed cases. I have read somewhere that he lost but four of his last fifty operations. Some years afterward—unable to get my mortality much under the one in five, for I was then ignorant of drainage—I took to Mr. Brown's method in a sort of despair. For a time it was used irregularly, and only in the worst cases, or in those not favorable for the clamp. The results of the first fifty cautery cases, published in the *Lancet*, gave a mortality of less than one in twelve—eight per cent., and the results that followed were much better. Mr. Wells and Mr. Thornton have lately given their statistics of cases performed under careful antiseptic treatment, and with all the other improvements of these later years, and the mortality *is nearly eleven per cent.*

"So much for Mr. Baker Brown's as compared with the other methods. But, after all, what concerns us most now is, by whose method may ovariotomy be performed with the least risk to the patient ? Surely the one that gave us a death-rate of less than eight per cent., long before antiseptics were heard of, is the one to trust to now—such, at least, is my experience. The cautery alone gave the best results of all the methods before. It gave better results fifteen years ago than any other method can yet show with antiseptics. Helped by drainage—for where would the antiseptic system be without drainage ?—it gives the best of all results with them. Ninety-eight of my last hundred cautery operations have recovered, and in one of the two fatal cases the tumor was malignant with cancerous matter in the pelvis, practically an incomplete operation.

"Have I not reason, therefore, for saying that, had Mr. Baker Brown lived, the history of ovariotomy since 1864 would have been changed; and that, in making his calculations, Lord Selbourne would have had to add three times the number of years to the lives of women saved by ovariotomy?"

In 1858 Mr. Spencer Wells began his work, and in his third operation he adopted the clamp as a method of dealing with the pedicle, and this he retained in all suitable cases until about 1878. During this time he performed 627 operations with the clamp, having a mortality of 20.73 per cent. During the same period he operated 157 times with the ligature, with a mortality of 38.2 per cent., and on this point it is to be noted that he retained the intraperitoneal method only for those cases to which he could not apply the clamp. I mention this here merely to indicate my opinion that the introduction of the clamp was a decidedly retrograde step in the history of ovariotomy. When I began my own practice, in 1867, I employed the écraseur, a variety of the intraperitoneal method, and my results over a limited experience were extremely good. Like others, however, I was so impressed with the overwhelming experience of Mr. Wells, that I resorted to the clamp, and my results with it were so bad that its employment will ever be to me a matter for bitter and lasting regret.

In his lectures before the College of Surgeons Mr. Wells gives the following account of his results:

"With regard to the proportion of deaths to recoveries, taking my own cases only as a starting-point, of the 500 cases published in my book, 373 recovered and 127 died—a mortality of 25.4 per cent of the 300 subsequent cases, published in 1873, in the paper at the Royal Medical and Chirurgical Society, 223 recovered and 77 died—a mortality of 25.6 per cent. Since the 800 cases, I have now operated on exactly 100 more, making a total of 900 complete operations. Of the last 100, 83 recovered and 17 died—a mortality of 17 per cent. Adding the whole 900 cases together we have 679 recoveries and 221 deaths—a mortality of 24.5 per cent. It is satisfactory that in the last series of 100 cases the mortality is the least."

In the last hundred cases I believe some seventy-five or eighty were dealt with by the ligature, and nothing could be more condemnatory of the clamp than such figures, which show that after using it more than six hundred times in selected cases, Mr. Wells could not bring his mortality below twenty per cent., whereas, fourteen years before, Mr. Baker Brown had a mortality of only ten per cent. with the intraperitoneal method.

In 1862, Dr. Thomas Keith began his operations in Edinburgh, and he speedily found, as he tells us, that the results obtained

by the clamp were extremely bad, and he reintroduced, and by his brilliant work has completely re-established, the intraperitoneal method of Baker Brown. With the clamp his mortality was 19.2 per cent., which is close upon that of Mr. Wells, 20.73; with the cautery, on the other hand, out of 156 cases Dr. Keith has had a total mortality of only 3.85 per cent., and in addition to this it can be shown by his constantly diminishing mortality that with each series of operations his increased skill diminished his death-rate, so that in his fifth series of fifty cases he had a mortality of only eight per cent. No such progressive improvement is seen in Mr. Wells' 627 cases of clamp treatment.

Just as this is written I have completed a series of a hundred cases performed without any of Mr. Lister's so-called antiseptic processes, and in all of which the pedicle was treated by the "Staffordshire knot." Only two of these hundred cases have proved fatal, and in both cases death was due to the fact that they had been repeatedly tapped.

Dr. Keith (*British Medical Journal*, October 19, 1878) attributes his success to four conditions, of which he speaks as follows:

"1. To drainage of the abdominal cavity in severe cases by a large perforated glass tube going to the bottom of the pelvis. It is to Koeberlé that I am indebted for the idea. He kindly gave me two of his small tubes in 1866. These were soon found to be too narrow and too short. They got easily choked with clot or lymph. For the last ten years, I have used the large glass tubes now in common use. Till I had learned in what cases to drain, the tube was used in alternate cases of the severe operations. I am as certain as I am of my existence, that had I used them earlier and oftener the mortality would have been less by one-third. These tubes I supplied to ovariotomist friends in all parts of the world, though no one used them, so far as I know, till attention was called to drainage by the vagina by Dr. Marion Sims—a method which seems to me to be one calculated rather to give rise to blood-poisoning than to save the patient from it. It is remarkable that the only year in which the mortality of the Samaritan Hospital fell to 10 per cent. was in 1876, when drainage by these glass tubes was first generally used. 2. To the use of the cautery in dividing the pedicle, as proposed and practised by the late Mr. Baker Brown. How the lesson given by his last results has been so systematically ignored in London has always been a marvel to me. 3. To the employment of Koeberlé's compression forceps, in large numbers, whereby loss of blood is prevented. His model is still the best, notwithstanding the clumsy imitations of it lately invented. 4. To the substitution of ether for chloroform in my last two hundred and thirty operations,

whereby the after-vomiting is avoided, and the risk of hemorrhage when the wound is closed diminished. All these things have, I think, helped to lessen the mortality, but the drainage and the employment of the cautery in the division of the pedicle have contributed most."

"I wish, for the credit of my small hospital, which I carried on almost entirely at my own expense, to make this statement of results distinctly; and I would not make it prominent now, but that year after year the authorities of the Samaritan Hospital proclaim in their reports, in the largest of Roman letters—though one of the surgeons tells me that he has objected to the statement in vain—that the results got there are always the best that have yet been obtained, the mortality of the Samaritan Hospital down to the end of 1876 being nearly one death in every four operated on; of the last five corresponding years, one in five."

At this period the history of ovariotomy entered upon a new phase by the introduction and application of what is called the antiseptic theory and the Listerian method of putting this theory in practice. Keith's results without this method were so brilliant as to put all other efforts in the shade, and, startled by them, with one consent we followed in his footsteps. In my own practice the mortality fell from twenty-five per cent., which seems to be the normal rate following the use of the clamp, to seven or eight per cent., which seems about the rate possible when Baker Brown's practice is followed. An attempt was made by Mr. Spencer Wells and his assistant, Mr. Thornton, to cover their retreat from the use of the clamp by claiming for the Listerian method the merit of the reduced mortality; but in the proper place I shall give reasons which have fully convinced me that it is to Mr. Baker Brown and to Dr. Keith, but certainly not to Mr. Lister, that we owe our recent and most brilliant results in this department of surgery.

In the preface to Mr. Wells' first book on "Diseases of the Ovaries," and again in his second edition in 1878, there is the following sentence. "Dr. Clay had steadily continued in the career which he began in 1842, but his operations not being performed in a hospital, before numerous professional witnesses, and no connected series of his cases being published, his example had but little influence."

These words are again used in an anonymous article upon the history of ovariotomy in the *British Medical Journal* for July 17, 1880, and I think they give a very unfair representation of the value of Dr. Clay's work. They convey the impression, whether intended or not, that the writer considers Dr. Clay's

statements concerning his cases are not to be trusted, but for my own part I am perfectly satisfied that there is not the slightest reason for any such aspersion. It is quite true that Dr. Clay's cases were performed in private practice, but if this is to be a ground for suspicion of their authenticity, then at least half of the contributions made to medical literature may be equally doubted. We might just as well turn round and doubt the cases which are credited to Dr. Ephraim McDowell; and the statement made by Mr. Wells that Dr. Clay did not publish any "connected series" is not correct, for he not only published a pamphlet, from which I have quoted, and of which I possess a copy, but in 1857 he published a table in which he gave the results of fifty-one operations. In his first twenty there was a mortality of forty per cent., in the second twenty about thirty per cent. mortality, while in the last thirty-one operations it fell to twenty-five per cent., and that mortality was maintained by Mr. Spencer Wells for twenty years after the publication of what I am now quoting from. Further, in Dr. Clay's publications and in the publications of others, it is made perfectly clear that he had performed his operations before "numerous professional witnesses."

There can be no doubt, therefore, that Dr. Clay's example emboldened others to follow in his footsteps, and to him must be given the first rank amongst English ovariotomists for the revival and complete establishment of this most important operation. It must be borne in mind that, at the time in which Dr. Clay practised, the exact method of recording cases, particularly of ovariotomies, which now obtains, had not been introduced in any department of practice, and even now it is almost confined to abdominal surgery. No one seems to have published any cases with the idea that at some later date a carping critic might rise to assert that such a case had never existed. This is a kind of argument which has received an almost universal condemnation amongst honorable men, and it is one which can only be made by insinuation, and its authors dare not give it by direct statement. To Mr. Spencer Wells must be given the credit of having introduced the exact statistical method, and this there can be no doubt had a very large influence in consolidating both professional and public opinion as to the propriety of performing such operations; for it was not till then was there any possible answer to such criticism as that directed against Dr. Clay. But that Dr. Clay did not provide against that kind of criticism, which he probably never expected, forms no justification for the conclusion that his statements concerning his operations and their results are not quite as correct as those of Mr. Wells or Dr. Keith.

What I have to say of the history of ovariotomy may be concluded by an expression of opinion that the record of its progress passes from Clay and Baker Brown to Keith, passing over altogether the unfortunate interregnum of the clamp as something deeply to be regretted. Not only by the re-establishment of the intra-peritoneal method, but by the successful teaching of the necessity for the complete cleansing of the peritoneum and the occasional use of the drainage-tube, Keith has earned the lasting gratitude of humanity.

The treatment of ovarian tumors by therapeutics need not be discussed, further than to say that it is limited to the administration of tonics to sustain the functions of the patient, or to correct some errant condition which might diminish the chances of success for the surgical treatment of the case. Sometimes we are the victims of singular coincidences, which seem to militate against the general experience in this matter. Some years ago I was consulted by a woman with an enormous unilocular tumor, whose husband declined all operative measures. Some months afterward she received from the hands of a physician some inert *placebo*, and soon afterward the cyst ruptured and its contents were absorbed. For nearly five years she remained perfectly well and then the tumor reappeared. She was admitted into a large general hospital, was operated upon and died in a few days. The tumor proved to be, as I had anticipated, a parovarian cyst.

For the cure of an ovarian cystoma there is nothing known to have the slightest influence save an operation for its removal, and those patients who unfortunately are led to believe that some drug or other, or some fanciful form of treatment will relieve them from the necessity of an operative ordeal, are only induced to waste time which is valuable, and to run risks which may be avoided. On this subject Mr. Spencer Wells observes: "But I would also say that if the operation be delayed for a time, she should not be subjected to any useless treatment: that it is quite useless to attempt, by iodine, or bromine, or lime, or by gold, or by any other remedy, to attempt to diminish the size of the tumor or to check its growth. All that is quite useless, and might be very injurious to the patient."

Sir James Y. Simpson expressed his experience in equally strong terms when he said, "he had no belief whatever that iodine, or mercury, or muriate of lime, or aqua potassæ, or diuretics, or deobstruents, or aught else, were capable of absorbing and removing the complicated structure and contents of a multilocular cystic tumor of the ovary." Mathews Duncan says, "We know of no one example of the cure, otherwise than

by the operation of Ephraim McDowell, of an ovarian dropsy properly so-called; not one, however many may be found described, or whoever may be the describer. Cures by one or more tappings, cures by medicines, cures by spontaneous rupture, cures by advancing pregnancy, have been, if not mere egregious mistakes, almost certainly cures of parovarian cysts whose history, as already known, quite accords with and explains such erroneous allegations."

Of tapping I have said as much as I think necessary, but here I may repeat what every one knows now, that it never cures a tumor, and that it only brings about complications. It is my firm belief that if ovarian and parovarian tumors were never tapped, but were removed early in their history, we should have only a casual mortality from the operation of ovariotomy. Tapping, therefore, in my practice, has become only a palliation for tumors I could not remove.

Many other plans have been devised for the radical cure of ovarian tumors, but they are now all abandoned in favor of ovariotomy; and such methods of treatment as the injection of iodine or the establishment of fistulous tracks can only be justified under very exceptional circumstances.

Before the reintroduction of the intra-peritoneal method by Dr. Keith, we used to delay the removal of an ovarian tumor as long as the patient could get about comfortably, and this was justified by the fact that with the clamp we got only about seventy-five per cent. of recoveries. But now that we can get ninety-five, and when we might get ninety-nine per cent. of recoveries if there were no delayed and tapped cases, my rule is to remove an ovarian tumor as soon as it is discovered, and this will soon come to be the received practice. The earlier the operation is performed the more certain the patient is to recover, for the less likely are there to be any complications. However advanced a case may be, I never refuse to operate, for I have seen some of the most unpromising cases recover without interruption. Even when there is strong reason to believe that the tumor may be complicated with malignancy, I make an exploratory incision to remove all doubt. In this way I find that my proportion of exploratory incisions is increasing, for whereas formerly I made an exploratory incision where I thought the tumor might be removed and where I proved mistaken, I now make an opening often where I believe the tumor cannot be removed, and here again, to my great delight, I am occasionally in error. An exploratory opening never does any harm, and very often does a great deal of good, even where the tumor cannot be removed, for I have repeatedly known that after this operation there was no reaccumulation

of ascitic fluid, where formerly it was abundant, and we sometimes see an exploratory incision arrest the progress of an irremovable tumor for a considerable time. I have now under my care a patient with a large myxoma of the cæcum whom an exploratory incision completely relieved from distressing symptoms for nearly two years. Sometimes, therefore, I now begin an "exploratory incision" and end it as an "ovariotomy," while formerly I used to start an "ovariotomy" only to end it as an "exploratory incision." There is only one risk to a beginner in this, that he will have to learn when to stop at the mere exploration. To attempt the removal of a tumor and not be able to finish it, is the most fatal of all proceedings, and therefore the list of incomplete operations should always be a short one.

It is almost a matter of routine in the major operations of surgery, that it should be carefully ascertained that the patient is not suffering from organic or serious functional disease of any important organ, and this, for ovariotomy, must never be neglected. Especial care must be taken to examine the condition of the urine, for the state both of kidneys and bladder is a most important factor in the success of the operation.

I should not, however, hesitate to operate in a case where there was even distinct indications of important visceral disease. I have operated on two patients with marked disease of the lungs, and they are both still alive and one has got nearly well. I have operated in an advanced stage of Bright's disease, and the patient recovered, was greatly relieved of her distress, and died in the ordinary course of her kidney disease. Any visceral lesion which can be remedied before the operation should of course be put right, and this is most particularly true of the bladder. If there is any chronic catarrh of that organ it should be cured before the operation, for as the catheter has in many cases to be used for some days, this condition will almost certainly prove serious.

When an operation has been determined upon and every care taken that any defect discovered has been rectified, we come to discuss the stages of the operation, the precautions to be taken before, and the treatment to be followed after it. First of all, there is the position of the patient—where shall she be? Experience answers that the more nearly her surroundings resemble those in a healthy private house the better; and the statistics show that the performance of ovariotomies in a large general hospital is altogether unjustifiable. There is no operation in the whole range of surgery where the patient seems to be so apt to be infected by septic influences, and no precautions against them can be too great. For any surgeon to perform an ovariotomy

while he is engaged in dissection or in the performance of post-mortem examinations, or while he is attending any case from which he may be likely to convey septic infection, should therefore be looked upon as a professional offence of the gravest kind.

Mr. Spencer Wells has always expressed his views very strongly upon this subject, and they are well summed up in his lectures at the College of Surgeons. "Only two days ago," he says, "one of the most distinguished of the rising surgeons of the day told me that he had gone straight from a post-mortem examination to operate for strangulated hernia, quite confident that the spray and washing the hands with carbolized water would make him safe. This undue confidence in antiseptics may lead to danger rather than safety, and so far I see no reason for altering the opinion I expressed at Manchester last year, that 'for my part I would rather operate in a clean, quiet, well-warmed, and well-ventilated building, be it large or small, without any antiseptic precautions, than run the risk of trusting to the neutralizing or destructive power of chlorine or iodine, sulphur or tar, borax or the permanganates, salicylic or any other acid, in a place tainted by the presence of sewer-gas, or the seeds of some infectious or contagious disease.'"

As we have now had abundant opportunities of showing that the so-called "antiseptic system," even when carried out to its full, gives no such absolute immunity from septic poison as is claimed for it, this kind of teaching requires to be placed in prominence, and therefore I add my own opinion, in a quotation from a paper recently read before the Royal Medical and Chirurgical Society :

"Some of the warmest supporters of the antiseptic system uphold it, on the ground that under its protecting influence operations can now be undertaken successfully which formerly were impossible, such as laying open joints, etc. But I desire to point out that this is an argument which cuts both ways, and which seems to me to form one of the great dangers of antiseptic surgery. The immense favor with which the antiseptic system has been so widely received, is most undoubtedly due to the fulness of its promise as a royal road to surgical success, as a something which puts the skilled and the competent upon a level with the inexperienced and incompetent ; and I know that there have been abundant instances of bitter lessons already, that even an antiseptic spray will not condone the want of manipulative dexterity or the absence of readiness in emergency.

"There is, further, an inevitable result in the full acceptance of this germ-theory adaptation, that the other factors—the con-

dition of the patient and his surroundings—will be relegated to unimportant positions, and we shall have a great risk of inducing an inattention to general hygiene and the incursion of rash experiment, which will do more harm than antisepticism will do good, even if everything claimed for it is true. That this is no fancy sketch is proved by what Mr. Spencer Wells narrated in his lectures at the College of Surgeons on abdominal surgery."

I can only say further that in my opinion any man who deliberately performs an operation under circumstances from which his patient acquires fatal blood-poisoning ought to be the subject of a criminal indictment.

I am also strongly of opinion that no surgeon engaged in constant attendance on the promiscuous cases admitted to a general hospital should perform such an operation as ovariotomy, and I look on it as mere foolhardiness on the part of any one to perform it to whom it will probably never occur again to engage with such a case, or whose experience is likely to be limited to two or three such cases in a lifetime. It is an operation beyond all others requiring that readiness of adaptation for emergencies which wide experience alone can give. Its complications are far more varied and tax far more heavily the courage and presence of mind of the operator than those of any other operation in surgery; and one or two successful cases scarcely compensate for those which are unsuccessful by lack of experience.

It may be urged against these views that they are merely the opinions of a specialist, and therefore represent only a limited interest; but my experience, were I permitted to detail it, would be sufficient not only to convince my readers that my views are well founded, but that they really represent the best interests alike of the public and of the surgical profession. To any members of my profession who may be inclined to argue this point let me put the question as to whom they would entrust their wives or sisters for the removal of an ovarian tumor—to a surgeon engaged in general practice, whose experience of these cases was limited to some half dozen cases with some two or three deaths, or to one who engaged in this kind of work only and diminished in every way all risks of infection, at the same time that he increased his special experience? The fact is, as Simpson very well showed in 1845, it was to a large extent because the operation was not made a specialty that it was so long in receiving professional acceptance. "The diagnosis and the operation were, under the existing divisions and arrangements of practice, undertaken by two different sets of practitioners—the former by the obstetric physician, and the latter by the operating surgeon. It was, perhaps, the only capital opera-

tion in which the surgeon was required to proceed upon the diagnostic knowledge of another party; and no one was to be blamed if he felt a natural repugnance to incur so serious a responsibility on such grounds."

It was mainly this which made Mr. Syme hold out so strenuously against the operation and refuse, to the end of his life, to perform it, even after Dr. Keith had shown what splendid results could be obtained in Mr. Syme's own city. By common consent it is being handed over to special practitioners, for whom no special name has yet been coined, and whilst the department of obstetrics is being retained by physicians, abdominal surgery is passing over to the surgeons.

The room in which the operation is to be performed should be fairly large, and so arranged that ventilation may be possible from window or door to the fireplace without the current crossing the bed of the patient. There should be no unnecessary furniture, and as little upholstery work as possible. Two small iron couches, with firm hair-mattresses and a water-pillow, are needed; and an intelligent woman for nurse, who will do as she is told, *and nothing more*, is absolutely essential. If two such can be got to act as relays for the first eighty hours after the operation, it will be found a great advantage.

For my hospital practice I have now arrangements altogether different from those of my earlier experience, and the results are correspondingly good.

The patient herself requires a little preparation for the change that is about to be made in her alvine actions. For this purpose, I direct that her food should be limited to soup and a very little bread for forty-eight hours before the operation, and that on the morning of the day previous she shall have a small dose of castor-oil. The elaborate preparations which were in vogue twenty years ago, of both the patient and her surroundings, are now quite abandoned.

I prefer that the room should have a northern light, that the day should be clear, and I have the patient placed on a firm, narrow table with her feet directed against the window. The arms and legs are secured by bands to the table, so that only one assistant is required, one person to give the anæsthetic, and one nurse to look after the sponges. All other bystanders are earnestly requested to do nothing unless specially desired, and above all not to speak during the operation. My assistant is duly instructed in his work, and I never, if I possibly can help it, operate with any but my usual assistant, for it is quite as important that he should know how to help me as it is that I should know how to operate. All the preparations of the instruments and

sponges I make myself, and then I am sure nothing is forgotten —or if anything should be omitted, I have only myself to blame. As a successful ovariotomy is the resultant of a large number of petty details carefully attended to, no amount of care and precision can be too great in carrying them out.

The instruments to be provided are, a perfectly sharp scalpel, twelve of Koeberlé's scissor-forceps, four handled needles threaded with two thicknesses of silk, two pairs of cyst-forceps, a pair of large screw bull-dog forceps, a wire clamp, a pair of scissors, a number of pieces of fine silk, about a foot long, for ligatures; a number of pieces somewhat thicker, eighteen inches

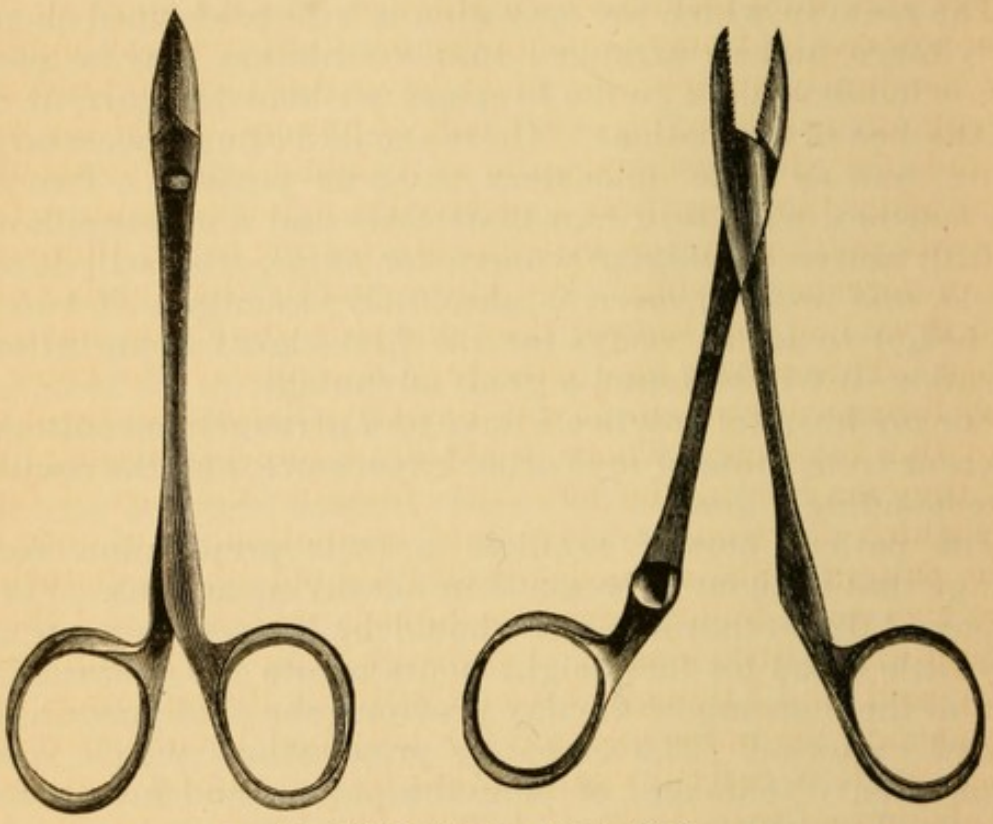


Fig. 30.—Tait's Modification of Koeberlé's Forceps.

long, for sutures; Paquelin's thermo-cautery, an aspirator, two sizes of trocar, and twelve good sponges. Of this list some of the items require special mention, and the first is the scissor-forceps of M. Koeberlé. Of the numerous minor improvements in our method of operating, none deserves to be spoken more highly of than the introduction of this instrument. I give above a figure of the pattern (Fig. 30) I use, though I do not know that it has much advantage over M. Koeberlé's original form beyond the fact that it is stronger and does not break, and that its pointed nose cannot be tied in the ligature. Its method of action will at once be seen. As soon as a bleeding point is seen it is seized

by one of these instruments and left in its clutches, so that when the operation has advanced as far as the treatment of the pedicle as many as eight or ten of these forceps may be hanging about the wound. More than twelve are rarely required, and if there should be need for more, one or two in use must be released by the assistant throwing a ligature round the points held by two or three. I have never more nor less than twelve, and I have them always ready to my hand in a small tray, covered with water and ranged carefully in order side by side, so that at a glance I can tell how many arein use. In this way I make sure that I never leave one inside.

Mr. Spencer Wells gives a very graphic account of an accident of this kind from his own practice, which I quote in full to show how easily it may occur in the most experienced hands and how constantly an operator must be on the alert to prevent it: "I took off, as I thought, every pair of forceps, closed the wound up, and everything seemed quite as it should be. But about two hours after the operation I received a message from a friend who was putting up the instruments for me to say there was a pair of forceps missing. We knew exactly the number of forceps; if we had not known that, one pair would not have been missed. This shows how necessary it is always to know how many forceps are taken. It was about five in the afternoon when I had this message: 'There is a pair of forceps missing; probably they may be in the patient.' Imagine the sort of feeling with which one would receive that intimation. I at once went to the patient. She seemed so well I did not like to disturb her; there was some doubt where the forceps might be, so I thought I would wait a little longer. I waited till night; she still seemed pretty well, and I thought I would wait till the morning; but in the morning the nurse told me the lady had been very restless. I then made a careful examination by the vagina and rectum and abdominal wall, to see if I could feel the forceps, but there was nothing to be felt at all. Still I was uneasy, and I thought I had better open the wound. So I asked Mr. Thornton to come with me and throw some carbolic spray over the abdomen, and making some excuse to the patient, just saying I thought it necessary to change the dressing, and it would be as well that she should not feel it, I gave her methylene, removed the dressing, and took out two stitches. I put one finger in, but at first could not feel the forceps. At last I found something hard, put another finger in, and found the forceps wrapped up in the omentum. From the way in which the omentum had insinuated itself into the ring-handle of the forceps and between the blades, it was easy to understand how difficult it was to find and re-

move the instrument; but I did it, returned the omentum, closed the wound, and the patient was none the worse. She got perfectly well, and to this day does not know that anything unusual occurred. Pray let me use this confession to impress upon you the necessity, not only of counting sponges, but of counting instruments also, that you may avoid any such painful experience."

These instruments are of great service in saving time, and this is a matter of importance in an operation which may extend over an hour. When the time comes for the removal of the forceps, after the pedicle has been dealt with, it will generally be found that the mere pressure has stopped most of the bleeding points. These instruments will also be found of great service in pulling out cysts and in many other ways which experience will indicate.

The handled needles, armed with silk, are items of the list upon which I take a great deal of trouble. They must be well made and well tempered, so that they will neither break nor bend. They must not have broad cutting points and must not make big holes. The eyes must be perfectly smooth and round, so as not to cut the silk. In fact, like everything else in ovariotomy, they must be perfection. The silk with which they are armed is of two thicknesses, for I always tie a pedicle or a mass of omentum with the thinnest silk I think capable of securing it. Therefore, for a thin pedicle I use thin silk, and for a thick pedicle I use somewhat thicker silk. The silk must be pure Chinese twist, with no cotton in it, an adulteration easily detached by liquor potassæ. I scald every piece of silk to be used before the operation in boiling water, to get rid of the gum, and then I stretch it tightly to test it and to reset its fibres. In this way I have secured myself against ever having had a ligature slip, an accident I have heard other surgeons complain of.

The form of trocar I use for emptying the cyst is figured on opposite page, and I claim for it the advantages that, being perfectly solid, it never admits air, and having no inside mechanism, it never gets out of order. The form of its point enables the operator to puncture secondary cysts without any alteration of the mechanism; it is not sharp and therefore can do no harm.

Of the sponges to be used it is almost impossible to speak with too great emphasis, as I distrust them more than anything else about the operation. I never let them out of my sight and I will not permit any one but the nurse in charge of them to touch them. They are prepared for each operation with the utmost care and the number in use is constantly *twelve*. They are counted before the operation, before the wound is closed, and

again afterward, so that by no possibility should one be left inside, an accident which has happened a great many times in the history of ovariotomy and which nothing but the greatest care will prevent recurring. Mr. Spencer Wells gives his experience in this point as follows:

"In one case I was a long time searching for a sponge before I could find it. No one who has not tried would believe the difficulty of finding a sponge in the abdominal cavity, if it be not very large, and have become saturated with fluid. The lady was the wife of a surgeon, and I operated upon her in the fifth month of pregnancy. After the pedicle was secured, and I was closing the wound, the nurse said there was a sponge missing. I said 'Are you quite sure?' She counted again, and said, 'I am sure there is a sponge missing.' I felt in every direction in this lady's abdomen; put my hand to the bottom of the pelvis, to the front of the uterus, and everywhere I could think, but

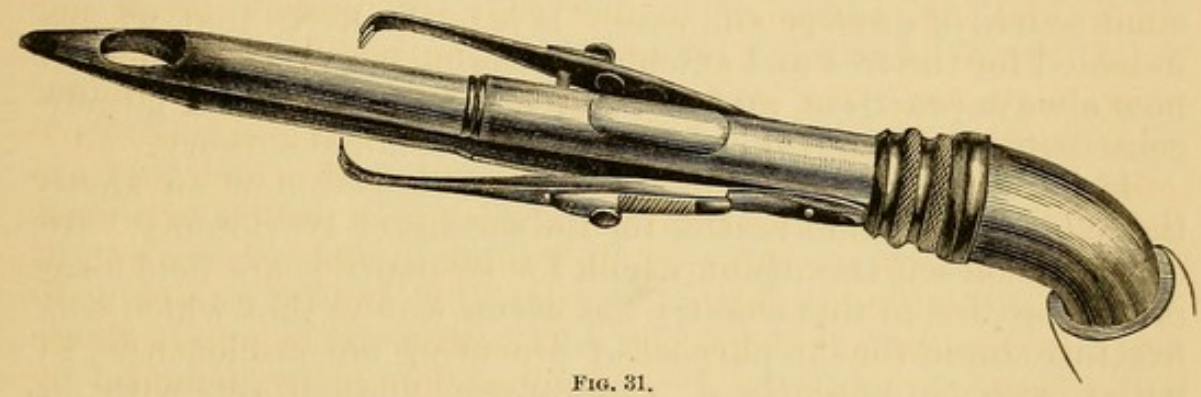


Fig. 31.

find the sponge I could not. But at last, up to the back of the liver, between the liver and the diaphragm, I found a small sponge, and removed it. The patient made an excellent recovery, in spite of all this groping. But this leads me to repeat the caution not to use sponges so small that they are not easily found."

Let me further say again that no one should touch the sponges but the nurse responsible for them. In one of my early operations, many years ago, a bystander hearing me ask for a small sponge tore one in two, so that there were thirteen in use instead of twelve, and both the nurse and myself were ignorant of the fact. The gentleman who tore the sponge alone knew of his act, and he left the room before the operation was finished. The result was that we found that thirteenth sponge four days after, and the horror of the circumstance is as vividly in my mind now as if it had happened yesterday. I shall never forget it as long as I live. I happen to have heard of ten other cases

in which sponges have been left behind, so that I need no excuse for the emphasis of my advice on this point.

The sponges used should be of the very best quality, should vary somewhat in size and shape, and should be perfectly free from tears or ragged points, from which pieces may become detached. When they are new I soak them for twenty-four hours in a solution of muriatic acid sufficiently strong to be disagreeably sour to the taste. This dissolves the particles of chalk with which they are infested and loosens the sand, and this must be completely washed out of them. After each operation I wash them free from color and then soak them for forty-eight hours in a strong solution of washing-soda or ammonia, to dissolve the fibrin. They are then washed repeatedly until the water comes from them perfectly clean, and after that they are placed for a week in a five per cent. solution of phenol. Finally, they are hung up in a well-made calico bag in a warm place till they are quite dry. I keep a very large stock of sponges, and they are a constant source of anxiety and care. It is to Dr. Keith that we are indebted for the free and efficient sponging out of the abdomen now always practised, and in this respect he has again largely contributed to the advanced success of abdominal surgery.

It hardly requires to be said that the selection of an anæsthetic is an important matter for the success of ovariotomy. By common consent the agent which for so many years held a supreme position in this country has given way to that which was first introduced for the purpose of procuring unconsciousness to pain in surgical operations. Chloroform, while it acts with an almost mathematical certainty upon a woman in labor, is a most unsafe and uncertain drug to use in surgical operations, save in the case of a very young child. It is not only that we have a considerable number of deaths from chloroform, but that in its use we have so much anxiety that the danger is ever present in the mind of the operator, and this distraction is destructive of that clear concentration which is necessary in the performance of ovariotomy.

Mr. Spencer Wells has for a long time advocated the use of the bichloride of methylene by means of Junker's apparatus, and this is certainly a very elegant, rapid, and, compared with chloroform, safe means of inducing anæsthesia. There are, however, two objections against its use; one is that it requires a special apparatus, and the other that this apparatus requires some one experienced in its use. In the hands of one who is accustomed to give the bichloride I think it is probably as safe as any anæsthetic well can be, but as it is wholly impossible always to obtain the services of one sufficiently skilled, I have for a long time dis-

continued the use of this agent. There are other reasons against its use which I do not think of sufficient importance to dwell upon. The agent I have employed for the last six years is free from any objection of an important character. I allude, of course, to sulphuric ether, which is now recognized as by far the safest anæsthetic in use, the kind of ether which I employ and which I prefer above all others being the anhydrous methylated ether manufactured by MacFarlane & Co., of Edinburgh, as originally recommended by Dr. Keith. The advantages of this agent are very numerous. In the first place, so far as its present use has gone, it is absolutely safe; in my own practice it has been used, on a rough estimate, between five and six thousand times, and not only have I had no accident from it, but its use gives me no anxiety whatever, and whilst I am engaged in operating my mind is absolutely at rest concerning the anæsthetic.[1] In the next place, it may be administered by any one, and when I say that in the whole of my hospital practice, for nearly three years, the ether has been administered for all operations by the sister in charge or by a nurse, I think I give testimony sufficient as to my confidence in the safety of the drug.

For its proper administration very few rules need be observed and no kind of special apparatus is needed. It is always given for me after the simple fashion which Sir James Simpson introduced for the administration of chloroform, that is by dropping it on the outside of a single fold of a towel, laid upon the patient's face. Bearing in mind that ether is extremely volatile, and that its vapor is very heavy, the following directions must be attended to. The towel used must not be too thin, because it must retain a sufficient body of ether for the continuance of the current of vapor; and yet it must not be so thick as to prevent the passage of air freely through it. The ether must be dropped on to the towel, not splashed on, but administered in a continuous stream, which must be allowed to drop from a small orifice on to the towel, above the level of the patient's nose, because the vapor of the ether will fall like a cataract over the patient's face. If the ether is dropped on the towel on a level with the patient's mouth, she will inhale, not the vapor, but a mixture of air and ether, which will act as a stimulant and not as an anæsthetic, and the ether must not be splashed on, for exactly the same reason. The towel should not be tightened over the face, but puffed out around it at a distance of an inch, or an inch and a half, from the skin, in order that it may enclose a body of vapor. The

[1] Since writing this I have had an accident (British Medical Journal, July 14, 1882) due, as I think, to the use of an inhaler.

whole of the piece of towel covering the face must be kept continually moist with ether, and in this way a continuous volume of pure ether vapor will be inhaled by the patient. After a few minutes the part of the towel in use must be changed for another part, because anhydrous ether absorbs with intense avidity the moisture of the breath, and the towel will be found coated with ice, and this, by its interference with rapid evaporation, prolongs the process: this is the chief argument against all ether inhalers. Only one other caution need be given, and that is to avoid bringing any light or red-hot cautery near the patient's face while the ether is being given, for it is explosive.

When these rules are attended to, ether will be found to be the safest, quickest, and most satisfactory of all anæsthetic agents. It has only three objections, and these are of but slight importance. The first objection is that the smell of the ether hangs about the clothes of those engaged in the operation, and this is offensive to many persons; the second is that the quantity of ether used is much larger than that of any other anæsthetic and therefore it is necessary to keep it in large bulk; and the third objection is that the taste of the ether is by no means so pleasant as either chloroform or methylene; but upon this point I have heard considerable difference of opinion. The cost of the ether is less than that of any other anæsthetic which I have used.

During the administration of ether there should be absolute silence in the room. No one should be allowed to talk; more particularly, no allusion should be made to the patient, or to the prospects of the operation, because for a long time after insensibility has apparently been reached ideas may be picked up by the patient from suggestions made at the bedside, and these often have a lasting and most disagreeable effect. This was one of Simpson's most stringent rules, and one which never ought to be broken. At the beginning of the process the quantity of ether should be small, in order to avoid giving the patient the sensation of being suffocated, because it is at this stage that the state of mind is induced which leads to struggling. Nothing makes any one struggle more than the sense of being choked. As soon as it is evident from the regularity of breathing that the patient has become accustomed to the presence of the ether vapor in the air the quantity given may be increased to that which has been already indicated, and if this condition be attended to it will be found that struggling will be altogether avoided, save in the case of children. When the patient does struggle, the only thing to be avoided is her getting hold of the towel, and it will generally be quite enough for the nurse or as-

sistant to keep the hands quiet while the administrator of the ether steadies the head. Whoever gives the ether should absolutely mind his or her own business, and take no interest whatever in the surroundings during the whole of the process; for even with an anæsthetic so safe as ether it is absolutely necessary that the whole attention should be engrossed with it.

Sickness is with ether, as with every other anæsthetic, a somewhat frequent trouble, and indeed it is difficult to see how it can be altogether avoided. It is very rare, however, that it need be a source of anxiety; but in one instance in my practice it was extremely serious, and the story of the case will give a lesson more emphatic, perhaps, than any mere directions which I can give.

A lady was placed under my care some three years ago for the purpose of having an ovarian tumor removed. She was, as usual, kept in bed for two or three days before the operation and on the morning of the operation nothing was given to her by her attendants, in order that sickness might be avoided. The operation was to be performed at nine o'clock, and she, being profoundly persuaded that upon an empty stomach she never could go through the ordeal, rose about six o'clock on the same morning, went to one of her boxes, which had been unfortunately left in her room, and in which she had secreted a quantity of biscuits and a bottle of port wine, and with these she made a hearty meal. In the middle of the operation sickness came on and she vomited a large quantity of purple pulp. She went on incessantly vomiting for eight days, and long before that time I had given up all hopes of her recovery. The straining caused by the retching tore the wound open, and upon the sixth or seventh day a large sloughy mass was extruded, which proved to be the rectus muscle of the left side, which had apparently been destroyed by the continuous vomiting. For ten days more she lay with a large, open wound, through which could be seen the movements of the intestines, and between her and another world, there was nothing but a layer of peritoneum. Fortunately, she did not open the wound any farther. She ultimately recovered, and is now in perfect health, save that there is a large intestinal protrusion at the seat of the wound. I do not think that in the whole course of my practice I have ever had a case which gave me so much anxiety, and it also gave me a powerful argument by which to impress upon others the absolute necessity of obedience.

The anæsthetic should, therefore, be always given upon an absolutely empty stomach. I do not even approve of the dose of brandy and water usually given, because ether is in itself one of

the most powerful stimulants, and we have only to watch the improvement upon a feeble pulse which takes place during its administration to be quite satisfied that no other stimulant is necessary. The quantity of ether given during an operation, especially during ovariotomy, is necessarily very large, but no one need ever be alarmed on account of the quantity of ether administered. It is absolutely necessary to keep all the patient's abdominal muscles, except the diaphragm, perfectly quiet; and long before there is the least danger the patient's deep snoring will indicate that the stage of profound sleep has been reached. When the patient snores the administration should for a short time be discontinued. The quiet regularity of breathing which always characterizes the unconscious state induced by breathing the vapor of ether is quite enough to indicate to any one of any experience in its use that the patient is in a condition of profound insensibility, and this will also serve as the best indication of safety. There is, therefore, very little need for the pulse being watched, or for the conjunctiva to be experimented upon to determine unconsciousness. Should sickness come on during the operation, the ether should be pushed a little more and this will stop it, because sickness really is an indication of returning consciousness. After the operation is over, should sickness occur, it is best checked by the administration of a little luke-warm water flavored with brandy, and without sugar. When the patient is sick she should always be turned on her side, as in this position the tongue falls forward and the vomit is got easily rid of from the mouth without risk of its entering the trachea. Forceps should never be used to drag the tongue forward, after the barbarous fashion introduced by the late Mr. Syme. It is quite sufficient to turn the patient on her side, or even to turn the head to one side, if any necessity arises for this during the operation. Artificial teeth should always be removed from the mouth before the ether is given.

There is only one risk about the administration of ether, and that is the occurrence of bronchitis in old people. I have not seen this often, but in several instances I have had reason for anxiety on this ground, and in order to avoid this risk I have devised an apparatus by which the vapor of ether is given at a temperature of 33° Centigrade, that is, very nearly its boiling-point.

The woodcut on opposite page will explain the apparatus which I have devised for the purpose. Anhydrous methylated ether (.720) is placed in the reservoir, A, which will hold about ten ounces, and which is furnished with a spring pump, which drives over about a dram of ether at each stroke into the glass boiler, B.

This boiler is suspended in a hot-water tank, C, beneath which is a spirit-lamp. From the boiler an exit tube, four or five feet long, passes to a Junker's mouth-piece.

When the apparatus is to be used, the tank is filled with water, the spirit-lamp is lighted, and about three drams of ether is pumped into the boiler. Care must be taken that there is no leakage from the boiler, otherwise there will be an ignition. It will soon be found that the boiling ether gives over a large volume of vapor at a constant temperature of a few degrees below the boiling-point of the sample, which of course will vary very much, but will generally be found to be pretty nearly the temperature of expired air, 31°—33° Cels. When given at this tem-

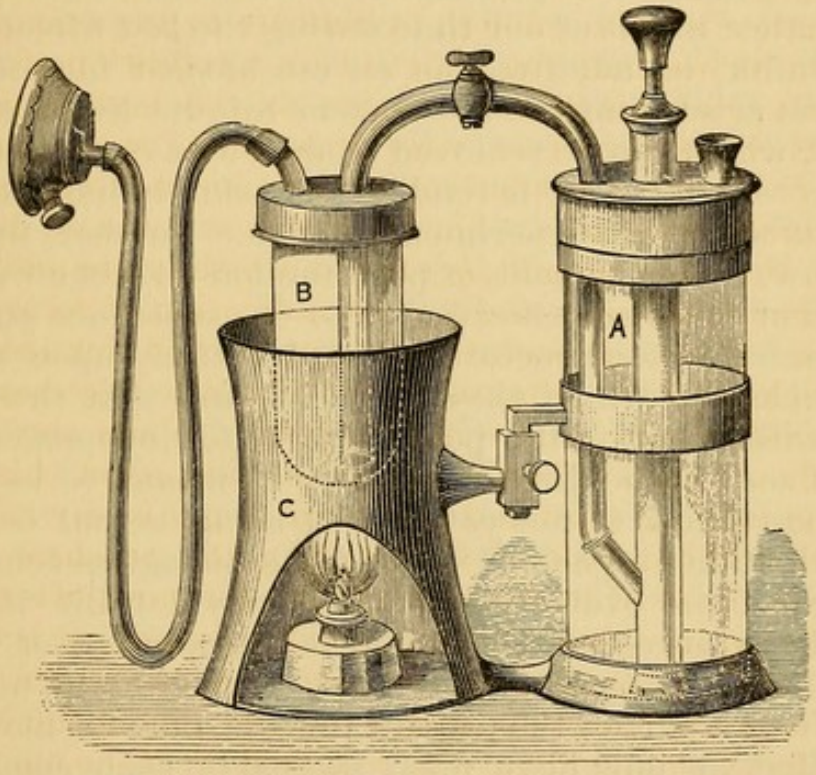


Fig. 32.—Apparatus for the Administration of Ether-vapor at Blood-heat in Cases of Old People.

perature, and free from air, the vapor is quite pleasant, and its taste or rather its comparative freedom from taste, reminded me when I tried it on myself, greatly of the flavor of nitrous oxide. It is, of course, certain that its administration will involve no risk of bronchitis.

I think this method will be found to be a substantial advance in the method of ether administration for old people; and if care be taken to keep the boiler vapor-tight, and the mouth-piece three or four feet away from the spirit flame, I think the apparatus will be found quite safe. Dr. Lauder Brunton made to me the ingenious suggestion of having a steam-jacket round the boiler, but that I have not found to be practicable without greatly

complicating the apparatus. The use of a hot iron bolt to boil the water, instead of the lamp, would probably be safer, but it would not be so handy, and would involve great wear and tear.

There comes in now the question of the adoption of what are called antiseptic precautions, and of these it is quite impossible to speak without a discussion, however brief, of the theory upon which they are based. This is necessary for many reasons, but chiefly because the followers of the antiseptic doctrine assert, with a vehemence worthy of the scholastics of the fifteenth century, that unless there be faith in the doctrine there can be no success in the practice.

To those who have followed closely the elaborate researches upon the phenomena of putrefaction, which have occupied some of the greatest minds of our time during the last fifteen years, it must, I think, be admitted as an established fact that these phenomena arise from the presence of minute living organisms in the air, which can be removed from it by a variety of physical means, by which the air is rendered absolutely harmless to the substances which are experimented upon. Further, that so far as we know, no phenomena of putrefaction do occur without the admission of these so-called germs to the substance putrefying, that the putrefactive processes depend entirely upon them and the organisms to which they give rise, and that the origin of such organisms within the putrefying fluid, independently of a sowing of seed in the fluid, though by no means to be regarded as an impossibility, is not yet proved as an actual occurrence. To any who takes a general view of the biological scheme from the aspect of the evolutionary philosophy I think it must be clear that the so-called "spontaneous" generation of the early forms of life is a necessary corollary, but it is by no means certain that it is a part of the present process, nor is it likely, in my opinion, that we now have upon the earth such conditions as would render biogenesis possible.

For my present purpose, therefore, it is enough for me to assume, as I do most fully, that the germ theory has been completely substantiated, and that no known process of putrefaction does occur save by the admission of resting spores or swarm spores of some of the many minute living organisms which are invariably associated with putrefactive changes. But concerning this there is another constant position associated with these phenomena. The materials upon which the experiments have been made, of infinite variety of kind and constitution, have all been dead, and no one has yet pretended that, by the admission of germs to living matter, he has produced the phenomena of the putrefactive changes which constantly result in matter which is

dead. To quote the apt illustration given by Dr. Wm. Roberts in his masterly exposition of this most difficult subject, the ordinary hypodermic morphia syringe will inoculate inevitably a sterilized solution of dead organic matter, but amongst the hundreds and thousands of hypodermic injections which are made daily, no one has yet declared a single instance of putrefactive changes resulting from it in the healthy, or even in the diseased human body.

It will, therefore, be seen that the application of the facts of the germ theory of putrefaction to the phenomena of diseases of living tissue is met at once by an overwhelming difficulty, to the removal of which none of the adapters, so far as I have seen, have as yet applied themselves. Granting that the same germs which would inevitably produce putrefaction in a dead infusion of beef are constantly admitted to wounds, there is not the slightest particle of evidence that they do produce any change whatever upon living tissue, still less is there any evidence that the changes which occur in the numerous varieties of what we call blood-poisonings, even when they are of an undoubtedly local origin, have the slightest analogy to those seen in a putrefying dead infusion. The mere presence of bacteria in the fluids of wounds, or in fluids enclosed in cavities, whilst offering many difficulties to the adapters of the germ theory, prove nothing for their position until they have shown that these organisms ever do occur in fluids or tissues which are truly living.

The difficulty, therefore, is this, that what we call vital action, for want of a name based upon a better understanding of what it is, places living tissue in an altogether different category from tissue in which the phenomena of life are no longer present.

Now, this is consonant with every-day experience. If a decaying hyacinth bulb or a rotting apple be examined, the presence of the minute forms of life is found to be absolutely confined to those parts where the changes have been effected, whilst those parts to which the rot has not extended are found absolutely free from them, and the difficulty of the adaptation of the germ theory is simply this, that its advocates have assumed that the invasion of the germs is the cause of the decadence of the vital phenomena and the ultimate death, while there is the alternative—still undiscussed and certainly undismissed—that the decadence of the vital powers, due to some cause possibly yet unknown, is that which gives the germs their potential ascendancy, and enables them to do what, during full vital action, they were wholly unable to effect.

If the views of the germ theorists were correct, we ought to expect that no operation could be done successfully without rigid

antiseptic precautions. The slightest cut of the skin ought to be followed by septic poisoning. There ought to be no difference in the mortality of operations in small and in large hospitals, in town and in country. In fact, if germs could have had the unbounded influence which is claimed for them by many antisepticists, surgery must long ago have been an extinct art, if, indeed, it ever could have struggled into existence.

The uniform experience of operating surgeons has taught them that the success of their work will depend upon three factors, the condition of the patient, the condition of his surroundings, and the nature and extent of the operation performed.

Of these three, undoubtedly the most uncertain factor is the first. What condition of the system it is which is favorable to operations is almost unknown. I must base my conclusions chiefly upon my own work, and in my special operation of ovariotomy I am perfectly certain that apparent perfect health is by no means a certain indication of a power of resistance to those conditions, whatever they be, which result in so-called septic poisoning.

The second of the factors, the condition of the surroundings of the patient, contains elements of far greater certainty. It has approached the position of a statistical law that the death-rate is in constant harmony with the density of the population, and that when the density exceeds a certain minimum of safety there are introduced specific septic diseases, as typhus fever, which are wholly unknown under other conditions, and which, even after the danger density has been reached, attack certain individuals only, and not all, for reasons which can be expressed only by saying, as I have already said, that the living tissues of those affected could not, and did not, resist the septic influence.

Every advance we make in sanitation shows that this factor, the condition of the surroundings of the patient, is of extreme importance.

The third factor which influences surgical success is the extent and importance of the operation performed. Everybody knows that while amputation of a finger is probably fatal in not more than one in ten thousand cases, nearly one-half of all amputations of the thigh die. Now, if the adaptation of the germ theory to surgical practice were as promising and as legitimate as some of its supporters allege, we should have had the remarkable result, previous to its application, that amputations of the finger and of the thigh ought to have approached one another in mortality to an infinitely larger extent than they have done.

If the contact of a bacterium germ upon a wound could be the source of blood-poisoning, then the size of the wound and

the nature of the operation could make but small difference in the result, and a wound into the theca of a finger tendon, and one of similar size into the peritoneum of another patient in the same ward, ought to have very similar risks. But, as a matter of fact, they do not, and we are forced to the conclusion that, even if bacterium germs lighting on wounds are the cause of much surgical mortality, that the power of vital resistance by the tissues, or the condition of the patient, and the extent and nature of the operation, are of infinitely greater importance as factors in the general result. This logical difficulty has evidently occurred to many of those who carry out Mr. Lister's adaptation of the germ theory to surgical practice. I have seen a rigid antisepticist occupy an hour and twenty minutes in making incisions a fraction of an inch in measurement, and barely skin deep, for the purpose of laying bare the tympanic membranes of an infant in whom they had been congenitally covered, the protraction of the operation being due solely to the antiseptic precautions. Such a proceeding produced in my mind a variety of emotions, chief of which were admiration for the enthusiastic consistency of the operator and sympathy for his evidently wearied audience. The just criticism of such a proceeding is the question, "Has any one ever seen such a trivial operation result fatally from septic poisoning, unless in some such hospital, as is described by John Howard in 1780, as the Hôtel Dieu, with three patients in each bed?" I certainly never have, and I have performed some thousands of them; and if it were necessary to take one hundred minutes to do what I could do in three I, for one, should seek my livelihood in some employment other than that of an operating surgeon.

The logical conclusion to be drawn from the facts is, therefore, that in minor operations germs have never, or at least hardly ever, any influence at all, and that in major operations the condition of the patient is of immense importance in enabling him to resist the influences, whatever they may be, which result in what we call the septic condition.

In any examination of the question there will of course be the primary difficulty, that it is by no means agreed as to what constitutes a major operation, and that between different operations which are admitted as major, there are known to be very different rates of mortality. Thus, amputation of the leg is proved, in my book on "Hospital Mortality," to be more than twice as fatal when performed for accident as when performed for disease. It must be perfectly evident, therefore, that any examination of this question must be conducted upon the usual rules of statistical investigation, the chief of which is that similar and not dis-

similar accidents should be grouped together. Any mere statement, therefore, of the general percentage of deaths from septic diseases on the general hospital or other population are absolutely worthless unless they be most carefully analyzed, and they are, of course, open to the still further objection that what constitutes a death from septic disease is by no means a perfectly accepted definition.

There is a popular belief that statistics can be made to prove anything, than which there is no popular belief more erroneous. Statistics alone seldom prove anything, certainly they never explain anything. Thus, the Registrar-General's tables tell us that there are certain death quantities which are perfectly constant, and they establish the fact that half of all our human mortality occurs before the fifth year of life. But this neither proves nor explains the cause of this mortality, nor does it even explain its factors, until a more careful analysis of individual cases is made. Therefore, nothing whatever can be proved for or against the adaptation of the germ theory to surgical practice by mere statistical statements. But in spite of this, statistics may be made to show exactly in what direction analysis of individual instances should be made, and, therefore, they alone are capable of forming the first step of accurate inquiry. First, let us ascertain, as fully as possible, what the facts are, and then analytical arrangements of them will certainly afford a more or less complete explanation of their method of production.

Thus, it must be evident to every one that a large group of one hundred ovariotomies must present features more similar to those of another set of a hundred than can probably be got in any other surgical comparison which is possible; and it is a probably correct assumption that if the same surgical skill and patience, the same attention to minute details, and the same state of the surroundings were common to the two groups, their resulting mortality would be identical or nearly so. But if there is one thing we value more than another, as being likely to contribute to success in surgical operations, it is personal experience; and we, therefore, may fairly expect that with each succeeding hundred ovariotomies the mortality will diminish, owing to the increasing skill of the operator. And this is the case notably in the practice of Dr. Keith, who, beginning with eleven per cent., went successively down to eight and six before he began to use antiseptics; and of my own experience I can only say that, while I had nineteen deaths in my first fifty operations, I had only three in my second fifty, and in all my subsequent practice these good results have been fully maintained; indeed, have been excelled.

A recovery after an ovariotomy is the sum of a number of details, all of which were efficient. A death, on the contrary, may be the failure of one only, and that may be or may not be under the control of the surgeon. Thus, of the three fatal cases in my second fifty, two were deaths due to details wholly beyond my control, and having no relation whatever to either the antiseptic system or any other of the operative details. The third death was due, as far as I could determine, to the irritative effects of thymol, used with full antiseptic details. Two of these deaths were antiseptic out of twenty-nine cases treated antiseptically; while of twenty-one cases treated without antiseptic precautions, I had only one death, and as the patient died within three hours after the operation, the want of antiseptic precautions could have had nothing to do with her death. From this group of cases, therefore, the argument would be wholly against the antiseptic system, and though my impression is that the conclusion would be a just one, yet the argument is absolutely fallacious, as all such are.

In the discussion of this question, which occurred some months ago, the only statistical argument of the slightest importance was given by Mr. Spencer Wells, who said that a very marked improvement had occurred in his results since he had used antiseptic precautions. But nearly concurrently with his adoption of germicides, he adopted the intra-peritoneal method of dealing with the pedicle, a method which has been superlatively successful in the hands of Dr. Keith, and to which, chiefly, I attribute my own rapidly increasing success. Thus, Mr. Wells' mortality improvement argues nothing in favor of antiseptics, but far more, in my opinion, for the short ligature.

The greater part of what I have just said upon this most interesting and important subject, is taken from a paper which I read before the Royal Medical and Chirurgical Society of London, in February, 1880, and which is published in vol. lxiii. of the "Transactions," and its general conclusions are quite in harmony with the opinion expressed by Mr. Spencer Wells, in his lectures delivered before the Royal College of Surgeons. I give his opinions as they are reported in the medical journals at length, for they are very important. He says: "If (say the supporters of Listerism) in one of the most serious operations ever performed upon the human body, where the largest serous cavity is opened and the freest access is afforded to any infective organisms which may be near the patient, Mr. Wells can obtain eighty recoveries out of one hundred operations; and as on two occasions he has had long series of successful cases—one of twenty and two of twenty-seven—without a death to break the run of success:

if in the last two years of his practice at the Samaritan Hospital, in 1876 and 1877, he had only seven deaths out of seventy-one cases—not one in ten—and all this without any of the special precautions supposed to be necessary by the advocates of the antiseptic system—never using the spray nor any carbolized solutions for sponges or instruments, no catgut sutures, and no protective carbolized gauze for dressing; if such results as these, in such an operation, have been obtained, is it possible that the germ theory can be true or the practice founded upon it necessary? It is troublesome; may it not be injurious? Cases of poisoning by carbolic acid—fatal poisoning—have been recorded, and, in many cases where recovery has followed, the patients have been exposed to great suffering and inconvenience."

"They claim the successes as proofs of the value of the [antiseptic] system; they explain the failures by some alleged neglect of some petty detail. In one case I thoughtlessly used a clamp which had not been carbolized, and was warned of probable failure; but the case appears, in spite of my negligence, on the list of successful antiseptic operations, and was one of the cases in which there was no fever after the operation. If there had been any fever, no doubt it would have been explained to the entire satisfaction of many people by the presence of some infective germs on the clamp. Of course, this explanation might possibly be true; and I freely admit that, in an experimental inquiry, no such carelessness should be permitted, especially as it gives rise to the suggestion, from zealous or enthusiastic partisans, that no one but themselves can be safely trusted to perform an operation antiseptically. "If," they say, "you do not believe in the presence and omnipotence of germs—infective germs—all about the patient and surgeon, nurses, bedding, instruments, sponges, dressings—indeed, always and everywhere—you will be sure to leave some loophole unprotected against the entrance of one or more of these dreaded enemies. You must accept the theory, or act as if you did, or your practice will certainly be faulty." My answer to this would be that I have watched the performance of a good many operations by surgeons who have complete faith in the system, and who believe they carry it out accurately, but I have never yet seen one case where a good many germs might not have escaped the action of the carbolic acid; and I believe that I or any surgeon present who wishes conscientiously to protect his patient from any of the atmospheric or other impurities which may possibly affect her injuriously, whether he uses carbolic acid or any other antiseptic, may, by careful attention, do so as completely and thoroughly as the most accurate experimenter can desire. Feeling all this, and feeling,

also, that undue faith in the system may lead to rash practice and the attempt to do things which had better be left alone, and knowing, also, how difficult it is to obtain any trustworthy, statistical, comparative facts to determine the relative success of any operation well performed, with and without antiseptic precautions—all other things being equal—I think what I have said of my own observations, although they only amount to twenty-two cases, may have some little value." What Mr. Wells complains of here is, I think, a very just object of criticism. The argument of many of the supporters of Mr. Lister's theories, and the promulgators of his practice, use an argument very like the schoolboy catch, "Heads you lose, tails I win." If there is success, then it is Listerism; if there is failure, then it is not Listerism; some important detail, like Mr. Wells' clamp, has been improperly managed, and the failure is no fault of the system. Another method of their argument is to say that their opponents, having no faith in the system, can have neither honesty nor intelligence sufficient to carry it out. To such a method of discussion it is impossible to reply with courtesy; and my only answer is that, as it is my best interest to cure my patient, I am ready and, I believe, capable of applying any practice for that purpose, but that I do not care willingly to approach the land of quackery.

Since Mr. Wells delivered his lectures he has seen reason to change his opinions, and he now attributes his diminished mortality to the introduction of antiseptics. There is this difficulty, however, left for him to explain: his mortality now is double that which Dr. Keith had secured before he used antiseptics at all, and at a time when Dr. Keith's experience was little more than a fifth of what Mr. Wells' is at present.

I think it very likely that if my own results with the Listerian details had been bad, it would have been said that I did not understand the system, and never could, and I think the charge, probably, would have been correct. But my results were good, and therefore they were allowed to pass. But I really believe that very few of my cases would have been admitted by an enthusiastic Listerian, and I am quite sure that for the last two years not a single case would have passed muster. Whether or not that be so, here are the results of the whole of my practice up to the date of my writing this:

	Per cent. Mortality.
Ligature, non-antiseptic (187 cases)	3.74
Ligature, antiseptic (52 cases)	3.84
Clamps, non-antiseptic (36 cases)	25.00
Clamps, antiseptic (26 cases)	27.00

Can any reasonable being doubt that here the improvement lies in the giving up of the clamp? In the figures for the "antiseptic ligature" I give credit for twenty-two consecutive recoveries in which Listerism was not really complete, as Mr. Lister's dressings were never used, nothing but plain dry cotton wool being put upon the wounds, and if the results had been bad among these, my Listerism would be scouted. If I put these cases to the credit of "non-antiseptic ligature," as I very fairly might, then the account would stand thus:

	Per cent. Mortality.
Non-antiseptic ligature (209 cases)	3.00
Antiseptic ligature (30 cases)	6.6

Thus it is that I am very hard of belief about the merits of the Listerian details as applied to ovariotomy, the more so as I have carried out all the processes with plain cold water, and got quite good results, and I find that the chief advocates of the "antiseptic system," those who attribute their success to it, never get their mortality lower than ten or twelve per cent., whereas without it I keep under five per cent. I can regard this sudden love for the antiseptic system as nothing but a shield to cover the retreat from the use of the clamp.

Mr. Spencer Wells' last utterances on the question of Listerism are very important. In his recently published work on "Ovarian and Uterine Tumours" he says: "Four of my last sixteen deaths were caused by septicæmia, so that antisepticism has not abolished this plague of abdominal surgery. Lister's antiseptic plans have not brought me to the point of seeing no deaths from septicæmia, as promised by some of their enthusiastic promoters, nor have they advanced my success in operating beyond what was obtained without it." These words seem to me to contain a practical and complete admission of what I have always contended, that Mr. Wells' greater success was due to his giving up the clamp. The mortality of his first 800 cases was 25.5 per cent., while in the subsequent 200 it was only 14.5.

My views received very valuable support from Dr. Granville Bantock, Senior Surgeon to the London Samaritan Hospital, in a letter he published in the *British Medical Journal* for January 8, 1881, from which I take the following extract:

"I quite concur with Mr. Lawson Tait, when he says that 'the method of recovery' is of far more value in estimating the merits of any particular system than 'the mere death-rate,' which is often a matter of luck, and especially when the difference in the latter is measured by units. I further agree with him in attributing to the present mode of treating the pedicle

much of the success which has recently been achieved. If there is one thing more than another, in the matter of ovariotomy, to which I look back with satisfaction, it is to the persistency with which I held to and urged the systematic use of the ligature at a time when (in 1875), owing to the powerful and sustained advocacy of Mr. Spencer Wells, the clamp was at the zenith of its fame. Notwithstanding the excellent results obtained by Dr. Tyler Smith, the ligature had fallen into unmerited oblivion, and was at that time used only as a *dernier ressort*, and in the most desperate cases. Thus, Mr. Wells says (at page 361 of his book), 'you will hardly wonder that I use the clamp whenever I can;' and at page 371, he says, 'the more I see of ovariotomy . . . . and the more I am driven to resort to cautery or ligature, the less I am satisfied with the results of these methods, the more reluctant am I to employ them, and the greater is my confidence in the clamp,' etc. The evidence I produced in favor of the ligature, as far back as 1872, before the Obstetrical Society, appears to have missed that notice which is claimed, and which subsequent events have shown it to have deserved."

Dr. Keith, in the record of his cases, does not give completely such details as afford a perfect statement of his results, based upon the various methods of treating the pedicle; but he tells us that in his first 50 cases he used the clamp 48 times, with 9 deaths. In his second 50 he merely indicates that his confidence in the cautery is returning. In his third 50, the clamp was used 34 times, with 7 deaths, and the cautery and short ligature 15 times, without a death; and now I understand from himself that he has entirely abandoned the extra-peritoneal method of dealing with the pedicle, as, indeed, has everybody else, by reason of Dr. Keith's unprecedented success with the intra-peritoneal method, even before he adopted antiseptics.

The basis of the antiseptic claim is that the system prevents septic poisoning, that is, septic or surgical fever. Every one who has watched a number of ovariotomies knows that by far the larger number of deaths occur from the incidence of fever, and that the pulse and temperature rise progressively, though perhaps with intermissions, till they reach the fatal vanishing points. With few exceptions this is true of all the deaths I have had. If, therefore, the antiseptic system favors a larger number of recoveries by preventing the so-called septic fever, it is an absolute certainty that the recoveries will be uniformly and correspondingly facilitated, inasmuch as in non-antiseptic cases the germs will enter every peritoneum and will theoretically produce fever in every case, and only in those cases where there is

a sufficiency of an unknown something which counteracts the septic poison will recovery be obtained.

Equally according to the theory will the germs destroyed by the antiseptic precautions enter the peritoneal cavity harmlessly, being dead and unfit to produce septic fever.

Another step in the syllogism is that as the temperature and pulse curve are uniformly admitted to represent the course of any case involving febrile action, if the antiseptic system makes its claims justly, ovariotomies performed under its precautions ought to indicate a more even and less febrile course of recovery than the non-antiseptic cases, and this should occur independently of all other details of the operation.

I would put the possible conclusion briefly thus: If we find a marked difference between the curves of cases treated antiseptically and those not so in favor of the former, then I think I may say that more has been done to establish Mr. Lister's view than anything I have yet seen. If there be no difference, then the question is just where it was; but if there be a difference on the other side, then I think the application of the germ theory to surgical practice will be certain to fade away from professional and popular acceptance just as many fair-looking visions have done before.

In order to test this point as far as I could I took the morning and evening observations of the temperature and pulse for each case during a period of ten days, and constructed for the morning and evening of each day an average of the total observations under discussion, and marked this upon graphic paper. I took ten days as the limit, because I believed that this exceeded by at least three days the average period of stable recovery in cases of ovariotomy, and because it was the limit to which the observations could be extended with full material. My general impression was that a successful ovariotomy was practically well on the sixth day; but it will be seen from the figures that, like other general impressions, this is quite a mistake, for convalescence is not fully established till the eighth day, and is certainly not complete on the tenth. Therefore, probably my conclusions would have been better with more extended observations. I also see now that my statements would have been more perfect if I had carried out my figures to two or even three places, but this would have involved a great amount of labor.

Concerning the mere duration of recovery, some interesting conclusions were indicated. From the curves which I constructed from the whole of the one hundred cases, it is quite evident from the pulse curve, still more from the temperature

curve, that recovery takes a sudden progress forward on the eighth day, but that it is not then complete. On the sixth and seventh days the temperature gives distinct indications of exaltation, especially nocturnal, and this is clearly seen, on examination of the constituent curves, to be due to the suppuration consequent on the separation of the clamp, and probably, also, on the formation of stitch-hole abscesses.

The consideration of this curve leads me to say that I attach less value to the temperature curves than to the pulse curves, for the reason that the temperature during the course of recovery from ovariotomy is liable to extraordinary explosions. I have repeatedly seen a patient's temperature rise three or four degrees, and in one recent case six degrees centigrade, without the slightest apparent reason, the exaltation lasting from half an hour to three or four hours, and then the temperature would fall quite as rapidly, leaving the patient without any appearance of effect, or any record of it, save on the chart. This is not the case with the pulse curve, for if that rises the general appearance of the patient, and other signs and symptoms, amply prove that something is wrong, and the changes of the curve do not occur or give way with rapidity, but always gradually. Therefore, temperature readings require to be far more numerous than pulse readings to give the same uniformity of result. Pulse readings are also not subject to such influence by limited suppuration as temperature readings, and this is shown by the marked difference in the temperature and pulse curves on the sixth and seventh days. Further, the temperature rises almost uniformly at night during the progress of recovery, while the pulse does not do so after the fourth night, and this confirms my general impression that the fourth night is the critical night of the course of an ovariotomy. My conclusion is finally confirmed by the fact that, while I have seen a case end badly without the temperature rising to any remarkable height, I have invariably found the pulse rise continuously till it disappeared.

It is made almost certain by all that I have seen, as far as my practice is concerned at least, that the improvement is due chiefly to the introduction of the intra-peritoneal treatment of the pedicle, and as far as I can discover there is nothing to be credited to antiseptic precautions, for the difference in result between the ligature used under antiseptic precautions and without them is not worth discussing, and depends really upon one death in one hundred and thirty-nine cases, and that death was not due in any either to septics or antiseptics, but would have occurred in any case. I have seen a case killed by thymol, and

this agent has been, by common consent, quite abandoned. Perhaps I have never seen carbolic acid kill a patient, as it is used now, but I have seen it produce very serious symptoms, and, therefore, I have entirely discontinued Mr. Lister's practice in abdominal surgery save in such cases as those conjointly responsible with myself desire that it should be employed. The only detail I have retained is the practice of having my instruments in a water-bath, which I find to be an easy and effective method of keeping them clean.

Dr. Keith has quite recently come to somewhat similar conclusions, and he has given up Listerism for abdominal surgery, and he told us at the recent International Congress, in London, that Listerism would add two or three per cent. to the mortality of ovariotomy. I am certain he is right.

For ovariotomy I generally have the patient anæsthetized on the bed and then lifted upon the table, the object of this being that she is saved the distress of seeing the preparations which have been made for the operation, as the instruments and other appliances are not brought into the room until she is unconscious. When upon the table, the arms and legs are secured by belts, and two clean towels are arranged so as to leave the abdomen exposed between them. I prefer this method to the rubber cloth with a hole cut in it, originally used by the late Sir William Ferguson, for it entails difficulty in securing the latter to the patient's skin, and, as a rule, I have not found that it saves very much mess. The bladder has previously been emptied by the nurse, and I have personally inspected all the arrangements of instruments, sponges, etc., to see that nothing has been omitted.

I begin the incision midway between the umbilicus and the pubes and cut downward, going completely through the skin and subcutaneous fat at one cut, for a distance of about two and a half inches. I then look for the white line and divide it to an equal extent, and after that I cautiously divide the fat and transversalis fascia until the peritoneum is exposed. A pause is then made, and every bleeding point is secured by a pair of scissor-forceps. I never, in the first instance, make an incision any longer than two and a half inches, because I am very averse to making the wound any larger than it need be, and, as a rule, three inches is enough. As soon as all the bleeding points have been secured, I open the peritoneum so as to admit my forefinger, and with that I make a brief preliminary exploration. The peritoneum is then laid open to an extent corresponding with the wound in the other tissues and the cyst is exposed, any further bleeding being immediately arrested. In this stage of the operation, indeed throughout the entire proceeding, I never use

any other director than my finger, for upon that I can place most reliance. If there should be no adhesions between the tumor and the peritoneum in front, this part of the operation is very simple; but if there be adhesions it is often no easy matter to determine the point of union between the two membranes, and it is in such a state of matters that inexperienced operators are apt to commit a grievous error. I have twice known instances, and I have heard of several others, where the glistening peritoneum has been mistaken for the wall of the cyst, and the operators have industriously set to work to separate the former membrane from the transversalis fascia. Sometimes, even when there is no adhesion, the peritoneum is so greatly altered in appearance, having become so thick, leathery, and gelatinous, as to deceive any but the most experienced operator. When the point of union between the cyst and peritoneum cannot be discovered, the best way is to cut cautiously inward until the cyst be opened, because then it can be emptied, the whole wall carefully taken up and examined, and the several parts accurately determined. This, however, wants great caution, for it may be that a piece of intestine lies in the way and may be opened by mistake; but the experiences of a few cases will enable an observant surgeon easily to determine when he is cutting through muscular fibre. When the cyst is reached, it should be tapped by a large-sized syphon trocar (Fig. 31, p. 261) and emptied as quickly as possible. Sometimes, however, the contents of an ovarian cystoma are glairy or even perfectly gelatinous and will not pass through a trocar, and nothing tries the presence of mind of an inexperienced operator more than this. The peculiar adhesive mass which sometimes fills an ovarian cyst will neither pass through a trocar, nor be seized by the hand nor lifted up by a sponge, and to remove it from the cyst is often a matter of the greatest difficulty. It was upon such a tumor as this that Houston performed the first ovariotomy. In such a case the walls of the cyst are generally thin and extremely fragile, so that they cannot be held by forceps. Such tumors are also often very adherent, the adhesions bleeding very freely when broken down, and many of these cases are lost on account of the mere extension of time required for the removal of the cyst contents.[1] In a case like this there is nothing for it but to enlarge the incision to the extent of four or five inches, or even more, and to remove the mass by the hands as rapidly as possible, the assistant aid-

[1] In a pretty table (III.) in his book Resultats Statistiques de l'Ovariotomie, M. Koeberle shows conclusively how fatal protracted operations are. It follows, therefore, that in cases of difficulty—and these are the only fatal ones—an experienced, skilful, and rapid operator is sure to have by far the best results.

ing in the process by squeezing upon the flanks. If, however, the cyst contents are fluid and the tumor consists mainly of one cyst without adhesions, nothing would seem more simple than an ovariotomy, and the removal of a parovarian cyst is usually a proceeding which takes a shorter time to perform than to describe.

When the cyst has been emptied, it should be drawn gently out through the wound; and if non-adherent, no obstacle will be met with unless it be the presence of some secondary cysts. The trocar having been maintained in its position by means of the spring catches at its sides, its point should be made to enter these cysts, and thus empty them; but very often the contents of these secondary cysts are much thicker than the fluid contained in the major ones, and sometimes they are so numerous and of such small size that they constitute a solid mass at the base of the tumor. In such a case I lay open the major cyst, and, passing my hand within it, I break up into its cavity as much as I can of the secondary cyst mass, in order to save any further enlargement of the abdominal wound. During the whole of these proceedings as much care as possible should be taken to prevent any escape of the cyst contents into the abdominal cavity.

Adhesions are mediate or immediate, the former being generally parietal or omental, existing in the shape of round or flattened bands of peritoneum. They seem to be formed by isolated patches of adhesion, which have dragged off the peritoneum from the abdominal wall, or by pieces of adherent omentum. They are seldom large, and are only of any moment when traversed by an important blood-vessel, which must always be secured by torsion or ligature, or by the cautery. The immediate adhesions require great patience to overcome them; but even when they unite the tumors to the most important organs, they may be separated, or a piece of the cyst may be detached and left. One of the dangers of the removal of an ovarian tumor with extensive pelvic adhesions—and still more of a uterine tumor—is damage to one or both ureters. It will be remembered that they run down obliquely from the kidney over the brim of the pelvis down to the bladder, lying over on each side of the cervix, and close to it. I have often laid them bare, but have never been unfortunate enough to injure them. In the celebrated case published by Simon (*Chirurgie der Nieren.*, 1870), the ureter had been dragged into the wound and clamped, and the patient recovered, with a urinary fistula of the left ureter. Simon heroically removed the left kidney and cured his patient, and I saw her alive some years after the operation, and at the

same time I saw her tumor in one bottle and her left kidney in another.

Nussbaum had a similar accident, and he cured his patient by making an artificial continuation of the ureter between its divided points. When dividing the pedicle of a large tumor of old standing, I have several times seen hugely dilated lymphatic trunks, which looked very much like ureters—a resemblance which more than once made me very uncomfortable for a few days.

Many curious and exceptional forms of adhesion occur, one of the most remarkable of which is occasional adhesion of the tip of the vermiform appendix of the cæcum to the tumor. I have seen this three times, twice in my own practice. The first time I saw it it was unfortunately divided by the scissors of the operating surgeon, with a fatal result; and it was only the discovery of this misfortune at the post-mortem which enabled me to recognize a similar condition, and avoid a like disaster in my own cases. In dealing with pelvic adhesions it must be borne in mind that the large venous trunks in that region lie immediately under the peritoneum, and that they must be carefully avoided. In the first operation I ever saw, one of these trunks was torn across by the rough handling of an inexperienced operator, and I need hardly say that death ensued in a very few hours, because it was quite impossible to secure the bleeding point, or even to find it. Immediate death was only staved off by stuffing the pelvis with a towel. This event made such a strong impression upon me that I thought it hardly possible I ever could take part myself in an operation fraught with such terrible risks. Hemorrhage from adhesions has always a very strong tendency to arrest itself, and most of the bleeding points will stop of their own accord, or can be stopped by simple pressure with a sponge; each point of importance should be seized by a pair of scissor-forceps, or touched with a cautery, or with a piece of solid perchloride of iron. For the arrest of a general oozing from a large surface, the pressure of two or three dried sponges is generally enough, and it is, therefore, my practice always to separate adhesions as rapidly as possible, and to follow their separation by the immediate application of sponges, leaving these *in situ* until I have finished the separation and removal of the tumor.[1]

[1] A few days ago I removed a tumor universally adherent, full of pus, and quite rotten. I tore across the pedicle before I had recognized it, and its vessels bled profusely. Like the rest of the tissue, it was quite rotten, and the more I tied the vessels the more they bled. The cautery was equally useless. I therefore secured all the points by means of forceps, and left them *in situ* in the abdomen, closing the

Suppose the tumor separated and withdrawn, the assistant, stationed opposite the operator, should immediately insert one or two sponges to prevent the exit of the intestines, and he should then take possession of the tumor and steady it while the surgeon examines the pedicle, and determines how it may best be dealt with.

At present, we may say, there are only three methods in vogue for the treatment of the pedicle, and of these one—that is, the clamp—should be reserved for extremely exceptional instances. Probably not more than two or three cases in a hundred require now to be dealt with by the clamp; certainly, I have not met with more than one for the last three years. The kind of pedicle requiring the clamp is thick and soft, and so short as to contain, perhaps, a small piece of the tumor. With such a pedicle the extra-peritoneal method is admissible, and probably is superior to treatment either by the cautery or the ligature; but I am not quite sure that a combination of a drainage-tube with either of these latter methods may not yet be found superior to the clamp. If to the pedicle of an ovarian tumor, unusually thick and short, or in that of a uterine tumor, it is thought desirable to employ a clamp, then it is evident that what is wanted is a form of circular constriction by some means which will completely arrest the hemorrhage, will not cut the pedicle, and which will allow the wound to be accurately adjusted round the stump.

To meet all these requirements, I have devised a clamp (figured on opposite page) which has served my purpose better than any I have seen.

After a large number of experiments I selected thick copper wire (No. 12, Birmingham gauge), nickeled, and completely softened by being made red hot and allowed slowly to cool, as being the best material to work with.

For use I bend it into a loop, as seen in the right-hand figure, ready for the operation. After it has been placed round the pedicle, the collar *B* is run on close up to the pedicle. The handle *A E* is then also run on, the ends of the wires running in the holes *F F*, and the end *A* fitting into a counter-sunk hole, shown by a dotted line at *B*. The pinch-screws *D D* are then closed tightly down on the wires, the screws *C C* being quite loose. The handle at *E* is then turned slowly till the loop of wire firmly constricts the pedicle. The tumor is then removed, and if there be any bleeding, a few more turns of the handle *E*

wound over them. I reopened it in twenty-four hours, removed the forceps, refastened the wound with a drainage-tube at its lower angle, and the patient made an uninterrupted recovery.

will secure it. When this is done, the pinch-screws *C C* are to be securely tightened down on the wire, and those at *D D* loosened. The handle will then come off, leaving the wire clamp with its collar, as seen in the right-hand figure. The ends of the wire are then to be turned slightly up, and the wound closed and dressed as usual.

I have used this clamp in thick pedicles in eleven cases with perfect success, and six of those were uterine myomata.

Of the two intra-peritoneal methods of dealing with the pedicle, which, by common consent, have been adopted as superior to all others, it is as yet extremely difficult to give preference to one over the other. In the hands of Dr. Keith, the cautery, as originally introduced by Mr. Baker Brown, has been brilliantly

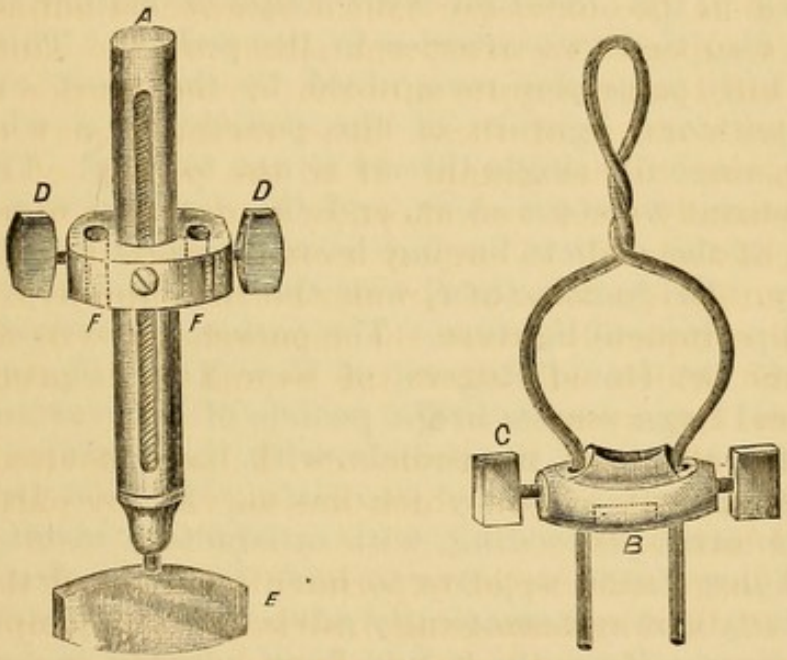


FIG. 33.—Tait's Wire Clamp.

successful; but in my own hands the silk ligature has proved not one bit inferior; and I shall retain it as long as it serves me as it has done up to the present moment; for it could only be by careful comparison of the results of a very extended experience that any determination would be arrived at as to the superiority of the one over the other. As soon as Dr. Keith has removed the tumor, he fastens on the pedicle the well-known cautery clamp of Mr. Baker Brown, screwing it up very tightly; he then divides the pedicle above the clamp by a common cautery iron at a dull red or almost black heat, searing the pedicle very slowly, but very completely. When he removes the clamp, that part of the pedicle which has been embraced by it is shrivelled and translucent, like parchment, and it very rarely gives him any

further trouble by bleeding; if it should do so, he screws it up again and applies the iron more effectually. He then drops the pedicle back into the cavity and proceeds with the further steps of the operation. I have watched him go through the process with great interest, but I was not impressed by anything beyond my previous convictions, that it is the intra-peritoneal method, and not any particular variety of it, which gives the success; and that the ligature as applied in "Tait's knot" occupies less than a tenth part of the time taken by the cautery, and, I think, must be more secure.

The history of the short ligature is given as follows by Mr. Alban Doran (*St. Bartholomew's Hospital Reports*, 1877):

"But Dr. Nathan Smith, of Connecticut, the second American ovariotomist, in his first operation in 1821, not only ligatured two arteries in the omentum with strips of leather from a kid glove, but also tied two arteries in the pedicle. This is in accordance with principles recognized by the most experienced modern operators, ligature of the pedicle, as a whole, being hazardous, since the single thread is apt to snap. The ends of all the ligatures were cut short, and the external wound closed, the stump of the pedicle having been returned into the abdominal cavity. Dr. Smith, then, was the first to adopt the complete intra-peritoneal ligature. The patient recovered.

"In 1829 Dr. David Rogers, of New York, ligatured separately several large vessels in the pedicle of an ovarian cyst, and returned the stump of the pedicle, with the ligatures cut short. The operation was perfectly successful. In 1835 Dr. Billinger adopted the same proceeding, with satisfactory results.

"Dr. Tyler Smith appears to have been the first authority who regularly and systematically advocated complete intra-peritoneal ligature. Recently it has been adopted in hundreds of successful cases where the pedicle has been found too short for the clamp to be safely applied. Ligatures of bleeding vessels in omentum are also cut short. As many as forty ligatures have been left in the abdominal cavity without any evil effects."

My own method of dealing with the pedicle is by a piece of silk thread, varying in thickness according to the mass of the pedicle; for a thin pedicle I use thin silk, and for a thick pedicle correspondingly thick cord, because, of course, in the latter case the ligature must be pulled with much more force than in the former. The method in which I use the ligature is that I have termed the Staffordshire knot, as it is the badge of the county of Stafford; and the idea of its use occurred to me while travelling in a Staffordshire railway carriage, in which it was a conspicuous ornament. Mr. Mazzinghi, the learned custodian of

the William Salt Library at Stafford, tells me that nothing is known definitely as to the origin of the badge, further than that it was granted by the College of Heralds to the county within living memory, and was copied from the arms of the old Stafford family at Maxstoke Castle. Such badges were used to distinguish the adherents of noble families, and their origin is generally altogether unknown. The legend in the present case is that rogues were at one time so numerous in Staffordshire that a knot had to be devised which would hang three at a time. If so, the original knot must have differed from its present representative, for the latter could only hang two. It is, however, its remarkable property in this direction which has led me to introduce its use in surgery.

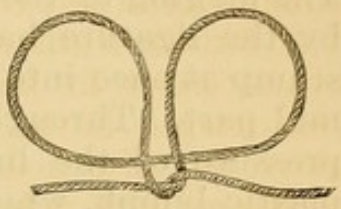


Fig. 34.—Tait's "Staffordshire Knot."

The woodcut will show how the knot acts, and a very brief description will probably make clear enough how it is used. An ordinary-handled needle, armed with a long piece of the silk required, is passed through the pedicle and then withdrawn so as to leave a loop on the distal side. This loop is then drawn over the ovary or tumor, and one of the free ends drawn through it, so that one end is above, while the other is under, the retracted loop. Both ends being seized in the hand, they are drawn through the pedicle, against which the thumb and forefinger of the left hand are pressed as a fulcrum, till complete constriction is made. A simple hitch is then made, as in the drawing, and tightened; and that is followed by another, as in ordinary ligature-tying. There is another and more complicated way of making the knot, by passing each end of the thread round the corresponding half of the pedicle, and crossing them within the loop in front, which is equally effective, and which may be used in cases of large solid tumors. But the former way is by far the more elegant and rapid method.

The advantages of this knot over all others are that, while it ties the pedicle in two halves, these halves are compressed really into one surface; the two halves are equally well compressed; and, from the mechanical arrangement of the knot, very great constricting force can be employed; and in this respect it greatly excels Dr. Peaslee's chain-ligature. I have used it now about two hundred times and it has never failed me on one single occasion. I cut the pedicle through about a quarter of an inch on the distal side of the ligature and drop it back. It may very reasonably be asked what becomes of a ligature so disposed of, and, fortunately for me, I am wholly unable to

answer the question, for I have not yet had an opportunity of seeing one on which I have placed it. I therefore gladly accept the description given by Mr. Alban Doran, in the *St. Bartholomew's Hospital Reports* for 1877 :

"In 1872 Dr. Bantock exhibited before the Obstetrical Society the stump of an ovarian pedicle from a patient who died of cancer one year after double ovariotomy had been performed upon her. The hempen ligature applied, with its ends cut short, to one of the pedicles, was found on dissection to have been completely absorbed, excepting its knot, which remained as a hard body the size of a hemp-seed, covered by peritoneum. The bulging of the tissues over each side of the groove formed by the ligature had brought the strangulated portion of the stump at once into close contact with the unstrangulated proximal part. Through the slight irritation produced at first by the pressure of the ligature, the proximal part had thrown out plastic lymph, which had conveyed nutritive plasma and also capillaries to the distal portion of the stump, and thus saved it from gangrene. In a case like this, the stump ultimately atrophies, for reasons evident to any surgeon with a superficial knowledge of pathology. As for the ligature, it is destroyed in the manner demonstrated by the experiments of Spiegelberg and Waldeyer."

Hegar has described a case where the pedicle sloughed and was passed *en masse* by the rectum, and on it was found the ligature which had been applied. The patient passed successfully through this terrible ordeal, and I hope her accident will remain without imitation.

The next step of the operation is to examine the other ovary, and if this be found diseased, to remove it in a similar way ; and I cannot help thinking that the cases are much more numerous than they used to be in which it is becoming necessary to remove the second ovary on account of cystic growth. Certainly, both in my own practice and in those of others which I have had an opportunity of seeing, the second ovary has been diseased and has required removal more frequently than used to be the case.

An exceptional method of dealing with the pedicle—the one introduced by Dr. Miner and called enucleation—deserves notice, because it must occasionally be resorted to in cases of sessile tumors. It is by no means easy of performance and always gives rise to very troublesome hemorrhage. This method consists in opening the peritoneal capsule at the base of the tumor, and scratching or dissecting out the cyst from the matrix in which its base is placed. Each bleeding point must be carefully

secured as it is divided, and when the tumor is removed the peritoneal stump must be gathered together and the edges fastened by sutures, just as in an amputation. I have three times employed this method with eminently satisfactory results; but I am bound to say that without a good deal of experience in the separation of adhesions I would have stopped in the middle of the process and left it incomplete, on account of its difficulties.

The next duty of the surgeon is to carefully cleanse the peritoneal cavity from all blood-clots and other *débris* which may be in it. For the introduction of this practice and the extreme care with which it is now done, as well as for the excellent results obtained from it, we are entirely indebted to Dr. Keith, though we find that he was partly anticipated by McDowell, who tells us, in his description of second operation: "Notwithstanding my great care, a quart or more of blood escaped into the abdomen; and, after the hemorrhage ceased, I removed, as cleanly as possible, the blood, in which the bowels were completely enveloped." So highly important do I consider careful attention to this precaution that I make no apology for giving the following somewhat lengthy extract from Dr. Marion Sims' description of Dr. Keith's method:

"When Dr. Keith performed his first operation in 1862, he was surrounded by old men in the profession who had a dread of wounding the peritoneum continually before their eyes. He was obliged to break up extensive adhesions, and, as a consequence, there was free exudation of blood. Before closing the external wound he began to sponge out the peritoneal cavity and suddenly thrust a large sponge down into the pelvis and brought it up saturated with blood. Squeezing it dry, he was about to repeat this process, when they all united in begging him not to do it, as from their stand-point there would be more danger in irritating the delicate peritoneum with the sponge than by leaving the blood there to be absorbed. He yielded against his judgment and closed the wound, leaving a large quantity of blood in the peritoneal cavity. On the third day afterward his patient was profoundly septicæmic, and in imminent danger. He recognized the source of danger and had the courage to open the lower angle of the wound, by removing two or three sutures. There was an immediate discharge of fetid bloody serum in large quantities, and from that moment the patient began to improve and soon got well. This made a profound impression on Dr. Keith's mind, and he determined from that time never again to leave extravasated blood in the peritoneal cavity if he could possibly remove it. It was not long before he had an opportunity of putting this principle to the test of experiment, for his

second case was a very bad one, with extensive adhesion. He had to tie many vessels and bleeding points. There was a large exudation of blood in the pelvic cavity, and he sponged it all out thoroughly, after which he closed up the external wound, and his patient recovered without a single bad symptom. From this time he adopted the principle of never closing an external wound till he had controlled all oozing of blood and made sure that the peritoneal cavity was dry and clean." I have seen Dr. Keith do this, and there can be no doubt that in this, as in other respects, he has greatly added to the success of ovariotomy.

This process has been called by the German surgeons the "toilet" of the peritoneum, and no care can be too great in its performance. I generally clean out the cavity of the pelvis and the hollow of each loin by two or three sponges, and then fill the whole abdomen full of tepid water by means of a tube running from a ewer or a suspended cistern, closing the wound as well as I can with one hand while the other is inside. I move my fingers rapidly about among the intestines and give them a good wash in the water. I then empty the cavity and refill it two or three times until the water comes out quite clear. In this way I very rapidly determine if there is any bleeding, because even a small point will perceptibly tinge the water I have poured in; and if the water continues colored I immediately hunt about until I have found and secured the bleeding point. After the cleansing has been satisfactorily accomplished I put a number of dry sponges down in the pelvis and over each kidney and then proceed to insert the sutures in the wound. By the time this is accomplished the dry sponges have soaked up all the water, and the peritoneum is generally found quite clean and dry. But if it should not be satisfactorily so, I repeat the process until I am quite certain nothing has been left behind, and in this way I believe I have added largely to the success of my practice.

Dr. Keith places great reliance on the use of drainage-tubes as originally introduced by Koeberle in 1867, and improved by himself; and this is a practice I have employed frequently of late and I think with very great advantage. Dr. Keith used to think the Listerian method obviated the necessity for drainage, and that by its employment any fluid left in the abdomen was prevented from decomposing; but my own experience satisfies me that neither of them need be used if the abdomen can be properly cleansed and dried. Dr. Keith has now given up Listerism and his results are quite as good without it as with it, perhaps better, for he has had two deaths from carbolic acid poisoning. I think the peritoneum itself will do a great deal of work in removing the *débris;* but the surgeon's object should be to give it as little un-

necessary employment as possible, and therefore I adhere most closely to Dr. Keith's practice in this respect, and to it I may largely attribute my increased success. Finally, I take the utmost care with the sutures to see that they are ranged evenly, that they include all the structures of the abdominal wall,[1] that the stitch-holes do not bleed, and that the wound is most accurately closed. I am never satisfied to leave an eighth of an inch of gaping wound, and I take the utmost care that the edges of the skin are in correct adaptation. I always use silk sutures, and introduce them generally by means of a large crochet needle. In fastening the sutures, as well as in tying all ligatures which are not transfixed, I always use the knot here figured, having two turns in its first hitch, so that when this is pulled tight it does not slip before the second hitch is made and drawn up. For ligatures that are transfixed I use the Staffordshire knot.

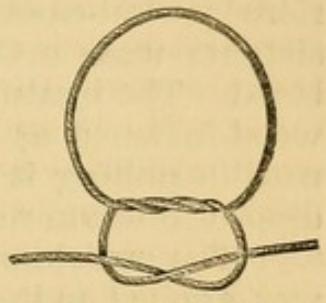


Fig. 35.—Double-hitch Knot.

For the first dressing of the wound I use nothing but the absorbent cotton-wool introduced by Mr. Sampson Gamgee, and I know of no greater addition to our means of treating wounds than this simple material. It is made up into pads of different sizes and shapes, averaging about five inches square, and from one and a half to two inches thick. Two or three of these are placed over the wound and are secured in their places by two or three narrow straps of sticking-plaster, the whole dressing being covered by a cotton binder round the patient's waist, and this is fastened with safety-pins. This dressing is rarely touched before the fourth day, when fresh padding is adjusted. On the sixth or seventh day I remove every alternate stitch and the rest are removed on the day following, and it is quite an unusual thing to find the wound otherwise than completely and permanently united.

In the *Medical Times* of March, 1874, Dr. S. G. Stephens, of Rio Bueno, Valdivia, Chili, gives an account of an "Ovariotomy under Difficulties," which is such a splendid instance of surgical pluck, crowned, fortunately, with success, that I wish to do what

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[1] A great deal of fuss has recently been made concerning some experiments performed by Mr. Spencer Wells on rabbits and other animals, relative to the inclusion of the peritoneum in the stitches, and it is claimed by Mr. Wells that by these experiments hundreds of women's lives have been saved. As it is a surgical rule to secure the co aptation of all divided structures, as it never was doubted that the peritoneum was an exception to this rule, Mr. Spencer Wells' experiments were altogether needless, and contributed nothing whatever to the advance of abdominal surgery. In many instances I have been obliged to leave the peritoneum out of the stitches, and I never saw that this exclusion made the least difference to the patient's recovery.

little I can to commemorate the hero, and therefore I give his account here in full :

"We had to send to Valdivia for chloroform—four days' journey. In the meantime I occupied myself in preparing the patient, in considering with what instruments I was to perform the operation, and selecting and instructing my assistants. The instruments were a trocar made from a piece of colhuihue[1] about ten inches long, hollowed out, and sharpened to a point at one end, and at the other connected with a piece of india-rubber tubing from an enema syringe ; the instruments from a 'Charrière' pocket-case, and a pair of craniotomy forceps. The assistants were a Catholic missionary, two Indians, and a half-blood. The ligature was made of raw-hide, with two pieces of wood fastened at the ends, in order that more power could be used in pulling it tight, and at the time of using it was to be dipped in warm neatsfoot oil.

"The pedicle was rather long, but flat ; the raw-hide ligature was applied to it, and tightened by means of the two pieces of wood pulled by the two assistants on each side of the body until it was almost buried in the parts, and then made fast with two lasso-knots,[2] the ends cut off, and the whole dropped into the cavity. The cavity was mopped out with cotton-wool, and the wound closed with fine iron-wire sutures, pushed through from within outward and twisted, and a superficial continuous suture of silk. Water-dressing was next applied, and a warmed bayeta flannel roller passed twice round the body. Consciousness returned before I could get her off the table, owing to the priest not attending to the chloroform, being too occupied and astonished at my movements ; in fact, throughout the whole proceedings I had constantly to attend to the pulse. Great exhaustion followed ; and I had first to administer warm wine-and-water, and afterward warm whiskey-and-water, apply friction to the extremities, until, finally, at five o'clock in the afternoon, she had improved very much, with a pulse at 115, and the surface warm and moist. My thermometer was broken, so I could not note the temperature. I remained in the neighborhood twelve days to attend to her, during which time she went on well, with the exception of a little vomiting the day after the operation, owing to the husband giving her warm lamb's blood without my knowledge. The first pair of sutures were removed on January 28th, and so on, day by day, one or

[1] A species of bamboo.

[2] One on each side—*i. e.*, one tied first, and then the ends carried round to the opposite side ; a slit made in one end, and the other cut in the form of a knob, which passes through it, thus preventing slackening through swelling.

more was removed, until the ninth, or middle one, was taken out.

"Never having seen the operation, nor read any special work on the subject, I had nothing to direct me but the short account given in the last edition of Dr. Tanner's 'Practice of Medicine.'"

Occasionally we are called upon to deal with an ovarian tumor in a woman who is pregnant—a complication which may or may not be discovered before the operation. Some years ago the question of the propriety of removing an ovarian tumor in a pregnant woman was discussed before one of the medical societies, and various opinions were given. By some obstetric physicians the opinion was expressed that it would be better to induce premature labor, and that after the patient had recovered from this, we should perform ovariotomy. Mr. Spencer Wells and myself, on the other hand, contended that it would be much better to perform ovariotomy, and leave the pregnancy alone, and this plan has now become the accepted practice. At that time Mr. Wells had operated upon ten pregnant women, and nine of these cases were successful. I do not know what his experience may have been since, and I have not found any record of the experience of any one else upon this subject; but since the discussion I have operated upon ten pregnant women with uniform success. Before that time I had only operated in one such case. The result was fatal, and was undoubtedly due to the use of the clamp, for the cause of death was gangrene of the pedicle. I do not now think pregnancy offers any bar to the operation. In all of my cases I have been able to recognize the pregnancy before I opened the abdomen; but I can easily imagine that it might occur to the most experienced surgeon to operate on a woman in whom he had not previously recognized the existence of the complication. Indeed, Mr. Wells tells us of a case in which he punctured a pregnant uterus with a trocar, having mistaken it for a cyst. He opened the uterus, emptied it of its contents, and the patient recovered.[1] This is one of the complications, therefore, to be especially borne in mind. The usual color and appearance of an ovarian cyst is as a rule sufficiently characteristic to make it easily recognizable from a pregnant uterus; yet I can easily imagine circumstances such as Mr. Wells encountered, that would lead to such a mistake; and should this misfortune happen, the bold proceeding he followed would certainly be the best practice.

We not unfrequently find tumors of the uterus associated

[1] A similar accident happened to Dr. Byford, of Chicago, and he successfully followed out the same practice as did Mr. Wells. (American Journal of Obstetrics, January, 1879.)

with cystic disease of the ovaries. It has happened to me very frequently to find a very large part of the mass which I believed to be entirely ovarian formed of a uterine myoma associated with an ovarian cystoma. Under such circumstances the practice now universally adopted is to remove the ovarian tumor and leave the uterine mass alone, but formerly it was regarded as the correct practice to remove both. Further on I shall deal again with this important subject, but here I will say it is my uniform practice now, when I find a uterine myoma in existence as well as an ovarian tumor, to remove both ovaries and tubes, as in this way we can arrest the growth of the tumor we cannot remove; indeed, in some of my cases the tumor has entirely disappeared.

One of the most interesting additions to our advances in abdominal surgery is that originally derived from an operation performed by Dr. Wiltshire, who removed an ovarian tumor from a woman suffering from symptoms of the utmost gravity, due to peritonitis and gangrene of the tumor. The case is described in the "Transactions of the Pathological Society" for 1868, the operation having been performed in May of that year.

Rapid increase of the tumor had taken place, and there were symptoms of the most urgent kind present. Vomiting had been incessant for three days, when, after unusual exertion, rapid enlargement of the tumor had begun. The pulse was quick and feeble, the extremities blue, and the patient's general condition one of collapse.

The extreme tension of the abdominal parietes was shown by the way in which the tumor shot up into the wound directly the incision reached the peritoneal cavity; it had also rotated. Blood escaped on puncture, and at one place the cyst wall gave way when touched, owing to extreme thinness. The pedicle was rotten, and the *right* cornua of the uterus had to be transfixed and tied to arrest hemorrhage.

The tumor proved to be of the right ovary and multilocular, the loculi being distended with blood. It had rotated on its pedicle four days before the operation, strangulation ensuing. The twist was from right to left, and appeared to have given two turns. The pedicle was quite small and short.

I think sufficient praise can hardly be given to Dr. Wiltshire for his courage in performing the operation under such urgent conditions, and it is not too much to say that to his success in this case we owe a new departure in the practice of abdominal surgery by which operations under acute symptoms are undertaken, and, apparently, with results as satisfactory as those obtained in cases free from emergency.

This remarkable axial rotation is an incident in the life-history of ovarian tumors, which has not yet received as much attention as either its importance or its frequency deserves, and, so far as I know, no perfectly satisfactory explanation of the method of its occurrence has been given.

So far as I can find, the first notice of the incident is made by the same author who has written most about it, Hofrath Professor Carl Rokitansky, who describes it in his "Handbuch der Pathologischen Anatomie" (vol. i.) in 1841. There the description is not full, but it is certain that he had then seen it, and in his future papers he tells more about it than does any other author; indeed, most other writers have taken their descriptions from him with more or less acknowledgment.

I have found reference to a note of a paper by him in the *Allgemeine Wien Medizinische Zeitschrift* for 1840, but have not been able to find the original paper. Possibly the note in question is a misprint, though the title is given in full, "Ueber Abschnerung der Tuben und Ovarien und ueber Strangulation der Letzeren durch Achsendrehung."

Rokitansky has also written very full papers in the *Allgemeine Wiener Medizinische Zeitung*, 1860; in the *Zeitschrift der K. K. Gesellschaft der Aerzte in Wien*, 1865; "Ueber der Strangulation von Ovarialtumoren durch Achsendrehung."

Dr. Van Buren narrates two cases in which he noticed the twisting of the pedicle of an ovarian tumor, in the *New York Journal of Medicine*, 1850 and 1851.

In the first the tumor was on the left side, but the direction of the twist is not given. The twist had not strangulated the tumor, and did not hasten the ovariotomy, which was successful.

The second case was one in which acute peritonitis was diagnosed on August 28th, and the patient died on September 8th. On post-mortem examination the tumor was found very dark in color, almost black. It was a tumor of the right ovary, but the direction of the twist is not stated. "The twisting of the pedicle interrupted entirely the circulation, the tumor thus became engorged with blood, thence peritonitis, followed by enteritis, causing death." The tumor had made one and a half revolutions only, the pedicle being short.

Dr. Patruban (*Oesterreiches Zeitschrift für practische Heilkunde*, 1855) publishes a case where the torsion produced rapidly fatal intracystic hemorrhage.

Dr. Crome, of Brooklyn (*American Medical Monthly*, 1861), had a case where the strangulation occurred twenty-four hours before labor in a small tumor, the patient dying of peritonitis

on the fifth day. The accident was indicated by the access of agonizing pain in the left side. The cyst was found ruptured and in a state of gangrene.

In his book on "Diseases of the Ovaries," Mr. Spencer Wells mentions that, during his first five hundred cases, he found the pedicle twisted in about twelve cases, but no mention is made of any of the tumors being consequently gangrenous, or that the operation was thereby hastened.

In the *Archiv für Gynecologie* for 1878, Dr. Veit, of Berlin, quoting Schroeder, says, that in his 94 cases of ovariotomy, axial rotation was observed 13 times, and Olshausen is of opinion that the tumors are generally non-adherent.

Dr. St. John Edwards, of Malta, has published a case in the *Lancet*, of October, 1861, in which he had recognized an ovarian tumor during the lady's first pregnancy. Her second labor occurred prematurely, sudden abdominal pain supervened on the second day after, and she died on the fourth. The tumor was found to be of a livid purple color, with patches of extravasated blood, and rents in its walls. The right ovary was flattened out on its under aspect (so that it must have been a parovarian cyst). The pedicle was two inches long, and had been twisted one and a half times round. It was intensely congested, and the ovary was full of dark extravasated blood (closely resembling one of my own cases). There was no peritonitis, and the tumor was absolutely free from adhesions. The contents of the sac were claret-like. He attributes the twisting to the expulsive action of the uterus, though the accession of pain was not till about forty-eight hours after labor.

In the *Edinburgh Medical Journal* I published the following case, which I desire to reproduce here, as it was the first of my experience of this remarkable accident:

On August 18, 1868, I was called in consultation by my friend, Mr. Lorraine, of Wakefield, to see Mrs. C——, aged forty-eight, who was suffering from a strangulated femoral hernia. I found the tumor of small size, that the symptoms had existed only two days, and that it was irreducible by the taxis under chloroform. I suggested a full dose of belladonna and a delay of six hours. At the end of that period I again tried the taxis under chloroform, but without being able to reduce the hernia, so I at once performed Gay's operations, divided Gimbernat's ligament freely, and without any trouble succeeded in returning the bowel.

At 7.30 on the morning of the 19th she was much relieved, free from pain, and the vomiting had quite ceased. Opium was

administered freely, and iced brandy-and-water or Moselle *ad libitum*.

August 20th, 8 A.M.—The abdomen was slightly tympanitic, and the pulse about 140, the patient being free from pain and sickness. 8 P.M.—Tympanitis increased; ordered a turpentine stupe.

August 21st, 8 A.M.—Tympanitis so extreme that I entertained the idea of puncturing the intestines. Temperature in axilla, 101.6°; no pain or sickness, and she takes beef-tea and stimulants freely; face very anxious in expression. 10 P.M.—Mr. Lorraine had seen her in the afternoon, and reported that she was somewhat better. When we met we found that the distention was much less; there was no pain and no narcotism, as the opium had been intermitted; rectum examined per vaginam, and found quite empty; temperature in axilla, 101°.

August 22d.—In the forenoon she had two moderately sized and very offensive stools; in the afternoon she was seen by my friend, Mr. Kemp (in whose practice the case occurred), who noticed, and remarked to me afterward, that the breath had the hay odor. At 10 P.M. I saw her with Mr. Lorraine, and we both noticed the musty smell of the breath. She was sinking then, and died at 8 A.M. on the morning of the 23d.

Twelve hours after death I made a post-mortem examination, with the kind assistance of Mr. Lorraine and Mr. J. Kemp. The wound made to relieve the strangulation had healed by first intention. On opening the abdomen I found the small intestines much distended with flatus. The sac of the hernia was empty and uninjured. On separating the intestines a black gangrenous mass was observed lying in the concavity of the right ilium. On passing my hand round it I discovered that it was a small ovarian tumor, consisting of two equal-sized cysts, one of which was totally gangrenous, and so soft as to break up with the most gentle handling, and discharge into the cavity a quantity of dark fetid serum; the other cyst was partially gangrenous. The tumor measured about eleven inches long and four inches in its greatest diameter, and it had a constriction between the two cysts. Its base was slightly glued to the brim of the pelvis; but, with this exception, there was no peritonitis. The tumor lay across the transverse diameter of the pelvis, the left end being buried in the pelvis, while the right lay over the brim on to the ilium. It was the right-hand cyst which was totally gangrenous.

When I passed my hand down the pedicle I found that it was long and thin, and twisted on itself, feeling more like an injected umbilical cord than anything else with which I am acquainted.

I remarked to my colleagues that the pedicle was twisted, and, keeping it in my left hand, with my right I slowly untwisted it, by rotating the tumor until the pedicle was straight. To do this, I had to alter my grasp of the tumor nine times; that is, the pedicle had been twisted by four and a half revolutions of the tumor. It was the right ovary which was diseased (and the twisting was from within outward toward the right side, as far as my recollection now serves me).

Concerning this case, I have ever since had a suspicion that my operation for hernia was an unnecessary one, and that all the symptoms were really due to the gangrenous tumor. If this were really so, I have the consolation that I did my patient no harm.

When this case came under my care I had never heard of the accident, indeed it occurred nearly twelve years ago, and my experience of ovarian tumors was somewhat more limited than it is now. It made a deep impression on me, however, and I resolved if ever I met with such symptoms in another woman, and could discover the presence of a tumor, I should not hesitate to attempt its removal. This determination I have been able to carry into effect on nine occasions with perfectly successful results. Of course, I cannot but regret that I did not recognize the existence of this tumor when I had the patient under chloroform, as I think I could do now with my larger experience in abdominal surgery, though, perhaps, my youth and inexperience at the time form a barely sufficient apology.

The next case I find on record is one published by Dr. Barnes in *St. Thomas's Hospital Reports* for 1870, where Mr. Spencer Wells, Dr. Tyler Smith, and Dr. Oldham had all recognized the presence of an ovarian tumor. Dr. Barnes saw her on August 26th, and on September 2d, when the diagnosis of pregnancy, in addition, was made. On the 25th there were all the indications of mischief in the cyst, and Dr. Barnes discussed the question, "Has the extra-uterine cyst ruptured?" On that day a premature fœtus was expelled, and she lingered on till October 4th without any attempt at surgical interference.

At the post-mortem "a cyst came into view, dark-colored, stained with blood in several points, having extravasated blood clotted in its walls. In places it was found very fragile; it had twisted twice axially from right to left during life."

I do not think there can be a doubt that if this case had been operated upon, as it might have been, seeing the tumor had been recognized, the patient would have recovered.

A still more curious case is related by Dr. Barnes in the

same paper, where the symptoms of strangulation were taken for those of labor, and where, on post-mortem examination, he says he found an ovarian tumor entirely free from adhesions, with its pedicle twisted twice into a rope, the appearances of gangrene being conclusive. Such a case would be just such a one in which ovariotomy would be, and has been in my hands, successful.

At a meeting of the Dublin Pathological Society, December 4, 1879, Dr. Kidd showed the preparation from a woman whom he had had under his care in the Coombe Hospital, and who had died under circumstances which clearly pointed to something wrong in a tumor which had been recognized some months before. The preparation was that of an uncomplicated ovarian tumor, with twisted pedicle and consequent gangrene. "There was a complete turn upon the pedicle; this had strangulated the tumor, and thus gave rise to the black appearance, and the woman died from irritative fever, produced by strangulation and sphacelation of the morbid growth." This is another case where I think there is cause for regret that an attempt at removing the tumor was not made. From the experience I am about to give of my own practice, I think there can be little doubt that the rule will be established that if the existence of an ovarian tumor has been, or can be recognized, and symptoms should set in which are of a serious kind, and can be referred to strangulation of that tumor, an exploratory incision should be made, and the tumor removed if possible, especially if it be found to be the seat of the mischief.

During 1879 I had the remarkable occurrence in my practice of three cases of gangrene of ovarian or parovarian tumors, due to axial rotation.

The first case was sent to me by Dr. Faussett, of Tamworth. She was forty-six years of age, her last confinement was four years before, and her menstruation was normal. I saw her first in March last on account of a small tumor, which I diagnosed to be monocystic, and probably parovarian. I advised her to defer any operation till it was larger. She returned on June 9th with the tumor greatly enlarged, and suffering from intense abdominal pain. Her face had a peculiar anxious expression, and her temperature rose to 39° C. at night. I therefore recommended the immediate removal of the tumor. On opening the abdomen I found the cyst of a black pearly color, universally adherent by recent lymph, its contents quite black, and its walls black, gangrenous, and in places quite rotten. The pedicle was twisted three or four times, and at the point of maximum constriction it was only as thick as an artist's pencil. I tied it just

below this point. After the operation she had no pain, the temperature never rose above 37° C., and she made an uninterrupted recovery. The right ovary was involved in the gangrene, but it was free from the tumor. The rotation had occurred from within outward to the right. The operation was performed without any of the Listerian antiseptic precautions.

The second case occurred in a patient from Sheffield, placed under my care by my colleague, Dr. Edginton. She was thirty years of age, had been married ten years, but had never been pregnant.

She had noticed a gradual increase in size for nine months previous to my seeing her. Sudden and violent pain in the abdomen occurred on the 4th of November, followed by incessant sickness. When I saw her on the 11th the diagnosis of an ovarian tumor was simple, and her anxious appearance, the green sickness, feeble pulse, and the intense pain, all pointed to the probability of strangulation of the tumor. I therefore admitted her at once to the hospital, and removed the tumor next day. It was found to be uniformly adherent to all the tissues in contact with it, the adhesions being recent and easily overcome, but they gave a great deal of trouble from free and abundant hemorrhage. This was controlled chiefly by the application of solid perchloride of iron to the bleeding points. The tumor itself was a multilocular cyst of the right ovary, of a uniformly dark purple color, extremely friable, having large extravasations of blood in the walls, and especially at the base, close to the pedicle. The pedicle was very short, and was twisted twice completely round, from within outward and to the right. The operation was performed with complete antiseptic precautions, but the temperature and pulse curves show that she made anything but an antiseptic recovery. The pedicle was secured by the Staffordshire knot. She left the hospital on December 14, 1879.

The next case occurred immediately after that just narrated. She was thirty-six years of age, had had children, the last four years ago. She had not menstruated for seventeen weeks, but had noticed an increase of size so rapid that it could not be explained by ordinary pregnancy. I saw her for the first time on November 10th at the out-patient department, and though the diagnosis was difficult on account of the patient being very fat, I made out early pregnancy and an ovarian tumor.

She came back on November 23d complaining of intense abdominal pain, which had come on suddenly two days before, followed by incessant sickness. She looked very ill, and vomited green matter while in the consulting-room. I at once sent

her into the hospital and called an emergency consultation with my colleague, Dr. Savage. He agreed with me that it was a case of pregnancy, with a strangulated cyst, the only argument against this view being the apparent absurdity of my having two such cases in the hospital at the same time, and the likelihood that our recent experience should lead us into too ready a diagnosis. However, we stuck to our view, and agreed upon immediate operation. This I performed, and found the case to be exactly as I had diagnosed. The uterus was occupied by a pregnancy of about the fourth month, and the tumor was a par-ovarian cyst of the right side, of a pearly black lustre, the ovary lying on its front in the line of incision, at least ten times as large as an ordinary ovary, being four inches long and two broad, the enlargement being due entirely to extravasation of blood in its tissue. The Fallopian tube stretched over about a third of the circumference of the tumor, running down toward its twisted pedicle, of which it formed part. In the wall of the tumor, and especially at its base, were effusions of blood. The contents of the tumor were straw-colored, but viscid. The tumor had made three complete revolutions from within outward and to the right side. There were no adhesions, and the operation presented no difficulty, and it was carried out with complete Listerian antiseptic precautions. She made a better recovery than the second case, but not so good as the first, to which it really had a very close resemblance. No symptoms of miscarriage showed themselves. She left the hospital on December 21st, and her pregnancy was satisfactorily terminated.

One feature which was characteristic of all three of these cases, and which I have omitted to mention in connection with the second, is that the abdomen undergoes a very rapid and unusual increase in size for a few days before, or coincident with, the access of the violent pain. In two it was noticed to have occurred to a marked degree before pain was felt, and this we may easily believe to be the stage of strangulation productive of œdema and precedent to that of gangrene. This points to the conclusion that the rotation is gradual. I have had six other cases in all of which the leading features were identical with those narrated.

The symptoms recorded in all these cases are closely alike. The chief feature is the sudden accession of severe abdominal pain and tenderness, followed immediately by vomiting, which soon becomes green. The pulse rises, but the temperature does not always do so. These symptoms in the recognized presence of an abdominal tumor which may be ovarian, should lead at once to abdominal section, and they would do so in my practice,

whether the tumor were ovarian or not, if there seemed to be any probability of its being possible to remove it.

As to the mechanism by which this singular rotation is produced, we may at once dismiss any explanation which attributes it to the condition of the tumors themselves, for we find it occurring in tumors of all kinds, large, small, smooth, and globular, multicystic and irregular, parovarian, ovarian, dermoid, and solid fibrous tumors, the only intrinsic conditions of the tumors being that they should be free to move, and have pedicles capable of being twisted.

Unfortunately, in the majority, or at least in a very large number, of the cases, the direction of the twist is not clearly stated, or not given at all, nor is the side on which the tumor grew clearly given.

Of the cases narrated by Rokitansky, the great majority, about four-fifths, were tumors of the right side, and in a still larger proportion the twist was from the left to the right side—that is, taking the vertebral column as the starting-point, the twist travelled to the left side, and then forward and over to the right, that being what I read as his "und ebenso kommt die Drehung nach aussen weitans häufiger vor, als jene nach innen," though it is by no means certain that my rendering is correct.

Certainly, in all of my own cases the tumor was on the right side, and the twisting in all those operated upon was as I have just described, and in the first case I have given my recollection as that it was in this direction also. It is not recorded so in my notes, however, and my memory may be in error, though I think it is likely to be correct, as the case made a more profound impression on my mind than, perhaps, any other incident in my surgical experience.

If we had exact statements on these points for a large number of cases, I think we might arrive at some conclusion as to the cause of the rotation.

In a few of the isolated cases explanations are given which seemed more or less possible to the narrators, but they do not bear the examination of extended experience. To two of these I have already alluded, and only a third requires to be mentioned. Dr. Barnes hazards the explanation that "the tumor being free from adhesions, and tolerably firm, may *roll over on its axis*. This may happen from the enlargement of the uterus tilting it over, or from over-exertion, when, one part of the tumor being more pressed upon than the opposite part, it rolls over." The part of this explanation which applies to cases where the rotation occurs in association with a pregnant uterus applies

only to a small number of the cases, even if it were sufficient, which I do not think it is, and therefore may be dismissed. The rest of the explanation simply amounts to a repetition of the fact that this singular phenomenon does occur, and is no explanation at all.

The only reasonable effort to explain the incident has been made by Klob, who has made some experiments, from which he concludes that it is the alternate filling and evacuation of the bladder which rotates the tumor. I have not been able to find the original paper, and am, therefore, unable to criticise the basis of his opinion, but on *à priori* grounds I think there may be something in his idea. But before I knew of this explanation, and entirely from my own cases, I had come to the conclusion that it was the alternant filling and emptying of the rectum which caused the rotation, and it is possible enough that the bladder may help. That the bladder alone should do it is, I think, unlikely, for being central its influence would be, in all probability, neutral. If it were the rectum, then this force acting on the left side of the point of rest, the vertebral column, would inevitably push the tumor in the direction in which, in at least nine out of ten of my cases, the movement took place; and it would certainly act more readily on right-side tumors than on those of the left side, for the former are anchored so that the pushing force of the rectum will be in the requisite oblique direction, in the plane of a screw, and very nearly at right angles to the axis of movement.

If I might venture to apply a dynamical illustration to pathology, I would say that an ovarian tumor growing on the right side with a free pedicle, and resting, therefore, with its axis inclined toward the top of the ninth or tenth rib on the left side, would be in the condition of a body having freedom of the first order—that is, free to rotate about a fixed axis, but not to slide along it. To such a body a screw, in the form of a wedge, would be applied by the rectum in the most favorable of all directions, in a direction obliquely from above downward, across the axis of freedom and below the equator of the moving body. Every piece of fæces which passed into the rectum, especially in the recumbent position of the patient, would act as a wedge to drive the tumor round. In obedience to the dynamic law, that by a successive repetition of the process an indefinite quantity of energy may be produced, however small the initial force may be, we have at once the explanation of the phenomena of many of these cases, notably of that published by Mr. Thornton. We have, in fact, this process of rotation going on slowly until the point of strangulation has arrived, when the sudden access of pain for

the first time indicates that something has gone wrong. For the bladder a similar wedge-like influence may be claimed, but from its want of obliquity, it is not likely to be so powerful an agent in the production of rotation. Both rectum and bladder would act, however, in the same direction, and if it be found on further investigation that the tumors are mostly those of the right side, and are generally twisted in the direction in which mine were, I think we may accept the rectum as the chief factor.

That this rotation may occur suddenly, that is to say, that an ovarian tumor may be twisted rapidly round two or three times in a few minutes or hours, is inconceivable.

Rokitansky publishes (1865, loc. cit.) the post-mortem accounts of fifty-eight cases of ovarian tumor in a period of four years, and in eight of these rotation of the tumor had occurred, but in four only did it seem to have given rise to strangulation and death. Rotation is, therefore, frequent, as Rokitansky says, occurring in about twelve per cent. of all cases, and in about six per cent. of all cases producing death. My own proportion is not nearly so high, my first hundred ovariotomies including only one of my cases, and in the practice of other ovariotomists we have not as yet heard much of gangrene from rotation.

The greater part of what I have just said on this interesting subject is taken from a paper which was read before the Obstetrical Society of London last year. In the discussion which followed, my theory of the cause of axial rotation received very material confirmation from Mr. Alban Doran, who said that it accorded with some of his own convictions grounded on experiments he had made in the post-mortem room of the Samaritan Hospital when examining cases of ovarian disease that had proved fatal before any operation could be performed. Supposing that a large tumor with an irregular surface lay to the right of the rectum, an accumulation of fæces might press upon the pelvic portion of the growth in such a manner as to push the whole tumor about a quarter of a turn round its vertical axis. Should the pedicle be very long, or short, yet inelastic, it would remain twisted after this pressure was removed, and might become still more twisted after it was reapplied. Should the pedicle be short and elastic, the tumor would slip back to its normal position every time that the pressure was removed ; only this pressure might be applied so long that the temporary torsion might involve damage to the vessels of the pedicle, producing all the bad effects of permanent and complete torsion after the pedicle has become untwisted. In examining the body of a patient who died in Mr. Knowsley Thornton's ward last December he found a large ovarian tumor pressed upon to the left side, inferiorly, by the rectum, which

was slightly distended owing to a cancerous stricture. A little artificial distention of the intestine caused it to press against the tumor so as to push its left side backward, stretching and twisting the pedicle. In examining this pedicle he found that it was not twisted, but that its veins were partially plugged, in all probability from the effects of intermittent pressure through frequent extreme distention of the obstructed rectum.

Mr. Wells suggested that the rotation was little more than an accident, but an accident of such frequent occurrence must have some kind of uniform cause; and if it be true generally, as it certainly was in Rokitansky's observations and in my own, that the great majority of the tumors twisted were right-sided tumors, and that they were twisted in one specific direction, it is clear that some special mechanism must be concerned in the process of rotation. The facts given by Mr. Alban Doran are emphatically in favor of the theory I have advanced, and, doubtless, if Mr. Doran continues his observations, some valuable results will be arrived at. If a left-sided tumor could be found rotated from within outward and over to the left, in a case where the rectum was on the right side, I should regard my hypothesis as proved.

Rokitansky says that, as one of the results of this rotation and strangulation of ovarian tumors, we may get involution and wasting of the growth, so that in many cases they may diminish and disappear; and Mr. Wells, quoting Rokitansky, seems to agree with him.

There is reason to believe that sometimes this axial rotation succeeds in destroying the pedicle altogether, and separating the tumor from its connection, one might imagine a tumor possibly being quite cured in this way. Dr. Peaslee (*American Journal of Obstetrics*, 1878) mentions a case of an ovarian tumor detached from its pedicle, in which, from the history, I believe it likely that the detachment was effected by axial rotation. Dr. Peaslee says: "After the tumor had grown about two years, it ceased growing entirely for six or eight years, then it began to grow again. It was very singular that he could find no connection between the first cessation of growth and the second accession. It was thought by her friends that she had hernia, for there was great suffering, referable to the inguinal region. It kept her in bed some time. After she got up, the tumor grew no more. At the operation the omentum was found to be quite extensively adherent, and there was near it an artery of peculiar development, about the size of the brachial artery, which divided into a great many branches. Upon putting his hand into the cavity, he found that he could run it all around the tumor; he found no pedicle whatever; all the attachment it had

was that referred to above. He then proceeded to ligate the vessels and removed the tumor. He found it to present all the characteristics of an ovarian cyst; there could be no doubt as to what it was. There was a notch in the broad ligament, showing that, at the time of the attack, there had been a pedicle. It had become twisted around and around, so that the circulation had been cut off, and it ceased growing. It had been nourished, no doubt, from contact, from which circumstance this series of larger vessels had sprung up, the vessels being larger in proportion as necessity required."

Besides the acute symptoms which are induced by the strangulation of an ovarian or parovarian tumor, we may have a condition of equal severity arising from peritonitis or from suppuration of the cyst. I have on seven occasions been obliged to remove tumors at a very few hours' notice, when summoned to patients suffering from acute peritonitis. Of these seven, six made excellent recoveries, all the symptoms disappearing in a few hours after the operation. The fourth case was one which occurred in my early practice, when I used the clamp, and from this cause the patient died. From the results obtained by the immediate removal of tumors when there is peritonitis, in the hands of Dr. Keith, Mr. Spencer Wells, Mr. Pridgin Teale, of Leeds, and others, the rule has become quite established to operate without delay when there is this complication.

Suppuration of a tumor is not unfrequently met with. Usually it is the result of tapping, but I have seen it occur without any ascertainable cause. The symptoms are not generally so pronounced as they are in peritonitis, but they are always sufficiently severe to attract attention, and to give rise to a suspicion of the actual state of matters. When there is reason to believe that so grave an accident has occurred, immediate removal of the tumor is the correct practice, more particularly if the symptoms have occurred after tapping. In such cases, of course, the cleansing of the peritoneum must, if possible, be made with even greater care than in any others, and I think that probably in cases where there is pus in the peritoneum, drainage after the plan of Koeberle and Keith will conduce to the recovery of the patient.

Dr. Keith was the first to set the example of removing an ovarian tumor under these desperate circumstances. His first case was in December, 1864, and was successful. He tells us (*Edinburgh Medical Journal*, 1875) : "Since then I have ten times met with cases of acute suppurating cysts, besides two chronic cases. In all of these, save one, the chance of ovariotomy was given, however hopeless-looking the case might be."

After the performance of an ovariotomy, and after the patient has recovered from the effects of the anæsthetic, the first symptom we have to deal with is, as a rule, sickness, and sometimes it is extremely distressing. It is due, in the first instance, to the anæsthetic. During my earlier experience I was under the belief that the use of ether was more seldom followed by sickness than was the use of any other anæsthetic, but I must now say I do not think ether possesses any great advantage in this respect; but as I do not find patients suffer so much from ether sickness after other operations as they do after ovariotomy, I am inclined to believe the constriction of the pedicle has something to do with the vomiting as well as the ether. Whatever be the cause, it is certain that in a good many of my operations there is a deal of trouble with sickness, sometimes lasting as long as twenty-four or twenty-five hours. I have tried a great many means of arresting this ugly symptom, and by far the most effectual I find to be the administration of tepid water, slightly flavored with brandy. I never now use ice for this purpose, as I have not found it very efficacious. It gives rise to intolerable thirst, and this is altogether avoided by the use of tepid water. Should there be any pain after the operation, I direct the use of a suppository containing one-fourth of a grain of morphia, but with this agent I am extremely cautious, for my patients never get a single dose of morphia or opium more than is absolutely necessary to relieve pain. Like other operators, I have long since discarded the routine use of opium, which was the fashion at one time, a practice brought into existence by the idea that it prevented the occurrence of peritonitis. With my patients as little as possible is done in the after treatment, each symptom being dealt with only as it arises, and it is quite exceptional for me to have to subject them to any active interference.

The ice cap seems to be in very constant use at the Samaritan Hospital, but I have never yet employed it. We hear of some thousands of pounds of ice being used every year for it, and I have been told that a large cistern has been erected at the top of the hospital, with pipes distributed all over the building, for the purpose of supplying iced water to the patients' heads. Dr. Bantock's explanation of this is to the effect that it is necessitated by the amount of carbolic acid used in some of the operations. In my own practice I have never seen cases of exaltation of temperature, except in cases where the Listerian details were fully carried out, or where the clamp was used. It never seemed to me that the use of the ice cap was at all a rational proceeding, but whether I am or am not correct on this point is

not a matter of much importance. I have never used it and my mortality is much smaller than the mortality where the ice cap is used.

When from some reason or another the patients begin to do badly, the first indication is an altered expression of the face. I am unable to describe this change of countenance, but I learned to recognize it only too well in the old days when, from the use of the clamp, my mortality ran high. Associated with this changed expression is a rapidly increasing abdominal distention, speedily followed by vomiting. At first the vomited matter is simply the fluid the patient has swallowed, but soon it becomes tinged with bile. Later on, should the patient grow worse, the vomited matter becomes entirely bilious, and toward the end it gets quite black and characterized by these features to which the name of "coffee ground" vomiting has been aptly given. In those instances where death followed the use of the clamp, the phenomena always began on the second or third day, the patient dying on the fourth or fifth; and when once these fatal symptoms had become fairly established, nothing I ever did could arrest them. To recapitulate here the therapeutic experiments based upon all kinds of hints, derived from reading and from the advice of friends, would be perfectly needless, for in not a single instance did I ever see beneficial effects from any of them.

On opening the abdomen after death the uniform appearances were found to be those of diffuse suppurative peritonitis. It was impossible to trace in every instance the exact cause of death, yet sufficient evidence was obtained to make me attribute the fatal result in such cases to the presence of a minute aperture in the wound, at the point where the pedicle was embraced, through which the discharges from the ulcerated surface under the clamp penetrated into the abdominal cavity. With the discontinuance of the clamp such cases as these have entirely disappeared, and now not only is it quite exceptional to have a fatal case, but it is quite unusual to have any anxiety concerning my patients' recovery. Sometimes bilious vomiting occurs, why I hardly know. I cannot think this a very dangerous symptom, for I can recall but one instance of its being so persistent as to give rise to anxiety. As soon as the vomited matter shows signs of bile, I give some mild laxative, generally a seidlitz powder or a teaspoonful of Epsom salts, and a small dose of calomel, and the symptom is speedily brought to an end.

With reference to movement of the bowels after an abdominal operation, I have entirely lost all the fears derived from tradition, and I never take any steps to prevent their ordinary

motion, indeed the administration of laxatives within a few hours after the operation is becoming quite a common practice with me, this innovation, in my opinion, being possibly conducive in some measure to my increased success.

Concerning fatal cases I am altogether of Dr. Keith's opinion, that the very first search to be made for an explanation should be in the details of the operation, and every fatal case occurring to me is subjected to most rigorous inquiry. Sometimes I have found reason to suspect some omission or commission on my own part; in others I thought the surroundings were at fault; while in other instances I have been totally unable to account for the catastrophe. Two of my fatal cases, where the ligature had been used, were due to intestinal obstruction, and this arose from a kind of paralysis of the intestines which was perfectly inexplicable. Dr. Battey tells me he has seen the same thing. In both instances the patients had made satisfactory progress until the sixth day after the operation, without the slightest interruption, when suddenly the abdomen became greatly distended, incessant vomiting occurred, and the patients rapidly sank. After death nothing could be found except enormous distention of the abdomen by fluid fæces and gases. In both cases temporary relief was produced by tapping the intestines. The only explanation I can offer of the fatality in these cases is that some mysterious influence, similar, perhaps, to that which causes tetanus, brought about this unexpected and inexplicable end.

One of my recent deaths arose from a cause which will serve to show how much care should be exercised in watching the actions of those who play less prominent parts in our operations, as well as in conducting our own proceedings. A death had occurred in the hospital from a distinctly septic cause, in a case under the charge of one of my colleagues, and after death the body was allowed to remain in the ward for an unnecessarily protracted time. My patient was placed in the same ward within a very few hours afterward, and though her operation was performed with complete Listerian precautions, within a very few hours she had all the indications of acute septic poisoning and died in less than eighty hours. Unfortunately, it was not until after the accident had occurred that I became aware that the directions which up to that time had always been carefully carried out, had in this instance been omitted. The lessons to be derived from that unhappy circumstance are twofold: that no precaution can be too great in guarding against such a catastrophe, and that the Listerian details cannot be relied upon to prevent septic poisoning.

Dr. Bantock has drawn attention to a distinct danger involved in the employment of carbolic acid after Lister's method, which I had not recognized until he pointed it out, but since that time I have seen three marked cases of it, and, looking back upon my own practice, I find record of what, I have very little doubt, was a fatal case of carbolic acid poisoning. What I allude to is the alteration of the urine, which is well known to be a result of the use of carbolic acid. Some months ago I had occasion to open the abdomen of a child, placed under my care by Dr. Totherick, of Wolverhampton, on account of a pelvic abscess communicating with the bladder. The operation was performed with complete Listerian details. Within twelve hours of the operation it was observed that the child's urine possessed the characteristic color given by indican, and that the small amount of albumen which had been recognized in the urine previous to the operation had increased to such an extent that the deposit after the urine was boiled occupied nearly half the tube. Twelve hours after convulsions occurred, and the child became comatose. My colleague, Dr. Heslop, whom I asked to see the patient, regarded the symptoms as perfectly characteristic of meningitis, and he gave a prognosis of an unfavorable kind. I was, however, strongly impressed with the belief that the child was suffering from carbolic acid poisoning. This opinion was, I think, sustained by the events, for the indican disappeared, the albumen diminished, the convulsions ceased, the child regained consciousness, and in about six days after the operation the whole train of symptoms had passed away. In another child upon whom I did abdominal section I saw similar, though by no means such severe symptoms, and in a full-grown woman of twenty-four years of age I again saw them repeated. One of the first operations I performed on the Listerian method died in thirty-six hours after the operation with complete suppression of urine; and my belief now is that she died of acute carbolic acid poisoning, though, as I did not recognize such a possibility at the time, I am not prepared to say absolutely that it was so, but I have never seen suppression of the urine, or any alteration of it, such as I witnessed in these cases, save where carbolic acid had been freely used. Years ago, when we used carbolic acid of the strength of one in four as the carbolic oil, it was a very common thing indeed to see serious urinary symptoms associated with its employment, and I am perfectly certain that a great many fatal cases of carbolic acid poisoning are caused by the too free and even indiscriminate use of this poisonous agent.

I have no very fixed rules concerning the diet of my patients

recovering from an ovariotomy, save that as long as there is any tendency to sickness they have no solid food. But I do not put them through a term of three or four days systematic starvation as I formerly did. Toward the end of the first or the beginning of the second day they are allowed to have some beef-tea or milk. The first solid food they get is either some custard pudding, some dry toast, or a little boiled fish, and these are cautiously followed by a little boiled chicken, or, indeed, whatever they may express a particular fancy for, if the preference be a reasonable one. If the bowels show no indication to move of their own accord by the third or fourth day, an enema is given and repeated at intervals until it has the desired effect.

The dressing of the wound is limited to absorbent wool or a little zinc ointment if stitch-hole abscesses should form, or some of the old red lotion if the wound should gape anywhere. The stitches are removed between the sixth and eighth days, and the patients are generally up between the fourteenth and twenty-first.

Before the patients are allowed to be up they are always provided with a well-fitting abdominal belt, and this they are earnestly enjoined never to be without, while in the erect position, for at least a year after the operation. If this injunction be obeyed, there is never any tendency to protrusion of the intestines; but with hospital patients it is almost impossible to get these orders carried out, and the consequence is that it is not unusual to find them return in about three or four months with a hernial protrusion, while in private practice this is almost unknown.

One of the many arguments used against the intra-peritoneal method of dealing with the pedicle was that the stump might contract adhesions to a loop of intestine and produce strangulation, or that an abscess might form round the ligature and give rise to much trouble. So far as I know the results of my practice, in not a single instance have these anticipations been fulfilled. I direct my patients to see me or write to me every two months after their operations, and I keep a correct record of their visits and letters, and therefore I think I may say that I watch the after-results of my operations with exceptional care.

In my earlier practice I laid great stress upon the practice of keeping the pelvis quiet, as I thought, by preventing the patient passing water for herself, and emptying her bladder every five or six hours regularly by the catheter. One source of trouble which arose from this practice was the frequent occurrence of cystitis, which in several instances assumed great severity and

gave rise to a very great deal of anxiety. I found that in most cases this arose from the carelessness of the nurses, for it was almost impossible to get them to understand what a clean catheter really meant, and I had to resort to the expedient of having the instrument, when not in use, always kept in a basin of water. If this simple precaution was properly attended to, my patients always escaped inflammation of their bladders; but even with the most scrupulous injunctions, a case of cystitis would occasionally occur. I therefore use the catheter now as little as possible, and my patients are always encouraged to pass water for themselves as soon as they can. Owing, however, to some reflex influence, arising doubtless from irritation of the nerves of the pedicle, they are sometimes quite unable to pass water for several days after the operation, and then the catheter must be employed with the precautions I have already indicated; and if it is to be used, let me urge the absolute necessity of these being attended to.

Another danger in this matter is to be avoided—one which is occasionally overlooked—over-distention of the bladder. I remember being told on two consecutive days, at each of my visits, by one of my most intelligent nurses, that a patient was passing her water all right. She was an extremely nervous patient, and the complaint she made of pain at first did not attract my attention; but at last I examined the bladder, and found it completely distended, although the nurse had informed me that she had passed a quantity of urine only a few minutes before my visit. The fact was that what she had passed was the mere overflow, and probably the patient's bladder had been distended for forty-eight hours. Thus, upon the slightest complaint on the part of the patient in the region of the bladder, a careful examination should be made. Fortunately in the case I have alluded to no harm ensued, but every consultant knows how frequently we are called in to see cases where the bladder has been allowed to remain over-distended for many days after a labor, and too frequently this is followed by terrible disasters.

For the treatment of cystitis arising from the use of a foul catheter, or from over-distention, I know of nothing equal to the injection into the bladder of a tepid solution (five per cent.) of hyposulphite of soda, and the administration every eight hours of a soluble pessary containing three or four grains of extract of belladonna.

The after-course of a case of ovariotomy is subject to many rude checks, which alter its history very much from the fortunate career supposed in the preceding pages. There are many dangers in the path of every patient submitted to this operation,

and there are many indications of their approach, but none so trustworthy as those derived from a close observation of the patient's temperature-curve. It should be the invariable practice of the surgeon to have temperature and pulse observations of his patient made night and morning for a few days before the operation, and afterward these ought to be repeated every four hours for at least ten days. Nothing has been to me more instructive than a comparison of a group of such charts; and I have repeatedly seen grounds for a prognosis in a case by the comparison of its temperature-range with those of former cases. It will almost invariably be found that, immediately after the operation, the temperature falls considerably. I have seen it do so as much as two degrees, indicating the risk the patient has to run in the form of shock. To obviate this being carried to a dangerous extent, it is always well to place hot water bottles to the sides and feet, and, if depression be severely marked, to administer a diffusible stimulant. By far the best is an enema of diluted champagne, with a little brandy. It is generally necessary to administer a small dose of morphia—one-third or one-fourth of a grain—immediately after the operation, by suppository, and by this I believe shock may be warded off in great measure, and the after-sickness prevented.

From the twelfth to the twentieth hour after the operation, the temperature slowly rises, unless the patient succumb to the shock; or, in the still rarer condition, where the operation has had to be undertaken on an emergency due to cyst-inflammation or an attack of peritonitis, in which case the temperature falls. In a case of the latter, where I operated with a temperature of nearly 40° Centigrade, it fell in twenty-four hours to 37°.

After the recovery from shock, the patient generally breaks out into a gentle perspiration, and this should be slightly encouraged, and the temperature may vary from 36.8° Centigrade to 38.5° without giving rise to any alarm. If it rise, however, above the latter point, especially if accompanied by an increased pulse frequency, dry tongue, pain, and inflation of the abdomen, green vomiting or hiccup, and anxious face, the access of peritonitis, in some form or other, may be taken for granted. The treatment of this must vary very much according to the circumstances of each case.

Vomiting and distention used to be, as I have said, an invariable indication of an ultimate fatal issue. In the old days of the clamp, when we saw these symptoms come on accompanied, as they always were, by the unmistakable expression of face, we knew only too well that our efforts had been in vain. Now it is extremely rare to see either of these symptoms of an extent

sufficient to give rise to much anxiety. We do see sickness, and sometimes it is green and bilious, and we occasionally have some little distention, but they very rarely cause us any trouble. For the treatment of the sickness, I find the most effectual remedy to be a small dose of sulphate of magnesia, thirty or forty grains in tepid water, repeated every other hour or every hour until the bowels have moved, or two and a half grains of calomel given every three or four hours until a similar effect is produced.

The distention is nearly always started in the transverse colon, and shows itself first at the scrobiculis cordis, which, after an ovariotomy, is usually concave. I look at every visit at this point, and teach all my nurses also to watch it carefully, and as soon as any distention is visible, a tube is passed into the rectum at intervals of two or three hours, and left in a short time to enable the flatus to escape. If either of these symptoms advance to any alarming extent, I use still more active measures to get the bowels moved, because I always find that as soon as a motion has passed they rapidly disappear.

If, at the time of the operation, it has been thought necessary to follow the plan of drainage, as introduced by Koeberle and Keith, a little extra care must be taken with the case. I have now had considerable personal experience of this method, and I am quite satisfied from the cases in which I have used it, and from what I know of Dr. Keith's work, that there will occur every now and then a severe case in which it will be found absolutely necessary to employ it. The tubes we use are made of ordinary glass, formed somewhat like a test-tube, with an overhanging lip with which they may be secured by one of the sutures against the possibility of falling inside. They vary in size and length, having a diameter of from three-fourths of an inch down to one-fourth of an inch, their length being from three to six or seven inches. In some of my operations upon the liver, I have had to use them a good deal longer; but for an ovariotomy this length will generally be found quite enough—in fact, it will rarely be necessary to use them more than four inches long. For at least half its length the tube is perforated with small holes through which the fluid drains into it from the cavity of the pelvis. Just before closing the wound, the tube is placed with its bulbous end down into the cavity of the pelvis, care being taken, of course, that it is placed behind the uterus, and that it does not interfere with a loop of intestine. Very soon after the wound is closed, it will be found that the serum drains from it, and the amount of fluid which, in the course of a very short time, will drain in this is simply amazing, for Dr.

Keith has shown me three huge bottles containing an aggregate amount of probably ten or twelve pints drained from one patient.

Now, whether it is or is not necessary to drain this large quantity away is not yet a settled question. I believe that a tube placed in a healthy peritoneum could be made to drain away an indefinite quantity, for there is no doubt that the peritoneum, being a huge lymph-sac, is constantly passing lymph either through the intestines outward or from the outer wall in toward the intestines. The direction of this lymph-stream is not known, nor, indeed, do we know anything about its physiology; but the facts of pathology are alone sufficient to determine its existence. My own view about drainage is that it will be useful only where some addition to this lymph-stream is made greater than the outlet can carry away. The fact that I have been so successful in my own operations without drainage makes me think it probable that I have unconsciously substituted purgation for drainage; for, on looking over my records, I find that in very many of the cases where Dr. Keith would have drained I have purged. This would make it seem as if the intestines were to a large extent the outlets of the drainage stream; but it will remain for some time an open question which of the two channels, the drainage-tube or the intestinal canal, will prove the better vehicle.

Dr. Marion Sims' proposal to drain every case by means of a tube passed from the abdominal cavity into the vagina was one which did not meet with any very favorable reception, and I feel quite sure that even in Dr. Keith's hands, where drainage of a reasonable kind is resorted to, the cases will become fewer and fewer; but still there can be no doubt the drainage will largely assist in relieving the strain upon that absorbent power of the peritoneum.

Dr. Keith, after fastening in the tube, places over it a wide sheet of very fine rubber-cloth, through which the end of the tube passes, and by the aperture in which it is firmly grasped; some carbolized sponges are then wrapped up in this cloth, and in this way a very ingenious absorbent dressing is formed, which prevents the fluid, as it drains from the tube, soiling the bed-clothes. Every hour or two the nurses squeeze the sponges dry, or replace them by fresh ones, and this is continued as long as the fluid is tinged red, sometimes for nearly a week. I employ a much simpler method. I merely apply two or three absorbent pads over the tube and change them when necessary.

The convalescence of a patient from an ovariotomy may be interrupted by any of the very many causes and accidental

complications common to all surgical operations, but these are found to be very greatly diminished by the increased care we now bestow upon the hygienic arrangements of our hospitals. Thus, formerly it was not an unusual thing to see a patient die eight or ten days after an operation from an attack of pneumonia, just the same as sometimes occurs in large hospitals after amputations; but in the admirable building of the New Infirmary at Edinburgh, Dr. Keith has already performed quite a large number of operations with splendid success; and but a few days ago he told me, I think, that there he had done some twelve or thirteen consecutive cases successfully without any of the Listerian or so-called antiseptic precautions.

Once or twice, after the removal of very large tumors from elderly women, I have seen a short, rapid cough set in, rapidly increasing in severity, and killing the patient in about thirty hours. What had occurred there was, I believe, perfectly analogous to the suffocative catarrh of old age. The expiratory muscles, perhaps chiefly the diaphragm, from long want of use, had become atrophied, and, missing their *point d'appui* in the tumor, were unable to carry on the process of expectoration of mucus.

Occasionally we have the occurrence of tetanus after ovariotomy, just as we have it after every other surgical operation. Only once has it occurred in my practice, and the patient recovered, probably because I did nothing at all to her, but left the disease entirely alone. It is not a subject upon which therapeutical experiments have produced any satisfactory results, and, therefore, I think the less its victims are interfered with, the more likely they are to get well.

Another nervous lesion I have seen occur twice after ovariotomy, and in both cases it has proved fatal, and several other operators have told me that they have had a similar unfortunate experience. I refer to a paralysis, or what appears as such, of the muscular coats of the intestine. The abdomen distends rapidly with gas, and when examined after death, nothing is found but the distention. In both of my cases there was nothing like peritonitis. I tried many things to relieve these cases, including galvanism and puncture of the intestines, but without benefit.

Great stress used formerly to be laid in all abdominal operations upon the necessity of keeping the bowels from moving, and it was formerly my practice to take pretty active measures to prevent them acting for ten or twelve days. This practice I have now entirely given up; and should there not arise any need for purging my patient, I let matters alone, desiring the

nurses to give her an enema of warm water as soon as she expresses any desire to have a motion, and to repeat this every three or four hours until it is effectual.

I have so far only made casual allusions to cases of exploratory incisions and incomplete operations. It is very difficult, indeed, to lay down definite instructions upon this matter which would be of much assistance to the beginner. Of course, in a mere exploratory incision, made, let us say, for the purpose of ascertaining whether the tumor is malignant or not, no special difficulty will be experienced. In my own cases they really are but open tappings; that is to say, now-a-days, when we know there is a quantity of ascitic fluid marking the outline of the tumor, instead of tapping with a trocar, I simply make an incision with a knife, large enough to admit my finger. I empty the cavity, and, with my finger and my eye, I can generally satisfy myself upon the points where I require information. This sort of operation is nothing more than a tapping, and has no more risk, so that I hardly class it amongst the list of exploratory incisions. What I mean by the latter term is when I open the abdomen by an incision big enough, perhaps, to admit my hand in order to determine whether the particular tumor can or cannot be removed. Here there are one or two dangers with which the inexperienced operator must be acquainted, and which are not always to be avoided even by the most experienced. Thus, in a soft uterine tumor, which may very closely resemble an ovarian cystoma, it may very often happen that the bladder is pulled with it right up out of the pelvis, and the surgeon's knife may go through the bladder before he recognizes it. This has three times happened to me, but I have always carefully stitched the viscus up, and no harm has resulted from the accident. Whenever, in an exploratory incision, the bladder is found pulled up and spread over the front of the tumor for a considerable distance, the proceeding may at once be brought to a conclusion, for it may be regarded as perfectly certain that the tumor cannot be removed. Another danger is in opening the capsule of a very vascular tumor, for it is often a matter of the greatest difficulty to arrest hemorrhage from such an incision; and when the tumor looks very vascular, and is probably uterine, let me strongly urge the operator, whose experience is not very large, to be very cautious how he touches it, unless he is prepared to proceed with and complete its removal. It is at this point that the great question arises; how to direct an inquirer upon this difficult path I do not know, unless it be to advise him that, before he engages much in abdominal surgery, he had better see a good deal of it in the practice

of some one else; for the success of an operator is to be marked not only by the number of successful operations he has performed, but also by the diminution of those he has left incomplete. When and how to complete the removal of a tumor in which there are grave difficulties can be learned only by experience, and, as I have elsewhere said, in my own earlier practice I too often left an operation unfinished which riper experience would have enabled me to complete. Let me here repeat once again my advice that the surgeon should most carefully consider what he is about to do before he turns an exploratory incision into an incomplete operation; but, after having once determined the tumor to be a fit one for removal, let him proceed as speedily and as surely as he may, attending carefully to the precautions I have already laid down, and, once having put his hand to the task, let me say it will be wiser for him not to turn back, but proceed to finish the work he has undertaken.

A most remarkable case was narrated in a paper communicated to the Medico-Chirurgical Society of Edinburgh, in May, 1874, by Dr. Mathews Duncan. In this paper Dr. A. C. Campbell, of Dundee, described a case of cystic tumor of the kidney, simulating ovarian disease. The patient, aged forty-nine, a mill-worker, had a tumor for about eighteen months in the left flank, as large as a man's head, with fluctuation, and with symptoms of ovarian disease. On exposure of the tumor and insertion of the trocar, nothing came, as the contents were like porridge. The tumor was therefore laid open, and two pints of the stuff cleared out. Both ovaries were perfectly healthy. It was found that the tumor was an altered kidney; it was, therefore, removed. The patient made a tedious, but complete recovery. About forty ounces of urine were passed daily.

As a fitting conclusion to this chapter I place a list of one hundred and one consecutive operations for the removal of ovarian and parovarian tumors, performed without any of the Listerian details, and forming, as may be seen from the dates, my most recent experience. The table shows what seems to me the best method of recording cases. The letters "H" and "P" mean that the cases in the respective columns were in hospital or in private practice. "R" means recovery, and "D" means that the patient died. Of the hundred and one cases there were only three deaths.

SERIES OF ONE HUNDRED AND ONE CONSECUTIVE OPERATIONS, WITH THREE DEATHS.

No.	Residence.	Medical Attendant.	Age.	M. or S.	Disease.	Operation, Removal of	Date.	Hosp.	P.	R.	D.
							1880.				
1	Malvern	Dr. Weir	64	W.	Cystoma.	Left ovary	Nov. 1	..	P.	R.	..
2	Feckenham, Worcester	Dr. Leacroft	50	M.	"	Right ovary	Nov. 20	..	P.	R.	..
3	Hednesford	Dr. Marsh Stiles	41	M.	Parovarian cyst.	.....	Dec. 2	H.	..	R.	..
4	Coventry	Dr. McVeagh	21	S.	Cystoma.	Left ovary	Dec. 7	H.	..	R	..
5	Stratford-on-Avon	Dr. Gill	42	S.	"	Right ovary	Dec. 21	..	P.	R.	..
							1881.				
6	Hambleton, Worcester	.....	56	M.	"	Right ovary	Jan. 4	H.	..	R.	..
7	Baddesley, Warwick	Mr. S. F. Palmer	49	M.	"	Right ovary	Jan. 5	..	P.	R.	..
8	Llanbedr, Merioneth	Dr. Williams	49	M.	"	Right ovary	Feb. 2	..	P.	R.	..
9	Birmingham	Mr. H. Bracey	15	S.	"	Right ovary	Feb. 2	H.	..	R.	..
10	Brierly Hill	Dr. D'Arcy Ellis	41	M.	"	Both ovaries	Feb. 5	H.	..	R.	..
11	Dyffryn, Merioneth	Dr. C. Williams	49	S.	"	Left ovary	Feb. 7	..	P.	..	D.
12	Chesterfield	Dr. Booth	32	S.	"	Left ovary	Feb. 14	..	P.	R.	..
13	Birmingham	Mr. Raffles Harmar	48	M.	"	Left ovary	Feb. 17	H.	..	R.	..
14	Leominster	Dr. Barnett	23	S.	"	Left ovary	Feb. 19	H.	..	R.	..
15	Nuneaton	Mr. R. B. Nason	56	M.	"	Left ovary	Feb. 27	..	P.	R.	..
16	Wootton-under-Edge	Dr. Forty	25	S.	"	Both ovaries	March 2	..	P.	R.	..
17	Leicester	Dr. Cox-Hippisley	31	M.	"	Both ovaries	March 3	H.	..	R.	..
18	Harbury, Warwick	Dr. Lattey	63	M.	"	Both ovaries	March 9	H.	..	R.	..
19	Lancaster	Dr. Cassidy	56	M.	"	Right ovary	March 9	..	P.	R.	..
20	Solihull	Dr. Page	22	S.	"	Both ovaries	March 12	..	P.	R.	..
21	Wolverhampton	Dr. Walton Hamp	21	S.	"	Left ovary	March 16	H.	..	..	D.
22	Ashby-de-la-Zouch	Dr. Betts	43	W.	"	Right ovary	March 26	..	P.	R.	..
23	Cannock	Dr. Moses Taylor	38	M.	"	Right ovary	April 2	H.	..	R.	..
24	Birmingham	Dr. Bailey	30	M.	"	Left ovary	April 9	H.	..	R.	..
25	Cradley	Dr. Standish	29	M.	"	Left ovary	April 29	..	P.	R.	..
26	Nottingham	Dr. Huthwaite	47	M.	"	Right ovary	May 7	..	P.	R.	..
27	Lichfield	Dr. Bastable	36	M.	"	Right ovary	May 7	..	P.	R.	..
28	Birmingham	Dr. Cox	57	M.	"	Right ovary	May 19	H.	..	R.	..
29	Darlaston	Dr. Cameron	40	M.	"	Both ovaries	May 21	..	P.	R.	..
30	Malvern	Dr. Weir	48	M.	"	Right ovary	June 15	..	P.	R.	..
31	Aston	Mr. Lawson Tait	31	M.	"	Left ovary	July 4	H.	..	R.	..
32	Wellington, Somerset	Dr. Edwards	22	S.	"	Both ovaries	July 5	..	P.	R.	..

SERIES OF ONE HUNDRED AND ONE CONSECUTIVE OPERATIONS, WITH THREE DEATHS—(*Continued*).

No.	Residence.	Medical Attendant.	Age.	M. or S.	Disease.	Operation. Removal of	Date.	Hosp.	P.	R.	D.
							1881.				
33	Walsall	Dr. G. Sharp	39	M.	Cystoma.	Left ovary	July 6	H.	..	R.	..
34	Ashby-de-la-Zouch	Dr. Betts	34	M.	"	Both ovaries	July 7	..	P.	R.	..
35	Derby	Dr. G. Copestake	35	W.	"	Both ovaries	July 13	..	P.	R.	..
36	Alfreton, Derby	Dr. J. J. Bingham	48	M.	"	Right ovary	July 28	..	P.	R.	..
37	Birmingham	Dr. D. Nelson	38	M.	"	Right ovary	Aug. 2	..	P.	R.	..
38	Chirk	Dr. Aylmer Lewis	17	S.	"	Left ovary	Aug. 2	..	P.	R.	..
39	Birmingham	Mr. Lawson Tait	40	M.	Parovarian cyst.		Aug. 8	H.	..	R.	..
40	Derby	Dr. Rice	18	S.	Cystoma.	Right ovary	Aug. 22	..	P.	R.	..
41	Sutton-in-Ashfield	Dr. J. J. Bingham	25	M.	"	Both ovaries	Aug. 24	H.	..	R.	..
42	Worcester	Dr. Coombes	52	M.	"	Left ovary	Sept. 3	H.	..	R.	..
43	Adderbury, Oxon	Dr. Colgrave	46	M.	"	Right ovary	Sept. 5	H.	..	R.	..
44	Horne Suckley, Worcester	Dr. Woodward	51	M.	Parovarian cyst.		Sept. 12	H.	..	R.	..
45	Bilston	Mr. Lawson Tait	35	M.	Cystoma.	Left ovary	Sept. 13	H.	..	R.	..
46	Birmingham	Dr. Kenny	30	M.	"	Left ovary	Sept. 20	H.	..	R.	..
47	Llandulas, North Wales	Dr. Turner	48	M.	Parovarian cyst.		Sept. 24	H.	..	R.	..
48	Birmingham	Mr. J. R. Harmar	63	W.	Cystoma.	Right ovary	Oct. 15	..	P.	R.	..
49	Wolverhampton	Mr. S. F. Palmer	57	S.	"	Both ovaries	Oct. 21	..	P.	R.	..
50	Ombersley	Dr. Roden	63	M.	"	Right ovary	Oct 28	H.	..	R.	..
51	Wolverhampton	Dr. Scott	34	M.	"	Right ovary	Oct. 29	..	P.	R.	..
52	Rugby	Dr. McKenzie	44	S.	"	Both ovaries	Nov. 1	..	P.	R.	..
53	Swansea	Dr. Rawlings	40	M.	"	Right ovary	Nov. 1	H.	..	R.	..
54	Machynlleth	Dr. Pratt	63	M.	"	Right ovary	Nov. 11	..	P.	R.	..
55	Birmingham	Dr. Nelson	39	M.	"	Both ovaries	Nov. 19	H.	..	R.	..
56	Birmingham	Mr. Hall-Wright	22	M.	"	Left ovary	Nov. 22	H.	..	R.	..
57	Derby	Mr. Curgenven	50	W.	"	Left ovary	Nov. 30	..	P.	R.	..
58	Denbigh	Dr. Turnour	53	M.	"	Right ovary	Dec. 7	H.	..	..	D.
59	Warwick	Dr. Tibbits	55	W.	"	Left ovary	Dec. 16	H.	..	R.	..
60	Kingswinford	Dr. Thomson	57	M.	"	Right ovary	Dec. 19	H.	..	R.	..
61	Llandulas	Dr. Wolstenholm	25	M.	"	Left ovary	Dec. 24	..	P.	R.	..
							1882.				
62	Dudley	Mr. Lawson Tait	46	M.	Parovarian cyst.		Jan. 11	..	P.	R.	..
63	Birmingham	Mr. J. R. Harmar	32	M.	Cystoma	Left ovary	Jan. 17	H.	..	R.	..
64	Birmingham	Mr. Leach	25	M.	Parovarian cyst.		Feb. 17	H.	..	R.	..

65	Birmingham	Dr. Drury	37	M.	Parovarian cyst.	.....	March 3	..	P.	R.	..
66	Derby	Dr. Carter Wigg	64	W.	Cystoma.	Left ovary	March 8	..	P.	R.	..
67	Bromsgrove	Dr. Wood	42	M.	"	Both ovaries	March 10	..	P.	R.	..
68	Birmingham	Mr. Hollingshead	58	M.	"	Right ovary	March 15	..	P.	R.	..
69	Tipton	Dr. Hickin	39	M.	"	Both ovaries	March 17	H.	..	R.	..
70	Whitchurch	Mr. Groom	64	W.	"	Both ovaries	March 19	..	P.	R.	..
71	Bootle	Dr. Young	27	M.	"	Both ovaries	March 23	..	P.	R.	..
72	Nuneaton	Mr. Nason	29	M.	"	Both ovaries	March 23	..	P.	R.	..
73	Derby	Dr. Carter Wigg	52	M.	"	Right ovary	March 24	H.	..	R.	..
74	Birmingham	Mr. Lawson Tait	29	S.	"	Both ovaries	March 25	..	P.	R.	..
75	Birmingham	Dr. Madden	33	M.	Fibroma.	Right ovary	April 5	H.	..	R.	..
76	Birmingham	Mr. Waterson	17	S.	Cystoma.	Right ovary	April 6	..	P.	R.	..
77	Birmingham	Mr. Hollingshead	42	M.	"	Left ovary	April 7	..	P.	R.	..
78	Birmingham	Dr. Edginton	38	M.	"	Both ovaries	April 13	H.	..	R.	..
79	Cheltenham	Dr. Simmons	50	W.	"	Both ovaries	May 3	..	P.	R.	..
80	Walsall	Dr. Oliver	28	M.	Parovarian cyst.	.....	May 5	..	P.	R.	..
81	Leicester	Dr. Marriott	26	S.	Cystoma.	(?) Removed	May 6	H.	..	R.	..
82	Birmingham	Dr. J. W. Taylor	67	M.	"	Left ovary	May 9	..	P.	R.	..
83	Birmingham	Mr. Leach	45	M.	"	Left ovary	May 12	..	P.	R.	..
84	Bilston	Dr. Smith	40	M.	"	Left ovary	May 12	..	P.	R.	..
85	Leicester	Dr. Cox-Hippisley	48	S.	"	Both ovaries	May 15	..	P.	R.	..
86	Lichfield	Mr. J. Clay	28	S	"	Both ovaries	May 16	H.	..	R.	..
87	Longton	Dr. Dawes	56	W.	"	Both ovaries	May 17	..	P.	R.	..
88	Wednesbury	Dr. Blackwood	49	M.	"	Left ovary	May 18	H.	..	R.	..
89	Birmingham	Dr. Hickenbotham	48	M.	"	Both ovaries	May 23	H.	..	R.	..
90	Sunderland	Dr. Dixon	28	S.	"	Both ovaries	May 27	..	P.	R.	..
91	Dudley Port	Dr. Price	34	M.	Dermoid cyst.	Left ovary	May 30	H.	..	R.	..
92	Stonehouse	Dr. Watters	27	M.	Parovarian cyst.	.....	June 7	H.	..	R.	..
93	Birmingham	Mr. Lawson Tait	27	S.	"	.....	June 24	..	P.	R.	..
94	Birmingham	Dr. Bull	54	M.	Cystoma.	Both ovaries	June 29	H.	..	R.	..
95	Birmingham	Dr. Hickenbotham	18	S.	"	Left ovary	June 29	H.	..	R.	..
96	Bickenhill	Dr. Quirke	50	S.	Parovarian cyst.	.....	July 10	H.	..	R.	..
97	Birmingham	Dr. Drury	22	M.	Cystoma.	Left ovary	July 21	H.	..	R.	..
*98	Coleshill	Dr. Jones	38	M.	"	Left ovary	July 28	H.	..	R.	..
99	Willenhall	Mr. Harthill	33	M.	"	Left ovary	July 29	..	P.	R.	..
100	Llanrwst	Dr. Jones	54	W.	Fibroma.	Left ovary	Aug. 5	..	P.	R.	..
101	Birmingham	Mr. Lawson Tait	29	S.	Cystoma.	Left ovary	Aug. 5	..	P.	R.	..
								47	54	98	3

H means hospital case. P, private case. R, recovery. D, Death.

* By an error of copyist case 98 was omitted in list published in Brit. Med. Journal, October 28, 1882.

CHAPTER VI.

RECENT EXTENSIONS OF ABDOMINAL AND PELVIC SURGERY.

In a former chapter, when speaking of the pathology of the ovary, I had occasion to point out that our recent advances in pelvic and abdominal surgery have enabled us to obtain a great deal of information concerning the diseases of the ovaries which formerly were beyond our reach. It has been very well said indeed that many of our recent operations, to which I propose here to devote some space, are quite as good as post-mortem examinations for the purposes of pathology. I would say that they are a good deal better, because we have by us, as part of the justification for the operation, a complete clinical history with which to compare the diseased appearances, and this we never have at post-mortems, as far as the ovaries and tubes are concerned.

In the chapter I have just alluded to, I give details of some of the cases in which I have performed operations which, until three or four years ago, I regarded as quite unjustifiable, but which I now regard as the legitimate outcome of our increased success in the removal of ovarian tumors. If Mr. Baker Brown had continued to practise ovariotomy for some years after 1867, he would speedily have brought his mortality down from ten per cent., at which he left it, to four or five per cent., at which Dr. Keith's mortality and my own now stand, and we should have been fifteen years in advance of our present position. From 1867, as I have already said, Mr. Spencer Wells exercised an impregnable influence on the conduct of ovariotomy, owing to the accident of circumstances, and with his mortality of twenty-five per cent. any real advance of abdominal surgery was wholly impossible. No one could venture to submit a woman to such a fearful risk unless her life was clearly menaced, and therefore ovariotomy was always delayed as long as possible; palliative tappings and other blunders were perpetrated; but worse than all, the diseases in the pelvis and abdomen which

were amenable to surgical treatment, but which did not immediately and evidently destroy life, were left alone, and the patients went unrelieved. Dr. Thomas Keith ended this dark period by showing us how to operate on the abdomen without fear and with little risk, and these recent advances in pelvic and abdominal surgery should, in great measure, be placed to his credit, though I do not know that he has engaged in them in his own personal practice to any large extent.

For my own part, so fearless am I now of abdominal surgery, so splendid have been my results in fields of practice which, until three years ago, seemed hopelessly enclosed, that I venture to lay down a surgical law, *that in every case of disease in the abdomen or pelvis, in which the health is destroyed or life threatened, and in which the condition is not evidently due to malignant disease, an exploration of the cavity should be made.* I have already published a great deal of work in support of this proposition, and some of this I consider of sufficient importance to reproduce at the close of this chapter.

In October, 1871, Mr. Hallwright, of Summer Hill, Birmingham, asked me to see with him a woman, aged forty-two, who had suffered for years from agonizing pain in the pelvis, chiefly on the left side, distinctly referable to the seat of the ovary, accompanied by a variety of reflex symptoms, of which the most marked was complete and persistent aphonia. She had for many years been under treatment at the hand of Mr. Hallwright and others, without the slightest relief. We found the left ovary large and very tender, displaced down behind the uterus, and the slightest pressure upon it gave rise to the characteristic sickening pain. From this pain she suffered on defecation. After having seen the patient many times, and after careful thought, I ventured to suggest to my colleague that removal of the ovary would probably cure her. I recognized the gravity of the proposal, for I had no fear that her sufferings would kill her, and she had only some six or eight years to live before the climacteric period would bring relief; but both Mr. Hallwright and I thought it was better that the suffering should be saved her for that period, even at some risk of life, than that it should be continued, making life a burden to her and to all her surroundings. The patient and her friends arrived at quite the same conclusions when the matter was explained to them, and the operation was decided upon. With the assistance of Mr. Hallwright and Mr. Bennett May, I performed it on February 11, 1872, and found the ovary non-adherent, as large as a pigeon's egg and full of thick, grumous matter, which, at the time, I took to be the fat of a dermoid cyst. More careful exam-

ination has since made me believe that it was a chronic abscess, for there is a complete absence of any skin structure in the walls, and there were no appearances of any of the hair, teeth, etc., which characterize dermoid tumors. So far as I know, this is the first record in the history of surgery of the removal of a small ovary on account of pain. The patient made a speedy and complete recovery, and has ever since remained completely free from pain in the pelvis. Her voice returned, and is now of normal power. She suffers now from some obscure disease which has stiffened her knee-joints and makes her a cripple, but all the symptoms which were in existence before the ovariotomy were completely and permanently cured by it.

The success in this case suggested to me the possibility of relieving other conditions of a kind involving risk to life by removal of the ovaries, more particularly menstrual hemorrhage due to uterine myoma. Every one knows what an intractable symptom this is, how rarely it yields to the most energetic treatment of a therapeutical kind, and how many surgical expedients have been devised for the purpose of dealing with it—enucleation of the tumor, hysterectomy, etc.—all of which have had to be abandoned on account of a mortality so high that the operations became wholly unjustifiable. Dr. Mathews Duncan and Professor Gusserow, estimate that enucleation has a mortality of fifty per cent., and hysterectomy one of seventy per cent. In my own experience the mortality of the former has been much higher, so that I have given the practice up, and have no hesitation in denouncing it as unwarrantable on account of its risk. Concerning enucleation, there is the additional objection that the tumors grow again, at least in the three cases in which I have enucleated successfully, this has been the uniform result. Dividing the cervix has also proved a wholly useless operation, for even where it has given relief it has done so only for a very short time. Hypodermic injections of ergotin and injections of astringents and styptics into the uterus have also proved useless and very dangerous, especially the latter; for I have had three deaths from it in some ten or eleven cases. Yet in these cases something must be done, for the hemorrhage proves fatal in a large number of them, and even when it does not do so, it utterly destroys the health and usefulness of the lives of the sufferers.

In a recent discussion on this subject at the International Medical Congress in London, Dr. Mathews Duncan expressed the astonishing opinion that such cases did very well if left alone, and did not demand any risky interference, yet he records with-

out condemnation the high mortality I have quoted in the cases of enucleation and hysterotomy.

I remember very well the first case of death which I witnessed from menstrual hemorrhage due to a myoma occurred in Dr. Mathews Duncan's own practice, while I was a pupil at the Edinburgh Infirmary in 1862. The case was impressed on my memory because I made a post-mortem examination of the patient and carried the tumor to Dr. Duncan. Since that time I have seen many deaths from this cause. But even if only a few of the cases died, their sufferings are severe and protracted, and they are permanent invalids. As an illustration of this, I cannot do better than quote a letter from Dr. Law Webb concerning a patient upon whom I have recently performed a successful operation of the kind I am now discussing, on account of a tumor which grew after its predecessor had been removed by enucleation: "When I first came to this neighborhood (Ironbridge, Salop), Miss F. was one of the 'confirmed invalids' of the place, and had been in failing health for some years before that date (1870). I must have attended her at short intervals for more than nine years. She had profuse hemorrhage at every monthly period, and sometimes every fortnight for months together. Treatment did so little good that she usually only sent for me when the loss was unusually great, or her anæmic condition alarmed her friends. Her life certainly has been a misery to her for the last ten years, as she has been ill and laid by more than half her time." This graphic description might be applied to scores of cases, and if our art is to be withheld from them, when we have an absolute cure in our hands for them, of what use can we claim to be? The answer might very fairly be made to Dr. Duncan that there must be a pecuniary inducement in keeping our patients in chronic ill health, when they might be promptly cured. That there is a risk of life attached to such an operation is no argument against it; at least it would be an argument equally strong against every kind of medical and surgical treatment, and would be equally logical against railway travelling. The only effect of the statement should be to reduce that mortality as close to a vanishing point as may be, and I am satisfied that can be largely accomplished.

I have said that the success with Mr. Hallwright's patient induced me to extend the principle, and on the first of August, 1872, I removed both ovaries for the purpose of arresting menstrual hemorrhage, of a perfectly intractable character, in a woman, aged forty, who had been for some months under my care.

The result was a complete success, and I heard of the patient being alive and well in 1874. The same idea, concerning the removal of small ovaries, had struck the minds of two other surgeons about the same time as it occurred to me, for up to July 27, 1872, five days before my second case, Professor Hegar, of Freiburg, removed both ovaries for neuralgia, with a fatal result; and on the 17th of August of the same year, Dr. Battey, of Rome, Ga., successfully operated upon a patient suffering from serious and complicated symptoms. Dr. Battey was the first to publish his cases and a defence of his proceedings (*Atlanta Medical Journal*, September, 1872), whilst I contented myself with discussing the principle only in my Hastings' Essay on "Diseases of the Ovary" (1873).

For the removal of small ovaries, Dr. Battey first introduced the phrase "normal ovariotomy," a great mistake, for it was at once assumed that we proposed to remove healthy ovaries, on slight or insufficient provocation, whereas, with very few exceptions, the organs are all diseased. This unfortunate phrase has been a great stumbling-block, and has excited an amount of opposition, both professional and public, from which abdominal surgery, and those who practise it, have suffered not a little. The terms "spaying" and "castration of women" (Hegar) were equally objectionable for the same reason, and further from the fact that, as far as my practice is concerned, they do not express the facts of the operation. Dr. Marion Sims has attempted to give it the name of "Battey's operation," but this will not do for very many reasons. Dr. Sims compares it with, and seeks the authority of precedent from "Amussat's operation" or "Syme's amputation." But there is no parallel at all in these cases, for in both the proceedings are definite, and practically do not vary; whilst in the operation I am now discussing, the details vary indefinitely, and so do almost equally the principles upon which the operations are performed. For an operation performed merely to "bring about the menopause," to quote Dr. Battey's definition of his own principle, there must be but a limited field, and my own experience of it would be vague and indefinite, and my conclusions would be doubtful. Again, neither Prof. Hegar nor Dr. Battey seems to have recognized the importance of the tubes in these cases, nor to have contemplated their removal for occlusion and distention. This is an extension of pelvic surgery entirely my own.

Similarly, I have a strong objection to the pedantic invention "oöphorectomy." This properly describes the removal of an ovarian cystoma as completely as it does a "Battey's opera-

tion." In my own practice the conclusion is indicated that removal of the Fallopian tubes is more important than removal of the ovaries, and in by far the larger number of my cases that alone might have sufficed; indeed, in many it has done so. Therefore, if Greek inventions must be introduced, I should ask for "salpingotomy" or "salpingo-oöphorectomy," or "prosthekotomy," if the pedantry were not ridiculous. But I do not propose to attempt any reforms or additions to our clumsy nomenclature. When I remove an ovary I call the operation "ovariotomy," and by describing the disease for which the operation was done, I leave each critic to class my cases as seems best to him. When I remove diseased tubes I generally, but by no means always, remove the ovaries with them, and in these cases I speak of the operation as "removal of the uterine appendages."

For the purpose of classification I arrange the operations which I have performed upon the uterine appendages on account of (*a*) pain, (*b*) intractable hemorrhage, and (*c*) reflex symptoms. Up to the date of writing they have all been published in detail, save some of the most recent, and it would be quite impossible, within reasonable limits, to republish these details here. A list of cases was submitted to the Obstetrical Section of the International Congress, with columns containing the residence and names of the medical attendants of the patient, so that each case might be identified if necessary. The first criticism which my work met with was that the statements of it were not true—a criticism which no one had the hardihood to make in public, but which was diligently circulated by some of whom I expected better things.

The next criticism was that I was "spaying women—removing healthy ovaries from healthy women"—a statement which has been reiterated by many medical journals, including so well-informed an organ as the *Lancet*. Even text-books in gynæcology, such as that of Hart and Barbour, speak of my "excising the ovaries for hydrosalpinx and pyosalpinx," so that I have become almost tired of the discussion.

I hope it may be for the last time that I give an emphatic denial to all this sort of thoughtless misrepresentation.

The principles of this extension of abdominal surgery are few and clear, and for their establishment I think I can give satisfactory arguments.

The first class of cases in which we may interfere is the most doubtful, and certainly the most restricted, and it is that to which the term "Battey's operation," if it must be used at all, should be confined. It is those in which there is no physical

evidence of pelvic disease, yet where there are serious symptoms so intimately associated with menstruation as to lead us to believe that an arrest of that function might cure or relieve the patient by the establishment of a "premature menopause."

It must be perfectly evident that this is such an extremely vague field that it may be either very limited or very much extended. I have been so very doubtful about it that I have limited it entirely to one well-pronounced disease—epilepsy.

There is no difficulty in defining true epilepsy, and we find that almost every epileptic woman is worse during the menstrual week. In some patients the fits are confined absolutely to the menstrual period, and then we speak of "menstrual epilepsy." I have had very many such cases sent to me for the purpose of having the operation performed, but I have limited the proceeding to five cases, all of them being patients where the disease had resisted all other treatment, where the intellect of the patients had become affected, and their usefulness to society completely impaired, their lives even being threatened. One such case is narrated in full in another chapter (p. 107 *et seq.*), and others have been published. All five patients recovered and are still alive.

The second case I operated upon was a girl, aged eighteen, who had been imbecile from birth, and who had developed the most violent menstrual epilepsy from the time of the molimen. For some months before the operation she had been so bad during the period as to require the constant care of two attendants, and Mr. Green, the superintendent of the asylum of which she was an inmate, was quite satisfied that the disease would shortly prove fatal. I operated on May 9, 1880, with the result of completely arresting menstruation and abolishing the epilepsy. She is still an inmate of the Birmingham Borough Asylum, and is quite manageable. She gives slight indications of an increased noisiness and loquacity at the time at which her periods should occur, and occasionally at these times she has an attack of *petit mal ;* but her violent epilepsy has quite disappeared, and remains in abeyance now, more than two years and a half after the operation.

In the third case the girl remained free from the fits for about six months, but they are now returning occasionally, so that I fear the case will prove a failure, though the operation has completely arrested menstruation.

The fourth and fifth cases are of too recent date to express any opinion about. In both menstruation is completely arrested, but the fits have not disappeared. Of the fifth, Dr. Knipe, of Melbourne, writes to me that she is very much improved so far.

It will be seen from this, and from the fact that I have suspended for the present further trial of the operation in such cases, that I am not greatly in favor of "Battey's operation." Its results, as far as the recovery of the patients are concerned, are satisfactory enough. The removal of the appendages in such cases is the easiest operation possible, and ought never to be fatal. Its secondary results are uncertain, and I am not disposed, in the present aspect of professional opinion, to hamper my work in other directions by its discussion further at the present time. To operate in this class of neurasthenic cases, where the symptoms are all subjective and the physical signs negative, is a question for further discussion, and for its settlement I have absolutely no material.

For the arrest of intractable hemorrhage the removal of the uterine appendages offers a most satisfactory field, as I have already shown (pp. 115 and 153). The primary risks of the operation are small, as is shown in the table on next page, which embodies the whole of my recent experience of such cases, dating from the time when the advance of abdominal surgery made such operations possible. For small tumors, where it is an alternative to enucleation, it is far safer, and it offers the security against a return of the disease which enucleation does not. In the case of large tumors it is an alternative, in the great majority of instances to hysterectomy, than which it is also much less fatal. In the great majority of cases it arrests the hemorrhage and the growth of the tumor at once, and in many the tumors shrivel and absolutely disappear. In such as do not disappear it might form a preliminary step to hysterectomy, though I never made such a use of it.

Hemorrhage was, of course, the leading feature in nearly all of these cases, but in many of them the pain and discomfort alone would have justified the operation. In the two cases which were fatal the anæmia was extreme, and the operation had not a fair chance. In case No. 33 the patient died of cancer about five months after the operation. At the time of the operation there was no appearance of the tumor being malignant, but by the kindness of Dr. Totherick I saw it after death, and there was no doubt of it being cancer. This is the kind of after-history in ovarian tumors of which I have already spoken at length, and doubtless it will be met with in myoma when we know more about that disease. One other case (23) has died since the operation, but I do not know anything as to the cause of death. So far as I have been able to learn, and that includes all the rest but two, the other cases are alive and well, and the secondary results of the operation are quite satisfactory.

FORTY-FIVE CASES OF REMOVAL OF THE UTERINE APPENDAGES FOR MYOMA.

No.	Residence.	Medical Attendant.	Age.	M. or S.	Date.	Hosp.	P.	R.	D.
					1879.				
1	Leamington.....	Dr. Tomkins.....	47	W.	Oct. 18..	..	P.	R.	..
2	Cannock.....	Dr. Tylecote.....	52	W.	Nov. 30.	..	P.	R.	..
					1880.				
3	Walsall.....	Mr. John Clay.....	34	M.	Jan. 13.	H.	..	R.	..
4	Southwell.....	Mr. Calvert.....	52	S.	Mar. 10.	..	P.	R.	..
5	Leicester.....	Dr. Clifton.....	42	S.	April 7..	..	P.	R.	..
6	Chasetown.....	Dr. Clarke.....	39	M.	April 22.	H.	..	R.	..
7	Solihull.....	Dr. Insull.....	46	S.	May 8...	H.	..	R.	..
8	Birmingham.....	Dr. Drummond.....	49	M.	Aug. 17.	H.	..	R.	..
9	Coventry.....	Dr. Fenton.....	47	M.	Sept. 1.	H.	..	R.	..
10	Stourbridge.....	Dr. Hammond Smith.	50	S.	Sept. 2..	..	P.	R.	..
11	Bloxwich.....	Dr. Somerville.....	35	M.	Oct. 26..	..	P.	R.	..
12	Bradninch.....	Dr. Stephenson.....	42	W.	Oct. 16..	H.	..	..	D.
13	Birmingham.....	Dr. J. W. Taylor....	44	S.	Dec. 18.	H.	..	R.	..
					1881.				
14	Coventry.....	Dr. Plowman.....	32	M.	Jan. 13.	H.	..	R.	..
15	Brierly Hill.....	Dr. D'Arcy Ellis.....	41	M.	Feb. 5..	H.	..	R.	..
16	Birmingham.....	Dr. Kenny.....	43	M.	Feb. 12.	H.	..	R.	..
17	Darlaston.....	Dr. Sutton.....	35	S.	April 20.	..	P.	R.	..
18	Droitwich.....	Dr. Cuthbertson.....	43	M.	June 15.	H.	..	R.	..
19	Birmingham.....	Mr. Hallwright.....	47	M.	June 17.	H.	..	R.	..
20	Iron Bridge, Salop.	Dr. Law Webb.....	38	S.	Aug. 25.	..	P.	R.	..
21	Birmingham.....	Dr. Kenny.....	43	S.	Aug. 27.	..	P.	R.	..
22	Wolverhampton...	Dr. Pope.....	40	M.	Sept. 19.	..	P.	R.	..
23	Broseley.....	Dr. Bartlam.....	51	S.	Oct. 4...	H.	..	R.	..
24	Ludlow.....	Dr. Brooks.....	37	S.	Oct. 30..	..	P.	R.	..
25	Bloxwich.....	Dr. Somerville.....	40	M.	Dec. 27.	..	P.	..	D.
					1882.				
26	Birmingham.....	Mr. C. J. Bracey....	36	M.	Jan. 4..	..	P.	R.	..
27	Wolverhampton...	Dr. Lycett.....	40	M.	Jan. 4..	..	P.	R.	..
28	Gloucester.....	Dr. Eshelby.....	37	S.	Jan. 10.	..	P.	R.	..
29	Conway.....	Dr. Pritchard.....	46	M.	Jan. 29.	..	P.	R.	..
30	Llandudno.....	Dr. Nicol.....	45	M.	Mar. 13.	..	P.	R.	..
31	Birmingham.....	Dr. Gaunt.....	49	S.	Mar. 21.	H.	..	R.	..
32	Birmingham.....	Mr. Fairley.....	45	M.	Mar. 29.	H.	..	R.	..
33	Wolverhampton...	Dr. Lycett.....	40	S.	Mar. 31.	..	P.	R.	..
34	Birmingham.....	Dr. J. W. Taylor....	44	M.	April 8..	..	P.	R.	..
35	London.....	Dr. L. Atkins.....	33	M.	April 11.	..	P.	R.	..
36	Dudley.....	L. T.....	21	S.	April 20.	H.	..	R.	..
37	Oxford.....	Mr. G. Jones.....	46	S.	April 27.	..	P.	R.	..
38	Alfreton.....	Dr. Fielding.....	45	M.	May 6..	H.	..	R.	..
39	Southampton.....	Dr. Seaton.....	44	M.	June 9.	H.	..	R.	..
40	Leicester.....	Dr. Clifton.....	35	M.	June 12.	..	P.	R.	..
41	Droitwich.....	Mr. Spofforth.....	35	M.	June 16.	H.	..	R.	..
42	Chesterfield.....	Dr. Hale.....	44	M.	June 27.	..	P.	R.	..
43	Birmingham.....	Mr. Bracey.....	45	M.	July 13.	H.	..	R.	..
44	Birmingham.....	Dr. Thomas.....	32	M.	Sept. 9..	..	P.	R.	..
45	Ludlow.....	Dr. Brookes.....	35	S.	Sept. 29	..	P.	R.	..

H means hospital case. P, private case. R, recovery. D, death.

I have already said enough about hydrosalpinx and pyosalpinx to render it quite unnecessary to discuss them further than to insert here a complete list of all the cases upon which I have operated for these diseases. Both conditions are far more common than was believed previous to my experience, yet they have been quite well known, and described for at least half a century.

Of the forty-four cases only four have occurred in single women, and the leading feature in the history of many of the cases was an attack of gonorrhœa. In one case I had to operate in the acute stage of the disease, which had arisen, on the admission of the husband, from this cause. In many other cases the origin of the condition could clearly be traced to an attack of inflammation after a miscarriage or after a labor.

A very frequent feature in the history of the cases was found to be that they had one child, and after that were never free from pain till relieved by the operation.

The leading symptom is persistent pain, intensified at the periods, especially just before their onset, and always made worse by intercourse. In the great majority of the cases married life had to be completely suspended, and the function was always restored by the operation.

Metrostaxis, sometimes so severe as to amount to hemorrhage, is a very frequent symptom, though in some of the cases menstruation is scanty.

Of course if the disease is bilateral the patients are sterile, and this is usually the case, though in some only one tube has been found to be affected, and then that only has been removed.

The operations are generally very difficult, for it is quite exceptional not to find the tubes and ovaries densely adherent to the viscera and to the pelvic wall, and in some of my operations the difficulty in overcoming these adhesions has transcended anything I have ever seen in the removal of cystic tumors of the ovary. In some cases the hemorrhage during the operation has been alarming, but it has always been controlled by sponge-packing. In three of the cases the diseased organs have been removed only at a second attempt; that is, in my early practice I had not the courage and necessary dexterity to complete the operation, the patients returned with increased sufferings and submitted to a second attempt in which I was successful. In one case I made three attempts to remove the tubes, the third being successful.

All the patients recovered, and, with two exceptions, are still alive and well. One of these died of English cholera, and the

other from acute melancholia, having been allowed to starve herself to death, the folly of her husband standing in the way of her removal to an asylum, where she could have been forcibly fed.

TWENTY-FOUR CASES OF REMOVAL OF UTERINE APPENDAGES FOR HYDROSALPINX.

No.	Residence.	Medical Attendant.	Age.	M. or S.	Date.	Hosp.	P.	R.
					1879.			
1	Birmingham	Mr. Watkin Williams.	28	M.	May 23	..	P.	R.
					1880.			
2	Birmingham	L. T.	37	M.	April 3.....	H.	..	R.
3	Birmingham	L. T.	28	M.	Sept. 24	H.	..	R.
4	Birmingham	Dr. Hoare	32	M.	Oct. 5.....	H.	..	R.
					1881.			
5	Redditch	Dr. Dodsworth	39	M.	July 14.....	H.	..	R.
6	Stourbridge	Dr. Hammond Smith.	27	M.	Aug. 19.....	H.	..	R.
7	Walsall	Dr. Sharp	37	M.	Oct. 7	H.	..	R.
8	Birmingham	Mr. Hallwright.....	34	M.	Oct. 19	H.	..	R.
9	London	Dr. T. Chambers	29	M.	Oct. 24	H.	..	R.
10	Walsall	L. T.	38	M.	Nov. 9	H.	..	R.
11	Warwick.....	Dr. Watson.....	33	M.	Nov. 14.....	H.	..	R.
12	Warwick.....	Mr. Bullock.....	37	M.	Nov 30.....	H.	..	R.
13	Walsall.....	Dr. Hubbard	32	M.	Dec. 10.....	H.	..	R.
14	Birmingham	Mr. J. R. Harmar....	49	M.	Dec. 16.....	..	P.	R.
					1882.			
15	Birmingham	Mr. Holbeche	34	W.	Feb. 27.....	H.	..	R.
16	Walsall	Mr. Willmore.....	46	M.	March 7.....	H.	..	R.
17	Birmingham	Dr. Hickinbotham...	38	M.	April 11.....	H.	..	R.
18	Lichfield.....	Mr. Clay	28	S.	May 16	H.	..	R.
19	Birmingham	Dr. Quirke.....	34	M.	July 13	..	P.	R.
20	Birmingham	L. T.	25	M.	Aug. 17.....	H.	..	R.
21	Redditch	Dr. Mathews	20	M.	Sept. 5.....	H.	..	R.
22	Coventry	Dr. Fenton.....	39	M.	Sept. 8.....	..	P.	R.
23	Birmingham	Dr. Sawyer	40	M.	Sept. 27....	..	P.	R.
24	Wanganui, N. Z. ...	Dr. Conolly	23	M.	Oct. 12.....	..	P.	..

TWO CASES OF REMOVAL OF OVARY FOR ABSCESS.

No.	Residence.	Medical Attendant.	Age.	M. or S.	Date.	Hosp.	P.	R.
					1872.			
1	Birmingham	Mr. Hallwright	42	M.	Feb. 11....	..	P.	R.
					1880.			
2	Wolverhampton	Dr. Lycett..... ...	37	M.	June 28.....	..	P.	R.

H means hospital case. P, private case, R, recovery.

TWENTY CASES OF REMOVAL OF UTERINE APPENDAGE FOR PYOSALPINX.

No.	Residence.	Medical Attendant.	Age.	M. or S.	Date.	Hosp.	P.	R.
					1879.			
1	Sutton	Mr. Evans	26	M.	Oct. 11	..	P.	R.
					1880.			
2	Hanley	Dr. C. H. Phillips	37	M.	Aug. 3	..	P.	R.
3	Birmingham	L. T.	28	M.	Sept. 24	H.	..	R.
4	Birmingham	Dr. Hoare	32	M.	Oct. 5	H.	..	R.
					1881.			
5	Church Stretton	Dr. McLintock	37	M.	March 6	..	P.	R.
6	Darlaston	Dr. Cameron	40	M.	May 21	..	P.	R.
7	Cradley	Dr. Standish	29	M.	June 13	H.	..	R.
8	Dudley	Mr. Samuel Berry	31	S.	Oct. 21	..	P.	R.
					1882.			
9	Birmingham	Mr. Green	28	M.	Feb. 2	H.	..	R.
10	Birmingham	Mr. J. R. Harmar	35	S.	Feb. 9	H.	..	R.
11	London		27	M.	Feb. 22	..	P.	R.
12	Stafford	Dr. Day	28	M.	March 4	H.	..	R.
13	Stonehouse	Dr. Eshelby	37	W.	March 10	..	P.	R.
14	Wednesbury	Mr. J. C. Garman	49	M.	March 13	H.	..	R.
15	Walsall	Dr. Holliday	31	M.	April 12	H.	..	R.
16	Birmingham	Dr. C. J. Bracey	38	M.	April 21	..	P.	R.
17	Walsall	Mr. G. Sharp	28	S.	April 27	H.	..	R.
18	Manchester	Dr. Lloyd Roberts	32	M.	June 28	H.	..	R.
19	Budleigh	Dr. Evans	28	W.	June 28	..	P.	R.
20	Birmingham	Mr. Briggs	27	M.	Aug. 17	H.	..	R.

The gratifying success which has followed these efforts in abdominal surgery has induced me to venture into fields hitherto untrodden, or very sparsely traversed, and in these my success has been quite as great.

The following is a list of operations of various kinds which have directly issued from the great advance in ovariotomy in 1878, all of which, with one exception, have been successful:

Nephrectomy	1
Nephrotomy	8
Cholecystotomy for gallstone	4
Hepatotomy for hydatids of liver	10
Laparotomy for pelvic abscess	20
Laparotomy for abscess of spleen	1
Laparotomy for hydatids of peritoneum	4
Laparotomy for chronic peritonitis	8
Laparotomy for acute peritonitis	2
Laparotomy for removal of extra-uterine pregnancy	7
	65

The only fatal case was one of extra-uterine pregnancy with a living child. The mother died of exhaustion, but the child is now about three years old and is thriving.

By no means the least satisfactory groups in the above list are those of acute and chronic peritonitis. In these cases absolute cures have been effected, in every instance, by the simple plan of opening the peritoneal cavity, cleaning it out, and draining it for a short time. That they were cases of an extreme kind might be shown by their details, but probably one will suffice. I take the following description of the patient's condition from a letter written to me by Dr. Justin McCarthy, who sent her to me: "The condition in which I found her was one of the greatest emaciation; seldom have I seen it greater, unless in the last stage of phthisis. There was an enlargement of the abdomen of rapid growth, and she had incessant vomiting and diarrhœa." I opened the abdomen, cleansed it, and drained it on May 18, 1882, and I saw her five months after, stout and robust, and able to perform any kind of work.

In Mr. Spencer Wells' recently published book is given a list of thirty-nine cases of removal of uterine tumor with twenty deaths. This terrible mortality would be more than enough to condemn the operation, and up to a short time ago, in my own practice, the results were nearly as bad, and I did condemn it very strongly. I found, however, from my failures, that it was, as in ovariotomy, the method of dealing with the pedicle which was at fault, and now that I have altered and amended this, I have obtained results which seem to promise that I shall do almost as well in the removal of uterine tumors as I have done in ovariotomy.

Of the twenty deaths in Mr. Wells' list, fifteen occurred with the use of the ligature for the treatment of the pedicle, and only one with the clamp. My experience is quite in harmony with this. I found when I used the ligature, however tightly it was tied, and however carefully I stitched it up like a stump, as advised by Mr. Wells, the stump shrank in a few hours from the escape of serum, the ligature loosened, and the patient died of hemorrhage. In only two cases have I been successful with the ligature, and in both I added the cautery, and did not sew up the stump. Therefore, it need hardly be said that my experience is not in favor of Mr. Wells' recommendations; and it seems to me more than curious to see Mr. Wells deserting the clamp in the very field where its use is promising to be an advance, after he had used it for twenty years in a field where it was a detriment and a hinderance to all progress.

I do not gather from his writings that in the cases where he employed the clamp, that it was other than the old caliper

form, the use of which he made familiar. He seems to have used it in six cases, of which only four recovered, which is much better than his results with the ligature, for with that treatment of the pedicle he had fifteen deaths and only fourteen recoveries. In my own practice I found that the caliper-clamp was fatal because it was quite impossible to close the abdominal wound accurately around the flattened stump, just as in ovariotomy.

I found that Koeberle's plan was open to objection because the small size of the wire cut the pedicle and gave rise to secondary hemorrhage (in two cases), and that (in one case) the wire broke some hours after the operation and allowed fatal hemorrhage to occur. I therefore devised the clamp which is figured on page 285, and which has proved completely successful in every case to which I have applied it, as may be seen from the following list:

EIGHT CASES OF HYSTERECTOMY FOR MYOMA IN WHICH THE PEDICLE WAS TREATED BY TAIT'S CIRCULAR WIRE CLAMP.

No.	Residence.	Medical Attendant.	Age.	M. or S.	Date.	Hosp.	P.	R.
					1880.			
1	Glasgow.....	Dr. Bruce.....	41	S.	Sept. 8...	..	P.	R.
					1881.			
2	Stoke-on-Trent...	Dr. Craig	43	M.	Nov. 17...	H.	..	R.
					1882.			
3	Ellesmere.....	Mr. J. W. Roe.....	32	M.	April 23...	..	P.	R.
4	Liverpool.....	Dr. Graham.....	32	S.	June 13...	..	P.	R.
5	Nottingham.....	Dr. Beddard	41	M.	Aug. 5....	..	P.	R.
6	Birmingham	Dr. Hickinbotham	45	M.	Sept. 8....	H.	..	R.
7	Cheltenham.....	Dr. Gooding.....	44	M.	Sept. 28...	H.	..	R.
8	Derby	Mr. Wright Baker	38	M.	Sept. 28...	..	P.	R.

H means hospital case. P, private practice. R, recovery.

In one of these cases (4) the responsibility of performing the operation had been declined by Mr. Spencer Wells, and another (7) came to me from the Samaritan Hospital, where she had been under the care of Mr. Knowsley Thornton.

CHOLECYSTOTOMY.[1]

Dr. Marion Sims attributes to Dr. Handfield Jones,[2] and I think correctly, the merit of first suggesting that the liver and

[1] Reprinted from Vol. LXIII. of the Medico-Chirurgical Transactions, published by the Royal Medical and Chirurgical Society of London.

[2] Mr. Whitaker Hulke has pointed out that Dr. Handfield Jones' proposal was anticipated by Jean Louis Petit (Memoires de l'Académie de Chirurgie, Tome I., p. 155). How curious that so valuable a suggestion should lie dormant for nearly a century and a half!

gall-bladder should be included within the field of surgical practice more fully than they had been up to that point, and particularly that surgical interference should be made in cases where death is threatened from the impaction of a gall-stone.

To Dr. Marion Sims himself must be given the credit of having followed out this suggestion with his usual boldness and ability, and he himself points out that the case in which he did it was not successful only because the operation was too long delayed.

To my good fortune it has fallen to be the first to follow out Dr. Handfield Jones' idea and Dr. Sims' plan successfully.

Elizabeth M——, aged forty, was admitted to the hospital on August 18, 1879, having been sent to me by Dr. Abraham Colles, of Bridgnorth, on account of an abdominal tumor.

She had been married eighteen years, had borne six children, her menstruation had always been normal, and she had enjoyed perfectly good health until the summer of 1878. At that time she began to suffer from severe spasmodic pains in the right side, these being always aggravated by walking or by lifting even slight weights. In September she noticed a swelling at the seat of pain, and this slowly increased. During last winter her pain became much more intense, her appetite failed, she lost strength and flesh rapidly, and on admission she presented an emaciated and almost cachectic appearance. She also suffered at that time from incessant headache and sickness, and obstinate constipation. The seat of pain was over the right kidney, where there was a heart-shaped tumor, firm and elastic, in which no fluctuation could be detected, and which was extremely tender to the touch. On examination under ether, this tumor was found to be perfectly movable toward each side; indeed, it could be pushed completely across the middle line to the left side. All round it a note of intestinal resonance could be produced. When pushed over to the left side, its heart-like shape became very apparent, and when it lay on the left side of the vertebral column, with its apex directed downward and to the left, its base evidently retained a connection with the right side.

A careful examination of the urine gave only negative results, though she spoke vaguely of its having been occasionally dark in color, muddy, and deficient in quantity.

At the consultation held upon the case, a variety of suggestions were made for diagnosis, the chief of which were cystic enlargement of a floating kidney, a tumor of the head of the pancreas, and dropsy of the gall-bladder. But no decided diagnosis was attempted, and my proposal to open the abdomen, and thus ascertain the nature of the tumor, was agreed upon.

On August 23d I opened the abdomen in the middle line to the extent of four inches, the umbilicus forming the centre of the incision. It then became at once evident that the tumor was a distended gall-bladder. I passed the needle of an aspirator into the apex, and drew off a quantity of white, starchy-looking fluid, probably amounting to between twelve and fifteen ounces, but I cannot speak positively as to its amount, as it was unfortunately thrown away by a nurse immediately after the operation. I then opened the gall-bladder at the point of puncture, so as to admit my finger, and came at once upon a large, round gall-stone, lying loose in the cavity. This I easily removed, and on further search I found another of rather larger size, and probably of pear-shape, at the entrance of the duct, impacted in it, and evidently the cause of the dropsical distention of the gall-bladder. The removal of this stone was a matter of very great difficulty; in fact, it took a very much longer time to effect than all the other steps of the operation put together. From the long, narrow, funnel-like cavity in which it was lodged, and from the mobility of the bladder, it was very difficult to seize, and when at last I did get hold of it, I found it adherent to the mucous surface. I had then to consider the extreme likelihood that in removing this impacted stone I might tear the walls to which it was attached, and thus certainly kill my patient. I therefore performed a very careful and protracted lithotrity, chipping little fragments off the stone regularly all over its exposed surface till I had the satisfaction of lifting out its nucleus. I then passed a blade of a fine pair of forceps on each side of it, and by a gentle squeeze broke up the remainder, and was then enabled to lift it all out. The weight of the stone removed entire is 4.2 grammes, and that of the fragments I could gather of the broken stone is 2.9 grammes, but of the latter stone as much again must have been lost on the sponges which were packed into the wound during the process of crushing, and upon which I had constantly to wipe my instruments. I washed the cavity out repeatedly, and took every precaution that I could to secure that no fragments were left. I then stitched the wound in the gall-bladder to the upper end of the wound in the abdominal walls by continuous sutures, leaving the aperture into the bladder quite open, and then I closed the rest of the abdominal opening in the usual way. The operation was performed with complete antiseptic precautions, and the anæsthetic employed was ether.

She rallied from the operation completely in a few hours. I dressed the wound antiseptically the same evening at 11 P.M., and found the dressings stained with healthy bile. In the fur-

ther progress of the case there is very little to report, save that the flow of bile from the wound continued till September 3d, when the dressings were discontinued and zinc ointment was used in their place. The stitches were removed and the wound was completely healed on September 9th, when she began to take solid food, up to that time her diet having been restricted to milk and beef-tea. On the 14th she sat up for the first time, and on the 30th she went home quite restored to health, free from pain and all her former symptoms, and having gained at least fourteen pounds in weight.

Looking back upon this case, I do not think that a more accurate diagnosis was possible, for there was an entire absence of those symptoms which usually characterize cases of gall-stone. After the operation she told us that one of her neighbors had said to her one day that she thought the patient was jaundiced, but beyond this no history could be got at of any symptoms pointing clearly to the true nature of the case. The singular mobility of the tumor was also a most puzzling condition. Fortunately, our advanced practice in abdominal surgery makes our limited powers of diagnosis in such a case of less importance, and I thoroughly agree with Dr. Sims that we should not wait till the approach of almost fatal symptoms puts the diagnosis in unmistakable fashion, but that "we shall make an early exploratory incision, ascertain the true nature of the disease, and then carry out the surgical treatment that the necessities of the case may demand."

Since the original publication of this paper I have operated three times for gall-stone, and these have recovered completely.

Four Cases of Hepatotomy.[1]

I.—On August 15, 1880, I was asked by Dr. Thelwell Pike, of Malvern, to see a lady, Miss E. G——, aged thirty-seven, whose illness had the following history :

Between 1870 and 1872 she broke down in health, suffered from obscure symptoms of which she can now give no very clear account, but which were referred, by three practitioners whom she consulted, to the spine. In 1872 she consulted the late Mr. Carden, of Worcester, who diagnosed some hepatic mischief, but gave no decided opinion.

In 1873 she had a severe inflammatory attack, the symptoms of which were regarded by her medical attendants as being due to diaphragmatic pleuritis. That illness continued for three weeks.

[1] Reprinted from the Birmingham Medical Review, October, 1881.

Since then she has never been well, suffering from bilious attacks, swollen legs and feet, dyspepsia, inability to walk, and great mental depression. She asserts that the right leg has always been more swollen than the left.

In 1876 she and her friends noticed an alteration in her size, she had to have her dress let out, her breathing became interfered with, and an enlargement on the right side became apparent. This increased slowly till 1879, when it was evident that the whole of the right chest and abdomen were enormously increased in size, but it was not till February of this year that any attempt at diagnosis seems to have been made, and the opinion then seems to have been that the enlargement was due to malignant tumor. In July she came under Dr. Pike's care, and he diagnosed it as a case of hydatids of the liver, and this diagnosis was confirmed early in August by Sir William Jenner, who advised the use of the aspirator. Dr. Pike and Mr. Dawson, of Malvern, aspirated the tumor on August 11th, and withdrew a few teaspoonfuls of clear serum, enough to establish the correctness of the diagnosis of multiple hydatids, even though they could find no scolices in it.

When I saw her on the 15th, I found her in such a condition that it was evident death from suffocation and exhaustion was imminent if relief could not be given. She was propped up in bed to relieve her breathing, and was vomiting incessantly. She was extremely emaciated, had a hay-like odor of her breath, pinched features, and yellow skin, and all the symptoms of extreme exhaustion. The hepatic dulness extended from the third rib down to the umbilicus, crossing the middle line to the left all the way for about two inches, and much more at the lower margin. The whole of the right side was occupied by the tumor, no air was entering the right lung, the left was greatly interfered with, and the heart was pushed much over toward the left. Below the right ribs distinct fluctuation could be obtained over the tumor.

Acting upon the principle which I have already advocated in previous communications to the Society, of opening the abdomen in all cases of tumors where life was threatened, and of the malignity of which there was no certainty, I had no hesitation in proposing abdominal section in this case.

Dr. Pike at once concurred in my proposal, and it was readily accepted by the patient and her friends.

I therefore returned to Malvern the next day (August 16th), and performed the following operation: Dr. Pike gave ether, and I was assisted by Mr. Dawson and Mr. Raffles Harmar. I made an incision four inches long and about two inches to

the right of the middle line, beginning at the edge of the ribs, and inclining slightly inward toward the umbilicus. Having carefully secured all the bleeding points, I opened the peritoneum, and found that there was no adhesion of the liver to the wall, and that I had exposed healthy liver-tissue. Into this I passed a large-sized aspirator needle, and evacuated a few teaspoonfuls of clear serum, as had been done before. Removing the needle I passed my knife into its track, and made an opening large enough for my forefinger. I then found that the layer of liver-tissue was from half an inch to three-fourths thick. I then fixed a pair of Koeberle's catch-forceps on each of the margins of the wound in the liver, and asked my assistant (Mr. Raffles Harmar) gently to draw them up as I enlarged the incision. This I did to the extent of about three inches, and the moment I freed my finger, myriads of transparent globes of all sizes, from a pea to an orange, shot out, covered the table and floor, and were afterward picked off the floor all over the room. When the tension was relieved, I dug them out with a large silver gravy-spoon, an instrument suggested to me by Dr. Pike, and this process took much more time than the whole of the rest of the operation, and during its performance, Mr. Harmar most skilfully prevented any cysts entering the peritoneal cavity, by keeping the flaps of the liver close against the abdominal wound. Finally, I perceived that my gravy-spoon was causing some hemorrhage from the inside of the cavity, which had no kind of lining membrane, and I had to leave a considerable quantity of cysts in the cavity. In the cut surface of the liver, two bleeding points gave me some anxiety, but I closed them temporarily with Koeberle's forceps, and finally secured them in the stitches. These I applied by a common short needle and piece of silk in the continuous method, fastening the wound in the liver, through the whole thickness of the tissue to the wound in the abdominal wall, so as effectually to close the peritoneal cavity ; I then fastened in a wide glass drainage-tube eight inches long. The quantity of hydatid cysts evacuated, is estimated by Dr. Pike to be about two gallons, and I think the amount is not exaggerated.

The patient rallied well from the operation, and seemed to suffer nothing from shock. Her sickness ceased immediately after the operation and did not return, and her breathing became at once relieved, so that she could lie flat on her back, or on either side.

I saw her again with Dr. Pike on the 19th, when I found her without a bad symptom, eating well, entirely free from pain, and with the hepatic dulness contracted to almost normal limits. A

large number of cysts had come through the tube daily with the discharge, which was faintly tinged with bile. Dr. Pike washed out the cavity twice a day with weakly carbolized water.

I saw her again on September 2d, and found the wound healthy, that Dr. Pike had removed the stitches, and that the cavity held only about half a pint. Only one very small cyst had come away since my previous visit. I found also that she was gaining flesh rapidly, and eating well, her diet that day having included bacon, cheese, and porter.

The daily details of the case possess but little interest, beyond the fact that at the end of the first week a short (three-inch) wide glass tube replaced the long one, and in a fortnight more, a $\frac{3}{8}$ rubber tube was inserted instead of the glass. Fragments of cysts continued to come away for about a month, and now (October 17th) there has been hardly any discharge at all for a fortnight, and nothing remains but a sinus.

Dr. Pike notes that one day during the syringing out of the cavity, she had a sharp, sudden pain passing round from right to left. This lasted some three or four hours, and after that about half a pint of bile was passed from the wound, and the pain gradually ceased.

The patient herself writes to me that she feels now quite well, and is able to walk about alone, not quite eight weeks after the operation. She is now (1882) in perfect health and has married.

II.—J. D——, aged fifty-six, was seen by me for the first time on February 5, 1881, in consultation with Dr. G. P. Hadley, of Lozells, under whose care, in conjunction with Dr. Heslop and Dr. B. Foster, he had been for some months. Dr. Hadley has favored me with the following notes: He saw J. D. for the first time in August, 1879, when he had an attack of severe illness which was regarded as due to the passage of a gallstone. In January, 1880, a large tumor was discovered occupying the whole of the epigastrium, right hypochondrium, and extending downward into the right iliac region. The tumor had an indistinct fluctuation. During 1880 the patient became greatly emaciated, passed generally clay-colored stools, and frequently had his urine deeply tinged with bile. In December, 1880, the cyst seemed to find an opening into the intestine, for the tumor became greatly diminished in size, and the patient passed large quantities of brick-red fluid from the rectum. After this discharge the cavity seemed to refill in a few days, and the process was repeated at intervals. In January, 1881, the process of emptying seemed to cease, and it was proposed to tap the cyst, but on account of the presence of intestines all over the front of the tumor it was

deemed more prudent to have an exploratory incision made, and I was asked by his attendants to undertake this. I had no hesitation in doing so, for I found the patient extremely emaciated, with a distinct icteric hue, and evidently sinking. There was a very large cystic tumor, apparently belonging to the liver, and on February 6th I performed the following operation, assisted by Mr. Wright Wilson and Dr. Williams, of Dyffrin, the ether being administered by Mr. Bennett, Dr. Hadley's assistant.

I made an incision about three inches in length over the tumor, in the axis of the right rectus muscle, and about three inches to the right of the middle line, beginning about two inches above the level of the umbilicus. The peritoneum was easily reached, but there I found intestines and omentum glued everywhere over the surface of the tumor, and I had to exercise much care in dissecting them off, so as to clear a part of the cyst about two square inches in area. There was, however, no adhesion between the parietal layer of peritoneum and the subjacent intestines. I then passed my small-sized trocar into the tumor, and evacuated seven and a half pints of dark, bilious-colored fluid. When the cavity was emptied completely, I enlarged the opening made by the trocar so as to admit two fingers, and came at once upon a loose mass, which I removed, and which proved to be a slough of liver-tissue weighing about one ounce. I then stitched the edges of the wound in the liver to those of the wound of the abdominal wall, and fixed in a glass drainage-tube. The cyst was clearly the liver itself, which had been distended into a shell, with apparently a pretty uniform thickness of about half an inch. The fluid removed was carefully examined by Dr. Saundby, the Pathologist to the Women's Hospital, and found to consist of nearly pure bile, mixed with pus.

The progress of the case must be given briefly, for there is very little to tell. No effort was made to conduct the treatment upon Mr. Lister's principles. The glass drainage-tube was left in for about a fortnight, and then a piece of rubber tube replaced it. The temperature and pulse curves were almost normal, the patient's appetite rapidly improved, and upon my last visit to him (March 30th) there was very little discharge from the drainage-tube, and he had gained fourteen pounds in weight in the seven weeks which had elapsed since the operation. (P.S.—He has gained forty-two pounds since the operation, September 16th.)

III.—L. B——, aged twenty-five, was placed under my care by Dr. Thompson, of Leamington, who had recognized the

presence of a large abdominal tumor, which caused the patient much distress. She had been married four years, but had never been pregnant. Her illness began with a sudden attack of pain at the seat of the swelling, in September, 1880, and since then the tumor had steadily grown till I saw her in February. The nature of the tumor was doubtful. It was in the position of the right kidney, was movable, but had an attachment above, which suggested an origin from the liver. No distinct fluctuation could be discovered in it.

On February 9th, assisted by Dr. Thompson and Mr. Raffles Harmar, I made an abdominal section, and found it to be a hydatid tumor of the liver, which had no adhesion to the abdominal wall. I opened the capsule, which consisted of a layer of liver-tissue, about a fourth of an inch in thickness, and scooped out the hydatids with a dessert-spoon. They were of various sizes, from a pea to a small orange, and amounted in all probably to a pint and a half or two pints. I was very careful to cleanse out the deep cavity in the liver very thoroughly, and Mr. Harmar very skilfully kept the edge of the hepatic wound up out of the abdomen, so that none of the parasites escaped into the peritoneum. The wound in the liver was stitched to the wound in the abdominal wall, and a glass drainage-tube was fastened in. The after-progress of the case was uninterrupted recovery, no effort being made to conduct its treatment on Mr. Lister's principles. The glass tube was replaced by a rubber tube at the end of a fortnight, and she returned home to Leamington on March 9th, just a month after the operation, and there she is rapidly recovering under the care of Dr. Thompson, the drainage-tube having been removed (April 23d), and the wound is now nearly healed. (P.S.—This patient is now in perfect health, September 16th.)

IV.—E. P——, aged twenty-one, and unmarried, was placed under my care by Dr. Wellesley Tomkins, of Leamington, in August last, for an abdominal tumor. This I recognized at once to be an enlargement of the liver, and unhesitatingly made a diagnosis of hydatid disease. Her illness began in April, 1880, with an attack of violent bilious sickness, followed by pain in her back and right side. The enlargement was noticed within six weeks, and had steadily increased. She suffered from repeated attacks of violent bilious vomiting. The hepatic dulness extended from the fourth rib down to an inch below the level of the umbilicus, and from the spine round to four inches across the middle line in front, and distinct fluctuation could be felt in the tumor below the ribs. I kept the case under observation from August till February, during which period she increased

two and a half inches in girth over the lower ribs, and fell off markedly in health. I had many consultations with professional friends over the case, more particularly did Dr. Heslop give me valuable assistance. The question, of course, lay between aspiration and hepatotomy, and this could be decided only by our being able to recognize which of the two varieties of hydatid disease my patient suffered from.

No indication of this could be obtained, and having a lively recollection of the disastrous effect of aspiration in a case already published by the Royal Medico-Chirurgical Society, and further, having a growing distrust in aspiration for abdominal surgery, and an increasing confidence in abdominal section, I determined upon the latter. I therefore proceeded similarly as was done in the other cases. It turned out to be a large monocystic hydatid. The thickness of liver-tissue through which I passed was nearly an inch, and I had a little trouble with hemorrhage, which was, however, completely controlled by pressure. I fixed in a wire drainage-tube, after having united the edges of the two wounds, and replaced it by a soft rubber tube at the end of a fortnight. This latter tube I finally removed on April 13th, and on the 19th the wound was almost healed, the patient was getting about, eating wall, and rapidly gaining strength. Possibly, in this case it might have been better to have tried aspiration first, and that may be the opinion of some. I do not agree with this, however, and I see no reason to regret my action. I am growing more and more satisfied that all such cases will be the best treated by abdominal section. (P.S.—This patient, also, is now in perfect health. I have operated upon six other cases of hydatids of the liver in exactly the same way, and all have done well, September 16, 1882.)

The Treatment of Pelvic Suppuration by Abdominal Section and Drainage.[1]

I have purposely used the words "pelvic suppuration" in the heading of this paper, in order to advocate a principle which I believe to be capable of a much wider application than it has already had at my hands. The cases, six in number, in which I have pursued this new method of treatment have all been, so far as I could discover, cases of suppuration occurring in pelvic hæmatoceles; but the difficulties in these cases have been no greater than I think would occur in pelvic suppuration of almost

[1] Reprinted from Vol. LXIII. of the Medico-Chirurgical Transactions, published by the Royal Medical and Chirurgical Society of London.

any kind, and the success has been most exceptionally encouraging. My experience is, of course, limited to suppurations of the female pelvis, but I see no good reason why the same proceedings should not meet with equal success in some cases, at least, occurring in the male.

Like others who follow the surgical specialty in which my practice lies, I have had a wide field for the observation of the various conditions classed under the head of pelvic abscess, and, like others, I have until recently confined my treatment of it to openings made from the vagina or in the neighborhood of Poupart's ligament. Experience, however, has driven me to the conclusion of Dr. Emmet, that "I cannot regard the introduction of the trocar into the inflamed tissues of the pelvis as a procedure free from danger under all circumstances." It is perfectly true that in very many cases where an abscess undoubtedly exists in the cellular tissue of the female pelvis, the fluid can be reached and removed by the needle of the aspirator. But, according to my experience, the relief obtained in this way is, in a large number of cases, neither complete nor permanent, and, in nearly all, the convalescence has occupied a time not at all commensurate with the extent of the lesion. This is quite as true of abscesses which have been allowed to open themselves, or have been assisted to open in the groin. They often continue as fistulous openings for years.

In many cases, even when the abscess can be reached by vaginal puncture, the nature of its contents is such as to make its evacuation an impossibility ; and I have seen several where a puncture made at random through an indurated pelvic roof has missed the disease. In these cases the symptoms of the presence of pus were conclusive, but no indication of its seat could be obtained. Dr. Emmet speaks of such in these words: "I can recall a number of cases which have been under my observation, with thickened tissues, where no treatment had the slightest effect, and finally, they have passed into other hands."

The course of such abscesses is so thoroughly described by Dr. West that his words cannot be improved upon, and therefore I give them at length :

"When suppuration takes place, the matter makes its way outwardly through the vagina, or through the intestinal canal, in almost all cases in which the inflammation is limited to the parts contained within the broad ligament. In those cases, however, in which the pelvic cellular tissue is implicated, the matter not unfrequently makes its way round between the muscles and the external surface of the peritoneum, and the abscess

points and discharges itself through the abdominal walls somewhere in the course of Poupart's ligament, or a little below that situation.

"Though the size of the abscess is not, in general, very great, it not unfrequently passes into a chronic state, and emptying itself, for the most part, through some narrow passage of communication into the bowel, the patient continues for months or years liable to occasional discharges of pus per anum, the commencement of which dates back to some attack of inflammation of the cellular tissue years before."

In an instance quoted by Dr. West, "occasional discharges of matter took place from the bowel, and pus was often intermixed with the fæces, five years after the first symptoms of inflammation of the cellular tissue about the uterus, the chronic results of which were still evident in a tumor which was closely connected with the rectum and the womb. These chronic abscesses generally contract, and the fistulous passages that lead to them become, by degrees, obliterated, but exceptions to this now and then occur, two of which have come under my own notice; and Sir J. Simpson has reported some very interesting cases where permanent fistulous communications have formed between the abscess succeeding to inflammation of the pelvic cellular tissue and the bladder, uterus, or intestinal canal."

In my own practice such disappointing cases have occurred with but too great frequency, and though I have had some successes by the employment of such means as the elastic ligature (*Lancet*, June 27, 1874) and counter-opening in the vagina (*Lancet*, April 3, 1875), yet the progress toward recovery has been so protracted as to contrast favorably only with those cases in which there was no recovery at all.

I have been, therefore, continually on the outlook for some means of dealing with such cases which would bring them as satisfactorily within our means of treatment as are collections of matter in most other parts of the body. This has been furnished by the wide, free, and successful application of abdominal section for the treatment of pelvic and abdominal tumors, and I have now to lay before the Society six cases, which include the whole of my experience in this novel proceeding, and in which success has been obtained far surpassing anything I have yet seen or heard of. In this comparison I am of course excluding those cases where pointing of the abscess in the vagina is evident at an early stage of the case, but even in these the recovery has always been, in my experience, more protracted than in the six now to be narrated.

A patient was sent to me in February, 1879, by Mr. Gwinnett

Sharp, of Walsall, suffering from a pelvic tumor, associated with very severe symptoms. She was twenty-two years of age, and had been married nine months. Her menstruation had always been too frequent and too profuse, and six weeks before I saw her it had stopped suddenly during its course, and this was associated with the onset of violent pelvic pain—one of the leading features of extra-peritoneal hæmatocele. A few days afterward she shivered and became very ill and feverish, and these symptoms had become intensified when I saw her ten days after their occurrence. She was then emaciated and hectic-looking, with a high night temperature, intense pain and tenderness over the lower abdomen, and when examined, a large fluctuating tumor, adherent to and behind the uterus, and going on either side of it, was found to occupy the pelvis and rise about half-way up to the umbilicus. The roof of the pelvis was fixed and hard, and no fluctuation could be felt there.

The nature of the tumor could be open to only two suggestions : that it was a suppurating parovarian cyst with peritonitis, or a suppurating hæmatocele. I leant to the latter view, as it was in consonance with the history, and I have never known a parovarian cyst suppurate, whilst hæmatoceles frequently do.

In any case, I determined to open it from above, and this I did. I found a large cavity, containing about two pints of fetid pus with decomposing blood-clots. This I carefully cleansed out, and after having united the edges of the opening into the cyst carefully to the abdominal wound, I fixed in one of Koeberle's glass drainage-tubes, five inches long. Seven days after the operation I placed a three-inch glass drainage-tube, and in another week this was replaced by a soft rubber tube. The patient got up on the twentieth day after the operation, and in ten days more went home perfectly well, with the abscess healed, and she remains now in perfect health (March, 1880).

The second case was sent to me by Dr. Flynn, of Birchills. She was forty-five years of age, and had never been pregnant, save one doubtful miscarriage soon after marriage, nineteen years ago. Symptoms resembling those of hæmatocele had occurred eight months before I saw her, and since that time she had been losing flesh, had lost her appetite, was troubled by constant thirst and night-sweats, and she had a rising night temperature. The uterus was fixed in a mass of effusion occupying the left broad ligament, and partly the right one also, and the mass on the left side encircled the rectum, forming a pronounced stricture, as hæmatoceles of the left broad ligament frequently do. No point of fluctuation could be felt in the pelvis, but the symptoms pointed clearly to the presence of pus. I

therefore determined to open the abdomen, and readily obtained the consent of my colleague to this proceeding.

On reaching the peritoneum the two layers were found to be adherent, so that the cavity was not opened. A large abscess was opened just behind the base of the bladder, between which and the uterus it principally lay, but stretching round behind the rectum. The floor and posterior wall of the abscess were found to consist of organized blood-clot, so that its origin was in a blood effusion into the broad ligament. A glass drainage-tube was inserted, and this was changed for one of Chassaignac's wire tubes on the eleventh day after the operation. She sat up on the twenty-first day after the operation, and the tube was finally removed on the twenty-sixth. She went home on the thirtieth day perfectly well, and has remained so ever since, now nearly ten months.

The third case was a patient of Mr. Hallwright's, in whom he had diagnosed hæmatocele some four weeks before I saw her. Symptoms of suppuration set in, and I performed exactly the same operation as in the first case, that is, the peritoneum was opened, and the abscess emptied and cleansed, and then the edges of its opening fastened to the edges of the parietal wound, and a glass drainage-tube fastened in. This case also was an undoubted hæmatocele of the broad ligament. Eight days after the operation the glass tube was changed for a wire one, and this was removed in twelve days more. She left the hospital perfectly well, only thirty-three days after her admission, and has since remained in good health.

Mary Ann B——, aged thirty, has been married eight years, and has had four children, youngest fifteen months old. Seen first by Mr. Hallwright on December 12th, when she stated that she had become unexpectedly unwell about five weeks ago, that this was accompanied by violent pain, which has never since been absent. She was seen by Mr. Hallwright, who diagnosed the occurrence of hæmatocele from the presence of a large hard tumor behind the uterus. About a fortnight previous to my seeing her the nocturnal exaltation of temperature, night-sweats, thirst, and increased pain, led Mr. Hallwright to suspect that the effusion was suppurating, and when I saw her I had no difficulty in confirming his opinion. I therefore admitted her to hospital, and on December 22d I performed abdominal section, as I found the intensity of the symptoms increasing, and on examination under ether the mass of the effusion seemed too high up to be opened safely from the vagina. The tumor consisted of a large effusion of blood in process of disintegration, contained in a cavity formed by the lifting

up of the posterior layer of the broad ligament, the rectum being carried up in front of it, together with the large vessels of both sides, as high as the bifurcation of the aorta, whilst anteriorly the peritoneum dipped to a most unusual depth, so that had I tapped the tumor from the vagina, I must have gone through the peritoneal cavity. The cyst was opened and emptied, and a drainage-tube fastened in, and the peritoneum closed in the usual way. Her recovery was neither so easy nor so rapid as any one of the others, probably because the cavity was the largest of all, and her condition before the operation was very bad, though another case to be related was, in the latter respect, much worse. The temperature in the present case was 38.4° before the operation, and it rose to 40° on the second day. It did not fall to 37° till the tenth day, December 31st. The drainage-tube was removed on January 10th, and on the 17th the wound had perfectly healed, and she left the hospital on the 26th. I saw her on February 26th, when she was hardly to be recognized, so astonishing was her restoration to health. From a thin, emaciated, and apparently dying woman, she had been transformed into a perfect picture of health, and she stated that she was able to do her work and get about as well as ever she did in her life.

In this case I am quite satisfied that the delay of the operation for a few days would have been fatal, and no vaginal tapping, even if it had missed the peritoneal layers, would have emptied the cyst of its clotted contents.

Ann S——, aged twenty-eight, placed under my care in January, 1882, by Dr. Gordon, of Walsall. She is the mother of three children, the youngest being three years of age. About four months ago she had symptoms resembling those of sudden effusion of blood into the broad ligament. For a month she was able to get about, but during the last three months she has been entirely confined to bed, the subject of symptoms clearly pointing to the occurrence of suppuration. Dr. Gordon had discovered the presence of a pelvic mass behind the uterus, in which no fluctuation could be discovered, and which was fixed.

I admitted her into hospital, and suspecting the case to be one of suppurating hæmatocele, I opened the abdomen on January 5th, and found the case to resemble the preceding one, save that the disintegrating effusion was not so large. It was dealt with in the same way, and the patient made a much more easy and rapid recovery, leaving the hospital on January 17th, and being restored to perfect health before the end of February.

Mrs. H——, aged twenty-nine, was married at eighteen, had

a child within the year, and has never been pregnant since. I saw her, at the request of Dr. Millington, of Wolverhampton, under whose care she had been, in conjunction with Dr. Blackford, of Cannock, whom I met in consultation over the case on the 15th of January, 1882. The history given to me was that about nine weeks previously, when driving in an open carriage with her husband on a very cold day, and during a menstrual period, she was suddenly attacked by a violent pelvic pain, and coincidently with this the discharge ceased. This pain had continued ever since, and had of late increased in severity. Menstruation had occurred at two irregular intervals since the beginning of her illness with great profuseness, and during these periods her pain had been much easier. A pelvic tumor had been discovered by Dr. Millington some weeks before my visit, and this he had regarded as an effusion of blood. She had suffered for about three weeks before I saw her from night-sweats, almost constant sickness, utter loss of appetite, intense thirst, with various other symptoms of pronounced hectic. The tumor, when I examined it, involved all the pelvic organs in a fixed mass of cartilaginous hardness, with the uterus embedded in it; the bladder spread over it in front, and the rectum encircled by a ring of hard effusion. The mass could be felt above the pelvis as a round and non-fluctuating tumor, with intestine in front of it. The patient had reached almost the final stage of exhaustion and emaciation. There was no difficulty in diagnosing the case as one of suppurating hæmatocele. With Dr. Blackford's concurrence we had her removed to Birmingham, and on the 21st I opened the abdomen and found matters quite as I had anticipated. The posterior layer of the broad ligament was lifted completely up out of the pelvis, and so was the anterior layer, as far as I could make out; at least, the only structure I could identify was the base of the bladder, and that seemed to form the anterior boundary of the tumor. From this point it spread backward, on a level with the brim of the true pelvis, and its posterior boundary was the bifurcation of the aorta. The contents were clearly fluid, and therefore I tapped it with an aspirator needle, and evacuated about half a quart bottle full of curdy blood-colored pus. I then laid the cyst open from the point of puncture, in a direction from before backward, and found its floor to consist of a thick layer of laminated clot, hard and rigid. I could make out the uterus rising out of this mass, but I could not discover the rectum.

I stitched the edge of the opening into the abscess to the edges of the parietal wound, and then closed the rest of the peritoneal opening, and fastened in a wide drainage-tube of

glass. After the operation the patient's temperature never rose above 37°, she had no more night-sweats nor sickness, and her appetite was really keen on the third day. A small-sized wire drainage-tube replaced the glass one on the twelfth day, as the discharge had become healthily purulent and free from clot *débris*. The smaller drainage-tube was removed on the fifteenth day after the operation, and on the twenty-fourth the sinus was quite healed, she had gained greatly in flesh and color, she was able to walk about, and on the twenty-seventh day she went home perfectly well, the uterus, however, being still quite fixed, as I expect it will remain for years. I have just had a letter from her (March 30th), in which she reports herself as being in perfect health.

In all of these cases I am satisfied that vaginal tapping would have been useless. In most of the cases, if the abscess had been opened by natural processes, it would have been into the rectum. In the last case it would probably have been in one of the groins; but I think in every one of the cases, unless it be in the second, death would have occurred long before a natural outlet could have been established.

My general conclusion from these cases is that the opening of such abscesses by abdominal section is neither a difficult nor a dangerous operation; that recovery is made in this way more certain and rapid than in any other; and that in future I shall always advise an exploratory incision where I am satisfied there is an abscess which cannot be reached nor emptied satisfactorily from below.

Since the original publication of this paper I have operated on fifteen cases, and the recovery in all the twenty has been complete and permanent.

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