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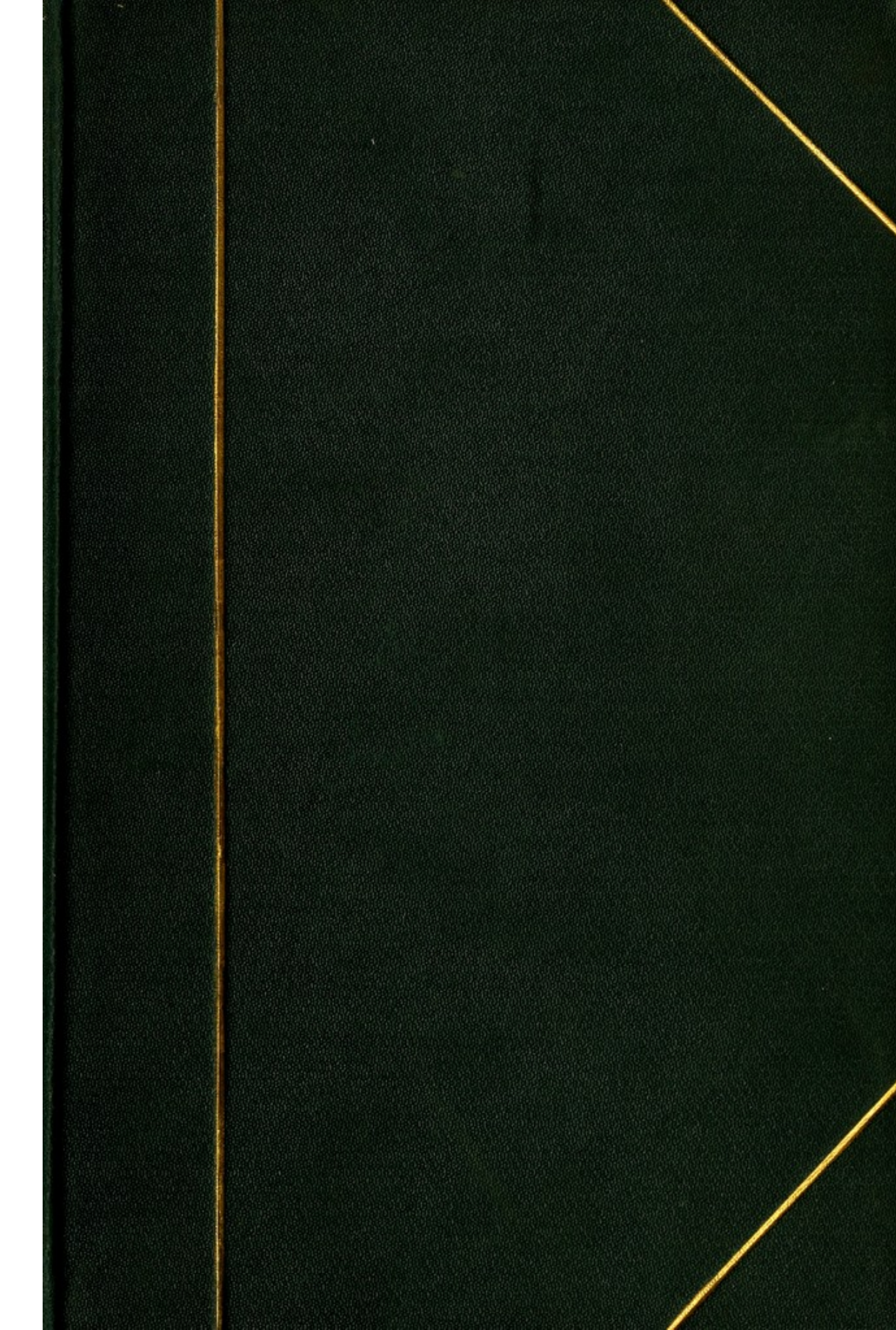
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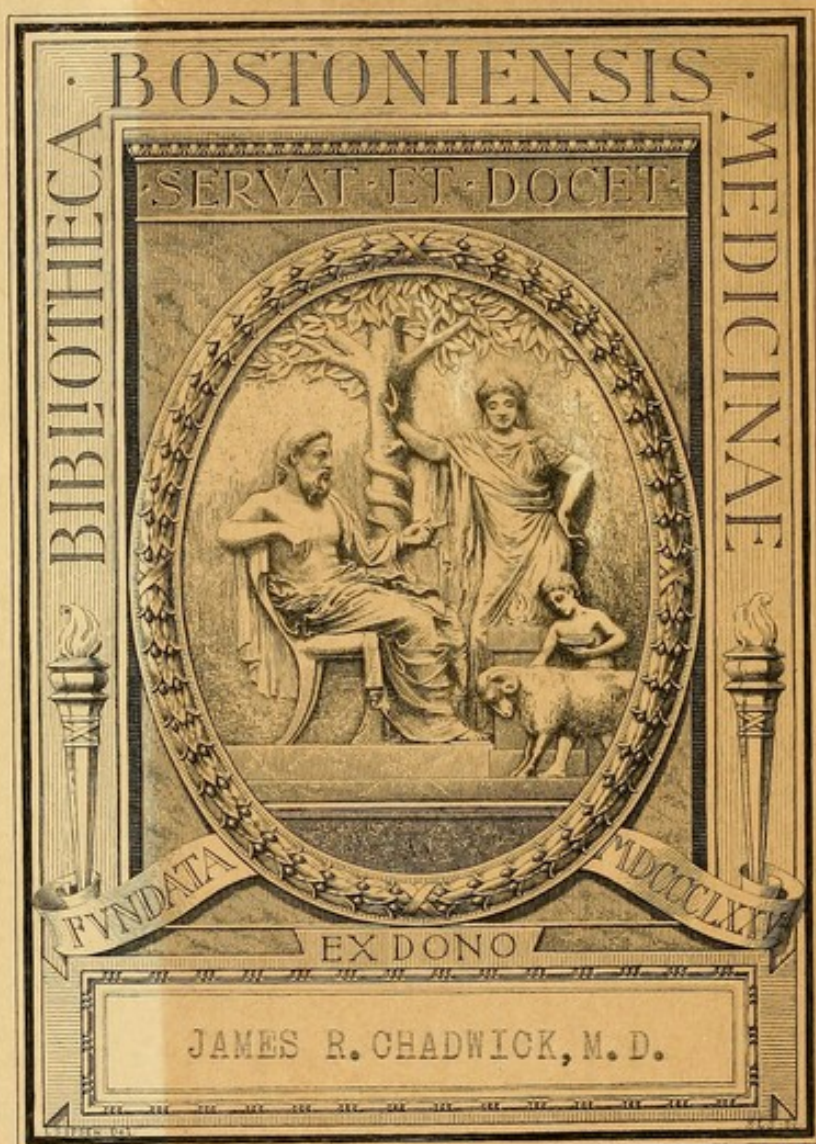
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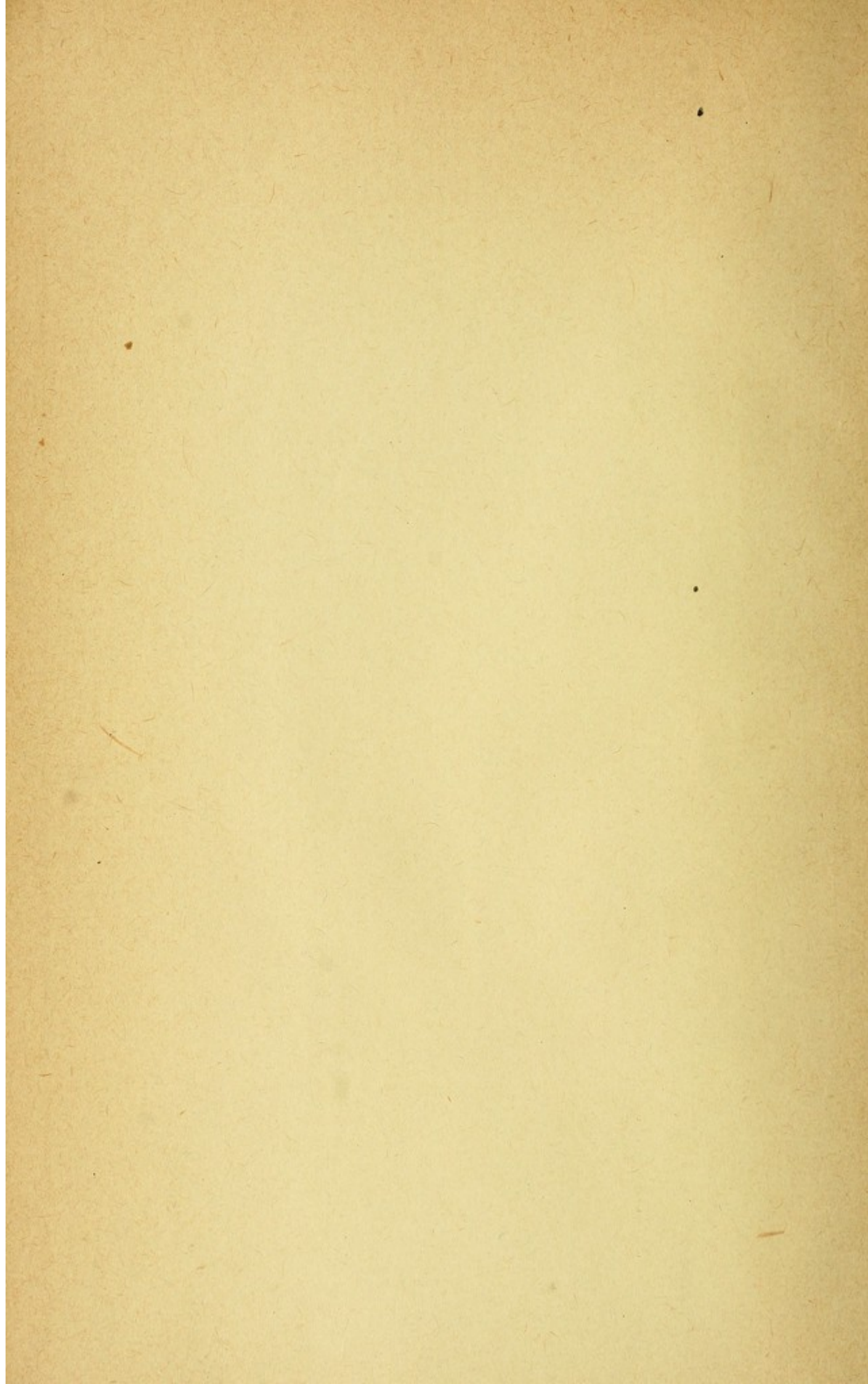
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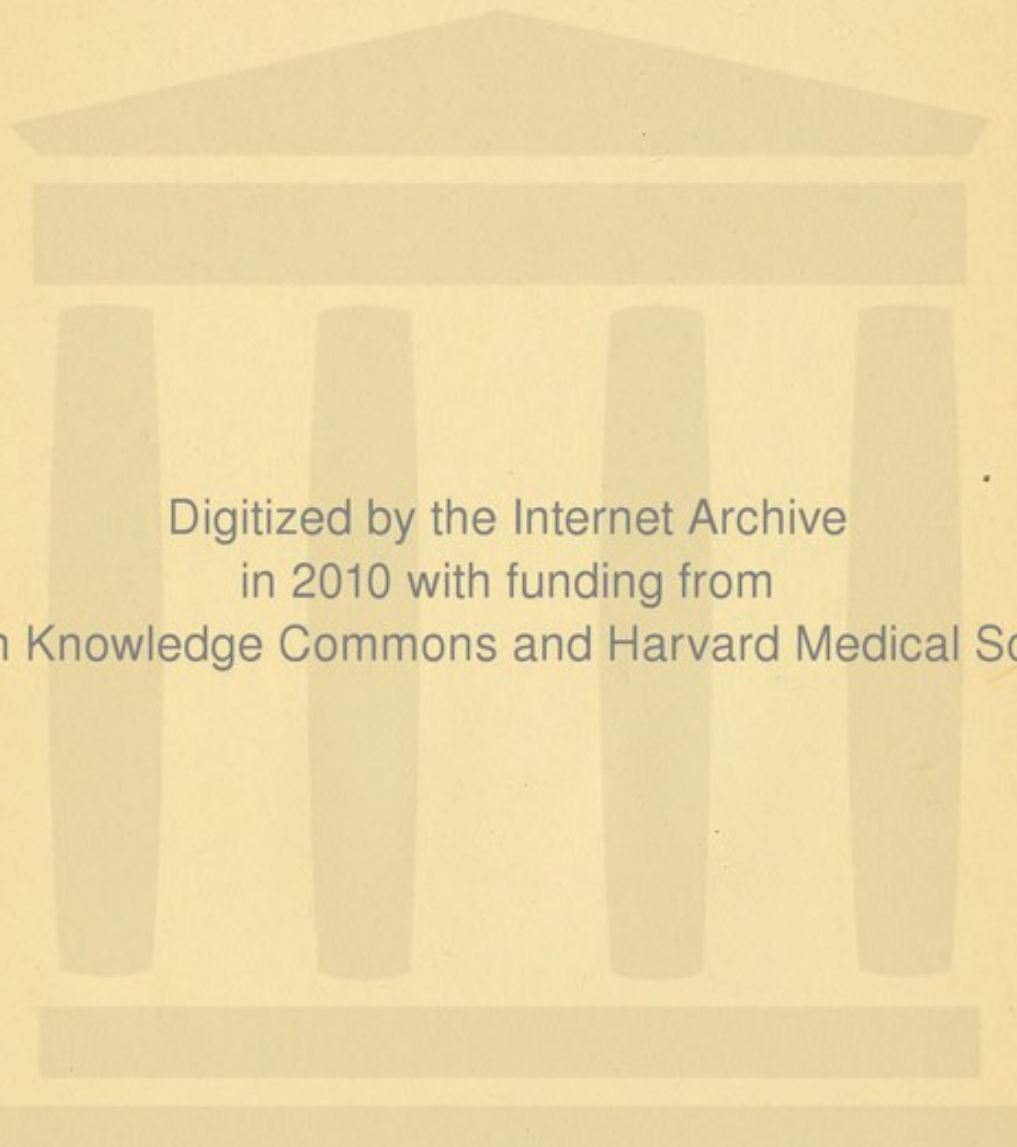


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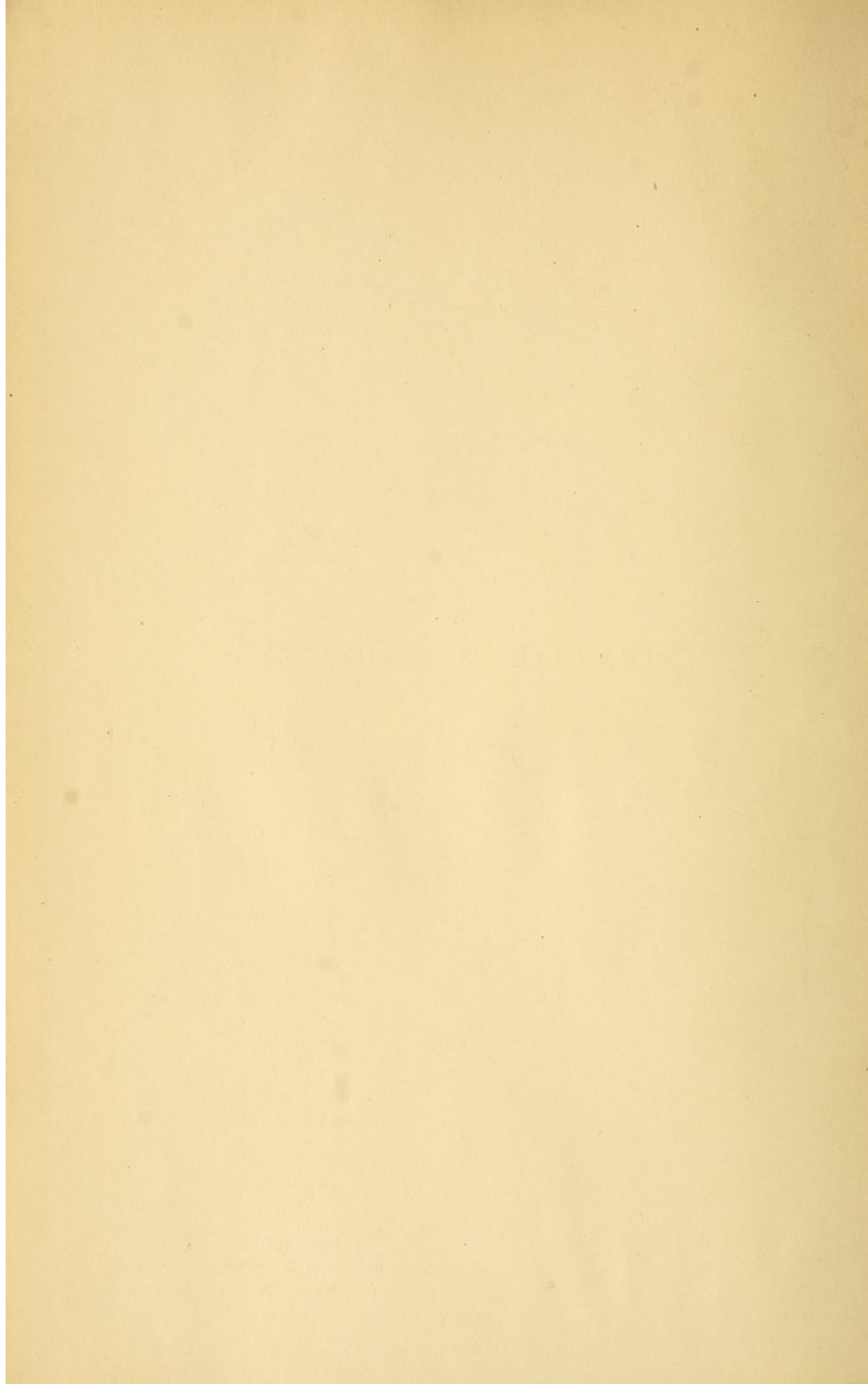


J. R. Chadwick





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EXTRA-UTERINE PREGNANCY.

"The resources of Surgery are rarely successful when practised on the dying."—W. T. LUSK.

THE
DIAGNOSIS AND TREATMENT
OF
EXTRA-UTERINE PREGNANCY.

BY

JOHN STRAHAN, M.D., M.Ch., M.A.O. (ROYAL UNIV. OF IRELAND),

FOTHERGILLIAN GOLD MEDALLIST OF THE LONDON MED. SOC., 1886, FOR ESSAY ON "TYPHOID
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JENKS PRIZE ESSAY OF THE COLLEGE OF PHYSICIANS
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TO
The Memory of
WILLIAM FURNESS JENKS, M. D.,

WHOSE EARLY DEATH ALONE PREVENTED HIS OCCUPYING A FOREMOST
PLACE AMONG

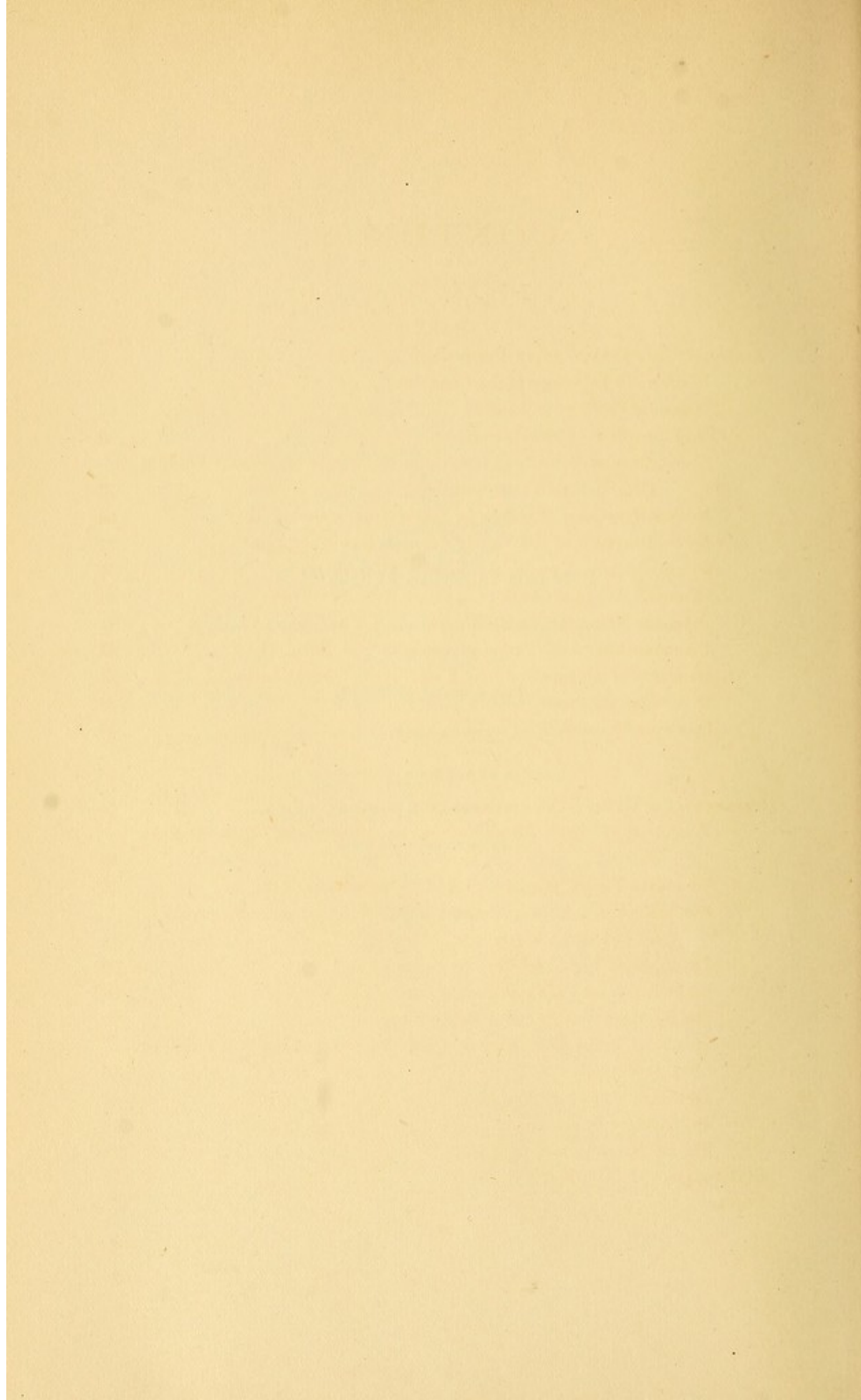
OBSTETRICAL WRITERS,

THIS ESSAY,

HONORED BY THE FIRST AWARD OF THE
WILLIAM F. JENKS MEMORIAL PRIZE,

IS RESPECTFULLY INSCRIBED

BY THE
AUTHOR.



CONTENTS.

PART I.

	PAGE
DIAGNOSIS OF EXTRA-UTERINE PREGNANCY,	1
History and Pathology of the Condition,	1
Diagnosis Previous to Rupture,	7
Diagnosis from Normal Gestation,	10
Conclusions as to the Diagnosis up to the Period of Primary Rupture (During First Three Months),	23
Normal Pregnancy Mistaken for Extra-Uterine Gestation,	30
Is the Diagnosis of the Variety Possible Before Rupture?	32
Diagnosis at the Time of, and Shortly After, Rupture,	34
Diagnosis After the Shock of Primary Rupture is Over,	42
Diagnosis About the Viable Period,	43
Diagnosis After Full Period of Gestation and Fœtal Death,	48
Diagnosis of Suppuration in an Extra-Uterine Gestation Sac,	51
Differential Diagnosis,	59
Summing-up on the Diagnosis,	66

PART II.

TREATMENT OF EXTRA-UTERINE PREGNANCY,	68
Treatment of Early Extra-Uterine Pregnancy, and When Rupture Occurs,	68
Treatment Before Rupture, Should a Diagnosis be Made,	72
Methods which have been Recommended for Bringing the Pregnancy to an End in the Pre-Rupture Stage,	79
Treatment at Period of Primary Rupture,	90
Resuscitation of Patient After Rupture,	93
The Operation for Ruptured Tubal Gestation,	96
Treatment Between Primary or Tubal Rupture and the Full Period of Gestation,	98
Treatment at the Full Period,	105
The Primary Operation,	111
Vaginal Extraction of the Child,	118
BIBLIOGRAPHY,	127
INDEX,	129

JANUARY 29th, 1889.

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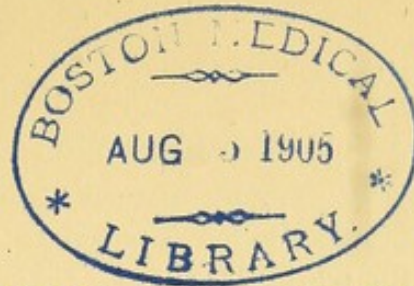
We, the members of the William F. Jenks Prize Committee, appointed by the President of the College of Physicians of Philadelphia, to examine the essays submitted in competition for the prize of two hundred and fifty dollars (\$250) offered under the provisions of the Deed of Trust executed by Mrs. William F. Jenks, herewith present our report.

We have received and carefully examined seven (7) essays upon the subject selected, viz : "The Diagnosis and Treatment of Extra-Uterine Pregnancy," and after a close comparison of their respective merits, we have unanimously awarded the prize to the essay which bore the following motto upon its title page : "The resources of surgery are rarely successful when practised on the dying."

On opening the envelope which accompanied this essay, and bore the same motto, we found that the author was John Strahan, M.D., M.Ch., M.A.O., Royal University Ireland, 247 North Queen Street, Belfast, Ireland, who is therefore entitled to the prize.

In conclusion, we desire to compliment the author upon the remarkable excellence of this essay—which will long be memorable in the records of the College for having received the first award under the above-mentioned Deed of Trust—and we also wish to express our thanks to the giver of the fund which has been instrumental in producing this very valuable contribution to the literature of our profession.

ELLWOOD WILSON,
THEOPHILUS PARVIN,
JAMES V. INGHAM.



DIAGNOSIS AND TREATMENT
OF
EXTRA-UTERINE PREGNANCY.

PART I.

DIAGNOSIS.

IN order to discuss the diagnosis of extra-uterine pregnancy it is essential to glance at the history and pathology of the condition, as an entire revolution has taken place within the last few years in pathology and treatment.

Until quite recently great ignorance and confusion prevailed as to the causation, varieties, and treatment, so much so, indeed, that the value of the older works on the subject is very small. In particular the value of the older tables of statistics and collections of cases, where those which have little or nothing in common are huddled together, amounts to almost nothing, except for some particular uses; and the deductions from such statistics are often most misleading. The recent enormous strides of abdominal surgery first revolutionized the treatment, and now post-mortem examinations greatly helped by ante-mortem sections have afforded new light on the pathology, which makes clear much which was previously very dark indeed. Lawson Tait has, perhaps, more experience of such cases than any man living, and more than any man who ever lived. The writer is inclined to agree with most of Tait's views upon pathology, although Tait is evidently too dogmatic on several points in diagnosis and treatment. Or, perhaps I should say, he looks on the treatment too much from the highly skillful abdominal surgeon's point of view, and does not consider what would be the most proper treatment to be followed where

such special skill was not available. With regard to pathology the weight of evidence seems to be in favor of most of his views, and now, since attention has been so much directed to the subject, proofs are coming in daily of the truth of positions which he took up years ago purely upon brilliant inductive reasoning. His theories, certainly harmonize with all that has been published on the subject, which no other theories will do. Most apparent contradictions of his views can be quite easily explained, and a great many of them can easily be made to convict their authors of error, out of their own mouths. As to old authors, their descriptions of even supposed facts require to be received with many grains of salt. Indeed, we might commence this chapter with the words of a famous author on this subject: "Special treatises on obstetrics, as well as periodical medical literature, teem with statements which are utterly unreliable, and which are calculated to mislead investigators of this subject."¹

The work of Parry, and that of Dr. William Campbell, of Edinburgh, published in 1842, contain about all that was known of the subject up till 1878, when it began to develop rapidly through Tait's wonderful success in abdominal section for rupture of extra-uterine gestation cysts during the first four months. One great change in our views, admitted on all hands, is that such cases are much more frequent than was formerly thought. In the light of recent experience, we can see quite plainly that formerly such cases were, as a rule, not only not treated, but not even diagnosed. Such cases were put down to "peritonitis" or to simple hæmatocele, extra- or intra-peritoneal, and the woman was simply allowed to die with whatever comfort could be derived from opium, hot fomentations, hot-water jars, stimulants, etc. The diagnosis of hæmatocele was right enough as far as it went, for the rupture of an extra-uterine gestation cyst is the most common cause of pelvic hæmatocele, of both varieties.

Without further preface or reference to the older literature of the subject I shall proceed to give Tait's classification of the forms which extra-uterine pregnancy may assume, either from the first, or after what Tait calls "primary" rupture. His amended table as given in the *Lancet*, September 1st, 1888, p. 409, is as follows:—

"Scheme of Ectopic Gestations in Tubo-ovarian Tract.

"1. Ovarian: possible, but not yet proved.

¹ John S. Parry, "Extra-uterine Pregnancy, its Causes, Species, etc." Philadelphia, 1876.

"2. Tubal, in free part of tube; and is—

"(a) Contained in tube up to fourteenth week, at or before which time *primary* rupture occurs, and then the process of gestation is directed into—

"(b) Abdominal or intra-peritoneal gestation, uniformly fatal unless relieved by abdominal section, primarily by hemorrhage, secondarily by suppuration of the ruptured sac and peritonitis.

"(c) Broad ligament or extra-peritoneal gestation.

"(d) May develop in broad ligament to full time, and be removed at viable period as a living child.

"(e) May die and be absorbed as extra-peritoneal hæmatocele.

"(f) May die, and the suppurating ovum may be discharged at or near the umbilicus, or through the bladder, vagina, or intestinal tract.

"(g) May remain quiescent, as a lithopædion.

"(h) May become abdominal or intra-peritoneal gestation by *secondary* rupture.

"3. Tubo-uterine or interstitial is (a) contained in the part of the tube embraced by uterine tissue, and, so far as is known, is uniformly fatal by intra-peritoneal rupture (as *b*) before the fifth month."

Regarding ovarian gestation, Tait is not alone in the opinion that it never occurs. Many writers of eminence deny the possibility of its occurrence, as Velpeau, Arthur Farre, Mayor, and, more recently, A. Willigk, who minutely examined several supposed specimens, but failed to find foetus or membranes in the ovary. Here we come to Tait's theory as to impregnation. He believes that, save in abnormal conditions of the tubes, spermatozoa never penetrate further than the uterus. The belief in ovarian gestation arose at the end of last century, from the theory that spermatozoa normally penetrated to the ovary. Hausmann made some researches in animals which were supposed to settle the matter, and the belief got up that most extra-uterine pregnancies were ovarian. This, at least, is self-evidently false at the present day. But Hausmann's conclusions are inconsistent with each other, and the lower animals cannot be said to have Fallopian tubes, as the horns of the uterus extend to the ovary. Campbell has collected a number of cases which might be looked on as genuine, only that they are so old (1682, 1697, 1735, and 1767). Tait alleges there are no preparations in museums, and that no modern preparation will bear examination. The objection to the old ones is that men were in those

days so incapable of minute examination and so prejudiced by their theories that their testimony is not worth much. At present we could only accept the evidence procured by post-mortem examination, or ante-mortem section, in which the examination had been made by a skilled pathologist, and not by an ordinary practitioner who would probably see whatever his theories inclined him to see; or, if not, who would use language too loosely for our purpose. Tait has seen a total of seventy-six cases, all of which were tubal except one, which was interstitial. Tait, in his work just published, disposes satisfactorily of all alleged cases, such as that of Spiegelberg, that of Puech, and those of Hildebrandt, of Berlin ("Lectures on Ectopic Pregnancy," by Lawson Tait, 1888, pp. 8-13).

Professor Arthur Willigk (*Vierteljahrbuch, für Pract. Heilkunde*, 1859) disposes of a number of specimens in various museums which had been labeled "ovarian," including one of Kiwisch. He depends on microscopic examination. On the other side we have men equally eminent, as Coste, Hecker, Kiwisch, and recently Puech (*Annal. de Gynéc.*, July, 1878); also Duverney, Goupil, and P. U. Walter give cases which it is difficult to reject.

In an essay on diagnosis and treatment it would be out of place to enter further into the question, but I may sum up with Parry, who says the weight of authority is in favor of the *probability* of ovarian pregnancy. While denying the probability of quite recent reported cases, one described in *British Medical Journal*, Vol. 1, 1887, p. 568, is a good example, where a Dr. Cargill, of Jamaica, says he found the placenta of a five months' foetus, embedded in a ruptured cystic ovary. This would seem conclusive, did we not know that in extra-uterine cases the placenta can migrate, change its ground, spread here and there from its original attachment, or may even be detached altogether and take root again (Dr. James Braithwaite, of Leeds, *British Medical Journal*, January 3, 1885).

Professor Freund (*Edinburgh Medical Journal*, September, 1883, p. 243; November, p. 397; December, p. 521), in a record and classification of fifteen cases of extra-uterine pregnancy, recognizes the ovarian variety, and says it is characterized clinically by "little or no pain in early pregnancy; rapidly growing flat tumor at side of uterus; may go on to full time." But in his description of the appearances at operation, he seems to indicate rather the broad ligament form of Tait.

With regard to the tubal, Tait believes that every ectopic gestation is originally located in the tube. He accepts the ovario-tubal as a possible variety which must always become intra-peritoneal on primary rupture, which would always occur within the time which limits the purely tubal form, that is, thirteen to fourteen weeks. All are agreed that the great majority of cases are tubal at first. There is no purpose in making further subdivisions of the tubal cases, as it is admitted on all hands that it is impossible to diagnose one form from the other, except at section or post-mortem examination. The important point here is Tait's theory, that according to the implantation of the placenta in the tube is the direction of the rupture, which *always* takes place within fourteen weeks. That if the rupture happen in the free part of the circumference of the tube, the bleeding is into the peritoneum (intra-peritoneal hæmatocele), which is almost invariably fatal unless the abdomen be opened; but if the rupture should happen in that fourth of the circumference of the Fallopian tube which corresponds to the folds of peritoneum constituting the broad ligament, then the foetus and blood escape into the substance of the broad ligament. In the latter case the bleeding is restrained by the compressive power of the adhering folds of the ligament, so may not be fatal, and the foetus may go on developing till term (extra-peritoneal hæmatocele). All the cases in Tait's unique experience (seventy-six cases) in which gestation has gone on beyond *primary* rupture have been of this extra-peritoneal kind. This is a most important advance in the pathology of extra-uterine pregnancy, as although an extra-peritoneal form was recognized by Dezeimeris, who called it "sous-peritoneo-pelvic," yet it was looked on as rare, or denied altogether. However, Drs. D. Berry Hart and J. T. Carter, in a paper read before the Obstetrical Society of Edinburgh, in July, 1887 (*Edinburgh Med. Jour.*, October, 1887), confirm Tait's theory from frozen sections of two cases. They also confirm what he taught for years, that in these intra-ligamentous cases the peritoneum is stripped off the back of the uterus, part of the front, sides and back of the pelvis and abdominal walls round to the front on each side, as far as the horns of the uterus. So that the cyst may present close to the abdominal muscles in the abdomen, and, although the placenta may be found extensively attached, for instance, to the back of the uterus, sides of pelvis, abdominal wall, and even to the bladder and intestines, still, if *secondary* rupture of the broad

ligament has not occurred the whole fruit of the gestation may be quite extra-peritoneal, and may even be removed by laparotomy without touching the peritoneum. So as Tait says, some of his operations are true abdominal sections, some are not. Hart and Carter propose a new name for the cases in which the peritoneum is stripped off the anterior abdominal wall—"sub-peritoneo abdominal" gestation. They also deny that either ovarian or purely intra-peritoneal abdominal pregnancy has ever been accurately proved to exist. Tait does not believe in the occurrence of primary abdominal pregnancy (intra-peritoneal). He thinks the digestive power of the peritoneum would dispose of the freshly impregnated ovum which dropped into it, in the same way as it does normally with the non-impregnated ova, which he believes are always dropping in.

The interstitial variety must be extremely rare; as far as known they always rupture into the peritoneum. The only other route they could take would be into the cavity of the uterus, when the case would be indistinguishable from normal pregnancy. Such statements merit no serious attention, as it is quite absurd to talk of diagnosing an interstitial case from a normal pregnancy. Tait has never seen a preparation which could, by any possibility, have been diagnosed from normal pregnancy during life, or, at least, previous to rupture. The time of rupture varies from three to twenty weeks.

My reason for accepting Tait's opinions, as a rule, in preference to those of others is, that he gives nothing as fact which has not been verified either by ante- or post-mortem section, or museum specimens. Against opinions thus established we have those of men who argue from impressions derived from symptoms, or pelvic examination, or deductions from lists of cases unearthed here and there, imperfectly observed and recorded, or records of post-mortem examinations made by men unskilled in anatomy. The acceptance of doubtful evidence has been the principal reason of the slow advance of medicine as a science. Imperfect observation, false experience, and the records of false diagnosis, lead to all the absurd and fanciful theories which have been the shame of medicine at all times. Then, authors laboriously collect hundreds of worthless records of cases which, probably, had little in common, and draw conclusions, and lay down rates of mortality and rules of practice for this and that condition; the whole thing being far more false and useless than is the Registrar-General's

Records of the causes of death in Great Britain. Having been a public Registrar of Deaths, I can testify that in most cases the medical certificate gives only the faintest idea of the seat of disease—not taking account of the many cases where the diagnosis is entirely at fault. Yet on these figures theories are being built up! The personal experience of highly qualified and highly gifted men, who systematically record and analyze their cases, is infinitely superior, as a basis for argument, to any form of collective investigation which has hitherto appeared.

I shall say no more regarding the pathology of extra-uterine pregnancy, except incidentally as it may bear directly on diagnosis or treatment. My justification for saying so much is that it is necessary to explain my position on this question, and to explain the terms which I shall have occasion to use. Unless we have an idea of what a man believes as to the nature of a condition, or disease, we can hardly comprehend what he says as to its diagnosis and treatment. I shall now consider the subject of diagnosis under two heads, viz: before rupture and after; the latter being subdivided into several heads.

DIAGNOSIS OF EXTRA-UTERINE PREGNANCY PREVIOUS TO RUPTURE.

Is such a thing possible? Tait practically says no, or that we shall fail so often that it amounts to the same thing. But in the literature of the subject, of recent date, there are many cases recorded where the diagnosis was made and verified either by rupture occurring later on, by operation, or by post-mortem examination. I do not argue from cases where cure resulted from some mode of treatment not involving opening of the abdomen, although it would hardly seem sound judgment to reject all these.

There are two elements in the diagnosis: First, we must diagnose pregnancy, and second we must ascertain with certainty that the pregnancy is extra-uterine. We may at once grant that, practically, in the great majority of cases the thing is impossible, but principally for three reasons: First, that the patients rarely apply for advice until rupture has occurred, that is, we rarely get an opportunity; second, the menses go on, and so the theory of pregnancy is apt to be dismissed; third, the whole history is apt to be misleading. The bulk of the patients have no suspicion of anything wrong till suddenly and without warning rupture occurs.

Well, then, when opportunity for a diagnosis offers, we have first to establish the fact of pregnancy, and this we know it is impossible to do with certainty till the middle of the fourth month, or beginning of the fifth, when the foetal heart becomes audible. Depaul believes he has heard the foetal heart as early as the eleventh week, and Routh believes the heart can be heard by vaginal stethoscopy at this early period. Naegle never heard the heart before the eighteenth week, more generally at the end of the twentieth. But for practical purposes pregnancy must have reached the fifth month before we can be sure of hearing the heart. Tubal rupture, however, always occurs before the fourteenth week, usually before the twelfth, and may be as early as the seventh or even fourth week. So that, as a rule, *primary* rupture, usually meaning death or laparotomy, has occurred before we can hear the foetal heart or be perfectly certain as to whether the patient is pregnant! I need not discuss the other ordinary signs of pregnancy, but will proceed to inquire: Are there any other signs by which we might make a probable diagnosis of pregnancy in the very early stage?

Several new signs of pregnancy have been introduced in recent years, but, except the character of the sphygmographic tracings in pregnancy, which have been studied by J. W. Ballantyne, M. B., Assistant Professor in Midwifery in Edinburgh University, there are none which can assist us much, under the circumstances. The pulse during pregnancy is one of high tension, as shown by the sphygmograph. There is also an increase in the rapidity of the pulse during gestation, the pulse rate usually varying from eighty to eighty-four per minute. The high tension is due to cardiac hypertrophy, as has been shown by the observations of Ducrest, Larcher, and Blot. But, unfortunately, this high tension is best seen in the later months of pregnancy, and in primiparæ. The sign, too, is of more prognostic than diagnostic value. Still, we might obtain a hint from it in obscure cases. The researches of Ballantyne are to be found in *Brit. Med. Jour.*, Vol. 2, 1886, p. 1094. The facts have also been attested by Marey, Mahomed, Macdonald, Fancourt Barnes and Louge. The experiments of Löhlein and others show that the French authors overestimated the amount of cardiac hypertrophy in pregnancy. Then there is Hegar's sign of pregnancy, viz., a characteristic softness, pliability and thinning of the lower segment of the uterus, that is to say, of the

part immediately above the insertion of the sacro-uterine ligaments. This condition can be easily made out, not only when the uterus is of natural firmness, but also when it is flaccid and yielding. In any case, it is possible, by depressing the uterus, to distinguish the upper portion and the rigid cervix from the lower portion, the softness of which is so marked that one could imagine the cervix to be simply in contact with a pelvic or abdominal tumor. No pathological condition known, not even hydrometra or hæmatometra, is capable of producing a similar group of symptoms. But Hegar's sign is not constantly present. (*Prager Med. Wochenschrift*, No. 26, 1884). I do not know of any observations as to the presence of this symptom in extra-uterine pregnancy, but as the uterus develops to a certain extent in this condition, I should think Hegar's sign would be present to some extent. Renil says the sign is present in the third month. Then, Dr. Henry D. Fry (*American Jour. of Obstetrics*, October, 1884) says that between the fifth and the eleventh week of pregnancy he found elevation of the temperature of the vagina, along with the purple color of the mucous membrane. In thirteen cases of non-pregnant women who were examined, the average vaginal temperature was found to be 99.1° F., whilst the average vaginal temperature of the pregnant women was as high as 99.74° F. He concludes that the increased heat is due to active hyperæmia necessary for the growth and development of the ovum and of the uterus. This causes increased intra-uterine temperature, which produces increased vaginal temperature. But, then, pathological conditions of the uterus produce an equal increase of heat in the vagina. Several authorities, as Cohnstein and Fehling, have recognized the intra-uterine rise of temperature during pregnancy. They say the temperature is still higher in the impregnated uterus than in the vagina, and look on that state of things, in the absence of pathological conditions, as indicating not only pregnancy but also a living fœtus. Fry thinks it could be said with more truth that a vaginal temperature equal to or more than 99.7° F. is a strong presumption in favor of impregnation, provided there are no pathological conditions present in the uterus, and no increase of heat in the axilla.

Dr. Routh declared at a debate on the treatment of extra-uterine pregnancy, held at the Brighton Meeting of the Brit. Med. Association, 1886 (*Brit. Med. Jour.*, December 4, 1886, p. 1093), that the uterine

souffle could be heard with the vaginoscope before the third month, though it could not be heard through the abdominal walls, as he has also declared he has heard the foetal heart.

These are the only observations I have to offer as to recent advances in the diagnosis of pregnancy in general, with the exception of the discovery of Dr. Adolph Rasch (*Brit. Med Jour.*, August 30th, 1873, p. 261), that pregnancy (uterine) could be diagnosed in some cases as early as the seventh week, and in most cases after the second month, by *fluctuation* in some part or corner of the uterus, wherever the ovum happened to be located. Some other fluid retained, as menstrual, is the only possible source of error. He says he has practiced this mode of diagnosis for ten years, and is only confirmed in his opinions. By two fingers in the vagina and the other hand on the abdomen it is possible to detect the smallest amount of liquor amnii just as easily as a few drops of pus can be detected in a subcutaneous abscess. One would, of course, require to be thoroughly accustomed to the bi-manual mode of uterine examination. Dr. Rasch also mentions a symptom not noticed in books which, at least, ought to provoke further inquiry, viz—an increased desire to pass urine, especially at night. This symptom might, of course, be due to any condition causing albuminuria, of which rising to urinate is an early symptom, to diabetes, and to bladder trouble; but then most symptoms are open to many interpretations.

This fluctuation symptom would be absent in extra-uterine pregnancy, but the point of it is that it could be used as a *substitute for the uterine sound* in ascertaining that the uterus was empty, which is a most important point.

After offering these remarks on the subject of the early diagnosis of pregnancy in general, I have to admit it cannot often be done with any reasonable degree of certainty before the foetal heart becomes audible, before which period rupture usually takes place in extra-uterine pregnancy.

ON THE DIAGNOSIS OF THE EXTRA-UTERINE CHARACTER OF THE PREGNANCY FROM NORMAL GESTATION.

In these cases all the usual sympathetic disturbances of pregnancy are present. Enlargement of the breasts, darkening of the areolæ, increase of Montgomery's tubercles, morning sickness, vomiting, etc.,

are all present. The condition as to menstruation, however, is a most difficult and misleading one. The text-books, still influenced by the opinions of Antoine Petit, who wrote in 1710, usually state that there is an arrest of menstruation, but that after the absence of one or more periods there is often an irregular hemorrhagic discharge, which Barnes attributes to partial detachment of the villi of the chorion, caused by the ovum growing out of proportion to the tube in which it is contained. This is very true as regards many cases, but, unfortunately, we meet with every condition of the menses. Petit says that "the menses, contrary to what is seen in normal gestation, continue to appear, but in smaller quantities, throughout the pregnancy." This is also true of a large number of cases, but, again, not universally. Sometimes the menses are suspended altogether, as in ordinary pregnancy; more commonly they continue irregularly and in *too great quantity*. Sometimes the loss amounts to metrorrhagia, from the enlargement and emptiness of the uterus, which would cause suspicion of a uterine tumor. In fact, the state of the menses is more apt to delude than to help us. The text-books emphasize the point that the patient generally considers herself pregnant, and they reason from this, or rather insinuate, that all the medical man has to do is to locate the pregnancy. Now, this may be sometimes true, but oftener the patient has no suspicion of pregnancy. The basis for the text-book statement is the fact that when the foetus lives on after the primary rupture the patient comes to know that she is pregnant, and firmly adheres to the idea, should the child be retained for fifty years. But before the period of rupture the case is quite different. Tait, with his enormous experience, finds that the great majority of his patients in whom primary rupture occurred had no suspicion of pregnancy. Consequently, no opportunity for diagnosing extra- from intra-uterine pregnancy was afforded to him. This is the great bar to the diagnosis. Had we cases under observation in hospital, no doubt the extra-uterine character of the pregnancy would be discovered, but from the circumstances, this can seldom be attempted. There is one point that should attract attention and lead to close examination, viz—the fact that the subjects of extra-uterine pregnancy, in a great majority of cases, have either never been pregnant previously, or not for a number of years. At all events, it is the rarest exception in the world, to the general rule, for a woman to make any complaint, or to consult a

doctor, until rupture has actually taken place. Tait has once only been consulted prior to rupture. In this case it was evidently impossible for any man to diagnose the cause of the tubal swelling. He describes it in the *Brit. Gynaecological Journal*, Part xiii, p. 38. She complained of obscure pelvic pain of several months standing, the usual symptoms, in fact, of salpingitis. She had not missed one period. The idea of pregnancy never entered the mind of any medical man, or any other person. Yet, three days afterwards she presented herself with the easily diagnosed symptoms of ruptured tube and abdominal hemorrhage. The history of the case, then, previous to impregnation is usually all we have to guide us. Often one confinement, then an illness with pelvic trouble, then sterility for a long number of years with menstrual pain and distress, then the extra-uterine pregnancy. Parry says, "women who have become pregnant with a child outside the uterine cavity frequently show a previous inaptitude for conception. The interval between marriage and the first impregnation is frequently long. If the woman has borne children a period of sterility frequently precedes the extra-uterine pregnancy." This fact strongly confirms Tait's idea that extra-uterine pregnancy is caused by whatever removes the ciliated epithelium from the Fallopian tubes. The recent discovery of Bland Sutton and Arthur Johnstone further strengthen the view.

If we could arrange the circumstances we could, no doubt, often diagnose either extra- or intra-uterine pregnancy. Given a woman who had incurred the risk of impregnation, who was uneasy about the matter, and who was quite willing to have a minute pelvic examination made, we could at a very early period make a highly probable diagnosis of pregnancy in any situation, irrespective of the menses; but the fact is that in real life we rarely or never get the chance. This is proved to demonstration by an extraordinary case of Lawson Tait's, described at page 49 of his work on Ectopic Pregnancy. This woman, if any one in the world would, was just the one to be uneasy enough to have herself examined, as she had already undergone abdominal section for ruptured tube. Yet with her actual experience of the condition and the dreadful results, when the same thing happened a second time, and *knowing she was pregnant*, she never thought of being examined the second, any more than the first time. In fact, there were no symptoms in the slightest calling for any examination.

The state of the menses during extra-uterine pregnancy may lead us into yet another error. From their profuse nature, where the patient has suspected pregnancy, we may actually fancy abortion has taken place, and thus be less prepared than ever for the coming rupture. Campbell illustrates the axiom "Put not your trust in histories," as follows: "In many instances of the different varieties of misplaced gestation, the catamenia are suspended; frequently, however, they appear regularly in each of the early months; in some cases they flow at uncertain periods; and in other examples they are either profuse, or limited in quantity. In many cases, at an uncertain period of gestation we have hemorrhage, uterine effusions, the extrusion of coagula, of bodies which resemble moles, or portions of placenta. These appearances have occasionally led to the belief that the patient has actually aborted, so that the ovum was originally not extra- but intra-uterine, and had escaped through a rent in the uterus into the peritoneal cavity, the extruded body in either case being viewed as the placenta. Cases attended with much uterine excitement, whether arising from unusual exertion, or some external injury, are the most likely to be accompanied by these latter phenomena." So that in every case of fancied abortion we should be on the lookout for extra-uterine pregnancy, among many other causes of uterine colic and hemorrhage.

We have now lingered long enough on the almost negative though usually considered positive signs of extra-uterine pregnancy in the early period. Let us now look at some of the positive changes which would aid us in the diagnosis—opportunity being given.

It is now universally admitted that the uterus undergoes a certain amount of development and evolution, as in normal pregnancy. The lining mucous membrane also undergoes development into a true decidua. Indeed, it has been suggested by an authority that we should dilate the cervix and ascertain the presence or absence of the decidua as a crucial test as to the existence of extra-uterine pregnancy. The hint might possibly prove useful, as in case it turned out to be a normal pregnancy the worst that could result would be an abortion. The cervix uteri also becomes softened and shortened, just as in normal cases, but these changes do not proceed so far. One feature of the abnormal cases, however, which is worthy of note is that the cervix very soon becomes open and remains so. At about the full period of

gestation Tait found, in most of his cases, that the cervix would admit the finger although it could not be passed through the internal os. Post-mortem the decidua has often been found wanting, but the description given by Campbell of what happens at the pseudo-abortions so common during extra-uterine pregnancy explains the matter. Duguet (*Annales de Gynécologie*, May, 1874) has also pointed out that it is usual for the uterine decidua to be thrown off, either in shreds or *en masse*, during the profuse menses which so commonly occur previous to rupture of the tube. The uterus undergoes involution after the death of the ovum, if the patient survive, unless the placenta be implanted into its posterior surface, which so frequently happens. The development of the uterus and the shortening and softening of the cervix, however, only go on to a certain point. Usually the uterus does not exceed the size of that at four months of a normal gestation, and it may not exceed the size normally reached in two months. Parry especially studied this point: he says, "the uterus, except in some rare instances, undergoes striking alterations, both in its structure and volume. Its development has been found to vary from twice the size of an unimpregnated organ to the volume which it is known to attain when gestation is four months advanced." Virchow, Kiwisch, and Oldham have demonstrated that no true decidua is developed in the tube. The mucous membrane there is wanting in the utricular glands which are found in the lining of the uterus. The chorion villi are directly implanted on the mucous membrane, which they even pierce. They even go deeper, encroaching on and weakening the muscular coat, which is, at places, quite displaced. This has an important practical bearing, as the situation of the placenta determines the site of rupture, and the site of rupture decides the further course of the gestation. If the rupture occur in the floor of the tube we have an extra-peritoneal hæmatocele, an effusion of blood into the folds of the broad ligament. The sac of the ovum may not burst. The limited space, the meshes of the connective tissue, and the resistance to further distention of a distended broad ligament may prevent fatal hemorrhage. The ruptured tube, and the cavity of the broad ligament together, may provide a capsule for the ovum, where, except secondary rupture into the peritoneal cavity occur, with its almost certainly fatal hemorrhage, the ovum may develop to maturity and, if suppuration of the sac do not occur, where the child may remain quiescent for

years, or from which it may be almost completely absorbed. In the Fallopian tube the placenta is developed on a different plan from that of the normal placenta. Hennig says it is developed on the plan of the same structure in dogs, cats, and rabbits. Being thus very slightly attached the tubal placenta is easily separated, and hemorrhage easily occurs. It can also take root again on a new site if displaced. This is the explanation of many supposed "ovarian" pregnancies.

A case which occurred in the Birmingham and Midland Hospital for women, under the care of J. W. Taylor, F.R.C.S., illustrates and enforces many points in the diagnosis and treatment of extra-uterine pregnancy in the earlier stage. The patient was 38 years of age; last child born *twelve years ago*. She was seen on 4th March, 1888; menstruation had been regular until 6th December preceding. On this date she had an irregular loss which continued more or less for two months. This loss had ceased three weeks before she was seen; *no amenorrhœa at any time*. Has had pain in right abdomen with a sense of illness for four months. For some time previous to being seen this pain had been increasing; when seen it was unbearable, and prevented sleep. A month before consultation there was some swelling above; this steadily increased up to that time. So here we had a case where the pain drove the patient to the doctor in the early stage, though not before rupture. What did he make of it? He found a hard, tense, *semifluctuant* mass on the right side of the uterus, pushing the latter to the left. The upper limit was quite plain above the pelvis, and formed a crescentic outline from the crest of the ilium to the pubes. He passed a sound, found the uterus slightly enlarged, empty, and distinct from tumor. Urine yielded half its bulk of albumen. What was the diagnosis? An inflamed and probably suppurating abdominal cyst. In fact, much the same as Lawson Tait made on the only occasion on which he was asked to examine an extra-uterine gestation previous to rupture. In the case before us also, as in Tait's, the fact that there had been no amenorrhœa was held sufficient to *exclude pregnancy*. Now, surely this would not be good practice, suppose it was normal pregnancy only we suspected. But with the well-known uncertainty as to the state of the menses in extra-uterine gestation, what shall we say of this summary exclusion? Would it not be nearer the real truth to guess that the possibility of extra-uterine gestation never crossed the mind of either examiner?

Well, abdominal section was done on March 7th. A blackish cystic mass was seen on the right side, very adherent to the intestine and omentum. The tumor was separated somewhat from its adhesions and at a fluctuant part a small *trocar* was inserted—for what earthly reason is not stated. Pure blood flowed freely. The tumor was then quickly separated, when at last the secret came out as a large foetus of about four months, with its placenta. The latter was mainly attached to the hypertrophied and expanded interior of the Fallopian tube, which had apparently ruptured much earlier. The tumor was then removed and the remains of the tube ligatured, a piece of omentum to which the placenta adhered was removed, the peritoneum washed out, a drainage tube inserted, and the patient was discharged on March 28th with only a trace of albumen in the urine.¹ This case affords obvious lessons in diagnosis and treatment, besides showing that in pregnancy, at least, severe albuminuria is no bar to operation. But the object of introducing the case here is to show that, supposing the hemorrhage following primary rupture should not be severe, and especially if, as in this case, the rupture occur into the broad ligament, we may not only have no symptoms calling for examination previous to rupture, but rupture itself may fail to attract the patient's attention enough to cause her to seek advice. In this case it was only the pressure symptoms of a rapidly growing mass which caused the patient to consult a medical man. The lessons of the case as to treatment are not in order here.

Now let us contrast what happens in real life with the statements of those gentlemen who consider the diagnosis of extra-uterine pregnancy previous to rupture perfectly easy. Dr. James Aveling, in a paper read at the Gynæcological Society on February 8th, 1888 (*Brit. Med. Jour.*, March 10th, 1888, p. 535), says truly enough that, as a rule, erratic pregnancy is found to occur during a prolonged sterile period following a first confinement; that the history is of the greatest importance and should never be overlooked; *that metrorrhagia is important when occurring with the usual sympathetic signs of pregnancy*. But in face of the instances I have alluded to, what shall we say of the statement that the patient usually believes herself to be pregnant, and that there is something unusual about her condition. Why, if this were so it would suggest the diagnosis to the most care-

¹ *Brit. Med. Jour.*, Dec. 1st, 1888, p. 1217.

less. But the statement seems the direct opposite of what usually obtains. Is this not one of Antoine Petit's ideas, which has survived since 1710, of which Parry says, "Could they be verified the detection of extra-uterine gestation would be an easy task; but unfortunately for the comfort of the obstetric surgeon scarcely one of them contains a grain of truth; yet strange to say the opinions of Petit influenced and impeded the progress of our knowledge on this subject for more than half a century." Tait says Parry might have said for a century and three quarters. In addition to the history, the patient's feelings and notions, all most dangerous as guides, of course, the results of pelvic examination are made the basis of diagnosis. What we want above all things is to have the possibility of extra-uterine pregnancy present to the mind. Without this, the most experienced will almost always go wrong. If we really have the possibility before us, and depend almost entirely on the pelvic examination along with the patient's history previous to impregnation, the probabilities are that we would go right in the very rare opportunities presented. It is the very rarity of the discovery which makes men miss it when the chance presents. Repeated examination at intervals of a week or two should certainly lead to discovery of the truth, at all events. A rounded, elastic, semifluctuant, tender tumor behind and to one side of a slightly enlarged and laterally displaced uterus, if found to be rapidly increasing under circumstances which permit the possibility of extra-uterine pregnancy, could hardly be mistaken for anything else. Dr. Barnes maintains (*Brit. Med. Jour.*, March 10th, 1888, p. 536), that the diagnosis of tubal gestation might be made with reasonable certainty, even at the seventh or eighth week. The ordinary signs of pregnancy being present, there are two signs which call for special attention. These are first, pain more or less acute in the pelvic region, of a spasmodic character; secondly, more or less hemorrhagic discharge from the uterus and vagina. *These are signs of abortion*—it might be intra-uterine—but they call for special pelvic examination. The uterus would be found not, as normally, in the middle line, but pushed across the pelvic brim to one side; on the opposite side would be found an extra-uterine swelling of an ovoid shape. The size of the uterus itself might or might not, most likely would not, equal that of the supposed period of gestation. If all these conditions had developed in a short period in a person hitherto free from pelvic distress, the case for extra-uterine

gestation would amount to more than suspicion. If in a week or two the extra-uterine swelling had increased considerably, and shreds of decidua had come away with the metrorrhagia, the case for tubal gestation would be all but complete. The process of abortion would be fairly commenced. If the abortion were uterine we could well wait, interfering only when necessary. But abortion in a tubal case means bursting of the sac, with commonly a fatal result. Dr. Barnes' views may be at once admitted to be in accordance with truth, experience and common sense, but unfortunately the conditions he lays down are rarely fulfilled, which means that the number of times we shall succeed in the diagnosis will be small compared with the failures. By the ordinary signs of pregnancy, I believe he means the sympathetic signs, as enlargement of the breasts, etc., and not the patient's idea, or the history as to menstruation, which, on the whole, usually lead us astray. Again, too often his special signals for pelvic examination, viz., spasmodic pain, and metrorrhagia, are either absent or so slight as not to cause complaint on the part of the woman. However, if we could fix in our minds the idea of Dr. Barnes, that abortion may be extra- as well as intra-uterine, it would perhaps occasionally save us from error. Dr. Routh (*Brit. Med. Jour.*, March 10th, 1888, p. 536) depends on hearing the uterine souffle by means of the vaginoscope as early as the sixth week. We know that a similar sound is heard in other cases, fibroid tumor for example, but Dr. Routh very ingeniously demonstrates how we can distinguish to which condition it is due. He argues, perfectly justly, that a fibroid tumor giving rise to a souffle must be much larger than a three months' pregnancy. Besides, there are other marks of distinction. Uterine tumors, while small, and so liable to be confounded with extra-uterine gestation, are always hard and solid. Their rate of growth is much slower and the history is usually a somewhat lengthened period of metrorrhagia. But if the surgeon resorted to bi-manual pelvic examination with the possibility of extra-uterine gestation and of fibroid of the uterus clearly before his mind, I do not think he could possibly make a mistake—especially if he used the sound. With regard to the use of the sound in the diagnosis of extra-uterine pregnancy, I would not hesitate to use it if really necessary, but I think the bi-manual method of examination has very nearly removed the necessity. With two fingers in the vagina, and the other hand in the pelvis from above, we can

usually ascertain all we want to know about the size, shape, and condition as to emptiness, of the uterus. Then we can employ Rasch's sign (*Brit. Med. Jour.*, vol. ii, 1873, p. 261), viz.,—fluctuation in some corner or part of the uterus, to detect the presence of normal pregnancy as early, sometimes, as the seventh week, and in most cases after the second month. For these reasons I think we should seldom feel called upon to employ the uterine sound in cases where the chances, perhaps, are in favor of the pregnancy being intra-uterine after all. There is generally supposed to be no danger in employing the sound, save that of causing abortion in case the pregnancy prove intra-uterine, but this is not altogether correct. We should remember that the great German obstetrician, Braun, in using the sound for the very purpose under consideration, passed it through the fundus without encountering any resistance to warn him, until the point could be felt quite plainly underneath the skin of the abdomen. That he really did so was verified by post-mortem examination, as we would naturally expect. Then the use of the sound has in too many cases been followed by pelvic cellulitis and septic inflammation, which are both dangerous to life and the cause of great suffering to the patient. What can we ascertain by the sound to compensate for its many dangers? Simply that the uterus is empty, its size, and the fact that it is distinct from the tumor. Now I maintain that all these points can be decided by a thorough bi-manual pelvic examination, in most cases. Possibly in diagnosing retroversion or retroflexion of the gravid uterus from extra-uterine gestation, the sound might be essential; but rarely if the examiner thoroughly understand the principles of bi-manual examination.

To return to the main question, W. T. Lusk, M. D., New York, read a paper on abdominal pregnancy and the desirability of early laparotomy, at the annual meeting of the British Medical Association, at Brighton, 1886, with tables and an analysis of 103 cases (*Brit. Med. Jour.*, December 4th, 1886, p. 1083). He gives the clinical history of a case on which he founds his opinions. This woman was under professional observation of her family doctor and another, all through her pregnancy; through the severe sufferings of the primary rupture and the sufferings after this, from the extra-peritoneal hæmatocele and growing foetus the result of rupture into the broad ligament. She was under observation even through the false labor, at term, when Dr. Mead satisfied himself that the uterus was empty, as he could so

easily do. After all this, Dr. Mead took her to a very eminent gynæcologist in America, who not only went wrong in the diagnosis, but expressed himself with great positiveness as to the absence of extra-uterine pregnancy, and stated that, without doubt, the tumor was a cysto-fibroma, situated in the posterior uterine wall. It seems a fairly typical case. Barnes' false abortion was markedly present, so far, at least, as pain was concerned. False labor at term was quite characteristic, yet, with a history which one would think, on reading it, could belong to hardly anything else, we find the case never diagnosed till long after foetal death at term. I think such a case as this should prove discouraging to those who tell us that extra-uterine gestation is not hard to diagnose. Dr. Aveling tells us extra-uterine cases are more easily diagnosed than cases of normal pregnancy, and that at the early period.

We are on the horns of a dilemma ; either we must consider the medical attendants in Lusk's case fools, or the statements on the other side false. The only way out of the difficulty seems to be, the conclusion that where everything conspires to aid us the diagnosis is extremely easy, but that practically, so many essentials are, as a rule, absent, that very few cases are really diagnosed till an advanced period. I do not question the diagnosis in the case of Dr. Aveling. This seems to have been one of the fortunate cases where it was quite plain. But he seems to think that is the rule, else his rules of treatment by electric current prior to rupture are unmeaning. For, as Tait says, it would be rather hazardous practice to apply the electric current to every kind of pelvic lump, on the suspicion that it might be an extra-uterine gestation sac.

Dr. Aveling says (*Brit. Med. Jour.*, December 4th, 1886, p. 1091) that the diagnosis is not so difficult as some writers would have us suppose. It has so many prominent and characteristic symptoms *that its detection is more easy than that of ordinary pregnancy in the early months*. He says his patient became pregnant soon after marriage, and then remained sterile for four years. After being quite regular, she ceased to be so, and symptoms of pregnancy appeared. Then she was seized at intervals with the severe characteristic colicky pains in the hypogastrium, and a continuous sanguineous discharge from the uterus. By *repeated* examinations a *pulsating* tumor, gradually increasing in size, was discovered behind and to the left of the uterus,

and this organ became, by degrees, evoluted, until it was at least four times its natural dimensions. Lastly, decidua was expelled from the uterus, making the diagnosis, as he says, of the left Fallopian gestation almost a certainty. This may be looked on as a typical case of the easy kind. It is hard to see how any man would either dispute the diagnosis, or, indeed, come to any other. But they are not all of that sort. In the very next case, described at the same meeting of the British Medical Association (*Brit. Med. Jour.*, December 4th, 1886, p. 1092) by R. Petch, M. D., London, the medical attendant missed the diagnosis, *although it is easier than in normal pregnancy*—mised, again, by the history, as the woman was suckling the previous child and the menses had not returned. Here there was neither a prolonged sterility nor any change whatever in the menses to suggest pregnancy, and accordingly we find it never suggested itself to the doctor. Her health had been good until two years before, when her only child was born; she suckled it for fifteen months, and became very weak and anæmic. The first return of the menses was in February, 1878, when she began to suffer from slight metrorrhagia at irregular intervals; she became very constipated and had pain after movement of the bowels; the characteristic pressure on the rectum described by Tait as “annular stricture,” which he looks on as a proof of effusion of blood into the *left* broad ligament. She gradually got weaker and suffered from occasional attacks of “obstruction of the bowels,” the pain becoming so severe and constant that she had to spend most of her time on the sofa. In April she first noticed a swelling in the lower part of the body, on the *right*; it was as large as an orange and tender. About the middle of June *she was aspirated by* her doctor for *supposed hæmatocele*, several ounces of blood being withdrawn, with some relief to pain. On July 8th there was found between the umbilicus and Poupart’s ligament, on the right, an oval tumor, well-defined, hard, and somewhat irregular on its surface, not movable, tender. No foetal heart sounds or movements could be detected, but at a point midway between the umbilicus and the anterior superior spinous process, a bruit resembling the uterine souffle was distinctly heard. On pelvic examination the uterus was found pressed to the left and forward, with the os low down and so patent as to admit the finger for $1\frac{1}{2}$ inches. A bougie passed $4\frac{1}{2}$ inches; there was slight muco-sanguinolent discharge. On July 12th the foetal heart sounds were very distinctly heard; a few

days after foetal movements were detected, and were recognized also by the patient. On August 20th treatment by galvano-puncture was commenced. Now, I hold that this case is just as typical of the difficult class of cases as that of Dr. Aveling is of the easy class. As the sac was on the right side the bowel obstruction must have been due to some other cause, probably, than effusion of blood into the left broad ligament, as insisted on by Tait. However, it is not at all mysterious that a child in the pelvis should obstruct the rectum, without regard to position. Now, Dr. Aveling was present at the discussion which followed on these papers, yet we do not find him expressing any surprise that the early diagnosis of this case was missed—nor offering the slightest explanation, or pointing out how it could “easily” have been done. Here we have a case under medical care and unlimited opportunities for examination, go on through the primary rupture and up to the sixth month without being diagnosed, or, at least, not fully diagnosed. The diagnosis of hæmatocele was right enough as far as it went. She was certainly suffering from extra-peritoneal hæmatocele, the result of tubal rupture, but the fact that there was also a living, developing foetus in the broad ligament was quite overlooked.

Dr. Routh, at the same debate (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1094), said that four signs made the diagnosis easy in the first class of cases, viz., up to the third month: (1) the general signs of pregnancy, (2) the shifting of the uterus to one side by a growing tumor on the other, (3) stoppage of the catamenia, and, often, subsequent delivery of either the whole or part of a decidua, and (4) detection of uterine souffle with vaginoscope, and not through abdominal walls. Yet he had just heard the cases of Dr. Lusk and Dr. Petch related, where the medical attendants failed to diagnose such cases till a late period. Dr. Hanks, New York, said at the same debate (*ibid.*) that tubal pregnancy ought to be diagnosed and could be diagnosed. A hæmatocele (simple?) was accompanied by excessive pain, and attended with general signs of loss of blood, but with no evidence of pregnancy. He related a case where the diagnosis was made apparently by the discharge of a bit of decidua which a microscopist after examination declared not to be decidua. The post-mortem examination confirmed the diagnosis. Mr. Lawson Tait said that he, with the largest experience yet on record, did not believe that even were opportunities given,

a diagnosis could be made before rupture with any degree of certainty. In fact there were no symptoms in extra-uterine pregnancy before the rupture had taken place. He also said he did not know of any one who had ever asserted that he had made a diagnosis of tubal pregnancy before rupture had taken place. He had never been called in to such a case certainly. Dr. H. A. Kelly, Philadelphia, immediately replied that he had done so and confirmed the diagnosis by operation, he did not say laparotomy (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1094). Dr. Kelly also said the diagnosis rested upon the rapid enlargement of the sac, pain, expulsion of decidua, and *subsequent contraction of the sac*. The last sign seems a strange one in the ante-rupture period. Surely he referred to the contraction of the sac which occurs subsequently to foetal death and absorption of the liquor amnii!

CONCLUSIONS AS TO THE DIAGNOSIS OF EXTRA-UTERINE PREGNANCY UP TO THE PERIOD OF PRIMARY RUPTURE (DURING FIRST THREE MONTHS).

To sum up the whole discussion, the symptoms are far from being definite and distinct. At the outset the sympathetic signs of pregnancy and the changes which occur in the uterus would tend to the idea of normal gestation. The woman may enjoy perfect health, or at least, better than she had in previous pregnancies. No reliance whatever can be placed on the cessation of the menses, as the discharge persists in about an equal number of cases, and in another class of cases irregular metrorrhagia is so common as to be actually looked on as of value in the diagnosis of extra-uterine gestation if associated with the sympathetic symptoms of early pregnancy. Very commonly abdominal pain is complained of from an early period. This may be intermitting but is more often constant and fixed in a certain part of the abdomen. But then it is often quite absent till rupture occurs. In many cases true decidua is discharged along with blood, and when present is of great diagnostic value. As the tumor grows, pressure symptoms may attract attention, as difficulty in defæcation and micturition, but these symptoms merely indicate pelvic trouble of some sort. Practically, the fact seems to be that in the vast majority of cases no opportunity for diagnosis is given. The women either have no symptoms, or their symptoms are not such as to lead them to seek advice. But certainly the diagnosis can be made and has been made.

In fact, in some few cases where the history does not, as it usually does, mislead, and where a number of symptoms are present, the diagnosis seems very easy. Still, in a far larger number it would be most difficult, whatever opportunities were given. Tait is, doubtless, too confident in his own unique experience, and too skeptical as to the assertions of others. We must remember that after all he has only seen seventy-nine cases (published up to date of his new book, December 1st, 1888) and this number is far too small to permit of any dogmatism as to what can and what cannot happen in other cases. There can be no doubt, however, that he is right as refers to the overwhelming majority of cases, and we may safely accept his last opinion on the matter, viz., "one might guess, but it is impossible to affirm; for one case we could diagnose, fifty would escape recognition." (*Brit. Med. Jour.*, March 10th, 1888, p. 536.)

Although I have a great respect for every opinion of Mr. Tait upon a subject he has made peculiarly his own, yet I do not slavishly follow whatever he may say unless it is well supported either by himself or others. But in his opinion on the practical impossibility of making an absolute diagnosis of extra-uterine pregnancy prior to rupture Mr. Tait does not stand alone. At the debate on the subject at the British Gynecological Society, Feb. 8th, 1888 (*Brit. Med. Jour.*, March 10th, 1888, p. 536), Dr. Japp Sinclair said that in three cases with the details of which he was familiar, there *were absolutely no symptoms* before rupture; and in two of the cases even after rupture the symptoms were most misleading. Again, he had met with many cases which simulated tubal pregnancy. At the same place Dr. Rutherford said that before the third month they could not be sure they had to deal with a case of extra-uterine foetation. It seems to me rather suspicious that those most famed for abdominal operations should, as a rule, express most doubt about the diagnosis of extra-uterine gestation prior to rupture, while those who go in for electricity, puncture, injections of morphine, etc., should profess to make the diagnosis offhand. Contrast this with the opinions as to diagnosis and treatment just after primary rupture. Here those who insist on abdominal section say the diagnosis is usually quite easy. Tait says we can make it seven times out of eight and guess at it the eighth time, while the stock argument of those who are against operation, the excuse under which they shelter themselves, is the impossi-

bility of anything like certainty in diagnosis. They feel if they admitted the diagnosis after rupture to be easy, they could then only plead want of skill in abdominal operations. Let us hear G. Ernest Herman, M.B., London, obstetric Physician to London Hospital, an obstetrician of skill and fame; a man who has done all abdominal operations with great success, yet a man who has not rendered himself prominent on either side of the question, who has neither denounced one particular mode of treatment nor unduly lauded another. In a very able lecture on the treatment of early extra-uterine gestation read before the Hunterian Society, London (*Lancet*, May 26th, 1888, p. 1021) he says: "There are a great number of published cases in which the diagnosis of extra-uterine gestation has been made, galvanism has been used, and the tumors have shrunk away. But the great mass of literature of this kind is far from convincing. The papers of Thomas (*Amer. Gyn. Trans.*, vol. vii, p. 219) and Garrigues (*ibid.*, p. 184) contain many cases in which the diagnosis rests upon such *slender grounds that it is impossible not to suspect error*. The strength of a chain is in its weakest link, and when one finds *seriously* adduced as *scientific evidence* cases of an *extremely doubtful* kind, without apparently a *sufficient sense of the possibility* of mistake, one is apt to judge of the strength of the whole chain of evidence by these *weak links*. There are cases, however, such as that of Dr. Petch (*Brit. Med. Jour.*, 1886, vol. ii, p. 1092), in which the foetal heart was heard, and after electricity was used the cardiac sounds ceased. This case, so far as I know, *is unique*. But there are some others in which the diagnosis seems *highly probable*, and the tumors went away I think we want a larger number of cases diagnosed with much greater accuracy than the great bulk of those hitherto published, before it ought to be accepted as a fact that we can by a galvanic current kill an extra-uterine foetus, and the evidence is not all on one side." He then cites the famous cases of Matthews Duncan and of Braxton Hicks where electricity failed. He then adds: "Now, in my judgment, a single well-observed case of this kind, in which the diagnosis and the effect, or rather non-effect, of the treatment were beyond all doubt, outweighs a great number in which the diagnosis was uncertain, and the possibility of natural cure by rupture or death of the foetus overlooked." Remember, this is the judgment of a man who does not deny the possibility of the diagnosis. Indeed, in this very lecture

(*Lancet*, June 2d, 1888, p. 1069), he gives minute and accurate details of three cases at the London Hospital, where he opened the abdomen. Two of these cases were advanced far past the rupture period, and he honestly confesses he failed to diagnose either. The third was seen *prior to rupture*, and this he diagnosed correctly, as was proved by the ruptured tube and large hemorrhage, etc. To show his fairness, because he did not find the foetus in this case which is not uncommon, he does not pretend to be sure of the nature of this case even yet (it was a two months' case), but remarks (p. 1070): "But if the diagnosis be assumed to be erroneous, the case is yet more interesting as one of unilateral dilatation of the Fallopian tube with *hæmatocele* and *subjective symptoms of pregnancy*. The whole tone of his lecture is that the diagnosis of extra-uterine pregnancy prior to rupture is difficult, and even impossible, from one circumstance or another, in the great majority of cases, yet easy enough in some. I believe the great stumbling-block in the way of a more frequent diagnosis is that we do not keep the possibility of the case being an extra-uterine gestation before our minds. Even famous gynaecologists and abdominal surgeons are too apt to have their minds fixed on ovarian cysts, the various forms of salpingitis, simple hæmatocele, and uterine myoma, with the result that when they turn out a foetus it comes on them as a complete surprise. Some of them have confessed, indeed, that because there was no disturbance of menstruation, or because the patient was suckling, the idea of pregnancy never entered the patient's head, and consequently never entered theirs. Diagnosis by exclusion is the most certain and the most scientific plan known, but how shall we exclude a condition that we have never considered the possibility of? In diagnosis by exclusion we lay down a mental plan of all the diseases we consider the case before us may possibly be. It follows, that if we leave one out we leave ourselves open to an egregious blunder so far as that particular disease is concerned. That the difficulties are often very great, the most casual glance through recent medical literature proves. For instance, Wassilieff (*Centralbl. für Gynäkologie*, Dec. 26th, 1885) details the case of a woman, aged thirty, married for sixteen years, healthy, last menses on Sept. 25th. Two months later she came to the hospital suffering from great salivation, pain in abdomen, and anæmia. The case was diagnosed as parametritis and toxic gastro-enteritis (on account of the salivation). She died on Jan. 3d. On post-

mortem examination peritonitis of lower abdomen was found, and in Douglas' pouch a foetus of four months. The umbilical cord was twenty centimètres long, placenta large. The foetal cyst was situated in the left broad ligament in the angle between the tube and the womb: the latter was not enlarged. One would be inclined to say that here is a case where the possibility of extra-uterine pregnancy never flashed across the examiner's mind at all, or he might at least have got nearer to the true diagnosis. With pain in the abdomen, a cyst large enough to contain a four months' child, and its placenta in the pouch of Douglas, and salivation, one would think the attendant might have made a better guess than "parametritis," if the bare possibility of hæmatocele or extra-uterine gestation had ever passed before his mind. Again, in the *Boston Medical and Surgical Journal*, July 15th, 1886, we have a case by Dr. E. A. Kemp, where the patient, aged twenty-six, had been married three months; healthy and strong. On the evening of March 25th, she became faint and had to be helped to bed; complained of pain in the stomach with symptoms of indigestion. Next day patient still had the pain and was very pale. Had menstruated regularly, last time only over two days. Did not seem to rally, and Dr. Frost saw her in consultation; died at 2 A.M. next day. At the post-mortem examination the surface was unusually blanched, and the abdomen full of blood. Both iliac fossæ were full of clots, with a tubal pregnancy of the sixth week on one side. Dr. Kemp publishes the case on account of the absence of the signs of pregnancy and the perfect regularity of menstruation, which he considers remarkable. Had he been a student of Lawson Tait, he would not have been at all surprised at these matters. In fact, it is hard to say how he missed the diagnosis of at least "abdominal collapse," as Barnes calls it, and hemorrhage into the abdomen. I think Tait would have had little difficulty in complete diagnosis here, although he is so modest and distrustful of his powers previous to rupture. This brings me to remark that those who diagnose, or fancy they diagnose, early extra-uterine pregnancies depend on two things principally, viz., the presence of all the most important signs of ordinary pregnancy and the interruption of menstruation, if only for a period or two, of course with the presence of an extra-uterine tumor. Now, the ordinary signs of pregnancy and the menstrual suspensions may both be absent, in which case they are apt to fail

most disgracefully. So, it seems to be that those who know most of the natural history of the affection are most cautious and those who know least the most confident. I suppose this holds in all branches of knowledge. That difficult cases can be and are diagnosed is proved by the following case (*Centralblatt für Gynäkologie*, April 14th, 1888):—

On Friday, February 17th, Dr. Zucker, of Berlin, saw a woman, aged 32. As she did not speak German, no history could be obtained. She had borne children and missed at least one period. Abdominal pain set in severely on February 12th, but disappeared in two hours when purged. She went to work again, but pain again seized her on the 17th (morning). The uterus was found pushed to the *right*, and an obscurely circumscribed soft tumor could be detected to the *left* of the cervix. There was much tenderness. "Abdominal collapse" set in and she was sent into Dr. Veit's wards. At 2 P. M., on February 18th, he operated. Even under chloroform bi-manual examination failed to detect any distinct tumor. The patient was pulseless on operation, and when the abdomen was opened nearly four litres of fluid blood escaped. The *left* appendages were found quite healthy. The hemorrhage was at length found to proceed from a rent two inches long near the centre of the *right* tube. A minute ovum was discovered, which makes the nature of the case certain. The tube was ligatured on each side of the wound and the pedicle tied, but it was not said whether the appendages were removed. A litre of blood was left in the pelvis, Dr. Veit hoping it would be absorbed. A compress was applied to the abdomen and the patient rallied a little, but soon died. Here the diagnosis of abdominal collapse and hemorrhage was plain enough, and as the operator was well acquainted with the subject, and knew the most common cause of intra-peritoneal hemorrhage, he referred it to its proper cause, although the hæmatocele proved delusive as to the seat of the pregnancy. This case shows on what different principles diagnosis after rupture is made from that previous to rupture, and how much more certain it is. But I introduced this case for another purpose. Dr. Veit takes occasion to say in relating the above case that he has operated in ten cases of tubal pregnancy. Seven of these were operated on *previous to rupture*; all these recovered. The other three were operated on after rupture, and two of these died.

Now there is nothing surprising about the deaths, if the cases at all resembled the one he describes. If we wait till the patient is dying, the chances of recovery are small. But what I want to point out is that here we have seven cases diagnosed previous to rupture, and the correctness of the diagnosis proved by operation. Again, F. Westermarck (*Nordiskt medicinskt Arkiv*, Band xix, No. 23) states that besides Veit's cases, Gottschalk has operated twice previous to rupture, both successfully. Westermarck advocates laparotomy prior to rupture, but remarks that the chance is seldom afforded, for women do not apply for aid, as a rule, until serious symptoms arise, that is, when rupture has already taken place. These cases, however, prove that the diagnosis can often be made, provided the opportunity be given. Another case from modern literature, proving the possibility of correct diagnosis before rupture, and I have done. In the *Meditz. Obozr.*, Fasc. vii, 1885, p. 689, Dr. Maria O. Prujanskaia, of Moscow, gives the case of a woman, aged 29, who had been twice confined at period, last time six years since. Aborted in August, 1884, and again ceased to menstruate in November, when mammary symptoms, nausea and vomiting set in. At the beginning of December, when stooping to tie her boots, severe pain in the abdomen suddenly appeared; this was soon followed by giddiness, dyspnœa, fainting, nausea, and bloody discharge from vagina. These symptoms recurred at the middle of December and at the beginning and middle of January, 1885. The author was sent for at the beginning of the fourth attack; found the uterus slightly patulous, enlarged and softened. The fundus was a little pushed over to the right by a firm, elastic, indistinctly fluctuating tumor in the left side of the pelvis. The tumor could be felt through the roof of the vagina, left side. A number of the usual symptoms of pregnancy being present, a three months' extra-uterine gestation was diagnosed. The author proposed to kill the fœtus by injections of morphine or electricity. These being refused, the case went on until by the end of January fœtal movements were distinct. The further history does not concern us here; suffice it to say that finally the vaginal operation (elytrotomy) by Paquelin's thermo-cautery was done, with death the night after. Fœtus alive, 4½ months. Here, again, the diagnosis seems to have been made by the ordinary symptoms of pregnancy having occurred, the presence of a pelvic tumor and pain pointing

to its extra-uterine character. The principal advance in our knowledge of recent years in this department, viz, diagnosis of extra-uterine pregnancy before rupture, seems to be that we know now that there may be no symptoms whatever, no thought of pregnancy in the patient's mind, no arrest of menstruation, no pain, and if sympathetic symptoms of pregnancy are always present, they may be so slight as not to attract the patient's attention. Then there may be all kinds of menstrual disturbances—metrorrhagia, extrusion of decidua, and “false abortion.” Probably a case presenting strong signs of pregnancy, with pain in the abdomen and an elastic, semi-fluctuating tumor at one side of the uterus, pushing it out of the middle line, with vaginal hemorrhage and passing of membrane, would have been diagnosed as an extra-uterine pregnancy many years ago. What we have to recognize and have always present to our minds is the possibility of extra-uterine gestation whenever we encounter a pelvic tumor in a woman who is within the fruitful age. If we keep the possibility before our minds, and remember that there may be no other symptom but tumor, we shall probably diagnose more cases in the future than has been the case in the past; at all events, our present knowledge should prevent the somewhat shameful missing of cases which ought to have been recognized, some of which I have instanced.

THE MISTAKE OF TAKING A NORMAL PREGNANCY FOR AN EXTRA-UTERINE GESTATION.

Extra-uterine pregnancy has on the other hand been diagnosed when the foetus was intra-uterine. Playfair quotes a curious example from Joulin, in which Huguier and seven of the most skilled obstetricians in Paris agreed on the diagnosis of extra-uterine pregnancy, and recommended an operation, when the case terminated by abortion and proved to be perfectly normal. No explanation is given, but probably it was one of those rare and curious cases described by Tait (“Lectures on Ectopic Pregnancy,” 1888, p. 62–3), who says that no other author except Parry has mentioned at all, viz., thinning of the uterine and abdominal walls, with the liquor amnii in abnormally small quantity, so that the foetus is felt so very plainly that it is impossible to believe it is not free in the abdomen. As this curious state more directly concerns the diagnosis of extra-uterine pregnancy at the full time and afterward, I shall say no more about it here. Although I have

advocated, principally, a dependence on the results of bi-manual examination of the pelvis, still, I would point out every peri-uterine tumor, rounded or oval in outline and producing more or less displacement of the uterus forward and to one side (the opposite to the tumor), is not to be considered an extra-uterine pregnancy. There are various conditions which give rise to very similar swellings, such as small ovarian or fibroid growths and extra-peritoneal, or ordinary simple hæmatocele, pelvic abscess, etc. The differential diagnosis of these we shall consider separately. But that the danger of mistaking a normal pregnancy for an extra-uterine one is real may be proved by a case contributed by Dr. Blackwood to the *Philadelphia Medical Bulletin*, and quoted in *Brit. Med. Journ.*, 1886, vol. ii, p. 34.

The patient, the mother of two children, had menorrhagia in December, 1883, which was treated by faradism of the uterus. She menstruated normally early in January, 1884. Had connection on January 8th, only time for a month past. Intra-uterine treatment resumed on January 15th. Had severe abdominal pain in February. Fallopian tubes both discovered dilated; intra-uterine treatment then abandoned. Ten applications made to interior of uterus since menses, —uterus empty. Signs of pregnancy showed, but foetus could not be found. By middle of March a round tumor the size of a walnut above right groin; mobile, not tender, and diagnosed to be a tubal pregnancy. Electricity employed up to 59 volts, to kill foetus. In a few days the mass disappeared and patient became weak and prostrate. A fortnight later *tumor reappeared but more central in situation*. Sound passed again; uterus *seemed* empty and could be drawn away from tumor. By the end of April, the *foetus which had been gradually coming nearer uterus entered it*, and internal os had closed. Normal delivery in the following December, six weeks beyond term—no complications. Dr. Blackwood thinks the electricity accounts for the six weeks the child was behind time, but does not attempt to explain the other curious phenomena. I think this case is an excellent example of a normal pregnancy mistaken for an extra-uterine one on account of some little swelling in the groin. To seriously bring the case forward as scientific evidence of *anything*, denotes a strong belief in the credulity of the profession. It teaches some lessons quite unintended by the author. It shows how hard it frequently is to produce abortion in a healthy woman, and it shows the amount of perfectly useless

and uncalled for local treatment women who can pay are subjected to in the present day. It also shows that electricity in the neighborhood of the uterus is not very dangerous to the life of the foetus. In fact, the case reminds one of the reasons given by Dr. Steavenson for questioning the opinion that shocks from faradic currents would produce the death of the foetus in early pregnancy. "The inhibitory apparatus of the heart was not developed in a foetus three or four months old; and the *tenacity* of life in a foetus was comparable to that which characterized the lower animals, such as tadpoles; and any current that was capable of destroying the life of the foetus was much more likely to destroy the life of the mother" (*Brit. Med. Jour.*, December 4th, 1886, p. 1094). Even did we suspect some pelvic swelling to be an extra-uterine gestation sac, it hardly seems necessary to run the risk which Dr. Blackwood did by passing the sound periodically to see whether the uterus was empty. Rasch's method of detecting early uterine pregnancy by the detection of fluctuation as soon as a little liquor amnii is formed, which he says is available sometimes at the seventh week and always at the second month, ought to satisfy us on that point. (*Brit. Med. Jour.*, 1873, vol. ii, p. 261.)

IS THE DIAGNOSIS OF THE VARIETY OF EXTRA-UTERINE PREGNANCY POSSIBLE BEFORE RUPTURE?

I think all are agreed that it requires a careful post-mortem or ante-mortem section to determine what variety of extra-uterine gestation is present.

"We are not warranted, except in rare instances, in asserting that the ovum is developed in any particular portion of the genital canal, unless we have the opportunity of making a post-mortem examination."—J. S. Parry. Tait admits that the pavilion of the Fallopian tube might become the seat of a gestation; he accepts the tubo-ovarian as a possibility. Tubo-ovarian cysts are common enough, but Tait has never seen anything like a tubo-ovarian pregnancy. Did such occur the only point about it would be that it must of necessity always end by rupture into the peritoneal cavity, and that, probably, within the period which strictly limits the pure tubal variety, viz., thirteen or fourteen weeks. According to the new pathology the bursting of the tubo-ovarian variety would, almost without exception, involve death by sudden or slow hemorrhage and

shock, unless saved by abdominal section. Tait has seen twenty-six post-mortem examinations for ruptured extra-uterine gestations; has operated on forty-three, and has seen about ten such operations by other surgeons, making in all the unexampled experience of seventy-nine cases. In every one of these the seat of gestation was without doubt the Fallopian tube, and only one was in the part of the tube embraced by the uterine wall—the interstitial variety. The interstitial kind he argues must thus be very rare. All other kinds he has yet to see, except gestations (secondary) in the broad ligament as the result of rupture of the tube in the fourth of its circumference corresponding with the two folds of the broad ligament. Of course he has seen the ovum in the cavity of the peritoneum as the result of tubal rupture. The interstitial variety agrees with the tubo-ovarian in *always* bursting into the cavity of the peritoneum. Such cases have been stated to sometimes burst into the cavity of the uterus. But Tait has *never seen* a preparation of interstitial pregnancy which *could by any possibility* have been diagnosed from normal pregnancy before the period of rupture. He says it is much easier to believe that such statements are due to errors of diagnosis than that the tissue of the uterus is ruptured and that the pregnancy becomes intra-uterine. The period of rupture in the interstitial variety seems to vary from three to twenty weeks, a fact he derives from post-mortem record and museum specimens only. Tait believes that, as a rule, the interstitial form takes a longer time to rupture than that in the free part of the tube. The solitary case of interstitial pregnancy which Tait has seen was one on which he performed laparotomy for right-ruptured tubal pregnancy on the 11th of May, 1885 (*Brit. Med. Jour.*, Dec. 19th, 1885). She made an easy recovery. Eighteen months afterwards she had another child—nothing remarkable about the labor. About fifteen months after this confinement she again became pregnant—no unusual symptoms except slight pain in the abdomen, not enough to cause her to seek advice, even after her terrible experience. The only point she emphasized was that she felt the child more plainly than ever before at a similar period. On March 9th, 1887, while in the act of stooping, she was seized with acute pain and a feeling of faintness. Stimulants were given, etc., but the patient died 5½ hours after the seizure, of internal hemorrhage. The abdomen was found full of clots and fluid blood. The cavity in which the foetus

was developed was separated from the true uterine cavity by a strong septum of uterine tissue. The true uterine cavity was lined with true decidua. The left tube communicated with the cavity of development in which the foetus and placenta lay, and the rupture had taken place in the upper and back part of the left uterine cornu. Here we have the almost incredible instance of a woman having tubal pregnancy twice with a normal pregnancy between. Yet during neither of her abnormal pregnancies was there any symptom to cause her to consult a doctor until the collapse of rupture seized her. I conclude, then, that differential diagnosis of the varieties of extra-uterine pregnancy is totally impossible previous to rupture.

DIAGNOSIS OF EXTRA-UTERINE PREGNANCY AT THE TIME OF, AND
SHORTLY AFTER RUPTURE.

The ovum, wherever it may develop, whether in the pavilion, the free part of the Fallopian tube, or in the interstitial part, as it grows distends the walls of the tube, forming a gestation sac. The chorion develops a placenta which penetrates the mucous membrane of the tube, encroaching on the hypertrophied muscular coat, so that where the placenta principally grows becomes the weakest part of the sac. Then, when the growth of the ovum exceeds the accommodating growth and stretching of the sac; when the stretching of the tubal sac reaches a certain point, spasm of the muscular wall is excited. Or, if spasm be present before, at length it detaches a part of the placenta, and bleeding occurs. Some of this blood usually escapes by the uterus, some is retained, increases distention, excites further spasm, and the end is rupture of the tube sac. Perhaps this process goes on many times before rupture takes place, which would account for the pain and hemorrhage which so often precede rupture. The ovum itself does not always burst when the tube does. As soon as rupture occurs fresh bleeding takes place from the torn tube wall—often it must be quite free. The blood already accumulated in the tube, along with the fresh flow, are now poured into the cavity of the peritoneum, in the vast majority of cases, causing shock and acute anæmia. When a Fallopian tube ruptures there are only two possible routes that the blood can take. If the tube give way between the folds of the broad ligament the blood will escape into the substance of the ligament, it will there be entirely outside the peritoneum, it

will, in fact, form an extra-peritoneal hæmatocele. In case of tubal pregnancy, it will form extra-peritoneal hæmatocele plus a ruptured or unruptured ovum. Now the amount of hemorrhage and the amount of shock in extra-peritoneal hæmatocele are moderate, as a rule, and the danger to life is not great. In the case of extra-peritoneal hæmatocele from tubal pregnancy, the ovum dies in a great many instances and the whole thing is gradually absorbed, no one, perhaps, suspecting what had given rise to the hæmatocele. However, if the tube bursts in any other part of its circumference except where the folds of peritoneum forming the broad ligament are, then the rupture must involve the peritoneal coat of the tube, and the blood is poured into the cavity of the peritoneum. This constitutes intra-peritoneal hæmatocele, and in tubal pregnancy, intra-peritoneal hæmatocele plus a living or dead ovum. Bernutz pointed out, in 1848, the vast difference between intra- and extra-peritoneal hæmatocele, but, even yet, most authors confuse the two. There is really all the difference in the world. If the blood is poured into the cavity of the peritoneum, it has free space there, the bleeding is encouraged because the blood meets with, and is diluted by, the lymph always present in the peritoneum, and easily caused to flow in abnormal quantity by any irritation. The result is that, except as the rarest exception, when a tubal pregnancy ruptures into the peritoneal cavity the *bleeding is fatal*, either immediately or in a short time. Tait says he has seen nearly eighty cases of intra-peritoneal hæmatocele, all resulting in death save those (with two exceptions) in which abdominal section was performed for the purpose of obviating death. ("Lectures on Ectopic Pregnancy," 1888, p. 31.)

It is easy to see how it is otherwise in extra-peritoneal hæmatocele. Here the blood is poured into the meshes of cellular tissue, an excellent medium for coagulation. Then there is only a limited space that blood can flow into. When the broad ligament becomes distended it actually makes pressure on the bleeding parts by resisting further distention. The only primary danger is that the broad ligament may give way and allow blood into the peritoneum, when the bleeding goes on and on till death happens. Tait has seen dozens of cases of broad ligament hæmatocele and has never met with a fatal one. (*Ibid.*, p. 31.)

A tubal pregnancy is bound to rupture; in the free part of the

tube it rarely delays beyond the twelfth week and may be as early as the fourth; in the interstitial part of the tube from the third to the twentieth week. This rupture takes two directions: into the peritoneum, which is the fatal form; and into the cavity of the broad ligament. The latter, or extra-peritoneal, alone gives all the cases which go on to the period of viability, all the lithopædia, all the suppurating cysts discharging into the bladder, rectum, vagina, and abdomen, and also all cases which by *secondary* rupture of the broad ligament into the peritoneal cavity are called "abdominal pregnancy." (Tait's "Lectures on Ectopic Pregnancy," 1888, p. 7.)

This little bit of pathology was absolutely necessary to prevent confusion and explanation at every turn. Let us now look at the symptoms and diagnosis of the intra-peritoneal bleeding, the fatal form. The injury sustained, as Barnes says, is compound. There is the traumatic violence attending the rent, and the sudden impression on the sympathetic centres producing shock, and the hemorrhage. The symptoms are also twofold: shock causes collapse, shown by loss of bodily heat, loss of all strength and energy, almost imperceptible pulse, intense paleness, vomiting, and often, in a very few hours, death. To this group of symptoms Barnes applies the term "abdominal collapse," and the name is a good one. These symptoms ensuing on rupture are so characteristic that violence and poisoning are the only things with which it would be easy to confound them. The whole thing usually happens so suddenly—some slight exertion, such as stooping at work, when a violent pain seizes the woman, she becomes cold, pulseless, collapsed, and is so often found dead or dying—that suspicions of violence are often aroused. Sometimes, especially if the unruptured ovum sticks in the rent in the tube, the hemorrhage may not prove immediately fatal. The woman recovers from the symptoms of shock, she may have no further attacks for a few days, when bleeding suddenly recurs with another attack of pain and fainting. This may be repeated several times before death occurs, but unless surgery steps in to her relief, death is all but certain. Some cases are so violent in the first onset that there is hardly time to do an operation, but, as a rule, death does not occur for several hours. The textbooks all describe recovery from the first shock as taking place sometimes. They then describe how the general signs of extreme anæmia show themselves; with the local symptoms of a feeling of an accumu-

lation of blood in the pelvis, retro-uterine hæmatocele, and how the patient may sink under the effects of continuous shock and hemorrhage. Also how, if she escape this danger, she has still another before her—peritonitis; how this may set in rapidly, with intense pain, swelling of the abdomen, pulse small and very rapid, temperature $100\frac{1}{2}^{\circ}$ F. to $101\frac{1}{2}^{\circ}$ F., and the drawn, anxious face met with in abdominal inflammations, and how she may recover even then. This is all true enough, but two things are confused. All these symptoms are rarely seen in *intra*-peritoneal hæmatocele; death rarely delays beyond a few days, and for the most part occurs inside twenty-four hours; but in broad ligament hemorrhage—*extra*-peritoneal hemorrhage, we may have all these symptoms—even a prolonged illness. The mistake arose through a false pathology. Where the foetus lived after the rupture, about the third month, it was almost always found encysted. Collections of blood, whether from rupture of extra-uterine pregnancy or other cause, were found encysted also. So, as peritonitis was the only means by which a free foetus or hæmatocele could become encysted, it was assumed that peritonitis must have been present. But even in extra-peritoneal pregnancy, although we may have inflammation and suppuration in the sac, still, even here, we rarely have peritonitis. The encystment is brought about by the foetus, or the blood distending the broad ligament and converting it, at once, into a sac or cyst.

Parry had a glimpse of the truth, but the point escaped him. He says, quite correctly, "It has been stated, however, that peritonitis, by which means alone intra-peritoneal effusions can become encysted, rarely follows the rupture of an ectopic gestation." Again Parry says: "There are few things in regard to extra-uterine pregnancy which excite more surprise than the *rarity* with which *peritonitis* is noted upon examination after death from rupture of the foetal cyst. The practical conclusions that may be drawn from a careful investigation of this subject are: That peritonitis is a rare sequel of rupture of the cyst, and even when pain, tenderness, and other symptoms of this affection supervene after the escape of the ovum, they do not necessarily indicate the existence of inflammation. *Peritonitis so rarely follows rupture* of an extra-uterine gravid cyst, that the *possibility* of its occurrence need not be taken into consideration in the decision of any questions relating either to prognosis or to treatment." Parry, like

all the older writers, argues against his own conclusions. He describes extra-peritoneal hæmatocele from rupture of a gestation cyst, and calls it *intra*-peritoneal, and then says it is so strange that peritonitis does not occur, although the hæmatocele gets a good cyst somehow. Here I should point out that I am not blindly following the opinions of Tait. Prof. Chas. A. L. Read, of Cincinnati, who himself agrees with Tait both as to pathology and treatment, points out that a great many authorities now agree with Tait as to the pathology of tubal rupture and the after effects. He says that in fact more have adopted the pathology than the practice, though if they accept the pathology he does not see how they can consistently refuse to follow the practice, which has astonished the world by its results. Schroeder ("Handbuch der Krankh. der weibl. Geschlechtsorgane," 7 Aufl. Leipsic, 1886), J. Veit (*Zeitschrift für Geburt. und Gynæk.*, 1884), and Kiwisch (quoted by Lusk: "Midwifery," p. 289) are among the leading Germans who agreed with Fritsch ("Dis. of Women," p. 289, New York, 1883), that "the most frequent source of the hemorrhage is the ruptured ovisacs of a tubal or other extra-uterine pregnancy." Among the gynæcologists, Tait, Imlach, Berry Hart, Thomas, and Emmet are among the most conspicuous who concur in the doctrine, while among the obstetricians, Lusk, Parvin, Barnes, Galabin, and Playfair may be mentioned.

The diagnosis of extra-uterine pregnancy at the period of rupture does not call for much further consideration. The case can nearly always be diagnosed as an abdominal hemorrhage, at least, and as the serious form, *intra*-peritoneal, is nearly always due to a ruptured gestation sac, the diagnosis is usually easy. Tait says he has never seen an *intra*-peritoneal hæmatocele that was not due to a ruptured tubal pregnancy. He believes the only other cause to be hemorrhage from some torn adhesions, or badly tied vessel after an abdominal section. Occasionally, but very rarely, other internal bleedings may eventuate in *intra*-peritoneal hæmatocele, such as ruptures of the liver or other abdominal organ, aneurism of some large abdominal artery, rupture of the ovary, etc. There is perhaps one point which ought to be noticed in the diagnosis, as it might prove useful in those cases where several attacks of hemorrhage occur before the case proves fatal. We must not expect to find a defined tumor caused by the blood. It is free and unlimited by any membrane. When contained in the rounded cavity of Douglas' pouch it bulges into the

vagina like a soft bag, but no defined tumor can be felt above the brim of the pelvis.

Should the rupture take place into the broad ligament, the case is not so plain. The symptoms may be very slight, or not noticed by the patient, as we find in many histories; or they may be nearly as severe as in the fatal intra-peritoneal bleeding. It is all a question of loss of blood. The symptoms will be those of ordinary (extra-peritoneal) hæmatocele. The essential diagnostic symptom between the two forms of bleeding is, that the blood is confined in the extra-peritoneal form, so that it forms a tumor which can be felt above the brim of the pelvis. Of course, the ordinary hæmatocele (extra-peritoneal) has many causes besides a ruptured tubal pregnancy: any sudden arrest of menstruation, or metrorrhagia, whether natural or the pseudo-menstruation so common after abdominal operations. The shape of the tumor is regulated by the broad ligament and pelvic fascia. Mostly the effusion is not great enough to be felt above the brim at first. When extensive the uterus is fixed and generally displaced forwards with a fluctuating swelling behind, or at one side. This swelling takes the shape of the distended broad ligament, concave beneath and rounded above the brim. It is not at all dangerous, even after abdominal operations, unless, indeed, the over-distended broad ligament gives way, and thus the blood enters the peritoneal cavity by a second rupture. For when the broad ligament ruptures into the peritoneum, just as in the case of the Fallopian tube, the bleeding is continuous or goes on at intervals, till death occurs, unless an operation puts an end to it. Of course, other dangers may arise in time from an extra-peritoneal hæmatocele; it may rupture into the peritoneum at any time; when due to extra-uterine pregnancy the ovum may develop and give rise to future dangers, and the cyst may suppurate, resulting in pelvic abscess. After extra-peritoneal hemorrhage has happened, as a rule, there is pain, faintness, a rise of pulse and of temperature. These symptoms gave rise to the older idea that when rupture of a tubal pregnancy did not kill by hemorrhage peritonitis occurred.

In the *New York Medical Record*, Feb. 25th, 1888, Drs. Taft and Gaillard Thomas report a case where apparently an extra-uterine pregnancy went on to full time without the tube rupturing. Dr. Taft has found records of eleven other cases where tubal pregnancy has

reached the eighth month without rupture. But with our present pathology such a thing is impossible. We must remember how difficult it is to determine the relations of an extra-uterine foetal cyst at such a late stage of pregnancy. The probabilities are that in these cases the tubes had ruptured early, the ova passed into the broad ligament and the ruptured tubes continued to form the upper part of the cysts which were completed by the ligament. Another case reported by Dr. Chambers, of Sydney (*Brit. Med. Jour.*, November, 21st, 1885, p. 984), is more difficult to explain. This case was only at the thirteenth week when abdominal section was performed. The case is described as abdominal pregnancy; the cyst was found in Douglas' pouch, but what concerns us here is that the ovaries and tubes are distinctly described as *intact*. On June 10th she was seized with severe abdominal pain and extreme collapse. The operation was performed on July 18th. This looks very like the usual tubal rupture, but is it possible the tube could contract and heal in a little over a month so as to leave no mark? I think not. Either the tubes were not properly examined, or if they really were intact rupture must have occurred very early and contraction and healing had been very complete. In this way Tait explains a somewhat similar case reported by Maticki (*Monats. für Geburtshülfe*, Mai, 1860), where the uterus and tubes could be traced and where the attachment of the placenta had become almost wholly omental. Tait says he has seen all stages of such a process and so believes it possible. He says there would be nothing more remarkable about it than the well-established fact that by axial rotation an ovarian tumor may be twisted off its pedicle and grow entirely from the omentum, or, as he says he has seen, from the ascending colon. He says he has also seen this strange transplantation in all its phases and in all stages. He also instances the fact reported by Lecluyse (*Bulletin de l'Academie de Belgique*, 1869), that an intra-uterine gestation became abdominal by the ovum escaping through an aperture left in the uterus by the defective healing of the wound of a previous Cæsarean section. The placenta became attached chiefly to the small intestines, and the history gives no hint of a sudden rupture. (Tait's "Lectures on Ectopic Pregnancy," 1888, p. 15.)

I shall conclude this part of my subject by a good account of an undoubted case of extra-uterine hæmatocele due to rupture of a tubal pregnancy. It is not often we get such a clear account of such a case,

at such an early period, and verified by operation. Indeed, it is the fact that operations are now so frequent which is teaching a new pathology. The treatment is revealing the disease. Dr. O'Hara reports (*The Obstetric Gazette*, June, 1886), that on September 25th, 1885, he was called to see a woman who had been in good health until two hours previously, when she was seized with severe rectal tenesmus (Tait's symptom of hæmatocele in left broad ligament) and pain in pelvis extending down the right leg and arm. When he arrived he found the patient collapsed, almost pulseless, with shallow respiration and cold extremities. She was the mother of three healthy children, the youngest a year old. She had missed one menstrual period about a week before the accident, and considered herself pregnant. Hence the diagnosis was extremely easy. She was considered to have internal hemorrhage due to ruptured tubal pregnancy at the fifth week. Opiates and stimulants were given. Next day there were signs of reaction, and laparotomy, though discussed, was deferred. Five days later Dr. O'Hara found the abdomen greatly distended, but no tenderness on pressure. Resonance was general on percussion, except over the right flank. The vaginal walls were œdematous, and the anterior wall was thicker at the cervix and to the left. The cervix was *moderately soft* and *patulous*. The urine was almost black, and the temperature 99° F. Two days later marked jaundice appeared with occasional vomiting and purging of bile. A few days later a swelling was noticed on both sides and in front of the cervix, with a painless, sanguineous discharge, containing fragments of decidua membrane. Micturition was difficult and painful. Three weeks after the first attack a terrible flooding occurred, lasting for an hour, and followed by slight hemorrhage, rigors and pain. On the thirty-third day Dr. Parish performed laparotomy. The incision was made in the *median* line, and the peritoneal cavity was found *empty* and the peritoneum normal. The exploring finger showed that the mass was *external* to the peritoneum, and extended upwards from the left half of the pelvis to a level with the umbilicus. Another incision was then made along the line of original selection—*i.e.*, above the outer border of Poupart's ligament—by which means the mass was reached without wounding the peritoneum. The incision was large enough to admit two fingers. About a quart of coagula, fluid blood, and pus escaped. The cavity was washed out with antiseptics, and the wounds

closed, a drainage tube being inserted into the abscess cavity. The patient rallied well, and there was not much discharge. A bloody discharge from the uterus, however, set in. Two weeks after the operation the uterine discharge had ceased, but free bleeding from the wound occurred. The temperature rose to 103° F., but there was no pain on pressure. Twenty-four hours after the operation the patient was permitted to sit up, but fresh bleeding occurred with same symptoms as at first. As the hemorrhage recurred the cavity was opened up and cleaned out. An injection into the rectum escaped through the wound, but not *vice versa*. Subsequently discharges of faecal matter, and a small gall-stone escaped from the wound. By May 1st the patient was convalescent, a small sinus alone remaining. The communication with the bowel had almost entirely closed. Menstruation had recurred. The patient's ultimate recovery was complete. It seems, in this case, that both foetus and placenta had either been absorbed or softened down into pus or débris indistinguishable from the other contents of the sac. The case is an excellent example of rupture of a tubal pregnancy into the left broad ligament. The ending is often far more favorable than this. Tait believes that both hæmatocele and ovum are absorbed in the majority of cases, so that there is nothing to distinguish such cases from ordinary every-day pelvic hæmatoceles.

DIAGNOSIS AFTER THE SHOCK OF THE PRIMARY RUPTURE IS
OVER.

Should we not see the case until after recovery from the shock of rupture, should there be such shock, we have then, according to the new pathology, to deal with a hæmatocele in the broad ligament. In a great many cases the ovum dies, so that the case becomes a simple extra-peritoneal hæmatocele attended by no great danger and tending to get well. In very many cases both ovum and blood are absorbed in the course of time, so that recovery is complete. In fact, the only dangers under such circumstances are that the distended broad ligament may give way at any moment, when the blood must flow into the peritoneal cavity, and peritoneal hemorrhage, as a rule, goes on to a fatal issue if surgical interference do not prevent it. Or, the sac may inflame at any time, and even suppurate, so that the hæmatocele may result in abscess of the broad ligament, which is, of course, indistinguishable

from abscess arising from any other cause. In fact, most recent writers believe that extra-uterine gestations will account for most of the pelvic hæmatoceles we encounter. So that, I say, if the ovum be dead, there is absolutely no distinction or means of diagnosis between hæmatocele from rupture of a tubal pregnancy into the broad ligament and hæmatocele from other causes, as sudden arrest of menstruation. The history is all we have to go upon, and that, as we have seen, is far more likely to mislead than to guide. However, in a very few cases, the ovum is not killed at the time of the rupture, but goes on developing. Of course, it may die at any moment between this period and the full period of gestation. If, however, it lives, the diagnosis is just as it was before rupture; that is to say, we must depend on any signs of pregnancy present to distinguish the case from one of simple hæmatocele or other pelvic tumor. Then, if there are enough signs of pregnancy present to make the diagnosis of that condition *probable*, by a pelvic examination we can get evidence enough to make it *probable* also that the gestation is extra-uterine. For instance, we have the position of the uterus in relation to the tumor, in front of and to one side. Then the uterus is absolutely pushed out of the middle line of the pelvis; it is more or less fixed; *the cervix is always open*; and the cavity is empty, as may be ascertained by bi-manual examination and the fluctuation test. In justice, I am afraid that this is all we can say about the diagnosis of extra-uterine pregnancy previous to the period when the foetal heart sounds become audible. But when the foetal heart becomes audible, the aspect of the case changes immediately. If we can hear the heart, and find the cervix open and the uterus empty, *the case is clear*. There is no possible source of error if *we are sure the uterus is empty*. If we are sure the uterus is empty, a little pelvic search will usually discover the whereabouts of the foetus.

DIAGNOSIS OF EXTRA-UTERINE PREGNANCY ABOUT THE VIABLE PERIOD.

As I have said, after the foetal heart becomes audible, the case is clear; still many are the cases in which diagnosis of an extra-uterine pregnancy is made while the child is safe within the uterus all the time. So our principal duty here is to point out some of the dangers of a false diagnosis in the direction indicated. One of these dangers arises when foetal outlines and movements can be felt more plainly

than is usual. This always does and always should suggest extra-uterine pregnancy, but the suggestion should not be allowed to go further or to become public property without sound evidence, which is only to be gained by rational examination of the pelvis. The false pathology of extra-uterine pregnancy which has existed so long, has so prejudiced the minds of medical men as to make them an easy prey to this mistake. The so-called "abdominal" pregnancy was supposed to be so common that whenever a doctor felt the foetus more plainly than usual, he immediately thought it was loose among the intestines. The fact is now ascertained that real abdominal pregnancy is the rare effect of secondary rupture of the broad ligament (this is the evident explanation of Jessop's case, which is the only reliable one on record). The consequence of the prevailing pathology is that no author except Parry noticed this important source of error until Tait illumined this point, as he has done so many. Parry says: "I have met with an example of *thinning* of the abdominal walls, a few years since, which was exceedingly puzzling. I was asked by Dr. E. W. Watson to see a young woman, to decide the nature of an abdominal tumor, which was the size of a seven and a half or eight months' gravid uterus. Upon making pressure upon the enlarged abdomen, a foetus was felt receding from beneath the fingers, against which it immediately rebounded. It was so superficial in its situation that it appeared impossible to believe that there was anything more than the skin of the abdominal wall interposed between the fingers and the child." Tait has seen eight cases where not only was there a "thinning of the abdominal walls," but an excessive thinness of the uterine walls also. Tait says he sometimes had the greatest difficulty in convincing the practitioners who called him in that the case was normal. He says that often the walls were no thicker than a single fold of towel. Sometimes the uterus was also displaced. In these cases one could forgive the medical men fancying the child was in the abdomen. Langley Browne, of West Brunswick, had a case where the thin uterus was extremely retroverted. In other cases there was retroflexion or latero-flexion. In such Tait confesses *patience* was necessary, to resolve doubts. He says, if with such a case any urgent symptoms existed, so that patience would be precluded, he would use the sound to see if the uterus were really empty. In all but one of Tait's cases there was a marked absence of liquor amnii. A curious point is that the cervix

uteri is usually thinned too, as much as the fundus, so that on examination the finger comes on the presenting part as if it were only covered by mucous membrane. And it is only by the greatest care that it can be ascertained that the os uteri is at the centre of the presenting part. Tait's eight cases were all under the seventh month except one. During the eighth and ninth months the walls of the uterus became thicker and the liquor amnii increased, and the cases became normal in every way, labor included. The exceptional case has not yet concluded, but is well advanced in the eighth month. Careful vaginal examination makes it clear the foetus is intra-uterine, but Tait says that an abdominal examination alone would convince any one that the child was free in the abdomen. There are some other conditions from which extra-uterine gestation requires diagnosis during the life of the child. The chief are intra-uterine gestations, with displaced uterus, during the early months, combined with fibro-myoma or cystic disease of the uterus; and as a rare condition pregnancy in one horn of a bifid uterus. For the first case there are three safeguards, viz.: pelvic examination, common sense, and patience. Doubtful cases must often arise, and the man who would omit patience or time from his list of diagnostics, providing no urgent symptoms arose, would thereby prove his shallowness of intellect. With regard to pregnancy in one horn of a double uterus, just as in interstitial pregnancy, it must often be impossible for the most careful to diagnose it from normal pregnancy on the one hand, or from extra-uterine on the other; the latter remark does not apply to interstitial cases. If pregnancy take place in a very undeveloped horn, it is practically extra-uterine gestation, and will end in rupture just as tubal cases do, unless very early abortion terminate the case. If, on the other hand, the horn be well developed, the gestation may go on to term and end by natural labor. Should a double cervix exist, the case would be evident. Suppose the tumor was clearly one-sided, although evidently attached to the uterus, and that by bi-manual examination we detected a fundus uteri close to the tumor and towards the middle line of the body, we should suspect pregnancy in one horn of a bifid uterus, and, I think, we would be fully justified in using Hegar's or Tait's dilators and exploring the interior of the uterus with the finger. Of course, this would be at the risk of producing abortion, which would probably be the best thing that could occur, in the interests of the patient. But still we should be

careful and trust to time in the absence of any urgent symptoms, as probably more intra-uterine pregnancies are supposed to be extra-uterine than extra-uterine mistaken for normal. We should always bear in mind the words of T. Gaillard Thomas with regard to such cases when urgent symptoms suddenly arise; but there is now no occasion to let the woman die, thus making the event of such importance as to overshadow a medical man's reputation for life. Every country village and town cannot possibly have a leading surgeon within range, but for that very reason every country doctor ought to think the matter out and be prepared to open the abdomen on a moment's notice as an urgent, life-saving operation. The operation is not as dangerous as herniotomy, nor half so difficult, yet any surgeon will do herniotomy on emergency, whether he likes it or not, as a life-saving operation. But the surgeon must have planned it in his mind beforehand, reflected on how he would do it, and the difficulties and dangers he might meet with. If the mind be not familiarized with the idea of taking the responsibility of doing such operations beforehand, it will simply shirk the duty when suddenly presented, and the patient be deprived of—not the chance—but almost the certainty of recovery. In the 42 operations Tait has done for abdominal collapse and hemorrhage, which it is safe to say would *every one* have died with any other treatment, he has only had two deaths, making a mortality of 4.7 per cent. This is about the mortality of mild cases of measles, so that the most inexperienced, if a qualified medical man at all, ought to cheerfully face the operation *as a duty*. The fact is that it is the will that is wanting. Most ordinary medical men have tacitly agreed with themselves that they will not endanger their reputation by doing any risky operations, such as tracheotomy, for instance. Now, this is another quite easy operation, and any medical man worthy the name could do it right well if he only prepared his mind beforehand and resolved not to shirk it when it became a duty. T. G. Thomas says: "Very often we hear of physicians being blamed on account of diagnosis in those cases which die suddenly from rupture. Every medical man who countenances such a charge *demonstrates his want of experience*, or his want of professional loyalty, by so doing. *Very often there is nothing in these terrible cases to excite suspicion; very generally nothing to decide us positively, even when suspicion is excited.*" The late Prof. J. Thorburn, of Manchester, after an extensive experience of extra-

uterine gestation and of diagnosis in doubtful cases, sums up the whole question thus: "*Granted an absolutely certain pregnancy, with very strong suspicion of its being extra-uterine, the probabilities are nevertheless very greatly in favor of its turning out to be intra-uterine.*" If obstetricians would remember these words they would avoid making themselves ridiculous on many occasions.

Before the foetal heart sounds become audible, and after foetal death, extra-uterine pregnancy has to be diagnosed from quite a list of other conditions, but I prefer to consider them in detail when we come to diagnosis after foetal death. I shall only allude here to the fact that extra-uterine pregnancy may cause retention of urine, because whenever a tumor gets behind the uterus from above it presses it downwards and *forwards* as well as to one side. When the cervix is pressed close to the pubes the uterus may press on the bladder and the cervix on the urethra, thus causing retention of urine. Now, this retention would probably direct our thoughts to retroverted gravid womb, in which retention is a prominent symptom, as it often is in small ovarian tumors and ordinary simple hæmatocele (retro-uterine hæmatocele). What I wish to remark here, then, is that retention of urine, which may occur in extra-uterine gestation, is far less common in that condition than in either retroversion of the gravid uterus, small ovarian tumors, or retro-uterine hæmatocele. Retroversion is very apt to be confounded with extra-uterine pregnancy, and here the ordinary signs of pregnancy are of no avail, as the retroversion contains a foetus usually. Retroversion may be best distinguished by tracing the firm rounded body of the uterus by vaginal and rectal examination and finding it continuous with the tumor. Another good point is that the supposed tumor in retroversion is much lower, much nearer to the pelvic floor, than is at all usual in cases of extra-uterine gestation. As to small ovarian tumors they have probably given rise to more protracted illness; they do not interfere with menstruation; the uterus does not undergo evolution; the purple color of the vagina is wanting.

The case of gestation external to the uterus goes on with intermitting attacks of pain until we arrive about the full period, when we have some characteristic phenomena. Whether the foetus dies at maturity or not—

Spurious labor is sure to come on. This consists in more or less

frequent and powerful uterine contractions, perhaps some vaginal hemorrhage with discharge of what decidua has hitherto escaped expulsion during the false abortions which may have occurred. Often the contractions of the abdominal muscles rupture the gestation sac and even the broad ligament (secondary rupture), so that blood and liquor amnii, if not the foetus, escape into the cavity of the peritoneum, which accident is, of course, usually rapidly fatal unless the surgeon steps in. Perhaps, more frequently, rupture does not occur, the labor pains continue at intervals until the child dies from effusion of blood into the substance of the placenta, causing asphyxia. In other cases again the child lives through, and after, this false labor. It has been known to be alive several months after full time; but, as a rule, it dies either before false labor commences or during its course. In any case, after from several hours to several days the labor pains gradually pass off and often *lactation* is established for a little. We shall now pass to a most interesting portion of our subject—

THE DIAGNOSIS OF EXTRA-UTERINE PREGNANCY AFTER FŒTAL DEATH—OR RATHER AFTER FULL PERIOD OF GESTATION AND FŒTAL DEATH.

When foetal death occurs at maturity we, of course, lose the sound of the foetal heart, and after a little, the uterine souffle also. There has been some discussion as to how long the uterine souffle continues after foetal death. Some have maintained that it lasts as long as uterine enlargement does, but this seems to be a mistake, as uterine enlargement often lasts for months, until the placenta begins to be absorbed. That is to say, when the placenta is implanted partly on the back of the uterus, which is very often, the uterus remains enlarged until the placenta begins to disappear by absorption; but, as a rule, the uterine souffle disappears shortly after the foetal heart sound. Observations have been made specially on this point by Dr. R. Petch, in his celebrated case of killing the foetus in extra-uterine pregnancy by electro-puncture (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1092). In his case, though the foetal heart sounds ceased on August 24th, the placental, or uterine souffle continued till September 7th, a period of fourteen days longer. That the bruit was not simply a sound conducted from the abdominal aorta, or other large pelvic vessel, he thinks is proved by the sound disappearing when it did; if it had been

simply conduction the sound would not have ceased so soon, as the tumor in that time had not shrunk sufficiently in size to remove the pressure necessary to produce conduction. This persistence of the utero-placental circulation is most interesting, both in diagnosis and treatment.

We may safely say that after the death of the child (should the case be first seen then) is the most difficult period of all in which to make a diagnosis. This may seem absurd, as we have a large, perhaps fully grown, child there to help us; but this child so alters when the liquor amnii begins to be absorbed that it can be taken for almost anything. First, then, we should, of course, pay great heed to the whole history, although here in particular it is usually most misleading, and we should never place the least reliance on the statements of patients any farther than they are backed up by physical signs in the pelvis or abdomen. The history as to absolute sterility, or sterility for a number of years, is of importance; then a sudden arrest of the menses, a gradual increase in size, false labor about the time of expected delivery, all have their value, and especially if there be *any show of blood during the false labor and a decrease of size afterwards*. These two latter points, if carefully inquired into, would usually keep us right, no matter how the remainder of the history might mislead. Campbell emphasizes these points while warning us not to put our trust in histories: "In many instances of the different varieties of misplaced gestation, the catamenia are suspended; frequently, however, they appear regularly in each of the early months; in some cases they flow at uncertain periods, and in other examples they are either profuse or limited in quantity. In many cases, at an uncertain period of gestation we have hemorrhage, uterine effusions, the extrusion of coagula, of bodies which resemble moles, or portions of the placenta. These appearances have frequently led to the belief that the patient has actually aborted, or that the ovum was originally not extra- but intra-uterine and had escaped through a rent in the uterus into the peritoneal cavity, the extruded body in either case being viewed as the placenta." If this be so, the history of menstruation, at all events, and this is usually made the chief point in the history, is not of much assistance.

The hemorrhage at false labor and the diminution in size afterwards are, however, *constant* (Tait). Then we have the condition of

the uterus, which is also *constant*, viz, the uterus is always intimately connected with the tumor, most often in front of it; it is always enlarged until the placenta is absorbed from the back of it, and the os and cervix are always open, more or less. If the case is seen shortly after foetal death, the tumor will be soft, there will be more or less ballottement in it, and possibly some part of the child, a hand or a foot, may be recognized by rectum, vagina, or abdomen. Parry well says: "If the woman does not perish from rupture of the cyst during the first four or four and a half months of gestation, it is not likely that an opportunity will offer to inspect the body until at, or near, or even some time after, the close of pregnancy. . . . Though the presence of an encysted foetus is not incompatible with life, and even with comfort and usefulness, the woman who bears such a burden within her is in constant danger of the cyst taking on inflammatory action, which will greatly endanger and may even destroy her. . . . If the patient is not seen until after the *death of the child*, the diagnosis of an extra-uterine pregnancy may be very difficult. Many years may have intervened before the woman comes under notice. Of course, if the cyst has opened into the bladder, bowels, or vagina, or a fistula has formed through the abdominal walls, there will be *little or no trouble* in arriving at a correct conclusion. Difficulty will arise only *when the cyst has not ruptured*, or, having opened into the bladder or into the rectum out of reach, it has not discharged any of its solid contents. Under these circumstances a correct conclusion can be reached by carefully sifting the clinical history. No point is too minute for examination. As a rule, it will be found that all such women have a firm conviction that they were pregnant when the abdominal tumor made its appearance. Though more than a score of years may have passed, they will not have abandoned the idea that they still carry a child somewhere in the abdominal cavity. Such women will nearly always give the history of *labor at or near term*, attended with uterine hemorrhage, and followed by the secretion of milk, after which they will assert that the abdomen diminished in size, and that this diminution steadily continued until the tumor reached the dimensions presented when the patient comes under observation. This association of phenomena is very characteristic, and when they are all present, erratic gestation should always be suspected. The *diminution* in the size of the *abdomen after labor* is a most important symptom." When the

liquor amnii is absorbed, the uterus becomes smaller and more mobile, as does the tumor itself. The hands and feet are often found above the brim of the pelvis and may often be recognized, but are closely imitated by little nut-like cysts of small ovarian tumors; or, pelvic examination, especially by the rectum, may make the nature of the case quite clear by the unmistakable recognition of some part of the foetus. Dermoid cysts and cancer are the causes, however, of many mistakes. A pelvic cancer has often been quite confidently declared to be a foetus. Indeed, if the cyst be compressed down into the pelvis, diagnosis, except by exploratory incision, may be simply impossible. Here I should warn the reader against the great danger of the aspirator, or Barnes' favorite puncture, as a means of diagnosis. Both these procedures are *more dangerous in abdominal or pelvic cases than abdominal section*. All modern literature proves this. Parry says that unless it has been decided to operate for removal of foetus *immediately*, the trocar is utterly unjustifiable. "A few, but very few, women have long survived its use." Jonathan Hutchinson says: "The practice is in itself attended by great danger, nor shall I deal honestly with you or myself if I do not candidly admit that, with due care and patience, I do not think that paracentesis ought to be necessary in a case of foetal tumor simulating ovarian dropsy." Mr. Hutchinson had a case of fatal peritonitis after puncture. Dr. Jordan punctured a woman who had no bad symptoms, but in two hours complete collapse came on. He says: "The doubts cast on my diagnosis, and the variety of opposing views in regard to the nature of the case, which unfortunately resulted in the use of the aspirator, were nearly the cause of the patient's death." I shall discuss the diagnosis from ovarian tumor and from pelvic cancer separately. When the liquor amnii becomes almost totally absorbed, the cyst and its contents become so jammed together and so intimately related to the contents of the pelvis that diagnosis often becomes an utter impossibility, or a mere guess if attempted. In such cases, fortunately, that modern diagnostic means, the exploratory incision, comes to the surgeon's aid, and as it is essential to any treatment worthy of the name its use is the more laudable.

DIAGNOSIS OF SUPPURATION IN AN EXTRA-UTERINE GESTATION SAC.

Ultimately, in the great majority of instances, the sac containing a dead ovum undergoes inflammation and suppuration, becomes con-

verted into an abscess and, like any other abscess, discharges its contents. This happens no matter at what period of gestation the foetus may die; indeed, an immature foetal sac usually suppurates sooner than where the gestation has gone on till term. After the death of the ovum the liquor amnii is absorbed, then the soft parts of the foetus and the bones to a great extent follow, until only a much smaller tumor remains in the broad ligament, containing debris of foetal tissues and bones. Sooner or later inflammation takes place and we have a pelvic abscess. Here, in order to in the least understand the various outlets this abscess finds for itself, we must discard the "encystment" theory of the older writers and accept of the broad ligament theory of Tait.

Dezeimeris was the first to discover that there could be an extra-peritoneal pregnancy. His facts were confirmed by others, but the rationale of the occurrence could not be got at till lately. Campbell tried to explain the facts of Dezeimeris away by the favorite "encystment" theory. He says: "It is difficult to comprehend how the ovum can insinuate itself under the peritoneum, which is reflected over the organs situated in the brim of the pelvis. Through time, certainly, the connections of the *original cyst* with the adjacent parts become so numerous that, *when superficially considered*, the ovum may *seem* to be enveloped by the layers of the broad ligament; but how it can pass under this appendage it is impossible to conceive." There is now, however, ample explanation of the how and the why, as well as plenty of proof of the correctness of the doctrine. In the *Memphis Medical Monthly*, March, 1888, Dr. R. B. Maury gives details of a post-mortem examination of a case which affords complete confirmation of the conclusions of Tait, Hart and Carter, as well as of Werth and Dezeimeris. "The pelvic organs were removed, and it was then seen, beyond any possibility of doubt, that the foetal sac was entirely extra-peritoneal, that the gestation had originated in the right Fallopian tube, and had developed between the folds of the broad ligament, downward to the pelvic floor, laterally to the pelvic wall, and upward into the abdomen. The ovum, in its development, had lifted the peritoneum off the bladder and the anterior surface of the uterus, while the relations of the peritoneum to the posterior uterine wall and to Douglas' pouch were not altered. The sac extended quite to the pelvic and abdominal wall on the right side, but did not go beyond the left

cornu of the uterus on the left. At the time of the operation it was observed to be covered by peritoneum, and this was clearly shown after death. The gestation was therefore entirely extra-peritoneal, and belonged to the variety intra-ligamentous of Werth, or sub-peritoneo-pelvic of Dezeimeris. This autopsy corroborates the view taught by Mr. Lawson Tait, that in extra-uterine pregnancy, no matter where the foetus may be found, its development begins in the Fallopian tube, and that it may become intra-peritoneal or extra-peritoneal, just as the tube happens to burst." If this description had been invented by Mr. Tait to bolster up his views, it could not have been better fitted for the purpose. But in order to really understand the pelvic abscess resulting from suppuration in an extra-uterine foetal cyst, we must also remember that according to the same view all the ova that survive rupture are certain to be situated in the broad ligament.

When a cyst containing foetal débris suppurates, the matter *always* takes one of four directions: through the rectum, which is much more common than any of the others; through the posterior vaginal cul-de-sac, which is next in point of frequency; occasionally through the bladder; and most rarely of all through the abdominal wall near the umbilicus. Foetal remains have also been known to pass into the colon or intestine, and through the uterus. Such cases are due to implantation of the placenta on those structures, with its subsequent partial necrosis and partial absorption. Tait says he himself did not understand how foetal remains passed by the umbilicus, but the researches of Berry Hart give the reason as plainly as daylight. In such cases the peritoneum is lifted off the anterior abdominal wall as high as the umbilicus, and the cyst is in close relation with the anterior abdominal wall. Those who advocate a masterly policy of inactivity in regard to treatment represent this elimination of the foetus as a very safe mode of getting rid of it. All we have got to do is to assist at the delivery of each bone, enlarge openings where necessary, etc.; but besides the years of misery, uselessness and suffering which women have to undergo during the elimination of the dead foetus, at least a third of them die during the process, from putrid absorption. Parry has collected 330 cases; of these 105 died, which is probably under the truth rather than over it. The direction which the abscess takes depends on the seat of the original hæmatocele and ovum; this is a further proof of the truth of the theory. Effusion into the left

broad ligament dissects the peritoneum off the rectum, causing annular stricture and sometimes complete obstruction of the rectum; so we would naturally expect the foetal débris would take the same route. Tait says he has proved the seat of the sac to be in the broad ligament by his finger in the rectum and a sound in the bladder, in many such cases ("Lectures on Ectopic Pregnancy," p. 57). We would expect the posterior arch of the vagina and the bladder to be the next easiest outlet, and in cases with a vaginal opening Tait has again proved the foetal cyst to be in the ligament. He also relates the case of a lady who had been passing phosphatic calculi for years, the nuclei being the bodies of foetal vertebræ. He opened the abscess from above the pelvic brim without opening the peritoneum, removed foetal bones, hair, pus, and phosphatic débris, which resulted in immediate cure. He could put his finger into the bladder by an opening on its right side; and as the womb was quite fixed, he could not doubt that the abscess was the consequence of a dead ovum in the right ligament.

The diagnosis of these cases often cannot be made at all till a foetal bone passes or gets arrested in the opening. The history is not to be depended upon unless the pregnancy has gone on till near term. If the foetus die early, there is only a history such as we might have in any pelvic abscess. These cases cause tedious, painful and dangerous illnesses, and render the woman a useless invalid often for years. The vaginal form suffers less than the others, and the elimination is sooner over. Discharge by the bladder is far more dangerous than any other form, because here we have the danger of cystitis leading to surgical kidney, while the phosphatic encrustations of the foetal remains greatly increase the pain.

This abscess in the broad ligament may follow any extra-uterine gestation which is deposited in the broad ligament as the result of primary rupture, which practically means rupture of the Fallopian tube; but the earlier death occurs in the foetus, the smaller will be the abscess and the less the trouble, danger and pain in eliminating the contents. The older the foetus previous to its death, the larger the abscess and the more trouble the bones have in finding their way to the surface.

Broad ligament hæmatoceles may suppurate, whether dependent on extra-uterine pregnancy or not, and in the early stage, before any bones are formed which are permanent enough to give trouble; of course, any differential diagnosis is impossible. Such cases pass as ordinary cases

of pelvic abscess. Tait thinks suppuration in hæmatoceles, taken generally, is not very common, yet many cases are encountered. The history might obscurely point to the origin of the hæmatocele, but, by the time the hæmatocele was merged in the pelvic abscess, probably the whole history would be merged in an obscure account of pelvic pain and trouble. About thirty pelvic abscesses have been operated upon by Tait without a death, of which he believed the origin to be hæmatocele, as he found considerable quantities of laminated, broken-down blood-clot. All these were extra-peritoneal, and he suspects about half of them originated in extra-uterine gestations. He gives the history of one case which is typical ("Lectures on Ectopic Pregnancy," p. 36). She was 45 years of age; had never been pregnant, except one doubtful miscarriage nineteen years before; symptoms like those of hæmatocele had occurred eight months before, after three months' suspension of the menses; she had been getting thin; had lost her appetite; had constant thirst and night-sweats, and abnormally high temperature at night. Uterus was fixed in a mass of effusion involving left broad ligament and part of the right. The mass on the left surrounded the rectum, forming annular stricture, as Tait has observed hæmatoceles on left side often do. *No other writer has noticed this sign of left hæmatocele.* No fluctuation could be felt in pelvis, but the symptoms pointed clearly to presence of pus in pelvis. The pelvis was opened from above; a large abscess found just behind base of bladder, between which and uterus it lay, but extended round behind rectum. The floor and posterior wall of abscess consisted of old laminated blood-clot; so its origin was a hæmatocele, extra-peritoneal. She went home cured on the thirtieth day.

As to the diagnosis of extra-uterine pregnancy at periods more remote, it must be conducted on general principles. Where the pregnancy has advanced to term, the history of pregnancy is usually clear enough. In such a case the history is quite different to that in cases at the period of primary rupture, or of hæmatocele, or pelvic abscess following primary rupture, where the signs of pregnancy had never been properly developed. Without the history of pregnancy it would be impossible to say what the nature of such a pelvic tumor might be. But as the retention of a foetus or of foetal remains is, and necessarily must be, a source of danger, to the dying day of the longest life, the exploratory incision will be justifiable, and will provide both the

means of diagnosis and of cure at the same time. The most recent case of lithopædion with which I am acquainted was related by Dr. A. Martin, to the Obstetrical Society of Berlin, November, 1885. A woman, 52 years of age, multipara, noticed signs of pregnancy in August, 1872. In 1873 she appeared as an out-patient, and a diagnosis was made of extra-uterine pregnancy with a living child. The patient declined to submit to any operation, and the child ultimately died. In November, 1885, twelve years after, she returned, and was willing to submit to any operation to be relieved of very distressing abdominal symptoms, which were rapidly becoming worse. The extraction of the lithopædion, which was lying on the right kidney, was easy enough; but great difficulties were experienced in extirpating a tumor which was adherent on every side, and filled the right half of the pelvis. The Fallopian tube was found distended with pus, and situated in a funnel-shaped cavity, which probably corresponded with the original site of the placenta and ovum. No trace of placenta could be discovered. Recovery took place without a bad symptom.

In the *Arch. de Tocologie*, April 10th, 1887, we have a most interesting and instructive account of the sequel of an extra-uterine pregnancy, which illustrates the dangers of retained foetal structures very well. A woman, aged fifty, who had never been pregnant, suffered twelve years before from violent and unexplained crises of abdominal pain, which obliged her to keep her bed. Her doctor attributed these attacks to disease of the ovaries. The patient, however, firmly believed she was pregnant, and at about the full time, as she thought, the pain came on with unprecedented vigor. The doctor who was sent for waited for labor to take place for three days, and then, *without apparently a suspicion of the condition of affairs* (no nice diagnosis there) left, with a diagnosis of "dropsy of the ovaries." As subsequent events showed it was a case of extra-uterine pregnancy, but at the time this was not diagnosed; and three years later the patient had a huge abscess, from which were extracted fragments of foetal bones. After the lapse of a considerable period of time the abscess healed and a second formed on a buttock, a third in the front of the hypogastrium, and a fourth in the perinæum. They all ultimately healed, but not until ankylosis of the hip joints had taken place. The last fragment of foetus was a black, porous and friable scapula.

This took five months to effect its exit, and left a wound which took two years to close.

Dr. Notta relates another warning case in the *Progrès Méd.*, May 10th, 1884. Patient aged forty. Eight years ago a tumor began to grow; grew for a year, and then remained stationary for seven years. On admission she had all the symptoms of intestinal obstruction, and an abdominal tumor. Laparotomy was at once performed. The anterior abdominal wall was closely adherent to the sac, which contained a yellowish, oily liquid, and had no pedicle. Its walls were fibrous and $1\frac{1}{2}$ cm. thick. The intestines closely adhered to the sac, and a strangulation was discovered. Death on the third day.

At the autopsy no peritonitis was found, but a second strangulation was discovered. The pregnancy was "abdominal," the foetus had undergone fatty degeneration, the skeleton was unchanged, the membranes were partly calcareous. The placenta was not identified. The degree of development and position of the foetus are not mentioned.

A case by Dr. Angus Macdonald (*Edin. Med. Jour.*, February, 1884, p. 697), should prevent us despairing of any case. As it is so instructive in every way, I give a sketch of it. Aged twenty-eight; one child six years ago; *no history of pain till sixth month*, when she had deep pain in the lower abdomen, with great difficulty and pain in passing water. Three weeks later a sanious vaginal discharge, and œdema of left leg beginning at ankle. In the seventh month hectic fever; uterus five inches long, depressed; fullness in Douglas' pouch and on both sides of pelvis, bulging into rectum, from which blood and pus flowed. An abdominal mass rose nearly to the umbilicus; breath sweet. The symptoms of septicæmia subsided, but much pain was felt, and threatening symptoms appeared on sitting up. At about the eleventh month operation was decided on. The macerated foetal remains were found attached to the right and posterior abdominal parietes. *The cyst was formed by six inches of almost gangrenous intestine, and contained fæces.* A fæcal fistula was also found. Resection of this part of the intestine was performed, and drainage ended the operation, which was performed with antiseptic precautions *without the spray*. Fæces flowed through the wound for a few days only. Three months after the operation the patient was quite well, and no trace of deposit could be felt. *Diagnosis:* abdominal pregnancy, death of foetus at end of sixth month, inflammation of sac,

peritonitis, thrombosis of left iliac vein, absorption of putrid contents of sac, and septicæmia, bursting of sac into bowel. Surely after the success of an operation even dealing with a sac containing fæces and complicated by resection of half a foot of gangrenous gut, we might enter on the simple section which is alone necessary to remove the ruptured tube at the end of the third month, without much nervousness as to the result.

At the Royal Medical and Chirurgical Society, London, on October 26th, 1886, Dr. Henry Gervis reported a most interesting case, the only one on record in which the foetal sac communicated with both small intestine and uterus. I shall give a sketch of the case, and of the remarks by the members on it. (*Brit. Med. Jour.*, Oct. 30th, 1886, p. 818). A woman, aged thirty-five, was admitted to St. Thomas' Hospital on Nov. 19th, 1885. She had ceased to menstruate on May 9th, and her pregnancy proceeded without incident until July, when she experienced severe pains in the abdomen, which continued until admission. In September she passed by vagina a pint of thick, greenish fluid; similar discharges continued at intervals until she came under observation. There had been no uterine hemorrhage, but a piece of skin was said to have come away with the discharge on one occasion. On admission, an ill-defined swelling was noticed in the hypogastric region, extending toward the right of the middle line; the lower segment of the uterus appeared expanded and fixed. A large quantity of offensive discharge, yellow in color, was issuing from the vagina through the os uteri; this discharge was found to consist of broken-down muscular fibre and vegetable tissue. On Dec. 14th she passed, by the vagina, the shaft of a foetal tibia. On exploration, the decomposing body of a small foetus was found lying, as it were, transversely in the uterine cavity. It was tightly fixed in its position, and all attempts to extract it were in vain; slight hemorrhage occurred during the operation, and the patient died on the following day. On post-mortem examination, the great omentum and the intestines were adherent to the brim of the pelvis, and, under this mass the remains of a partially disintegrated foetus were found, in the posterior half of the pelvis. Almost the whole of the posterior wall of the uterus had sloughed away. Dr. Gervis pointed out that he was unable to find any similar case of extra-uterine gestation occupying Douglas' pouch, communicating both with the uterine cavity and the

small intestine. Dr. C. H. Routh suggested that, as a rule, the only conclusion which could be arrived at when there was evidence of a *growing tumor* not in the uterus, with signs of pregnancy, was that there must be extra-uterine gestation. He had found it an invariable rule in extra-uterine pregnancy that the *catamenia ceased at once, but came on again in about three months*; and that pieces of decidua then came away. In this case, such a piece of decidua was probably passed. He thought that if a sponge or sea-tangle tent had been used, followed by a Barnes' bag, it would have been possible to reach the cavity of the uterus and remove the foetus. Dr. Gervis, in reply, said the tumor was not a growing one when it came under observation. The first diagnosis was extra-uterine gestation, but this was rendered doubtful by the passage of portions of the foetus from the uterus by the vagina. The interest of the case was the unique direction taken by the foetus (through the uterus).

No doubt the fact of portions of the foetus passing by the vagina from the uterus would shake the opinion of the most confident. The case illustrates the value of Tait's rule to trust no history, or statement, or theory any further than you can get physical evidence in the pelvis to back it up; and even if the case was supposed to be intra-uterine, finally, the treatment seems weak and vacillating. The indication was to remove the foetus wherever it may have developed and in whatever way might prove necessary; but it was left sticking within reach and after efforts had been made to dislodge it. One fancies the treatment would have been much more decided and firm in other hands and, perhaps, with a different result.

DIFFERENTIAL DIAGNOSIS OF EXTRA-UTERINE PREGNANCY.

Probably the following list as given by Prof. Thorburn contains all the conditions that have been and are liable to be mistaken for extra-uterine gestation: 1. Intra-uterine pregnancy; 2. Normal pregnancy with retroversion or retroflexion; 3. Pregnancy in one cornu of a bifid uterus; 4. Ovarian tumor; 5. Cyst of broad ligaments; 6. Fallopian distention; 7. Uterine tumors; 8. Pelvic hæmatocele (simple); 9. Pelvic inflammatory exudations or abscesses (simple); 10. Cancer of pelvis or peritoneum.

1. *Intra-uterine Pregnancy*.—During the first two or three months it is very rare that anything unnatural is suspected. It is extremely

seldom that the medical man has any opportunity to attempt the diagnosis. As a rule, the opportunities for diagnosis commence after primary rupture in the very cases in which this does not carry the patient off, or lead to an operation which removes the pregnancy. I have already discussed the diagnosis from intra-uterine pregnancy so fully that much here is not required. After three or four months the occurrence of occasional metrorrhagia, or the return of natural menstruation with an increasing tumor, or frequent or constant pain, may lead to an examination. The practitioner should insist on an examination on hearing of any such symptoms. Then a good knowledge of the signs of pregnancy, a knowledge of what can be ascertained by bi-manual examination of the pelvis, and great care in forming his opinions, are the guides to success and the lights to warn from disaster. He should not build too much on any one symptom; he should not lay too much emphasis on the lateral situation of the swelling, especially if to the right, unless there are corroborative *physical* signs. In the advanced stage (seven or eight months) abnormal thinness of the uterus and abdominal walls is a great source of confusion, but then there is in almost all such cases an utter absence of any corroborating evidence, and bi-manual examination, as usual, is our only guide and safety.

2. *Retroversion or Retroflexion of the Gravid Uterus.*—Retroflexion or retroversion of a gravid uterus has a definite history not so liable to mislead as the history of extra-uterine gestation. There has probably been previous displacement of the uterus which the patient may be aware of, or if retroversion occur during pregnancy the history is definite and clear. There is a sudden onset of symptoms of pelvic pressure, obstructed defæcation, urination, pain in the pelvis, and *no vaginal hemorrhage*. If we only employ the old vaginal examination, we might easily be misled, but an intelligent man, who makes a careful bi-manual examination, can scarcely be deceived. The point is to establish the continuity of the uterine cervix with the mass behind, which always lies lower, nearer the pelvic floor than is the case with an extra-uterine tumor. Then the results of treatment complete the differentiation. It may be added that though difficulty of urination may occur in extra-uterine gestation it is never so prominent as in retroversion of a gravid uterus.

3. *Pregnancy in one Cornu of a Bifid Uterus.*—I have already

touched on this subject and have only to repeat that unless the horn be well developed this is practically an extra-uterine gestation, only more dangerous, as whereas a tubal pregnancy has a chance of rupturing into the folds of the broad ligament where it is quite extra-peritoneal and comparatively without danger, the gestation from a ruptured horn, unless removed through the uterus by very early abortion, is sure, on rupture, to be extruded into the cavity of the peritoneum, which practically means abdominal section or death. Unless we can detect a double cervix or os uteri, bi-manual examination is the only guide, and it must be confessed that such cases are most difficult to diagnose. Moreover, as in tubal pregnancy, there is usually no opportunity for examination till rupture occurs without the least warning. Fortunately, abdominal hemorrhage is easy enough to diagnose and not really difficult to treat. The only definite hint that can be given to guide the bi-manual examination is that a decidedly one-sided tumor closely attached to the uterus with a fundus at its side ought to make us very suspicious; and if bi-manual examination of the uterus and search for fluctuation in it give no signs of an ovum there we should insist on dilating the uterus and exploring its interior with the finger. This treatment would probably cause the gestation to abort, the only hope of avoiding rupture and abdominal section.

4. *Ovarian Tumors*.—This disease is very liable to be mistaken for extra-uterine gestation. The mistakes would be frequent were extra-uterine gestations going beyond primary rupture frequent. The history of ovarian tumor is more definite than the history of extra-uterine pregnancy. But it must be confessed that in many cases time and repeated bi-manual pelvic examinations are the only means of distinction. Some cases of extra-uterine pregnancy could be diagnosed from ovarian tumor at once and with ease; in other cases it would be totally impossible, at one visit, to the most skillful.

If we happened to have most of the symptoms of even early pregnancy present and a vascular, *growing*, semi-fluctuating tumor at the back or side of an enlarged uterus, with suspension of the menses for a time or two, and then irregular hemorrhages with severe crampy pain, the diagnosis would be evident to the simplest. In fact, as soon as symptoms of life, of real pregnancy somewhere, arose, the difficulty would be over to any intelligent man. But either before definite signs of life in the tumor, or after these have disappeared, diagnosis is

practically impossible in many cases without resort to a means which ought to be the more used, as it is also the only treatment worth anything, as a rule, that is abdominal section. This has now been proved to be less dangerous than puncture by aspirator or trocar, and with the progress of another twenty years is bound to be used by everybody. No doubt the difficulty between extra-uterine pregnancy or its results and ovarian disease is very rare; this is fortunate for the reputation of medical men. However, rare things seem to be always turning up to the annoyance of practitioners. I think the principal reason of the confusion and helplessness is that we allow the possibility of extra-uterine gestation to slip out of our mind when we are examining a pelvis to ascertain the nature of some tumor, hence the difficulty. The question does not arise in the mind thus—is this an ovarian tumor or a dead ovum? But is this tumor a cyst of the ovary, broad ligament, sub-peritoneal uterine tumor, cancer, etc.? Consequently it is impossible to diagnose what has never even received consideration. If we admitted the possibility of the presence of a dead ovum any evidence in that direction would be duly weighed and might at least prevent us making a false diagnosis by preventing us from making a positive one at all. Then on section for *diagnosis* and treatment we should not be thunderstruck by turning out a foetus of which we had never thought. I am convinced that we all, consciously or unconsciously, diagnose in all cases by exclusion. We may exclude quite a number of things by half a second's reflection, but still this must be done if we wish to avoid blunders. I believe the difference between the man who makes many blunders and the man who makes few (who does not make some?) is that the latter takes a more extensive mental survey of the *possibilities* of the case: the former merely reviews a few of the more common, and thus judges on imperfect evidence. Defective diagnosis, especially when positive, usually arises from error in the management of the understanding more than from any other cause. We instinctively recognize this at the moment the blunder is exposed, hence the feeling of shame. If we had judged fairly on all the evidence obtainable why any shame? Hence, I repeat, bi-manual examination and reflection are the only beacons against error. Prof. Thorburn ("A Practical Treatise on Diseases of Women, London, 1885, p. 477) tells of an operation at a case of this kind by Dr. Atlee, in 1867, Sir Spencer Wells, Dr. Nicholay-

son, of Christiania, Drs. Burpee and Drysdale, of Philadelphia, and Dr. Atlee's elder brother, were present, and acknowledged that they had never been called on to diagnose a case of the kind.

5 and 6. *Cysts of the Broad Ligament, and Distention of the Fallopian Tube.*—The point here, as in the last case, is to give the possibility of early extra-uterine pregnancy or of a dead ovum a chance of being considered in our minds. We may say that where the symptoms of pregnancy, early or advanced, are present error would be almost impossible if any care were taken. But just as in the case of ovarian tumors, many cases might arise in which patience or abdominal section, according to the urgency of the symptoms, would be the only possible means of diagnosis. In pyo-salpinx there would be a history of pain with fever, etc., quite different from anything we could have in an early tubal pregnancy. Except affections of the tubes the other possible pelvic cysts agree pretty closely in their characters with ovarian cysts. The great point is not to forget the possibility of an extra-uterine ovum. These pelvic cysts are not, as a rule, urgent in their symptoms, and if we have the least doubt as to the possible presence of any form of pregnancy that doubt itself would save us from blunder, as we would wait until time would tell. In case of a dead foetus, past term, we could hardly be said to blunder if we had carefully considered the possibility and then made an incision to see what it was. These cysts may, of course, be combined with extra- or intra-uterine pregnancy and with ovarian disease in the most puzzling and confusing manner. It would take a special treatise to point out and consider the possible combinations. But still a careful bi-manual examination of the pelvis and some sense will guide the most inexperienced past blunders almost as successfully as the greatest experience, unless combined with the greatest care and caution. When in doubt wait, at least till the possibility of *normal* pregnancy can be eliminated, then, if possible, have the tumor removed, except it be evidently an extensive cancer.

7. *Uterine Tumors—especially Sub-peritoneal.*—These are not very liable to be mistaken for extra-uterine pregnancy except the latter is suspected to be in the early stage. Consequently it is the small tumors which are most liable to be suspected. But these are always solid and have none of the semi-fluctuating feel of an ovum in the Fallopian tube. Then, they do not increase at the rapid rate an ovum does. The history usually has a longer duration than would be com-

patible with extra-uterine pregnancy, and is marked usually by metrorrhagia for a length of time. In fact, it is hard to understand how with bi-manual examination and a little common sense and prudence, a mistake could be made between the early stages of extra-uterine gestation and a small fibroid of the uterus. Sometimes, indeed, the metrorrhagia of extra-uterine foetation previous to rupture might suggest myoma, but the myoma would be an interstitial one or a polypoid, so that a tumor at one side and behind the uterus, of a painful, fluctuating nature, could hardly be twisted into a myoma causing the metrorrhagia.

But it is a very different story in the advanced stage, or rather when extra-uterine gestation is over and the remains of the foetus are consolidated into a mass close to the uterus, when all liquor amnii has been absorbed and nothing characteristic of a foetus remains. This constitutes one of the most difficult cases for diagnosis which could be imagined, and a diagnosis of fibro-cystic tumor of the uterus is very generally made, often as a mere cloak for conscious ignorance. Those who, on paper, can diagnose anything should listen to the words of Parry on this subject: "Although from the careful perusal of numerous histories of cases of this nature some degree of facility of distinguishing their presence may be acquired after a certain period of their duration, and of deciding even, in *occasional* instances, on the particular variety of such pregnancies, yet assuredly every practitioner who *has attentively studied the subject* must admit the distinction to be a task of no ordinary difficulty." In fact, often there only remains the exploratory incision as a means of diagnosis.

8. *Pelvic Hæmatocele*.—In the vast majority of cases it must be impossible to distinguish between a hæmatocele arising from other causes and one the result of the rupture of an extra-uterine gestation. As the accident always takes place before the fourteenth week, the ovum, or foetus, if the ovum be also burst, is too small to be felt among the clots in the pelvis. Fortunately, the differential diagnosis between simple hæmatocele and that from ruptured tube is not of the least consequence to treatment, or in any other respect, except perhaps in the case of an unmarried woman it might be important to the friends to know whether she had been pregnant. Intra-uterine hæmatocele is always attended by "abdominal collapse," and the bleeding goes on, except in rare instances, to death, unless the bleeding vessel be

tied, just as if the hemorrhage proceeded from the trunk of the femoral artery, no matter what the cause may have been. Extra-uterine hæmatocele may be attended by fainting, great pain, etc.; but in the great majority of cases, without any regard to the cause, the affection is without danger, and does not require any active interference. We may, if we please, try to find out whether an extra-uterine pregnancy was not at the bottom of the accident, as in the case of the extra-peritoneal variety we have the danger of the pregnancy still going on and giving any amount of trouble later. But if we have any doubt in the world, we will reserve our opinion for a few weeks, when the secret will be revealed. As regards the intra-peritoneal form attended by collapse and death, if not relieved, we may consider all cases, in the first instance, as due to ruptured tubal pregnancy, and we shall rarely be wrong. In the extra-peritoneal, the history of pregnancy may be so clear as to make the diagnosis for us at once, as soon as we ascertain that the foetus is not inside the uterus. Or, on the other hand, the history may leave us just where it found us, or rather mislead us from the truth. Where pregnancy was possible, or likely, it will be well to await the progress of events before committing ourselves.

9. *Pelvic Inflammatory Exudations and Abscesses.*—The same remarks exactly as were made of hæmatocèles will apply here. No man can tell, in many cases, whether an inflammation or a suppuration in a broad ligament be due to a ruptured tubal pregnancy causing hæmatocele or not. Abscess of the broad ligament is the same disease, from whatever cause it may arise. And till it makes its way to the surface, and *till bones appear in the discharge*, the diagnosis is impossible—then it is self-obvious. Fortunately, as in the case of hæmatocele, it does not matter as to treatment in the very least.

10. *Cancer of the Pelvis or Peritoneum.*—This is a most important topic, as most of the glaring blunders have been in mistaking a pelvic cancer for a foetus retained beyond full time. Strange to say, Thorburn is the only author who dwells on this point, except Tait in his recent lectures on ectopic pregnancy. Thorburn has seen four or five cases where cancer has been taken for a foetus, even by intelligent practitioners, yet he denies that the diagnosis is difficult. It is another illustration of the general absence of correct management of the understanding, or of trusting to one symptom. Malignant tumors, as we all know, are liable to assume the most fantastic shapes; the irregu-

larities of surface, sharp projections and indentations may certainly very closely resemble a child, or the hand, foot, head, leg, arm, or ribs of a child. They imitate the shape and features of a foetus more closely than any other known condition. But because there is a tumor like a foetus in the abdomen, is that any reason for declaring it to be a foetus, when an examination into the family and personal history of the patient would, in most cases, correct the fancy started by the shape? Tait tells us that a slow-growing cancer of the ovary, or behind the uterus, or elsewhere, in the neighborhood, might be difficult to diagnose, if mere pelvic examination were depended on, from extra-uterine gestation of long standing. The history, however, would here come in as a great help. The rate of growth would probably be steady, or, if a rapid period set in, the temperature would decide; for the only condition which could possibly cause a rapid increase in the size of an extra-uterine gestation sac would be suppuration; this a study of the temperature would soon reveal. As usual, the history of the case, supposing it to be an extra-uterine gestation, is not to be depended on, as it is more likely to mislead than to guide. Tait tells of a case of this kind where a man undertook an abdominal section on the supposed history of an extra-uterine gestation of long standing. In this case there was nothing distinctive about the shape of the tumor, so he had the less excuse. Upon making incision, the tumor was found to be a mass of cancer of the omentum, adhering to everything and involving everything (*op. cit.*, p. 69). This was one of those blunders which are worse than a crime as regards the reputation. The moral of all this is, never put your trust in any one mode of diagnosis, not even the most certain of all, in most cases, pelvic examination. Always have something else to corroborate any theory you may form before you let it past the stage of a working hypothesis, and above all, never *trust* the history of an extra-uterine pregnancy. Other histories, as of cancer, may be of the utmost moment, and often the history of extra-uterine pregnancy may be as clear as the day; but, again, the history may be worse than useless—the history on either side, either for extra-uterine pregnancy, or against it.

SUMMING UP ON THE DIAGNOSIS OF EXTRA-UTERINE PREGNANCY.

In the early stages it is seldom diagnosed from normal pregnancy, or from anything else, as in the great majority of cases rupture is the

first sign of *anything* being wrong. Many cases could be diagnosed were opportunity given, with great ease, but Tait is probably correct in saying that were opportunity given we should fail fifty times for once that we could be *certain*.

The cases in which the ovum survives rupture do not present any very distinctive features until the foetal heart is heard. From that time till some little time after foetal death, diagnosis is extremely easy. In cases where the ovum perishes at rupture, the results of rupture, extra-peritoneal hæmatocele or pelvic abscess, have absolutely no features by which they can be distinguished from cases in which the hæmatocele or abscess is due to other causes. If abscess forms and discharges, when foetal bones appear the diagnosis is self-evident.

When, after false labor, the foetus dies and the liquor amnii becomes absorbed, the difficulties of diagnosis begin again. And though a certain proportion of cases is almost self-evident as to diagnosis, it may justly be said that there is another and larger class where human skill and wisdom at their best can only guess. It is under such circumstances that the diagnosis from ovarian and other cysts, myoma of the uterus, pelvic cancer, etc., is likely to arise. Contrary to all authors, except Tait, I would impress on the practitioner that whatever may be trustworthy, the *history* of extra-uterine pregnancy is not so. That is to say, sometimes it may point almost without leaving room for doubt to what is the matter; but, perhaps, in a larger number of cases it only leads astray. The form of the tumor also must not be depended upon. Indeed, according to Prof. Thorburn, when the form of the tumor strongly suggests a foetus, it is almost sure to turn out to be something else. He says that in *every case* in which he has been consulted regarding a supposed encysted foetus the diagnosis turned out wrong (*op. cit.*, p. 438). He says (*ibid.*), that he showed one such to many experts, the late Dr. M'Clintock among the number, at the time of the meeting of the British Medical Association in Manchester in 1877, with what *appeared* to be a *perfectly typical history*, and the only difference of opinion was as to the exact position of the child. Yet it turned out to be a case of pelvi-abdominal cancer.

PART II.

TREATMENT.

As the treatment of extra-uterine pregnancy varies so much according to the period at which gestation has arrived as well as according to the state of affairs at the time the woman comes under observation, we must divide the subject somewhat as we have done the subject of diagnosis. It seems most convenient to discuss the treatment under three principal heads: First. Treatment up to time of rupture and shortly after rupture; Treatment under the various circumstances which may occur from rupture up to full term and death of the foetus; Treatment after the death of the foetus. Of course, these three principal divisions have to include many subdivisions.

TREATMENT OF EARLY EXTRA-UTERINE PREGNANCY, AND WHEN RUPTURE OCCURS.

Of all the revolutions in medical or surgical science, perhaps the most rapid and the most thorough has been in the treatment of ruptured extra-uterine pregnancy.

Even so recently as 1876 (when Parry wrote) the reform had not commenced. In fact, Lawson Tait's first operation proved a failure in June, 1883. Yet in five years what a change! A new operation has been established in that time. As much progress has been made in the last five years as has often taken a thousand to accomplish. Not only is the operation established, but taking the mortality from the very start it has only amounted to 4.7 per cent.! The whole thing is wonderful. To think how women have been allowed, since the earliest times, to die of hemorrhage owing to a ruptured tube, without an effort to stop the bleeding, although most strenuous efforts would have been made to have arrested almost any other bleeding. Nay, operations many times as dangerous as abdominal section would have been considered the imperative duty of any surgeon in charge of almost any other variety of hemorrhage. The principle, indeed, was often enough

asserted, but no one attempted to carry it out. In 1866 Bernutz and Goupil (p. 202) laid down the law that, "The indication in such a case is plain—we must stop the hemorrhage." But Blundell, writing in 1830, said he did not doubt that "many women die in this way, but being buried without examination, the real cause of their death is never ascertained." He even suggested the expediency of abdominal section for the control of the hemorrhage. Since his time many writers touched on the subject, but no one ventured to put the suggestion into practice until Mr. Tait (*Lancet*, October 26th, 1886; also, "Diseases of Ovaries," p. 348) broke the ice. Before 1883 the mortality was practically 100 per cent. No one can dispute the general surgical principle, "For surgical hemorrhage, cut down and tie the bleeding point," and, as Tait puts it, "If a big branch of the femoral artery were bleeding, my colleagues would cut down on and tie it. Why should Poupert's ligament be a line of demarcation within which this writ will not run?" His colleagues would tie the external iliac artery for an aneurism, why should he not tie the internal branches of the same artery? The operation comes under the category of life-saving procedures, like tracheotomy, but it has also other sufficient reasons to justify any operation, unless the mortality greatly exceeded 4.7 per cent. within the first hundred cases on record. As Professor C. A. L. Read, of Cincinnati, says, "The damaged tube, the foetal structures and the placental tissue demand attention. Of the latter two, it may be said that, even though they become incapsuled and partially absorbed, and the residue remains innocuous, they cannot be considered, in a surgical sense, other than as foreign bodies, and as such, constant menaces to the health and life of the patient. The tube, of necessity, becomes destroyed as an oviduct, and, if left intact, it will only be to figure at a later period as a hæmato-salpinx or a pyo-salpinx, and consequently to demand extirpation."

To show the general opinion that the rupture of an extra-uterine gestation has always been considered fatal when left alone, we may quote Parry, the best and most recent writer on the subject, previous to the revolution in treatment: "From a careful examination of this subject, it must be acknowledged that a happy termination of the rupture of the cyst is exceedingly rare. Of 149 cases in which the ovum was located in that portion of the tube which does not traverse the tissues of the uterus, 145 died." Or, four cases were supposed to sur-

vive out of 149; now, out of 43 similar cases operated on, two died. Indeed, no one could make out a more powerful case for operation than Parry did. He pointed out that so few recovered, that all hope of such a result should be dismissed in considering the treatment. He pointed out that Albucasis was probably the first to describe extra-uterine gestation, in the middle of the eleventh century. That, "since then men have, doubtless, watched the life ebb rapidly from the pale victim of this accident as the torrent of blood is poured into the abdominal cavity, but have never raised a hand to help her. Surely, this is an anomaly, and it *has no parallel in the whole history of human injuries*. In the whole domain of surgery—for we cannot look to other than surgical measures under the circumstances—*there is now left no field like this. In this accident, if in any, there is certain death*. The only remedy that can be proposed to rescue a woman is gastrotomy—to open the abdomen, tie the bleeding vessels, or to remove the sac entire." He thinks the first suggestion of this operation came from an American, Dr. Harbert, but to Rodgers belongs the credit of reducing the arguments to order, and advocating it. Since the time of Rodgers, laparotomy has been advocated by Meadows, Hewitt and Greenhalgh, in a discussion before the Obstetrical Society of London. Koeberle, Behier, Schroeder and Atlee countenance it, but only one performed it until 1883. Why? Parry says: "The great impediment to the adoption of this treatment is the uncertainty of diagnosis." Is this true? Decidedly not. The greatest tyro can diagnose the case sufficiently closely to tell him that it means operation or death. We have no clearer clinical entity than "abdominal collapse,"—the signs of peritoneal hemorrhage. I think, the bugbear was the fear of opening the peritoneum, that sacred ground on which the surgeon dare not trespass on pain of death to his patient and his reputation. Had it not been for the success in other abdominal operations, ovariectomy especially, I dare say we would still have been where we were in 1883. The antiseptic system, although it has had its day, will always deserve the blessings of mankind, as it gave surgeons the courage and moral support necessary to introduce abdominal surgery, and all the further advances which abdominal surgery has already led to, and will yet lead to. 1878 saw the beginning of the particular advance we are discussing, when Tait laid down his famous law, that when a patient was "in danger of death from conditions within the abdomen which do not

seem to be clearly of a malignant nature, but a correct diagnosis of which is impossible, I open the abdomen, and at once make the diagnosis certain, and a successful treatment possible." Tait points out that exploration, as a principle, is nothing new. He has seen a baronet and court surgeon remove a female breast for a tumor, and then found it was only an abscess. Here the exploration was made after the needless mutilation. Any medical man could relate dozens of such incidents, showing that complete accuracy of diagnosis is to be had in no region of the body, and that exploration is a sound, surgical principle not half enough cultivated. Tait adds, "*Absolute accuracy of diagnosis in the abdomen is very far from being possible; only the ignorant assert that it is, and only fools wait for it.*"

Some other objectors to abdominal section for ruptured extra-uterine pregnancy argue that death occurs so suddenly that in the majority of cases there is no time for operation, that the patient is too much exhausted, or moribund, before being seen. This may be true of some cases, but, as medical literature of recent date proves, it is not true of the majority. Recent medical literature swarms with cases of extra-uterine gestation; by reading these we can plainly see that, as a rule, there is time enough to obtain the services of a couple of local men, at least; and as I argued elsewhere, every country surgeon must study the matter up and prepare *his mind* to do the operation when absolutely necessary, as he has to operate for a strangulated hernia—a much more difficult and dangerous procedure. Tubal pregnancy cannot be so rare as some writers would persuade us, as between 1883 and 1888—just five years from first operation till last one published—Tait has operated on 42 cases and saved 40 lives otherwise as good as lost ("Lectures on Ectopic Pregnancy," pp. 44 and 45).

Again, at a recent meeting of the Obstetrico-Gynæcological Society of Vienna, Prof. Edward Hoffmann showed specimens of a number of cases of tubal pregnancy in which death had occurred through rupture (*Brit. Med. Jour.*, Oct. 20th, 1888, p. 903). His analysis of these cases will prove useful in many ways. He shows that: Among the eight cases, six were over 30 years of age; in one instance the age could not be determined; one of the patients was 22 years old. As to one of these women, it could be stated with certainty that she had already been confined; in another this was doubtful. In two cases the right Fallopian tube, and in six instances the left was the seat of

pregnancy. In three cases the pregnancy was situated in the middle, and in the rest of the cases in the internal portion of the tube. In half the number of the cases peritonitic adhesions of the Fallopian tube were detected. In seven cases rupture occurred in the second, and only once in the third, month of pregnancy. In one-half of the cases, several hours (*from four to twelve*) elapsed between the first symptoms of the disease and the death; in the rest of the cases, however, it was questionable whether death occurred suddenly, as there was no exact history of disease. It is thus obvious that in most of the cases, provided the diagnosis is made in proper time, there is sufficient time for surgical interference. In two cases of fresh hemorrhage the foetus was already found in a macerated condition; this, as well as the strong torsion of the umbilical in one of these cases, led the author to conclude that the *critical hemorrhage* did not in *all instances* occur simultaneously with the *rupture of the Fallopian tube*, but *only at a later date*. That is to say, that in some of the cases the death of the ovum preceded the rupture of the tube.

Now, as I intend discussing the proper treatment in the very few cases in which a clear diagnosis is made before rupture of the tube, in some detail, I cannot do better than start with this important remark.

TREATMENT OF EXTRA-UTERINE PREGNANCY BEFORE RUPTURE,
SHOULD A DIAGNOSIS BE MADE.

Of late it has been held in many quarters that the proper treatment of such a case is to kill the foetus in some way, so as to cut the gestation short and allow the whole ovum to be absorbed.

Now, the remark that in some cases the ovum died before the rupture took place suggests a consideration of the utmost importance, at the very beginning of our inquiry. If rupture takes place after the ovum is dead, does this not seriously diminish the value of any plan of killing the foetus? Nay more; as in intra-uterine pregnancy the death of the ovum is the great cause of abortion, may not our killing the foetus bring on efforts at its expulsion, the false abortion which ends in rupture of the tube? In fact, may not the killing of the foetus promote the very accident we are so anxious to guard against? Of course, it may be urged on the other side that as it is bound to end in rupture sooner or later, the sooner the better, as the ovum will be

the smaller the earlier the rupture. But do ruptures at three weeks or at two months give at all better results than those occurring at twelve or fourteen weeks? The latter question may at once be answered in the negative. Rupture at three weeks is just as certainly fatal as at three months.

In *Centralbl. für Gynäk.*, No. 15, 1888, is a case by Dr. J. Zucker, of Berlin, of an extra-uterine gestation which ruptured at the third week of its development, and where the tubal sac was "at most" not larger than a hazel-nut. No disease of the tube could be found. This is a well observed case operated on by an authority (Dr. Veit), and confirmed by a post-mortem examination, yet the rent was two inches long in the right tube, and the abdomen full of blood, four litres being removed at operation. The entire minute ovum was found; so there is not a point wanting.

Again, Dr. Parish (*New York Med. Jour.*, Oct. 20th 1883, p. 447) reports a case, also confirmed by post-mortem examination, where a woman six weeks after last menses had slight metrorrhagia; eleven days later, sudden onset of intense pain with great prostration, a mass in Douglas' pouch pushing the uterus forward against the pubes. He diagnosed ruptured tubal pregnancy with internal hemorrhage. The diagnosis was disputed. Death in thirty hours. He adds, the autopsy showed the cyst could have been removed. So here we have a case in the second month just as fatal as one in the fourteenth week. Many such cases could be cited from recent literature, and, too often, proved by autopsy, so we may consider the point proved. But what evidence is there as to the death of the foetus producing rupture of the tube? I may confess at once that the idea is theoretical, that there is no evidence further than deductions from finding that in some cases of early rupture the ovum had been already dead for some time, and the reasoning from the parallel case of a dead ovum inside the uterus causing its own expulsion. Barnes says (*Brit. Med. Jour.*, March 10th, 1888, p. 536) that when "shreds of decidua have been extruded with uterine hemorrhage the process of abortion had fairly set in. In the case of *uterine* abortion we might safely wait, aiding nature according to her indications. But nature's course in the case of tubal gestation was to *burst the sac*, and that might be fatal. At this point rupture might be prevented by puncturing the sac, thus relieving tension by drawing off the liquor amnii and any blood which

might have been effused into it." At the same discussion the idea was mooted in a more direct way by Dr. Imlach, but he brought forward no evidence in support of his theory. He said (*ibid.*) "that opinions had advanced much since 1872, when he had seen a case at full time allowed to perish for want of surgical interference. He maintained that the same thing happened in a tubal pregnancy as did inside the uterus when the foetus died. As soon as the child died, or was killed, the tube endeavored to discharge its contents, and, as it could not possibly pass into the uterus, the tube was ruptured. If they killed the foetus they were doing the best they could to bring about the very thing they wished to avoid. Let it go on to full term or to rupture, and then operate at once. Laparotomy could always be performed *almost without danger*, so that there was really no excuse for killing the foetus. He considered it to be an absurd piece of meddlesome gynæcology."

This is all the evidence I can find in favor of the idea that killing the foetus may cause rupture of the tube sooner than it would naturally occur; but as I am not prepared to throw overboard wholesale all the evidence as to ova killed by puncture, electricity, etc., so I cannot place theory against facts, and I therefore abandon the idea as non-proven. I think it worth further investigation, however, as there may be peculiar circumstances under which death of the foetus will provoke rupture of the tube. There are many other objections to all plans of killing the foetus, such as those of Tait: that it is immoral, that the foetus has a right to its chance of life, that the placenta may grow after foetal death, that death of the foetus does not free the woman from the many dangers which encompass her, that there is neither certainty nor safety in any of the methods employed to kill the foetus, that most of them—all those involving any puncture—are actually more dangerous than an abdominal section, and that they will commend themselves only to those who lack courage and skill to obtain good results by abdominal section. The last objection is, I think, the principal reason for further research to determine whether there is any good in killing the foetus. We cannot have a Lawson Tait within reach of every poor woman in every small town or country village, and if there is any easier way of relieving her of the danger which an extra-uterine pregnancy involves, it would be well that all should know of and be familiar with it. We will discuss Tait's objections

presently, and try to ascertain what truth there may be in them ; but, first, I would remark that most of his objections only apply properly to killing the foetus at a rather advanced period of pregnancy, whereas these methods are intended specially for the pre-rupture stage. But, unfortunately, no matter what treatment may be discovered for the pre-rupture stage, it could only be of the most limited advantage, as in the vast majority of cases the rupture itself is the first warning of anything being amiss.

To take Tait's objections in order: I will not waste words on the objection that killing the foetus is immoral, as in the stage of which we are considering the treatment the chances of life for the foetus are so poor that they cannot be much worse. The great majority die at rupture (with the mother or by the operation to save her life). Then of the cases where the rupture is into the broad ligament (calculated at one-fourth by Tait), the great majority of this minority die either at the period of rupture or, at all events, before the viable age. So that, as Tait himself says, it is only the minority of the minority which reaches the viable age. Then, suppose we could nurse an extra-uterine pregnancy to the viable period, would it really be so very moral and praiseworthy, if we could terminate it safely, to continue a pregnancy so fraught with danger to the mother? I certainly do not see the immorality of killing the ovum before rupture. Next, as to the danger of the placenta continuing to grow after foetal death. At the debate at the British Gynæcological Society, Feb. 8th, 1888. where Tait made this assertion, Dr. Inglis Parsons said that there was no evidence to show that the placenta would continue to grow when the child had been killed by electricity. The force that could produce the death of the foetus was not likely to leave the placenta untouched. No subsequent trouble had resulted in the twelve cases mentioned by Dr. Garrigues. (The paper of Dr. Garrigues is to be found in the "Transactions of the American Gynæcological Society" for the year 1882.) Now, I do not know that there is any evidence of growth of placenta after the killing of the child *by electricity*. But if Dr. Parsons meant that there was no evidence of the growth of the placenta after foetal death generally, he was mistaken. Unless we are prepared to deny the truth of reports by Tait, he has proved it over and over again. In his "Lectures on Ectopic Gestation," 1888, pp. 73, 74 and 75, quite a number of cases are reported. He says it is a

frequent experience to find a large placenta embracing a small ovum cavity without any, or with only very slight, traces of a foetus in the so-called uterine "moles." If this be so, we have no reason to do other than expect that the same thing will occur occasionally in tubal pregnancy. But Tait is not alone in this belief. Dr. Champneys and Mr. Thornton gave evidence to the same effect before the London Obstetrical Society, and Hart and Barbour, in their "Manual of Gynæcology," 1882, have the following:—

"Case of extra-uterine gestation, with death of the foetus, but *continued growth of the placenta*, which led to fatal hemorrhage." A. B., æt. 24, had passed two periods without menstruating; three months ago she began to have irregular hemorrhages three times a month, and in considerable quantity. The tumor was found in the pelvis, the vagina being compressed against the pubis, the cervix reaching above the brim, and the bladder displaced into the abdomen. The tumor was as large as a uterine pregnancy of $4\frac{1}{2}$ months. After a puncture of the cyst with the aspirating needle, the patient died, with symptoms of internal hemorrhage, and on a post-mortem examination, by freezing the pelvis and cutting sections, the uterus was found to be $5\frac{3}{4}$ inches long, the fundus being five inches above the symphysis, and the cervix so drawn up that the fornices are obliterated. The gestation sac lay in the pouch of Douglas, and was chiefly occupied by the placenta. The cavity of the amnion contained but little fluid, and the foetus was about the size of a three months' pregnancy. The continued growth of the placenta after the death of the foetus had led to fatal hemorrhage.

We have thus evidence enough of the growth of the placenta after foetal death. But Tait himself admits when describing the way he now deals with the large placenta found in operations at full time or after, viz., cuts off the cord close and hermetically seals up the wound, that the placenta, as a rule, is the first of the tissues to disappear by absorption, notwithstanding the numerous cases in which it grows for a time after foetal death ("Lectures," p. 97). Still, if the placenta grew for a time after the child was killed in the pre-rupture stage, it would be enough to cause rupture of the tube. The next objection to killing the foetus is that it does not save the woman from the many dangers which encompass her. This is such a good argument in the advanced stages of pregnancy that I wonder any one persists in the killing plan

after, say, seven months. We find that in the vast majority nature kills the child, yet is the mother any better except once in a hundred cases? The child dies naturally in all cases at some time or other, yet we have serious troubles after that. Indeed, when the foetus dies some time after rupture, inflammation and suppuration in the sac usually take place followed by elimination of the foetus as a foreign body. It is a very rare case, indeed, where this does not happen sooner or later.

But I do not think this argument would have much weight against killing the foetus before rupture, or even shortly afterward, as we know that should the foetus perish shortly after rupture, as a rule, foetus and blood are alike absorbed, as in simple hæmatocele, so much so, indeed, that it is impossible to tell a hæmatocele from ruptured tubal pregnancy from one due to other causes. The next argument against the various methods of killing the foetus is that there is neither certainty nor safety in any of them. This is confessed by their advocates. There is ample proof of this proposition. The horrible case of Matthews Duncan, in the Bartholomew's Hospital Reports for 1883 proves it by itself. In this case the induced current as strong as the faradic coil of a Coxeter's combined battery could give was used first. The positive pole placed over the abdomen and an insulated negative pole in the vagina. The current was passed and withheld during spaces of two seconds for a minute and a half. Then the continuous current from forty modified Leclanché cells was passed for six minutes, producing slight blistering of the skin and a dried surface in the vagina. Heart sounds audible. Next day two grains of morphine were injected into the amniotic cavity. Then the liquor amnii was drawn off through the abdominal wall by the aspirator; eight ounces were removed. Heart sounds still audible. Five days later, $\frac{1}{4}$ grain morphine was injected two inches deep into the body of the foetus at the place the heart was best heard. This was twice repeated at intervals of two days without any result. Galvano-puncture was now used. Two insulated electrolysis needles were passed an inch and a half into the tumor and connected with the negative pole; positive pole to abdomen. A current from forty Leclanché cells was passed for six minutes with some interruptions. After all the foetal heart was beating, but more slowly. The foetal heart still being audible four days after all this, the tumor was aspirated again, and thirteen minims of equal parts of

water and a hypodermic solution of morphine injected again into the body of foetus over the heart. Foetal heart ceased at last. The patient died two days afterward, which no doubt was not surprising to the great gynæcologist who conducted this horrible series of experiments. At the autopsy, twenty-six hours after death, the contents of the cyst were very fetid, the soft parts of the foetus completely macerated; the bones exposed. The foetal internal organs were diffused in the fetid fluid or were semi-fluid. Such a case is a serious impeachment of all the modes of killing the foetus. Here they were all combined to a sickening and fatal extent. The child was only killed two days in advance of the mother, whereas, as Tait points out, both might have been saved by following the ordinary principles of surgery. A few more such victories as this and all these methods would be lost. If this case had occurred since Tait demonstrated the comparative safety of laparotomy, we would consider this case of Matthews Duncan a disgrace to the medical profession; but even in 1883 the peritoneum was still a sacred ground from the knife, though not at all sacred from the needle and the trocar, which are, at least, quite as dangerous. The last sentence contains the next argument against foetus killing. The trocar or aspirator in the case of abdominal diseases may tell that the tumor contains serum, blood or pus, but how much is the information worth? Then as to treatment, for what condition could the aspirator be considered final and sufficient treatment? Hear Parry on the use of the trocar in extra-uterine pregnancy. "In cases of doubt, the foetus being dead, the trocar has been used to draw off some liquor amnii in order to confirm the diagnosis. *Unless it has been decided to operate immediately* for the removal of the foetus, the use of the trocar is utterly unjustifiable. A few, but *very few, women have long survived its use.* Jonathan Hutchinson has had fatal peritonitis after its use. He says the practice "is itself attended by great danger; nor shall I deal honestly with you or myself if I do not candidly admit that, with due care and patience, I do not think that paracentesis ought to be necessary in a case of foetal tumor simulating ovarian dropsy." We have now reached the end of Tait's objections to the process of foetus killing, and will next inquire into the various plans proposed, a little in detail.

METHODS WHICH HAVE BEEN RECOMMENDED FOR BRINGING THE PREGNANCY TO AN END IN THE PRE-RUPTURE STAGE.

Puncture of the Sac.—This is said to be easily effected by the introduction of an exploring trocar through the vaginal or rectal wall. The aspirator needle has also been employed. But supposing the diagnosis made, it might not be so easy as it looks to put the needle into a freely movable tumor, or, at least, to put it into such a position that it would draw off the liquor amnii. Then suppose the liquor amnii drawn off, is the foetus sure to die? We have the authority of Dr. G. Ernest Herman, Obstetric Physician to London Hospital, for it, that he has known it to be drawn off in cases of five or six months' pregnancy without killing the foetus, and there seems no reason why it should be more fatal in the early stage. But if harmless to the foetus, it can hardly be said to be harmless to the mother, and it has not been attended with very brilliant results in any sense. Parry says mothers rarely survive it long, but recoveries have been recorded by Tanner, Stoltz, Jacobi, Koeberlé, Greenhalgh, and E. Martin (two cases). However, death from peritonitis and septicæmia have followed this "simple" procedure in the hands of Sir J. Y. Simpson, Martin, A. Simpson, Braxton Hicks, Thomas (two cases), Conrad, Routh, Netzel, Hutchinson, John Scott, Gallard, and Depaul. Fränkel ("Zur Diagnostik und operativen Behandlung der Tubenschwangerschaft," *Arch. f. Gynäk.*, Bd. XIV, p. 197) drew off nearly three-fifths of an ounce of liquor amnii without killing the foetus. Then the blood vessels in early extra-uterine gestation sacs enter from below, so that they are in danger of being wounded, and recent literature shows many cases of hemorrhage following tapping. In the more advanced stages of pregnancy the danger of tapping is very great. In the case of Dr. Braxton Hicks, the patient died, when four months pregnant, of internal hemorrhage, the result of an attempt to kill the child by puncture. In *Brit. Med. Jour.*, Dec. 1st, 1888, is a case to which I have already referred for another point. The patient recovered after laparotomy at the fourth month. As the urine gave half its bulk of albumin, it was urged at the consultation that under the circumstances it would be wiser to tap. The operation showed that tapping would have been fatal through hemorrhage. If the patient had by a chance survived, she would have been rendered so much the worse by the complication of an intra-peritoneal hæmatocele. But modern experi-

ence seems to decide that this condition is always fatal by repeated hemorrhages, if not the first time, so that the woman would have been practically killed by this "simple" and "harmless" operation. The truth is that people without the skill or courage to do an operation will have the audacity to plunge a trocar in anywhere, because it is not usually attended with immediately fatal results. However, in Jordan's case, such was very nearly the result, as two hours after the tapping "complete collapse came on" (Tait's "Lectures," p. 69). Therefore, puncture is, as Tait maintains, quite as dangerous as abdominal section. The result is at least uncertain; indeed, it has never been properly proved to be efficient. Certainly cases are recorded in which a pelvic tumor has been tapped, some serum has been drawn off, the tumors have subsided, but anything like positive proof that these tumors were extra-uterine pregnancies is wanting. Such is the proceeding which, even in the present year (*Brit. Med. Jour.*, March 10th, 1888, p. 536), Dr. Robert Barnes, "with the present state of knowledge, prefers to *all others*," reserving, of course, the right to proceed to ulterior measures, according to circumstances. He distinctly says: "In the early stage, before bursting of the sac, electricity to kill the fœtus might be applicable; but simple puncture of the sac was better." Dr. Routh (*ibid.*) agreed with Dr. Barnes. If electricity is really worse still than simple puncture, I should say it will not remain long in fashion. Barnes declares it is probable that the puncture which forms a part of the other methods, such as electricity and morphine injections, is by itself adequate to account for the successful results. If so, so much the worse for the other methods. Playfair joins in the opinion that the good results of the other plans would follow on simple puncture. One of the arguments used in favor of simple puncture by aspirator is that, in case of a mistake in diagnosis, it would do no harm. This is very different from Parry's view that few women survived it (*op. cit.*, p. 204).

Injections of Solutions into the Sac.—Joulin ("Traité complet des accouchements," p. 968) proposed that, in addition to puncture, we should inject something into the sac which would destroy the fœtus. He must have had his doubts about the efficiency of simple puncture. He proposed injections of sulphate of atropia, one-fifth of a grain dissolved in a few minims of water, by means of a hypodermic syringe, with an extra long needle. His idea was carried out successfully by

Friederich of Heidelberg (Cohnstein, "Beitrag zur Schwangerschaft ausserhalb der Gebärmutter," *Arch. für Gynäk.*, Bd. xiv, p. 355). He recommended that the needle of the syringe should be put into the sac through the abdominal or vaginal walls, a few drops of fluid first withdrawn and then the poison injected. Friederich preferred a fifth of a grain of morphine. He repeated the dose every second day, until the diminished size of the sac gave proof that the foetus had been destroyed. This he did in two cases. There was but slight inflammatory reaction, and the woman did not seem affected by the morphine.

Rennert ("Extrauterin-Schwangerschaft im fünften Monate," *Arch. für Gynäk.*, vol. xxiv, p. 266) afterward killed a foetus in the fifth month of extra-uterine pregnancy by one injection of half a grain of morphine. The patient had a severe illness, but finally recovered. In the Gynæcological Section of the Eighth International Medical Congress at Copenhagen, Koeberlé related a case of advanced extra-uterine pregnancy where morphine was successful. The woman made a complete recovery, the placenta and child being both completely absorbed. But Hennig reports also a case operated on by Koeberlé, where profuse hemorrhage occurred. He does not state whether the case was fatal. ("Die Krankheiten der Eileiter und die Tubenschwangerschaft," p. 138.)

The slight successes scored by this method of injecting poisons into the gestation sac, are surely more than counterbalanced by the horrible case of Dr. Matthews Duncan, already referred to. Any one who is at all inclined to favor this mode of treatment, should read the full account of the case which is to be found in "St. Bartholomew's Hospital Reports," vol. xix, 1883, p. 35. My opinion is that this one case is quite enough to discredit the morphine treatment altogether. It was by accident more thoroughly tried than the experimenter wished, as, on one occasion, two grains of morphine were injected in mistake for one-fifth of a grain. The only perceptible effect of this large dose was on the mother, whose pupils contracted and who felt drowsy.

Elytrotomy.—Thomas's operation of cutting from the *vagina with the cautery knife*.

Professor Gaillard Thomas relates a case where he cut into the sac with the red-hot knife attached to the electric-cautery. He very nearly lost the patient, but she finally recovered.

Lately, he recommends Paquelin's cautery at a red heat. After slowly cutting the sac, he recommends that the foetus should be removed, the placenta being suffered to remain. The sac is then to be filled with antiseptic cotton, and this renewed once in thirty-six hours. He recommends the operation only where instant action is required, owing to urgency of symptoms. I do not see on what possible grounds this operation could be preferred to simple abdominal section. It seems much more difficult, and could not possibly give better results. It has been spoken of by an authority as "a bold and judicious proceeding." But I quite agree with Dr. Herman (*Lancet*, May 26th, 1888, p. 1022), in thinking it "bold, but not judicious." As Herman remarks, the operator cannot tell into what tissue he is pushing his incandescent knife, and, should the diagnosis be erroneous, the operation would be likely to prove fatal. Dr. Herman ("Delivery by the Vagina in Extra-uterine Gestation," "Trans. Obstetrical Society, London," 1887) has collected the cases of this operation, and criticises it very severely. His conclusions are fatal to the operation at any stage of pregnancy, but here we are only concerned with it previous to rupture. He characterizes the opening of an extra-uterine gestation sac from the vagina early in pregnancy, before rupture has taken place, by the cautery knife or otherwise, as "a dangerous and unscientific" proceeding. Abdominal section should always be preferred. It seems a mere vagary of one in search of novelty or notoriety. Tait is equally opposed to it.

Electricity.—This agent, in various forms, has been used to kill the foetus in extra-uterine pregnancy since the middle of the present century. The suggestion came from France. Moreau states that Voilemier was the man who first proposed it. Cazeaux also made the suggestion ("Midwifery," 1850) by inquiring whether the death of the foetus were not possible by passing an electric shock through the uterus. Bachetti, an Italian, was the first to actually make trial of the suggestion. He introduced two needles into the tumor and connected them with the wires of an electro-magnetic machine, on February 2d, 1853. He reported pregnancy arrested after two shocks. This was a case of galvano-puncture. Dr. Braxton Hicks was the first to try electricity minus the puncture. In 1866 he applied one pole to the abdominal prominence of the tumor and the other in the vagina. He abandoned the plan after two trials, resorted to the favorite puncture

from the vagina, and killed his patient. Dr. J. G. Allen, of Philadelphia, was the first to employ electricity, minus the fatal puncture, with success. His first case was in 1869, three years after Braxton Hicks had failed by the exact same method. Dr. Allen had another successful case in 1871. Electro-puncture had succeeded in shriveling up the ovum in a case of tubal gestation in the hands of Burci in 1859. He used two acupuncture needles. Dr. Allen did not report his successes. It seems the story transpired incidentally, at a debate before the Obstetrical Society at Philadelphia. As in the case of so many inventions, it was forgotten for a time, or, at least, no more was heard of it until, in 1877, Drs. Landis and Lovering had a successful case. Since 1877 the practice has become quite a favorite among American practitioners, and it may, at once, be admitted to be a vast improvement on the puncture in any of its shapes or forms. It would be very hazardous to say that in case of error of diagnosis, electricity, even minus the puncture, would be harmless; but, at least, it would be comparatively so, as compared with the puncture. Quite a number of successful cases are now on record, principally by Americans, and in no small measure due to the enthusiasm of Prof. T. G. Thomas, of New York. Thomas saw most of the cases in consultation, and urged a trial of electricity in the form of the faradic current. Dr. J. C. Reeve ("Trans. of the Amer. Gynæc. Soc.," vol. iv, p. 313), Dr. H. P. C. Wilson (*Amer. Jour. of Obstet.*, vol. xiii, p. 836), Drs. Harrison, Herrick, Cushier, Cocks, Lambert, P. F. Mundé, and Garrigues, have each reported one successful case, Landis a second one, and W. T. Lusk two. In six cases treated, one by Dr. McBurney ("Case of Tubo-interstitial Pregnancy," *New York Med. Jour.*, vol. xxvii, p. 273), one by Dr. C. E. Billington, two by Rockwell, one by Mundé, and one by Dr. Herrick, equal success was attained by the employment of the galvanic currents with interruptions (120 to the minute). In McBurney's case two applications, in Billington's four, and in Rockwell's one only, were required. Drs. Thomas and Garrigues prefer the faradic current, as they consider it equally successful, and not so unpleasant to the patient. Details of most of these cases will be found in the two papers of Dr. Thomas in the "Trans of the Amer. Gynæc. Soc.," 1882 and 1884, and in the paper of Dr. Garrigues, *ibid.*, 1882. In Prof. Lusk's first case the faradic current was applied at the end of the tenth week from last menses. The sac was very low

down on the right of the vagina ; ballottement of the entire ovum could be obtained by the bi-manual method, and fluctuation was plainly felt. Thomas confirmed the diagnosis. For the first three applications no change was observed, but at the tenth distinct changes were found. Treatment was then abandoned and the tumor almost disappeared. Fifteen successful cases in all had been published up to the autumn of 1886. The Americans have only used electricity during the first four months of extra-uterine pregnancy. According to Dr. Lusk (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1094), the galvanic current usually kills the foetus at a single séance, but it must be used of great strength. The faradic current might be used, of moderate intensity. It should be applied for five minutes at a time, twice daily, for several days, until distinct shrinkage of the tumor is detected. The most convenient and successful method consisted in placing the negative pole over the tumor through the rectum, and the positive pole over the abdomen.

Dr. Landis, in a recent communication (*American Jour. of Med. Sciences*, Oct., 1885) arrives at the following conclusions after conducting a series of experiments based upon the supposition that the foetus can be killed if subjected to a current strong enough: 1. In using the faradic current in extra-uterine pregnancy, the applications ought to last an hour, if the patient can bear it. 2. The current should be repeatedly applied, in order that the vitality of the foetus may be completely exhausted. 3. For at least one séance the current should be used in great strength. 4. The current probably acts upon the placental circulation, as well as by destroying the foetus.

Duchenne, Barnes and others have suggested the use of a spark of Franklinic electricity in the form of a shock from the Leyden jar, but the galvanic has been preferred, because by a more prolonged application it was hoped that the chemical effects of the current would aid the shock. In fact, it was hoped by the electrolytic action to obtain results beyond the mere death of the foetus. That is, it was hoped that the chemical action, of which we have many examples in the coagulation and decomposition of albuminous tissues and its power of splitting up both organic and inorganic substances into various absorbable products, would aid in the absorption of the dead foetus, the placenta and sac. We have a striking example of this electrolytic action in the recent application of electricity for the decomposition and subsequent absorp-

tion of cicatricial tissue in the urethra. This hope has received a certain amount of confirmation in the famous case of Dr. R. Petch (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1092) where a six months' foetus was killed and completely absorbed. But then he inserted two needles into the body of the foetus, so that the treatment was galvano-puncture, as distinguished from the American mode minus the puncture. Then it is very hard to say what nature can, and what she cannot do. We know that many ova are absorbed entirely at four or five months without any treatment, and that the placenta can be absorbed even when of full growth. Leopold (*Arch. für Gyn.*, Band xviii, S. 53) experimented on animals by opening the uterus and turning the embryos out into the peritoneum, and he found they were quite absorbed. Even the liquor amnii can absorb a very young embryo. Smellie ("Midwifery," New Syd. Soc., vol. ii, p. 68) relates a case in which the amnion was expelled separately from the chorion and the foetus visibly inside it. After twelve hours most of the foetus was dissolved. Then Tait in forty cases found the placenta every time, but only twelve foetuses. Certainly when an extra-uterine foetus dies about the fifth or sixth month suppuration, as a rule, takes place in the sac, sooner or later, and the foetal bones are eliminated; and even when the sac does not suppurate the foetus may be retained for many years without either bones or flesh being absorbed. So that, as far as we dare draw conclusions from one case, the case of Dr. Petch seems in favor of the power of electricity to favor absorption of the foetus. Then we have Apostoli and Doléris recommending the treatment of circum-uterine hæmatocele by negative galvano-puncture—*puncture again*. According to these authorities it rapidly cures. They say its action is twofold; surgical, by creating a fistula in the solid effusions, the neoplasms, the walls of cysts, etc., and thus effecting a permanent means of elimination; and medical, by provoking disintegration of the tissues, followed by rapid and manifest resolution (*The London Medical Record*, Feb. 15th, 1886, p. 77). But Apostoli seems to explain the wonderful results he claims for electricity in peri-uterine inflammation, fibroids, etc., more by the sinus effects of the puncture than by electrolytic action (*Brit. Med. Jour.*, Nov. 19th, 1887, p. 1096). Again, he confesses in regard to myomata that his method "was in no sense a radical cure, but that it would, under favorable circumstances,

lead to an amelioration of the most distressing symptoms." (*London Medical Recorder*, Aug. 20th, 1888, p. 322.)

It should also be remembered that shortly after M. Apostoli had astonished the world by his revival of electric treatment, he suddenly improved upon his *modus operandi*, and instead of merely passing currents through the uterus and its tumors by means of one electrode in the vagina or uterus, and the other applied to the abdominal walls, arrangements were made for plunging insulated needles into the substance of the tumors. The action set up in this way was ascribed to electrolysis, that is, electrical decomposition of the tissues into their component parts. In the opinion of Alfred S. Gubb (*Lond. Med. Recorder*, Aug. 20th, 1888, p. 322) the idea of electrolytic action was probably erroneous. He says, so far, no trustworthy evidence has been adduced of the existence of any such action; and that it is more than probable that the success of the treatment is due to the purely caustic action of the current, as it is well known that if the degenerative process be once started in these tumors it is very apt to go on until the tumor is destroyed.

In one of the most recent cases of the employment of electricity in extra-uterine pregnancy, reported by Dr. Buckmaster, of Brooklyn (*Medical News*, July 21st, 1888), Lawson Tait seems, to my mind, to have proved that the whole case is inconclusive, as he says most of these cases are. The case is too long to discuss here, but Tait's remarks will be found at pp. 71, 72, 73, and 74 of his "Lectures on Ectopic Pregnancy."

We have not had much experience of the electric treatment of extra-uterine pregnancy in England. The famous case of Dr. Petch was the first successful one, but galvano-puncture was employed. It was done on August 20th, 1878. The first successful case of the American plan, without puncture, was that of Dr. James H. Aveling, senior physician to the Chelsea Hospital for Women, done on May 10th, 1881. The case was in the pre-rupture stage, and his diagnosis was confirmed by Sir Spencer Wells. Gaiffe's induction apparatus was employed, with two cells, but not more than half its power was used. The negative pole was applied to the swelling through the vagina, the positive pole being placed on the abdominal wall. It was applied for ten minutes on four days, when the tumor was found

smaller, the pulsation had ceased, the attacks of pain were no longer felt, and in about two and one-half months only a small, flat, elongated body could be detected in which there was no tenderness. The pregnancy was between the second and third month when treatment commenced. The current caused no actual pain. He believes the foetus died by shock, as there could be little or no chemical action in the electricity from a Gaiffe's induction machine. He believes electricity is the only safe and justifiable method of foeticide during the first four months of extra-uterine pregnancy (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1091). Now, regarding the electric treatment of extra-uterine pregnancy, I think any fair-minded man must admit that the greater part of the literature of the subject is far from convincing. As Herman says, "There are many cases in the papers of Thomas and Garrigues in which the diagnosis rests on such slender grounds that it is impossible not to suspect error. The strength of a chain is in its weakest link, and when one finds seriously adduced as *scientific evidence* cases of an extremely doubtful kind, without, apparently, a sufficient sense of the possibility of mistake, one is apt to judge of the strength of the whole chain of evidence by these weak links." Of course, no one doubts Petch's case, but even here, where there was only one application, neither foetal heart sounds nor movements were influenced by the current at the time, the next day they were weaker, and could not be heard on the fourth day. It is not uncommon for the foetus to die at any time during extra-uterine gestation, and besides, there was the puncture, which Barnes says is the active agent. The conclusion is that we require more cases, and far more conclusive cases as to diagnosis, before we can admit it as proved that the galvanic or faradic current can kill the foetus. Then there is an accurate enough case against the idea, the horrible one of Matthews Duncan. One case like this weighs more than many such as that of Dr. Aveling. Indeed, the case of Matthews Duncan gives great support to the opinion of that eminent electrician, Dr. Steavenson, who has had so much experience in treating urethral strictures by electricity. Steavenson admits that *galvanic* shocks, when the puncture is used, can kill the foetus, but he questions whether *faradic* currents without puncture can. His reasons are—1st. That the inhibitory apparatus of the heart is not developed in a foetus three or four months old; 2d. That the tenacity of life in a foetus is comparable to that of the lower animals, such as tadpoles, and

that any current that was capable of destroying the life of the *fœtus* was *much more likely* to destroy the life of the *mother*. Tait says he cannot accept the twenty cases collected by Dr. Aveling, which disappeared under electricity, as cases of tubal pregnancy, and disbelieve his own observations. Besides, he believed, if such cases went on to the viable period *all the children* and *most of the mothers* might be *saved* by proper operation at the proper time. At the close of the latest debate on the subject before the British Gynæcological Society, on February 22d, 1888, the President, Dr. Arthur Edis, said the facts before us did not justify a clear expression of opinion. He would not recommend any one for whom he had a regard, and who was suffering from an extra-uterine gestation, to be electrified, but would prefer opening the abdomen and removing the mass at once. The cases in which dead *fœtuses* had been left for long periods of time inside the body without giving rise to symptoms were very exceptional, and the electrical treatment might be the means of causing the very mischief they were endeavoring to avoid.

Then as to the alleged harmlessness of the electric current in case of mistaken diagnosis, there have been deaths accredited to electrical treatment of abdominal tumors. From the dangers we have shown to follow the simple puncture, it follows that galvano-puncture cannot be without very serious dangers. But even the electric current without the puncture, according to an American, Dr. Baldy, is not without serious risks. At all events, serious accidents have followed its use in an alarming number of instances, considering how few cases have been so treated. In the *Obstetrical Gazette*, June, 1888, will be found the most interesting and instructive article of Dr. Baldy. "He regrets that the majority of American surgeons favor the electrical treatment of extra-uterine gestations. He thinks after rupture there can be no two opinions about abdominal section being the *only treatment*. He believes the electrical treatment will undoubtedly kill the *fœtus*, and that it has advantages, such as doing away with the so-called mutilation of the woman and with the worry and excitement of an operation. There are, however, disadvantages which cannot be overlooked. There are cases on record in which the *fœtus* has *ulcerated its way out*. Dr. Mann states that there are *only two* such cases on record, and that, as the number is so small, they need attach no great importance to them, but, if these two cases are compared with the total number of

cases of extra-uterine pregnancy treated in this way and reported within the last few years, the proportion is decidedly large. Then, there have been *four cases* reported of rupture of a blood vessel without rupture of the cyst. In one case, that of Dr. Janevin, the vessel ruptured some days after the foetus had been killed by electricity, and the *patient lost her life*. We might as well attribute this ruptured blood vessel to the electric current, as Dr. Petch may the death of the foetus in his case, which occurred four days afterward. Death from rupture of a vessel was a positive danger, and one which could not be ignored. As far as Dr. Baldy knew, there had never been a death which could be attributed to the knife. A number had died after the operation, but they had died in consequence of the rupture, and not from the operation. The fact that the foetus had been killed (assuming for the sake of argument that this really took place) did not render the Fallopian tube pervious, but leaves the woman sterile from that side as effectually as if it had been removed by the knife and ligature. Dr. Lusk had reported two cases, in one of which, *after six years*, there was a *mass still remaining* as large as a pigeon's egg, and in the other, *after four years*, the tumor is as large as a hazel nut. This was the usual history of these cases. He was in favor of removing any pathological growth whatever from any portion of the body, at any time, if it can be done without risk to life. The masses which were allowed to remain were as likely to set up subsequent trouble as some of the tubal and ovarian troubles for which they operated every day. It seemed to him that the gentlemen who *accept the knife* for the *least dangerous*, and refuse it for the more dangerous, troubles are extremely inconsistent, to say the least of it. By operation the cyst is removed, the fear of subsequent danger is done away with, and the risk of the continued growth of the placenta, however small that risk may be, is also avoided."

It seems to me that Dr. Baldy has quite snuffed out the "electrolysis" party—that is, if they needed any snuffing out. But the electrical treatment was a noble and ingenious effort to do something for a class of cases for which medical science at the time could do nothing. As Parry said, it was an anomaly in surgery, that for these cases of bleeding to death from ruptured tubal pregnancy there was no hope and no help. But, before long, every surgeon will see that Tait's discovery that the bleeding can be stopped and the whole present and

prospective danger removed by an operation which at the outset is attended with a mortality of only 4.7 per cent. supersedes the electric discovery and every other uncertain, unsafe, and elaborate plan of treatment. People, as yet, can hardly realize the fact that an accident, which since the earliest ages was looked on as a fatal accident, can be remedied by such a very safe procedure as abdominal incision. When the fact is realized, all other plans of treatment will speedily die away and be heard of no more. Had Tait's discovery not taken place I would have said the electrolysis idea should be pushed and developed to the utmost; but now, it is really not worth while. Any procedure with any danger at all attached to it—and what operation has not?—would probably have as high a mortality as Tait's operation, and no other could be so certain, so thorough, and so free from after-dangers. I quite believe, with Tait, that tapping for ascites, or for any other purpose, is quite as dangerous, if not more so, than abdominal incision. Yet the most timid and ignorant country doctor will thrust a trocar into the abdomen without one uneasy thought. But we must allow time for the facts to be realized. Most of the people on whom Tait operated were already nearly dead with shock and hemorrhage, and the two he lost out of forty-three, no doubt, as Dr. Baldy says, died not from the incision but from the shock they already suffered from. I dare say, if Tait could operate on a number of extra-uterine gestations previous to rupture, the mortality would be about *nil*; but then he says he can't get hold of them till rupture occurs, as they don't feel much the matter; and really this objection is *fatal* to the electrical treatment also. I would say, then, that the proper treatment of extra-uterine gestation in the pre-rupture stage, whenever diagnosed, or suspected with great probability rather, is instant abdominal incision and removal of the whole trouble.

TREATMENT OF EXTRA-UTERINE PREGNANCY AT PERIOD OF PRIMARY RUPTURE.

Every authority is agreed as to the only treatment of any value once rupture has occurred. We have discussed this so fully in other parts of the essay that it only remains to describe the operation and the various matters connected with it. We need not touch on the symptoms, as they have been fully considered under diagnosis. We shall first look at the history of the operation.

History.—Heister, in giving the indications for abdominal section, places extra-uterine gestation first, “when the foetus is contained in the Fallopian tube, the ovary, or the cavity of the abdomen.” Simon (“*Mem. de l’Acad. de Chirurg.*,” Paris, vol. ii) speaks of the same indication in his account of Cæsarean section. It is difficult to say when the first real laparotomy was done, as the very early operations were probably only done when suppuration had taken place in the gestation sac and the pelvic abscess was in process of pointing in some direction. The earliest of which we have any account was in 1540, when Christopher Bain operated, but the probabilities are that this operation was only the opening of a broad ligament abscess. The same may be said of the next operations, those of Noierus in 1591, and of Cyprian in 1694. The operation of Primerose, in October, 1594, was a true laparotomy. His patient had two extra-uterine pregnancies. The first time in 1591, when Jacob Noierus extracted the child, as already mentioned, when the pelvic abscess containing it had already burst, that is, he enlarged the fistula. The second time she was pregnant, Primerose removed the child by a real abdominal section. Three years later we have the report by Felix Platerus of another successful case. Then, for more than a hundred years there is no mention of a case. In 1714, a case was operated upon in France, reported by Calvo. The next case there is any record of was one by John Bard, of New York, who operated in 1764—the first time in America. The next operation was also done in America, and was successful. It took place on January 14th, 1791, the operator being Dr. Wm. Baynham. Baynham had another success on February 6th, 1799. Another American case, by a Mr. Knight, was published in 1795. After these four cases in America a quarter of a century elapsed before the operation was repeated by Dr. Wishart, on the 6th of October, 1823. Dr. A. H. Stevens, of New York, did the next, on February 6th, 1846. Except the last, the other five operations in America were done by country doctors, and the country doctors will have to take heart and do the operation again, as they well could. About 1850 ovariectomy commenced, but in 1876 Parry could only collect 62 operations for extra-uterine gestation. Even this, however, the first list ever made, showed so much success that it is wonderful the operation was never done as a life-saving procedure during the collapse of rupture. In Parry’s list of 62 there were 30 successes to

32 failures. Dr. Harbert, also an American, was the first to definitely suggest the opening of the abdomen for rupture, in 1849, and Dr. Stephen Rogers, of New York, also urged it in 1866, '67. It is very strange how America has been to the front in connection with this subject, and it is to be hoped that Americans will be more than ever connected with it, in carrying out the life-saving operation first urged by Americans. Nobody, however, found courage to carry out the procedure which was indicated by the ordinary principles of surgery until in June, 1883, Lawson Tait commenced a new era in surgery. Although his first operation was a failure, in five years from that date he has placed the operation in the front rank of procedures for the direct rescue of human lives from immediate death.

Extra-uterine gestation has hitherto, and with good reason, been considered as one of the most fatal of diseases.

About three-fourths of all cases die, and more than half die shortly after the primary rupture.

In considering the treatment of a ruptured tubal pregnancy, the great point to remember is that it is, so far as the tendency to death goes, a "concealed hemorrhage," and that every moment is of value. The patient may not die for many hours, but every hour that operation is postponed diminishes her chances of recovery, even when the operation is at last done. The blood tends to keep oozing into the peritoneal cavity all the time, with gushes at intervals. This is owing to the peculiarities of the condition of affairs as before explained. Owing to the free space for distention in the peritoneal cavity, and the fact that the effused blood hardly coagulates, owing to being diluted by the constant flow of peritoneal lymph, there are no natural hæmostatic processes in operation at all. Fainting is the only means nature can employ to check the vital stream for a little. One fainting fit follows another, but with revival of consciousness comes revival of the flow. The result is that we might almost as well delay about starting artificial respiration in an apparently drowned man, as delay operation in a woman with a constant soaking of blood into her abdominal cavity. Then, when time has been lost in trying to rally her with stimulants, which only remove fainting, dilate the small arteries, increase the force and frequency of the heart's action, and so promote the bleeding, everybody can see the woman is doomed, so that friends naturally refuse to allow the operation, and surgeons

are just as well pleased to be relieved of an operation which would only bring them discredit, seeing they have allowed the woman to become practically moribund. The death in the majority of cases is purely from hemorrhage, as the abdomen full of blood on ante- or post-mortem section shows. That the free space in the peritoneum, and the dilution of the blood with fluid, as it flows, encourage the bleeding, is proved by the fact that, considering the size of the rent in the tube and the comparatively small vessels opened, the hemorrhage is enormous. As much as two gallons of blood have been found in the abdomen, and two or three pints is not an unusual amount. The hemorrhage, indeed, has no relation to the size of the rent in the tube. In fact, to delay the operation is to remove the last chance of recovery. In many cases the patient is in a hopeless condition before operation has been decided on, for, as Lusk says, "*the resources of surgery are rarely successful when practised on the dying.*" In some cases where a little delay for help was absolutely necessary, or in cases where the patient was almost dead before being seen, it might be possible to either prevent the coming on of the moribund state, or remove it enough to give the patient a chance of life after operation. We shall now consider the means to be used to this end.

RESUSCITATION OF PATIENTS AFTER RUPTURE, TO GIVE ABDOMINAL SECTION A CHANCE.

Death does not generally follow rupture for some hours. There is, therefore, always time for operation, except under peculiar circumstances, but when seen, extreme prostration may forbid operation. Should delay be absolutely necessary after the arrival of the surgeon he should make pressure on the abdominal aorta to prevent further hemorrhage, as suggested by Playfair. If collapse be too extreme to give hope of operation, we should try to resuscitate the patient sufficiently to make the operation hopeful. Transfusion naturally suggests itself, but intravenous saline injections are safer, much easier of access and management, and just as efficient. In a parallel case, the algid stage of cholera, Dr. Francisco Aguirre, of Santiago (*Revista Argentina de Ciencias Médicas*, Feb., 1888), produced quite a "resurrection" from a state of profound collapse by intravenous injection of artificial serum composed of 20 grammes of sulphate of soda and 10 grammes of sodium chloride to 2 litres of water. 600 grammes were injected

at intervals into the left saphena vein. Dr. Aguirre refers to other recorded cases of intravenous injection by Lorain, Dujardin-Beaumetz, Rouvier, and Hayem, and urges that the results justify a continuance of this plan of treatment.

Still better, the experiment has actually been tried in collapse from extra-uterine rupture of a gestation sac, and the operation might have been done under favorable circumstances, but the patient was supposed to be cured, which, of course, she was not, so the hemorrhage recurred and carried her off.

In the *Vratch*, No. 33, 1885, p. 543, Drs. J. F. Zemacki and Kotelnikoff, of the Obukhovsky Hospital for Women, in St. Petersburg, give the case of a soldier's wife, aged 31, who had been brought pulseless, insensible, with all the signs of acute anæmia. The patient had been reduced to this state by profuse uterine hemorrhage of three days' duration. The uterus was found slightly enlarged, the os being closed, but discharging some sanguinolent fluid. The subcutaneous injection of camphor and the internal administration of valerian, Hoffman's solution, and wine, brought but slight improvement. In view of the danger immediately threatening the patient's life, the authors resorted to intravenous injection of a litre of a solution of common salt (6 grammes), liquor potassæ (1 minim) and pure carbonate of potash (3 grammes) in water (1000 grammes), heated up to 40° C., and poured through a funnel from the height of a mètre. The transfusion lasted about fifteen minutes. A very marked improvement immediately ensued, the cardiac action becoming stronger, the pulse fuller and less frequent, the consciousness returning, etc. The patient being now able to give account of herself, the authors learned that she had been previously healthy up to three days before, when painless flooding suddenly appeared; the last menstruation had occurred three months ago; nevertheless, the patient did *not* regard herself as pregnant. Having taken all together (including the negative results of a repeated careful examination), the authors arrived at the conclusion that they had probably to deal with a case of *intra-uterine* growth. However, during the subsequent days, there were developed all the signs of acute peritonitis with intra-peritoneal *accumulation of fluid*, and on the fifth day after her admission the patient died in collapse. The post-mortem examination revealed rupture of the left Fallopian tube, which was greatly distended, and contained a

foetal sac with a large blood coagulum and rudimentary placenta, but no embryo. The peritoneal cavity was filled with an enormous amount of sanguinolent fluid—blood diluted by peritoneal lymph. The authors seemed to think the rupture of the tube took place *after* the patient's life had been saved by the injection of saline fluid.

This is a most instructive case. The authors, though evidently skillful in general medicine, as evinced by the very rational treatment of the collapse, yet seemed to be totally ignorant of the subject of extra-uterine pregnancy. When they found collapse from acute anæmia, they never suspected what the *fluid* was which had suddenly filled the peritoneum, and even after death they did not interpret the case aright. The woman evidently was brought to hospital after tubal rupture, was restored to life, but the bleeding recurred from time to time, till her abdomen was full, when she suddenly died. However, the case shows the possibility of completely resuscitating a patient collapsed through ruptured tubal pregnancy, thus allowing the operation to be done with almost no danger whatever. In the case described the new supply of circulating fluid replaced the old so well that the operation could have been done any time for *five days after rupture*.

As the faintings of acute anæmia are extremely dangerous, it is well to know of some means of restoration. For this purpose hypodermic injections of one or two drachms of ether, or of brandy, have been employed, and are very useful indeed, as a temporary resuscitator, though when the collapse was very great from hemorrhage I do not believe they would revive the patient sufficiently for the operation, as we see the intravenous injection of salines does. However, recently a more powerful remedy for resuscitation of those apparently dead has been hit upon, which might do us good service on an emergency in abdominal hemorrhage. Dr. M. H. Lackersteen (*Arch. für Gynäk.*, July, 1888) publishes three cases in which patients apparently dead were resuscitated by means of hypodermic injections of nitro-glycerin. The first case was that of a young lady who became suddenly collapsed during the passage of a gall stone. Neither respiration nor pulse were perceptible. The hypodermic injection of ten minims of a one per cent. solution of the drug was promptly followed by signs of returning life, and she ultimately made a good recovery. Case second was one of *drowning*, and case third was that of a *still-born child*. In each case atropine was injected as soon as the effects of the nitro-glycerine were apparent.

THE OPERATION FOR RUPTURED TUBAL GESTATION.

The objects are the arrest of hemorrhage, removal of the blood already in the peritoneal cavity, ovum and ruptured tube. The incision should be made in the middle line, and midway between the pubes and umbilicus, and should be about one and a half or two inches in length. The linea alba is exposed at the second or third stroke of the knife. Pressure forceps are applied to any bleeding points and left attached. A small opening is made in the fascia, the fibrous septum between the recti found, then the fascia is slit up, the point of the knife directed *forward*. The muscles are pushed aside with the handle and the sub-peritoneal part found. The fat is lifted up with two pairs of catch forceps and carefully opened. While the forceps pull the tissues up the peritoneum can easily be made out; a small opening is made in it while thus elevated, the finger is inserted, and the remainder of the incision made on the finger by scissors. By this method the bowels run no risk, as each tissue is lifted up before division; then the first little hole made in the peritoneum admits air, and the bowels fall quite away from the parietes. The index and middle fingers are now inserted. If the omentum cover the intestines it must be worked up by the fingers. When the way is clear the fingers are passed right down to the fundus uteri. At the first incision through the peritoneum blood will probably escape and the pelvis be found full of blood and clot. The fingers have to find the fundus uteri among the clots. Then with a finger on each side of the broad ligament, the Fallopian tube is traced first on one side, then on the other. The gestation sac will be made out as a soft enlargement, of varying size, according to stage of gestation, and the consistence will vary according to whether the ovum has escaped or not. The sac is brought to the surface and the tear searched for. The best site for a pedicle is selected, ligatures put on and tied in the Staffordshire knot, or, if the pedicle is very broad, two Staffordshire knots or a chain ligature may be required. The whole sac is then cut away. In this way we need not trouble about stopping bleeding at the torn spot. The ovum is seldom found complete in situ, the placenta is often found in the sac, but both foetus and placenta are often free among the clots. The peritoneal cavity is then cleared as far as possible by the fingers or hand; then irrigation with hot water is employed, and, lastly, the cavity dried by the sponge. To clean out the peritoneum it may be necessary to enlarge the incision;

a little flat sponge is laid upon the bowels under the incision to protect them during the stitching, and the sutures, four or five, are placed in position. The sponge is then removed and the sutures tightened and tied. The operation is a most easy and simple one, and need not occupy more than ten minutes. As this is not a treatise on abdominal surgery, I think it would be a mistake to load it with purely surgical details as to operations, use or non-use of antiseptics, precautions as to cleanliness, details of after-treatment, etc. All these are discussed in every work on operative surgery. Suffice it to say, that the operation should at least be *aseptic*, if not *antiseptic*, in all its details; the hands, instruments, sponges, dressings—everything touching the wound or surrounding the patient—should be scrupulously clean. This is confessed to be the secret of the antiseptic system, which has done glorious service in teaching us the absolute necessity of these things if we are to have any success. Indeed, let leading surgeons say what they will, it is the antiseptic system they have to thank for all the modern advances in surgery. We have now found out what parts of it are essential and what needless, or perhaps injurious, that is all. But, perhaps, as I have used a term, "Staffordshire knot," which may not be familiar in all parts of the world, I had better explain that simple contrivance. The Fallopian tube, when the diseased one is found, is pulled out tight, and the broad ligament spread out ready for the ligature. The ligature is passed double by means of a very blunt needle with the eye at the point, which is then withdrawn. The ligature is made to include the tube and as much of the broad ligament tissue as may seem necessary to include all vessels supplying the bleeding part. When the needle is withdrawn we have a loop of the ligature sticking through the broad ligament, the two free ends are lifted and brought round the Fallopian tube, and *one* of the ends is put through the loop and both ends then pulled tight with one hand, while the fingers of the other steady and make counter-pressure on the Fallopian tube. At this stage we have the loop with one end through it at the one side of the Fallopian tube, the two ends coming round the free border of the tube, one tucked through the loop and one not; then the two ends are taken up and treated just as if it were a simple single thread we were about to tie round the tube, that is, a reef-knot is cast on the ends, perhaps with the aid of an assistant pulling one end, so that the surgeon may keep the knot close down on the tube. The purpose

of the first part of the knot is to make sure that it is tight enough to arrest the circulation through the vessels by a mere pull, as the surgeon is working out of sight among tissues slippery with blood. When the one end is put through the loop and both ends pulled, the tube is tightly encircled, and the tightened loop does not relax when the ends are relaxed, although a further knot is added to make sure the loop does not relax in time.

In many cases the diagnosis has to await the section; the rule must be to operate when signs of alarming abdominal hemorrhage occur. We should never allow a patient to die because the diagnosis is not completed. This has always been the objection urged against laparotomy hitherto, viz., the uncertainty of diagnosis. But, surely in these days, when men insist that it is quite easy to diagnose extra-uterine pregnancy before rupture, the old excuse will fail. The diagnosis is of no importance as to the treatment; we operate to arrest intra-peritoneal hemorrhage, the alternative being death, and knowing that *intra*-peritoneal hemorrhage is usually due to ruptured tube.

TREATMENT OF EXTRA-UTERINE PREGNANCY BETWEEN PRIMARY OR TUBAL RUPTURE AND THE FULL PERIOD OF GESTATION.

"If the woman does not perish from rupture of the cyst during the first four or four and a-half months of pregnancy, it is not likely that an opportunity will offer to inspect the body until at, or near, or even some time after, the close of pregnancy." Tait's theory, as I have said, is that, almost without exception, foetuses which survive tubal rupture have been discharged between the folds of the broad ligament. These folds are easily opened out by the growth of the foetus, so that often the foetal sac comes to encroach on the general cellular tissue of the pelvis, strips up the peritoneum from the back and sides of the pelvis, the back of the uterus, and even from behind forward, until the whole lower part of the anterior abdominal wall is stripped except a piece between the cornu of the uterus and extending down to the base of the bladder. Tait does not deny the possibility of abdominal pregnancy. He only says it is rare, and explains how it comes about. Jessop's case is a proof that abdominal pregnancy can exist. Nonat's case, 1855, and that of Duverney, 1712, show that the account Tait gives of their origin also occurs. The post-mortem record in Nonat's case is most important in another way: it

illustrates one of the dangers of extra-uterine pregnancy after primary rupture (supposing it to be in the broad ligament). In this case 25 ounces of black fluid blood and clot were found—*intra*-peritoneal hæmatocele, and as usual an almost sure cause of death. When the intra-peritoneal blood was removed, an ovoid tumor was observed, covered by the peritoneum of the broad ligament. This tumor was formed of blood also, with a foetus added; it was what Tait holds to be the usual form of extra-uterine pregnancy where the case goes beyond tubal or primary rupture. It was, in fact, an extra-peritoneal hæmatocele plus a gestation sac and foetus. But the order of events was plain. The extra-peritoneal hæmatocele and gestation had been in the broad ligament, but the broad ligament had given way and poured blood into the cavity of the peritoneum, and this had caused death, as no operation was done. At the bottom of the left recto-uterine cul-de-sac, the peritoneum forming the posterior layer of the broad ligament presented a perforation with a communication between the recto-vaginal cul-de-sac and the cellular tissue separating the peritoneal layers of the left broad ligament. This case points out the treatment when this "secondary" rupture occurs, as it may occur at any moment from primary rupture till the foetus is removed by operation or eliminated by nature. The woman is liable to secondary rupture into the peritoneum and to death therefrom any day while she has an extra-uterine—generally called an abdominal—foetus inside her. But operation would save her when this happens at any time, just as in the case of primary rupture. At least the chances would be immensely in her favor, whereas, without operation, her death is almost certain. The operation in secondary rupture would be different, and rather more difficult than that for primary rupture. Abdominal blood would be cleared out on incision, then the secondary rupture searched for, the gestation sac incised, clots and foetus removed—foetus often will be absent, from absorption—any hemorrhage still going on from interior of sac stopped by styptics, as vinegar, actual cautery, or ligature to actual bleeding points. Then when the sac had been cleaned and dried, the edges would be returned to the edges of the incision in the abdominal walls, a drainage tube put in, and the wound closed except at the bottom. If a definite placenta be present in these cases it should not be touched, as great hemorrhage is likely to take place from its site unless the foetus has been long dead, and there is no

efficient means of arresting the hemorrhage. The woman will not be out of danger, of course, till the placenta has come away in the discharge along with the foetal membranes, and the great point of the after treatment is to prevent septic absorption. This is done on general surgical principles, on which I need not enter, as they are in no way special to my subject.

In the *Brit. Med. Jour.*, December 15th, 1888, p. 1339, will be found a discussion on a case of extra-uterine gestation before the London Obstetrical Society. As this may be considered the very latest expression of opinion I notice it. Dr. Griffith asked if the case were proved to be extra-peritoneal according to Tait's pathology, as there was a specimen in St. Bartholomew's museum which almost disproved the theory. Now, how one specimen could disprove the theory that the majority were extra-peritoneal is hard to see, and the discussion as a whole seemed to considerably corroborate Tait's theory. Griffith further asked if benefit seemed to arise from the median incision (the case was one operated on eight months after foetal death) and the opening of the peritoneal cavity, as it had been *shown by Mr. Thornton and others* that it was *usually better to cut down directly on the sac and avoid opening the peritoneum*. Here, it seems to me, Dr. Griffith either stultifies himself or shows he knows nothing whatever of the subject. How, in the name of common sense, could Mr. Thornton show that it was *possible*, let alone *better*, to avoid opening the peritoneum if the foetus were *within it*, as he alleges? How could the foetus be removed without opening the cavity in which Dr. Griffith maintains it is?

Again, Dr. Champneys said that the statement that all cases of extra-uterine gestation which went quite, or nearly, to term were intra-ligamentous was unfounded. In a case by himself (*"Obstet. Soc. Trans.,"* 1887) the foetus was simply kicking free among the intestines. Very well; but what then becomes of the theory about peritonitis and encystment? It is probable that the foetus was quite free through all its career? The difference between Dr. Champneys and Mr. Tait is that Champneys offers a very rare case as a refutation of Tait's theory, but does not attempt its explanation, while Tait points out that such is a rare exception, and explains it, as he has done Jessop's very similar case. It seems to me that the least objectionable explanation of Jessop's and of Champneys' cases is that offered by Tait, viz., that in such cases secondary rupture sets the child free in the abdomen; that its

original situation from the time of tubal rupture had been external to the peritoneum, but that the distended broad ligament had given way and the fœtus slipped into the peritoneum. In such cases hemorrhage, for some unknown reason, must have been slight, or the woman would have died of abdominal collapse, as after primary rupture. Dr. Cullingworth, in reply, gave reasons for believing the pregnancy was *originally* tubal, and had *become a broad ligament gestation* through rupture of the tube. Operation *without opening the peritoneum* would have been impossible in this case, owing to the relations of the parts. I cannot give more space to this most recent discussion on my subject. My conclusions, from a study of it, are that there must be a great amount of truth in Tait's theory, when this sort of thing is all that can be urged against it by his rivals, the London obstetricians, some of whom hardly seem qualified to discuss the question at all. A man who maintains that abdominal pregnancy, in the old sense, is the rule, and then asks if the advantages of *removing the gestation without opening the peritoneum* are as great as alleged, is certainly an opponent who will not greatly injure (or benefit) any cause.

When a woman has recovered the shock of the primary rupture, which often, in these cases, is too slight to be noticed, the case then becomes practically an extra-peritoneal hæmatocele, and unless the signs of pregnancy be very numerous and decided, it could not be diagnosed from simple hæmatocele until the fœtal heart sounds become audible. The treatment should be symptomatic. If possible, the case should be let alone, or such palliative and sedative treatment adopted as pelvic pain, obstructed defæcation, or obstructed micturition may call for. The woman is perfectly safe during this period, save for possible accidents which, however, may occur at any time. The dangers of broad ligament hæmatocele with fœtus or without it are—secondary rupture of broad ligament into peritoneum, usually causing fatal hemorrhage unless abdominal section be done at once; when inflammation and suppuration occur in sac; and when fœtus, having died, when the case is due to gestation, nature tries to eliminate the fœtus by fistulous openings in various directions. Suppuration in hæmatocele from gestation is not very common, as the gelatinous fœtus is often completely absorbed. In forty cases where Tait removed a placenta he only found twelve fœtuses ("Lectures," p. 38). This is too large a propor-

tion to be accounted for on the theory that he had overlooked the foetus among the clot and débris.

The treatment between the primary rupture of the Fallopian tube and the viable period should therefore be expectant. Laparotomy should be done in case any symptoms arise putting the mother's life in danger. Laparotomy is always safer than any other kind of interference. The objections to tapping the gestation sac through the vagina or abdominal wall, to electricity whether with puncture or without, are a hundredfold increased as the foetus grows larger. If there be any excuse for killing the foetus in the very early stage, there is none now. Nature, in the majority of cases kills the foetus, but safety is not often procured to the woman. Should a woman carry a dead foetus for fifty years she is always in great danger, always at the mercy of accidents. She may take ill with symptoms of abdominal collapse at any moment, and the alternative then is laparotomy or death. No puncture, no electric current, will restrain bleeding from an abdominal artery; nothing will do but an operation. So such cases should be nursed up to full time, if possible, we being prepared, of course, to interfere at once if necessary. Pelvic abscess, if it occurs, should be opened from *above* the brim, as this gives best results in modern times, and, on the first sign of foetal remains being eliminated the sac should be opened *above* and cleared out. I cannot enter into the advantages of this modern practice over the older let alone policy, within the limits of this essay. I shall merely say that the arguments will be found in Tait's "Lectures on Ectopic Gestation," at page 36. It will be found that Tait operates to prevent a pelvic abscess causing fistulæ, here and there, as this involves immense suffering, years of invalidism, and death in the end, in a large number of cases. He has opened the pelvis above in about thirty cases, with *recovery in all*. He says he can't say surely how many of these suppurating hæmatoceles were due to tubal pregnancies, but he suspects more than half were, as this is perhaps the greatest cause of hæmatocele. Here Tait is by no means alone in his opinion. Gallard (*Gazette Hebdomadaire*, 1858, p. 461) thought that most hæmatoceles were extra-uterine gestations. Lesonef, who was the great authority on extra-uterine gestation before Parry, is ("Remarques sur Trois Cas de Grossesse Extra-uterine," p. 18) of the same opinion. Leopold (*Arch. für Gynäk.*, Band xviii, S. 74) says: "How

often may it happen that rupture of the gestation sac in the second month may run its course with the clinical phenomena of a retro-uterine hæmatocele, and end favorably with complete absorption of the effused blood? I do not for a moment doubt that such hæmatocèles occur much oftener than we usually think, and Schroeder, (*Lehrbuch der Geburtshülfe*, 6th Auf., S. 408) believed that he had seen cases of tubal pregnancy run a favorable course in this way so often that he believed termination in cure to be the rule." Veit (*Zeitsch. für Geb. und Gyn.*, Band xi, S. 384) goes so far as to declare, "that extra-uterine pregnancy is the *most frequent of all tube diseases* for which patients come for treatment; that all cases of hæmatocele occurring in women who think themselves pregnant, or have suffered from irregularities in menstruation before the illness, *should be taken to have arisen in tubal pregnancy*; and that cases of *apparent parametritis after abortion* should be suspected of being *hemorrhage from extra-uterine pregnancy*." Herman has seen three cases of large hæmatocèles, preceded by a history pointing to extra-uterine gestation, and accompanied by the passage of a decidua, in each of which the patient recovered. Indeed, Herman (*Lancet*, May 26th, 1888, p. 1022) has seen many cases in which, with the signs and symptoms of hæmatocele, there was a history more or less consonant with the supposition that extra-uterine gestation had been present.

The diagnosis in these cases, though far from certain, yet rested upon grounds quite as strong as those supporting the diagnosis in the great majority of the cases reported as having been cured by electricity. On this supposition, that so many cases which escape the fatal primary rupture recover by early death and absorption of the foetus, we build one of the heaviest arguments against electricity, tapping the cyst, etc., and also give an explanation of the supposed successes by such methods. When the foetus undergoes any great development, however, danger properly only begins when the foetus dies. From some obscure reason, some foetuses remain for a long time—perhaps many years—quite fresh and undecomposed. In such the sac does not much incline to suppuration without some traumatic cause added, as a blow. In other cases, probably where the placenta adheres to bowel and septic gases pass into the foetal circulation, the foetus soon decomposes, inflammation commences in the sac, septic absorption ruins the

mother's health and threatens her life, and abdominal section becomes the only rational treatment.

Dr. R. P. Harris has inquired into the subsequent histories of the cases that have been *cured* by electricity. Some of these, including one of Dr. Allen's, had suffered from repeated attacks, supposed to be peritonitis, and continuous ill-health for years. A dead foetus cannot be an innocuous mass unless in the very early stage, and not always then (see *Brit. Med. Jour.*, Dec. 4th, 1886, p. 1094). Routh, although an advocate of electricity prior to tubal rupture, says that from that period up to the viable age, we should open the abdomen to prevent possible rupture (secondary) (*Brit. Med. Jour.*, as above). This seems to be uncalled for except circumstances demand it.

Dr. Gill Wylie, New York (*Brit. Med. Jour.*, Dec. 4th, 1886, p. 1093), reports that he did abdominal section on a woman who had been previously subjected to electrical treatment and *reported cured*; and Dr. Gordon, Montreal, admits (*ibid.*), although the foetus may be killed by electricity, it may *not prevent the necessity for subsequent abdominal section*. Indeed, Dr. Petch, after reporting his famous case where he caused the death and absorption of a six months' child, pleads that even if it be necessary afterward to remove the dead child by abdominal section, the condition will have been in no way made worse for operation by the previous use of electricity. He also admits that in his famous case the dead foetus would have been a great danger for a year after, in case the woman had conceived again. But Petch used galvano-puncture, and I can't admit the truth of his argument, that in any case the galvanism can do no harm, as I fancy I have proved that any puncture is dangerous—if not quite as dangerous as abdominal section. And if he did not kill the patient at the time by hemorrhage or septicæmia, she would be more liable afterward to decomposition of the foetus on account of the puncture. The danger of septic mischief would continue until the whole results of gestation were completely absorbed (*Edis, ibid.*). All authorities seem agreed in discountenancing electricity or puncture after the sixth month.

Conclusions.—It seems, then, to sum up the treatment of extra-uterine pregnancy between the primary rupture and the viable age, that there are only two alternatives worthy of consideration, viz., expectancy and laparotomy.

The life of the child is an element which should enter into the consideration, and in the absence of symptoms endangering the mother's life, and as operation *before* the full period is no more dangerous than before the foetal sounds are heard, we should give the child a chance for its life. Why should we destroy one life without any corresponding advantage to the other? We should, then, leave the case severely alone, enjoining on the mother precautions as to the avoidance of exertion and all manner of accidents likely to provoke secondary rupture, or death of the child. When operation is rendered necessary by rupture of the sac into the peritoneum, the foetus is generally macerated, so that it is probable that foetal death makes all the circumstances demanding operation more likely, as in normal pregnancy foetal death is the great cause of abortion.

TREATMENT OF EXTRA-UTERINE PREGNANCY AT THE FULL PERIOD.

The mother should be carefully guarded and watched till false labor sets in, and the onset of labor seized as the most favorable time for both mother and child.

The statistics of this operation, until recent times, have been very discouraging. The operation during the life of the child, was called the "primary" operation, in contrast to that after its death, which was called "secondary." But when all vital action is over and the circulation through the placenta ceased and succeeded by thrombosis of the maternal sinuses, opening the sac is hardly more dangerous than opening an abscess. Parry reported twenty cases of "primary" operation, with eight children saved, and six mothers, but, according to Lusk ("Midwifery," p. 338), the record is worth nothing, as to the mothers at least. Litzmann ("Zur Feststellung der Indicationen für die Gastrotomie bei Schwangerschaft ausserhalb der Gebärmutter") gives nine operations, with only one recovery—the now famous case of Jessop. Werth ("Trans. of the Gynæcol. Section of the Eighth International Congress at Copenhagen") collected seven cases, with one recovery—that of Martin, of Berlin. Litzmann reports that only four children survived the third day, out of ten operations. Koeberlé reports nine cases, seven children and four mothers being saved. Meadows related a good case to the Obstetrical Society (November, 1883.)

But this alarming mortality must not be taken as that which is likely to occur in the future. Many reasons conspired to give results such as the above. First, no tables of operations previous to 1878 are of much value, as the final abandoning of the clamp in ovariectomy revolutionized abdominal surgery. In the operations before this date, the peritoneum was fouled in the operation and not cleaned out. The only hope of the surgeon was in "adhesions," which would shut off the gestation sac from the remainder of the cavity of the peritoneum. It is now known that the danger lies not in opening the peritoneum, but in allowing decomposable matter to escape into it, or, if it gets in, neglecting to wash it out again. The peritoneal cavity being a gigantic lymph space, putrescible matter might as well be injected into a vein as left there. Then the older statistics are of little real value from another reason. Most of the patients were operated on "in extremis," and hardly two were by one man, or agreed in anything. Hardly two had anything in common for comparison, yet they are all thrown together as if nearly similar.

It may be said that in these old days the "secondary" operation, after foetal death, gave excellent results, although the same want of skill in abdominal surgery, the same want of asepticism, prevailed. Yes; but in these chronic cases the adhesions did cut off the peritoneal cavity, as a rule. A great many of them were done after suppuration of the sac, and the fact that the woman still lived—most of them died before this stage—showed that the peritoneal cavity was shut off, or the pus would have poisoned the patient. So, the woman was hardly susceptible to the want of cleanliness in the operation. Besides, the "puerperal condition," to which Parry and others attributed the great fatality of operations at term, certainly did make the woman much more susceptible to bad surgery and ignorant operators. But, without taking bad surgery into account, septic influences are fatal to the woman in the "puerperal state." Tait says he never succeeded in getting a woman through a Cæsarean section, and he thought it was the puerperal influence. But since he began to amputate the uterus at the same time, and thus get rid of metritis and its septic consequences, all his patients recovered easily, just as they recover in ovariectomies; so he now concludes the puerperal influence is all nonsense. ("Lectures," p. 78.) Taken as a rule of practice, the comparison of the "primary" and "secondary" operations was quite delusive.

This method compared together the fatality attending operation before the death of the child with operation at a time when large numbers had already died. It compared a woman's chances when operated on during the life of her child with the chances of women to survive a final stage of deliverance, but took no account of the numbers who had already perished. Parry himself says: "During the discharge of the decomposed child the mother is subjected to all the dangers which result from the absorption of purulent and putrid matter." He has tabulated 330 cases, and of these 105 died without any operation. This gives a mortality of 31.8 to start with, and, if we are to truly estimate a woman's chances of life, this should be added to the results of the secondary operation, as the primary operation gives a final cure when recovery follows. That this argument is not mere advocacy of operation may be shown by the fact that, unless a long time is allowed to elapse after foetal death, the operation is still nearly as dangerous as the primary one. So, if we are to give our patients the safe operation, we must wait till 31.8 per cent. of them would be dead. All of Litzmann's patients who underwent operation during the first month after foetal death (seven) died, but, in cases operated on at from five weeks to twelve months after foetal death, only seven died out of twenty-six. Now, in modern times, Tait has operated on seven cases, saving *five* mothers and three children ("Lectures," p. 35.) This is not much greater than the mortality in Litzmann's cases of secondary operation; Tait's mortality for the dreaded primary operation being 28.5 per cent., Litzmann's, for operations from five weeks up to a year, 26.9 per cent., while the primary cases and those for a month after foetal death *all died*. This shows the progress we have made in abdominal surgery in a few years. Of course, the figures on both sides are much too small to base conclusions on, but they seem to indicate great advances in surgery. The deaths of all the cases operated on in Litzmann's "primary" list were due partially to the desperate state of the patients necessitating early operation, and partly to bad surgery provoking profuse hemorrhages from removal of the placenta.

There are even yet no means of knowing when the thrombosis of the maternal sinuses renders hemorrhage from the placental site unlikely. Schroeder removed the placenta three weeks after foetal death and had no loss, while Depaul, in an operation at four months after

foetal death, had *fatal* hemorrhage. Modern surgeons, however, leave the placenta alone, as a rule, Martin, of Berlin, excepted. Sometimes, indeed, it is found in the line of incision, when more or less separation is unavoidable. As a rule, the placental attachments are in the pelvis. The placenta may adhere to the bladder, uterus, omentum, intestines, etc., but not often to the anterior abdominal wall; of course, the operator might encounter it on opening the gestation sac. Lusk says that the placenta has been found in the line of the abdominal incision in the proportion of one case in six ("Midwifery," p. 339), but he must refer to borders of it only, as it is far more spread out in extra-uterine pregnancy than in normal cases. But the figures of Litzmann's secondary operations which I have above compared with Tait's are quite too favorable to be used for that purpose, as they evidently include operations which were merely enlargements of fistulæ. If we take the cases he has collected of secondary laparotomy, as distinguished from that performed during the life of the foetus on the one hand, and incisions for the purpose of extracting foetal bones from fistulæ on the other, even cases collected in recent years fall far below the success of Lawson Tait in the primary operation. Litzmann collected thirty-three cases of secondary laparotomy (twenty-four of them between 1870 and 1880); of these nineteen recovered and thirteen died, giving a mortality of 42.4 per cent. against Tait's mortality of two cases out of seven = 28.5 per cent. Koeberlé saved four mothers and seven children out of nine operations, giving a maternal mortality of 55.5 per cent. As far as it is wise to draw conclusions from such very small numbers, the primary operation in the hands of Tait seems to be in the position (or better) in which the secondary operation was in the last decade. Of course in good hands the secondary operation has also greatly advanced. Gaillard Thomas saved all four lives in as many operations, and Tait only lost one out of seven.

Lusk, in a paper entitled "A Plea for Early Operation in Extra-uterine Pregnancy," tells us he did not collect his figures with a view to supporting any foregone conclusions. He gives us 103 cases, occurring between 1875 and 1886, excluding cases of rupture in the early stages. Of twenty-nine cases of "abdominal" pregnancy terminating in fistulous openings, nine died. These are often looked on and reported as cases of *spontaneous cure*; and if to the actual death rate the highly probably permanent injury to health and usefulness is added, it will be

seen that natural elimination of the foetus is not a very hopeful lookout. Eight cases died before fistula had formed, at from eight months to a year and a half. Of fifty-two cases of secondary laparotomy at different periods after foetal death, thirty-seven recovered and fifteen died. In only three of the operation cases could death be attributed to the laparotomy; these were all cases of profuse hemorrhage. In all the other cases, fatal after operation, the patients were almost moribund. Although operation is not usually successful upon the dying, still some cases recovered which seemed past all hope. It is often difficult to say under which class an operation should be placed, as Litzmann says that furious hemorrhage has occurred from the placental site four months after foetal death (*Brit. Med. Jour.* Dec. 15th, 1888, p. 1340). Such a case would have all the dangers of a primary laparotomy.

Medical opinion at the present day is mostly in favor of not awaiting foetal death, but of giving mother and child both the best chance for their lives. Tait's latest utterance on the subject (*Brit. Med. Jour.*, March 10th, 1888, p. 535) is "that *all* children and *most* of the mothers might be saved by proper surgical intervention at the opportune moment." Dr. Playfair, at a debate before the Obstetrical Society, London, on Dec. 5th, 1888 (*Brit. Med. Jour.*, Dec. 15th, p. 1340), said that Dr. Harris's tables of cases of primary operation showed a most gratifying success. Primary laparotomy has been advocated by Velpeau, Kiwisch, Koeberlé, Schroeder, Barnes, Levret, Gardieu, Meadows, and, indeed, most recent authors. Jonathan Hutchinson and others prefer the secondary operation. If we delay operation the cyst may rupture during the efforts at labor, and death occur at once, or other risks, which cannot be predicted, are always liable to occur. The woman may sink from peritonitis or exhaustion consequent on rupture of a suppurating sac, which in nearly all cases sooner or later occurs, so that "the patient's life may be said to be at the mercy of accidents, of which we have no sufficient warning." By delay we deliberately sacrifice all hope of saving the child, while it is quite doubtful whether we increase the mother's chances in the long run. Indeed, in the opinions of all modern writers of the least intelligence, the question is between primary and secondary laparotomy; nobody recommends abandonment of the case to "nature," nor the allowing of a woman to go about with even a quiescent gestation in her abdomen for the remainder of life. The best that Parry can say

for his do-nothing policy is that when the case is left until nature has shown through which channel elimination is to be effected, the mortality is 17.35 per cent. less than in the cases in which the primary operation was postponed. He should, in justice, add on the large mortality which occurs during the process of elimination, which is, according to Lusk, nine deaths in twenty-nine cases allowed to proceed to formation of fistula. According to Parry himself, out of 330 cases, 105 died trying to eliminate foetal remains. If we consider the risks of septicæmia, etc., which women run from the death of the foetus until it is eliminated, which must happen in most cases, unless death anticipate it, I think we shall conclude that primary operation is safer than total neglect, not to speak of the immense saving in suffering and loss of usefulness. But to also omit secondary laparotomy, in these days, would be almost criminal. Parry himself saw this, as he says: "After the death of the foetus, and the restoration of the normal condition of the system, the retention of an extra-uterine foetus is not incompatible with a long and useful life, but a woman is *never free from danger* while she is carrying an encysted child. Violent exercise, injuries, blows, strainings, and similar mechanical irritations, may be the exciting cause of inflammation of the sac at any time. Hence, violent pain, with fever and evidences of inflammation following these, always demand a cautious prognosis. Depressing diseases, as any of the continued fevers, or local affections which introduce a profoundly typhoid condition, endanger the woman by impairing the nutrition of the cyst, and leading to destructive inflammation."

The great defects of Parry's tables are that the survivals he compares with the cases operated on, are the mere remnants of the hosts which have died before the period of comparison, and that the mortality in his day was wantonly, as it were, run up by the most stupid delay until recovery was well nigh hopeless, and a great deal of most unscientific bungling before the case was resigned to operation, as a last resort; for instance, one case was operated on after over a week of labor, and another after most persevering efforts for a *whole day to deliver her with forceps!!* The operation should not be left till it assumes the character of a last resort; it should be looked on as a mode of treatment. Harris gives a case in the "International Cyclop. Surg.," vol. vi, p. 784, which illustrates this:—

"In October, 1885, three prominent Philadelphia obstetricians met

daily in consultation for sixteen days over the case of a lady who was suffering the pains of false labor. . . . As they could not promise the husband that an operation would probably save the life of his wife, they waited for the time to come when they could do this; but, while doing so, and when the lady appeared to be getting better, she was suddenly seized with agonizing pains, followed at once by a state of collapse, and died in thirty minutes."

THE "PRIMARY" OPERATION.

On account of the re-arrangement of the peritoneum—to avoid opening it if possible—the incision should be made well to one side of the middle line. The long tube-like process of peritoneum extending from each side of the fundus uteri to the base of the bladder, would be opened by a medium incision. No doubt this has occurred many times when the gestation cyst presented close behind the abdominal muscles, a little to one side, and so gave the operators the idea that most of the extra-uterine foetuses were *in the cavity of the abdomen*. The history and the physical signs will show which side the gestation is on. The incision should be two or three inches from the middle line of the body. When the sac is opened the foetus is to be carefully lifted out by the feet, using great care not to lacerate the sac or abdominal wall. If the child is living some one disengaged should take immediate charge of it. Another important feature in the latest form of operation (Tait's "Lectures on Ectopic Pregnancy," p. 95) is, that the umbilical cord should be cut off quite close to the placenta, the placenta squeezed as empty of blood as possible, the sac cleansed of all blood, loose membranes, etc., and then washed with warm water, the sutures carefully placed, the sac again washed out with clean water by means of a siphon trocar, and the stitches drawn tight, with a small trocar still in the wound. The sac should then be emptied of all the water possible, the trocar taken out with precautions against admission of air, and the *wound totally closed*. This is quite a new operation, and promises to be a great advance on any yet tried. Tait says he thought of it in consequence of the striking success of the same operation in congenital cysts. The results, in case of cysts, are much better than where drainage is employed. Tait has tried all ways of dealing with the placenta, and thinks this best. He has removed it by vaginal section disastrously. He has twice removed it in cases where both

the mothers and the children still survive. The hemorrhage in these cases was easily arrested by perchloride of iron; but in these cases he found it possible to tie a big pedicle of the remains of the tube and broad ligament, which contained most of the blood vessels, to the placenta. He thinks it is often possible to tie a pedicle with the main vessels in it, and it should always be done, and under such circumstances the placenta should always be *removed*. But in many cases the placenta is spread out on various tissues, and is so intimately adherent that it could only have been removed with great difficulty and hemorrhage. Tait thinks under such circumstances, although it could be removed, yet it is more prudent to let it alone. In three other cases he kept the placenta in situ, bringing the umbilical cord out at the lowest part of the wound and closing up all but this part. The whole three mothers recovered. In two of them it was unnecessary to open the peritoneum. In one it was opened, and in this case it was carefully closed again, except a little hole for the umbilical cord and the drainage tube. These patients had all to pass through an offensive course of suppuration, which lasted several months and came near killing each of them. Tait believes by his new operation he can get rid of all this suppuration in most cases. The histories of cases where extra-uterine gestation has gone on for long periods show that all the tissues of the foetus except the bones can be absorbed, and even the bones to some extent. The placenta is the first gestation tissue to disappear, although it continues to grow for a while after foetal death in many cases. But should the placenta grow and come to require subsequent removal, Tait says it would be an *immense gain* to the woman's chance of life to leave it over for a second operation when her health was restored. So he says "deal with the foetus only at the first operation." In fact, by this method Tait almost converts the "primary" into a "secondary" laparotomy; he gets the advantages of each, and avoids the drawbacks of each. One great drawback of the ordinary primary operation, when the wound is not hermetically sealed, is that decomposition is wantonly induced in the sac as a means of getting rid of the placenta, which, in the majority of instances, it is too dangerous to remove. The necrosis of the placenta is evidently a fearful risk to a woman in the puerperal condition. I think it will be admitted that a woman during the puerperal period is far more sensitive to all septic influences than women are at other periods; hence surgery which

permits of sepsis is apt to prove fatal to the puerperal woman. The reasons, perhaps, for this sensitiveness are not far to seek. The modern discovery of leucomaines and ptomaines in the healthiest state gives us the clue. As soon as involution of the uterus commences, waste matters are thrown into the blood in greatly increased quantity. These products of retrograde tissue changes have to be *oxidized* into substances which the organs are capable of eliminating. During this activity of the chemical processes of the body, no doubt more leucomaines and ptomaines are formed than is natural. In fact, during the puerperal state the woman is at the verge of poisoning, under the best circumstances; then, if, in addition to these self-bred poisons, others from decomposition are added, the poisoning becomes visible as some acute disease—septicæmia, or some fever with inflammatory complications. It may be objected that this is only hypothesis. This criticism is hardly true, as we now know that poisons are constantly being thrown out of the body by urine, sweat, bowels, and breath. We also know that anything which increases tissue change, as exercise, increases the amount of leucomaines excreted. We also know that if by excessive exercise of any kind we increase the production of poisons beyond the ability of the various organs to eliminate, disease is produced, as a simple or severe fever. We also know, and here there is a perfect analogy with the puerperal state, that men over-marched or over-fatigued, fall easy victims to cholera, or any other infection. The fatigued man is excreting self-produced poisons to the top of his power; if you throw in any more poison he fails to eliminate it, and perhaps it works in some zymotic way on his waste-laden blood, at all events, he takes infection very easily. The puerperal woman is excreting poisons to the top of her power, throw in another, and you very easily cause disease. It is well known how sensitive puerperal women are to the poisons of scarlatina, erysipelas, sewer-gas, and especially septic matter in the genital canal. Is there not more than theory, then, for delaying the decomposition of the placenta in primary laparotomy by closing up the wound? A case by Dr. Champneys ("Obstet. Trans.," 1887) illustrates the principle. The placenta was emptied of blood, but the cord was not cut short, but brought out of the wound, as it was thought the antiseptic system of dressing would prevent decomposition. The operation was done on the 19th October, and on the 19th November "the incision was completely healed, but the hypogastrium markedly

distended, and a swelling, which was supposed to be the placenta, was markedly smaller." It was soon evident, however, that there was decomposition of the placenta, and peritonitis in consequence. The patient was very ill: pulse, 114; temperature, 104° F., and even 106° F. She got worse till the 7th January, when death took place. On examination the placenta was found lying in the sac like a round ball as large as a foetal head, and of dark maroon color. The placenta was quite detached, save for a few bands and one or two adhesions. The patient could easily have been saved by re-opening the abdomen at the outset of serious symptoms and removing the placenta. Serious symptoms did not begin for five weeks after operation. She was allowed to suffer from septic poisoning for nearly six weeks without the slightest effort to remove the mass of decomposition.

The lesson is to close the wound hermetically to give every opportunity for absorption of the placenta, without decomposition. But should it not be absorbed, and, still more, should symptoms indicating decomposition come on, it should then be removed. This secondary removal of placenta would, probably, be both easy and safe; hemorrhage would be very unlikely, as thrombosis of the maternal sinuses would have occurred. Tait does not pretend that the question is yet settled by experience, but he claims that this is the most rational way of doing laparotomy for extra-uterine pregnancy at the full period or afterward, and all his experience in abdominal surgery of all kinds leads him to expect it will be a success. He says it is certainly not a question which will be settled by the tabulation of a number of cases, mostly dissimilar in the most extreme degree from one another, and incapable of leading to anything but confusion when put in the form of statistical evidence. All must admit that his description of the getting up of statistics of an operation, or of a disease, is only too true, and that, usually, the conclusions drawn, and rules of practice laid down, from an analysis of such jumbles of all sorts of things thrown together, are most misleading, and effectually prevent the advance of medical science. Simple ignorance is safe and hopeful, compared with error. An erroneous rule of practice, or an erroneous opinion based upon a lot of figures arranged so as to lie, remains in our text-books until it is learned by the whole profession. It probably holds the field for years, and when it is at last doubted, no heed is paid, for a long time, by the great bulk of the profession. When it is finally

acknowledged to be false, the whole profession has to gradually unlearn the lie; this mostly takes a new set of text-books, and, alas! a new crop of practitioners. Nothing is accepted as more than a new-fangled fancy till it appears in the text-books as an unquestioned truth; so that, when a lie so appears, it is easy to see how long it will take to undo the mischief caused. Simple ignorance is conscious of its want, and stands still; error thinks it knows the road, and rushes against fatal obstructions.

Campbell anticipated Tait in his idea, as, at page 152 of his work, he says: "As the placenta, when long retained, is destroyed during the suppurative process, except in some rare instances, and removed from the abdominal cavity with the other decomposed structures, or cannot be discovered, this discloses to us the important fact that the retention of the mass may be permitted without any detriment to the parent, while it can scarcely be doubted that the irritation, which could not fail to be produced by groping for it among the abdominal viscera, or the hemorrhage arising from its detachment, might be succeeded by formidable effects. At one period, it was supposed that the placenta could not be suffered to remain in the abdominal cavity with impunity; but it may be asked, Can the retention of the mass be more injurious to the patient than that of a full-grown foetus, which, as we are now aware, may remain in the abdominal cavity for a long series of years without any injurious effect?"

Supposing we do not care to do the operation in the way proposed by Tait, we can do it in the ordinary manner, except that it is very important to avoid the *linea alba*, as, no matter how decidedly the case may be extra-peritoneal, if we *open at the linea alba*, we are *sure to open the peritoneum*, thus adding greatly to the danger of the operation. No doubt, this is the reason why the fact, that most cases are extra-peritoneal, has failed to become generally known. The custom is to make the incision in the middle line, and it so happens that the peritoneum is stripped off from behind and below forward, so that in every case there is a strip of peritoneum left attached in front, the breadth of the uterus, and reaching down to the base of the bladder. If the middle line is incised, the peritoneum must be opened, and if a man sees that he has opened the peritoneum to get at a tumor, that, usually, settles the matter in his mind as to its being intra-peritoneal. So, no wonder the belief in "abdominal," or rather "intra-peritoneal,"

pregnancy has gone on so long. Then there can be no doubt that Tait is right on the

Management of the Placenta; that is, that it is much safer to leave it, as a rule. Thomas, Hicks, Barnes, Koeberlé, all agree on this point. Dr. A. Martin, of Berlin, indeed (*Brit. Med. Jour.*, Feb. 11th, 1888, p. 290), advocates its removal, and has had a successful case, and Tait has removed it twice, saving both mothers and both children ("Lectures on Ectopic Pregnancy," p. 96). But I think Tait proves that the dangers of removal in most cases more than counter-balance the dangers of leaving it, even as has been the case hitherto, when decomposition of the placenta has been invited by leaving the wound open. Even if we should prefer drainage of the cyst to hermetical closure, I fail to see what good can be gained by leaving the umbilical cord to rot in the cyst and in the wound; as it, at least, is easy enough removed, why not remove it? The drainage tube will keep the wound open, if we wish that. Now, Lusk states that the placenta is found in the line of incision into the abdomen in one case in six ("Midwifery," p. 339). I fancy this must be a mistake, unless we are to understand it as a mere tail or border of the placenta. The reasons for this belief are—1st. That Dr. James Braithwaite, of Leeds (*Brit. Med. Jour.*, Jan. 3d, 1885), in describing a case where operating, as usual, in the middle line, he came on the placenta as he had beforehand diagnosed, he expressly says that "this *situation* of the placenta is rare, and I think it may without much difficulty be diagnosed by the thickness of the structures intervening between the foetus and the examining hand." 2d. Tait ("Lectures," pp. 83, 84) "The placental relations are *always chiefly pelvic, generally wholly so.*" Again, speaking of Jessop's case, Tait says, "The placenta was found *where it is found in the great bulk of broad ligament cysts*, plastered over the pelvic contents." Besides, it is well known that the placenta is very often implanted into the *back of the uterus*, the peritoneum having been stripped off first. This is one reason why the uterus does not undergo involution until after the child has been some time dead. Of course the placenta is thinner and more spread out than in normal pregnancy, and may be widely implanted, but it is assuredly not the case that the central parts, or body of the placenta, are at all commonly planted on the anterior abdominal wall. The disadvantages of leaving the placenta in primary laparotomy are (1) the tedious and protracted

convalescence; (2) the danger of death from hectic and exhaustion, owing to long-continued suppuration of the sac and its contents during the process of disintegration and throwing off; (3) the possibility of secondary hemorrhage, such as occurred in a case reported by Braxton Hicks. Parry states that the frequency of secondary hemorrhage after spontaneous separation of the placenta, as compared with that after its forcible separation, is one to five. Still, one principal point of greater safety in the "secondary" laparotomy is that bleeding is not to be so much feared. I introduce this matter for the purpose of pointing out to country doctors that the operation of laparotomy, as done by the best surgeons, is one of extreme simplicity, and that no man who has received a medical education need fear to undertake it if he has only reflected on how he would proceed beforehand. The best surgeons don't attempt to remove the foetus, and they don't attempt to remove the sac, so that, unless there is intestinal obstruction, the adhesions and their separation do not concern him, and certainly, in the majority of cases, he should never ascertain their presence, or give his head any trouble concerning them. All he has got to do is to cut down on the cyst, extra-peritoneally, if he can, or if he can't avoid the peritoneum, then he should sew the edges of the cut, or ruptured cyst, to the edges of the abdominal wall where incised—that is all the extra trouble, if he unfortunately opens the peritoneum. Having incised the abdomen and then the cyst, he has only to remove the foetus, clean out the sac, cut off the cord, and close his wound either *completely*, as Tait will do for the future, or leave the lower part open and put in a large glass drainage tube, in the old style. If he adopt the drainage plan, he should keep the interior of the sac as dry as possible by sucking out all effusions several times daily with a syringe. It is the *fluids* of decomposition that are especially dangerous, the solid parts of a decomposing mass can only prove harmful by evolving gases which can be absorbed.

Perhaps I should say a few words on the

Treatment of Interstitial Ectopic Pregnancy, should it happen to be diagnosed. We have seen that, as a rule, it is absolutely impossible to diagnose it from tubal pregnancy, or pregnancy in one horn of a bi-lobed uterus. It is probably very rare. There are only five or six specimens in all the British museums. Mr. Alban Doran has stated that there are two in the museum of London Hospital, but Tait says

these are evidently broad ligament pregnancies Parry has 31 cases of interstitial pregnancy in his 500 extra-uterine gestations, but we have seen that, contrary to the maxim that "figures cannot lie," they are peculiarly liable to do so in effect, so we can put no trust in this statement, although if admitted it would support the idea that interstitial cases are infinitely rare. But if a case could be diagnosed before rupture the treatment would be to dilate the cervix, divide the septum in the wall of the uterus and empty the pregnant cornu. It would be of no benefit to kill the foetus, as the placenta would probably grow sufficiently to burst the cornu unless it were very fully developed. If not, the foetus, in all probability, would decompose, as it does, sooner or later, in nearly all cases of "*missed labor*," as well as of extra-uterine pregnancy, and in the supposed case the results of decomposition, besides poisoning the system even while in the cornu, would probably be poured into the cavity of the peritoneum by bursting of the cornu. When a cornual pregnancy bursts, prompt opening of the abdomen and hysterectomy, or removal of the ruptured horn, would probably save life, or at all events offer the only chance.

VAGINAL EXTRACTION OF THE CHILD IN EXTRA-UTERINE PREGNANCY.

This is an alternative operation to laparotomy at any period of the extra-uterine gestation, but especially at the full period or after foetal death. We have shown that it is more dangerous than abdominal section in the early stages of extra-uterine gestation, and we hope to make it equally clear that it is equally objectionable in advanced gestation. It looks, like the plunging of a trocar into the tumor, a much easier and safer proceeding, and, like puncture, it is the resource of timidity and bad surgery. One argument is enough, I think, to exclude it, viz., the fact that the placenta is usually in the pelvis, so that the incision would usually have to go through it, and even worse, the child would have to be dragged through the highly vascular site of the placenta, which, in itself, would usually be enough to turn the scale against the patient. Of course, it has been done with success; what operation has not? but the question is, is it the best, the easiest, the most rational and, in the long run, the most successful operation? I think it can easily be proved that it is none of these. It was done with success, both as regards mother and child, as early as 1817, by

Dr. John King, of Georgia. Campbell reports nine cases, with the saving of five mothers and five children. Parry, up to his time, made a collection of fifteen cases, with six recoveries. But recently it has fallen out of fashion and favor. Billroth, in his "Handbuch der Frauenkrankheiten," 5ter Abschn., p. 87, relates a case where Bandl, in 1874, operated by vaginal section under what he supposed to be most favorable circumstances; the death of the patient followed on the third day. In former times, it was thought to be a great point in the woman's favor, if the foetus could be easily felt from the vagina, so that all the operation called for was incision of the vaginal wall and application of the forceps. It was thus that Dr. John King, of Edisto Island, operated in 1816 or 1817. Indeed, if the tumor could be reached from the vagina, operation was looked on as quite legitimate.

But elyototomy, or vaginal section, has been done in the early stage of extra-uterine pregnancy, as well as at full time or afterward, as in Thomas' famous case (*New York Med. Jour.*, June, 1875), where he opened the gestation cyst, previous to rupture, with a platinum knife made incandescent by the galvano-caustic battery, by which he hoped to prevent hemorrhage. Through this incision in the vagina he removed the foetus, and attempted to remove the placenta, when such violent hemorrhage occurred that he had to inject the cyst with solution of persulphate of iron. The placenta decomposed, and was discharged as débris, the decomposition causing septicæmia, which nearly cost the patient her life, although he freely washed out the cyst with antiseptics. We have already criticised this case, the results of which operation could hardly ever equal Tait's 4.7 per cent. mortality with abdominal section. There is another case of elyototomy done at about the period of primary rupture, which gives a good idea of the procedure.

In the *Meditz. Obozr.*, Fasc. vii, 1885, p. 689, Dr. Maria O. Prujanskaia, of Moscow, gives the case of a patient, aged 29, who had been twice confined naturally, the last six years ago. She aborted in August, 1884, and again ceased to menstruate in November, when morning sickness, vomiting and the usual breast symptoms of pregnancy appeared. One day, at the commencement of December, as she stooped to put on her boots, she was seized with very severe pains in the abdomen, then giddiness, sickness, dyspnoea, fainting fits and sanguino-

lent vaginal discharge appeared. In the middle of December, and in the beginning and middle of January, 1885, similar attacks occurred. During the fourth attack, the author found the uterus slightly enlarged, softened, the fundus slightly deviated to the right by a firm, elastic, indistinctly fluctuating tumor, which was situated in the left side of the pelvis, and could be felt through the left arch of the vagina. The cervix uteri was patulous. Various common signs of pregnancy were present, and the author diagnosed a three months' extra-uterine gestation. The peritonitic symptoms with prostration, which, evidently, were caused by primary rupture, were put down to "partial rupture of the superficial layers of the foetal sac accompanied by hemorrhage into the peritoneal cavity." What are the superficial layers of the foetal sac but the walls of the Fallopian tube? The author proposed to kill the foetus by morphine injections or by electricity. As these were declined, nothing of any import was done. At the end of January, foetal movements were distinct through the abdominal walls. In the middle of February, a fifth attack of peritonitic symptoms caused the patient to become submissive to any treatment. Professor Snegireff diagnosed a left tubal pregnancy *at the point of rupture*, and performed elytrotomy by Paquelin's thermocautery; an incision about three fingers' breadth long into the roof of the vagina permitted easy podalic extraction of a live foetus of four and a half months. The umbilical cord was tied and divided, the placenta left *in situ*, the sac and vagina washed out with sublimate solution, and the cavity of the sac and the vagina filled with iodoform gauze. The operation was almost bloodless (the uterine artery had been tied immediately after the incision), and lasted only ten minutes. Death occurred the same night. No *post-mortem* examination was obtained. Now, here we have a case where elytrotomy was done in the most scientific way, where the placenta was not interfered with, where the most powerful antiseptics were used, where there was no hemorrhage to lay the blame on, and yet the result is speedy death.

A successful case at term, where the child and mother were both in good health years afterward, was related to the London Obstetrical Society by J. H. Matthieson, of St. Mary's, Ontario, on May 7th, 1884 (*Brit. Med. Jour.*, 1884, p. 999). As the course of the extra-uterine pregnancy down to the false labor was typical, I need not burden my pages with it, but merely describe the mode of operation

pursued. It was done without chloroform, by cutting with an ordinary knife, and tearing with the finger-nail. The face presented, but the child was easily extracted with forceps. The placenta was at the *posterior and left side of the cavity*, its lower border some two inches from the vaginal roof. It was easily peeled off, a sponge soaked in perchloride of iron following it up. There was very little bleeding. The clots were removed from the cyst, and its walls touched with perchloride. Much exhaustion followed. The placenta was tri-lobed. The cyst was washed out antiseptically after operation. The wound healed in three months. The patient had severe pain in the side for two years after the operation. Reading of the case was followed by debate. Dr. H. Gervis (President) doubted the propriety of the vaginal operation and of the removal of the placenta. Each case must be judged on its own merits, but, as a rule, he favored primary laparotomy and leaving the placenta. Dr. Playfair fully agreed with the President. Dr. Edis also advocated primary laparotomy, owing to the advances of abdominal surgery (in 1884). Dr. Griffith thought that the presence of a contractile cyst wall necessitated the gestation being either interstitial or in an undeveloped form of a double uterus. Dr. Champneys said that the uterine *platysma*, which extended widely over the pelvic organs, was sufficient to account for contractions, and that these occurred in undoubted extra-uterine pregnancies.

Dr. Herman, Obstetric Physician, London Hospital, has arrived at a number of important conclusions with regard to vaginal section, which he communicated to the Obstetrical Society of London at a special meeting for the discussion of the treatment of extra-uterine gestation during the later months of pregnancy. The meeting was on November 23d, 1887, and is reported in *Brit. Med. Jour.*, December 3d, 1887, p. 1213.

Dr. Herman said no general rules could apply to all cases and at all periods of gestation. He gave a case of his own in a patient aged 40. The extra-uterine pregnancy was preceded by a long period of sterility. Symptoms like those of rupture occurred at about two months' gestation. At nine months false labor occurred, and lasted nearly a month. The labor pains were accompanied by dilatation of the cervix. Then the pains ceased, the breasts became smaller, and the cervix contracted. The cyst was opened by vagina, the child removed, and carbolic washings of the cyst freely used. The placenta

came away on the sixteenth day. Cyst closed in $2\frac{1}{2}$ months. Dr. Herman has collected thirty-three cases of elyototomy for extra-uterine gestation, and from an analysis he drew the following conclusions :—

1. The operation by the vagina, previous to rupture, by the cautery knife or otherwise, is a *dangerous and unscientific* proceeding. Abdominal section ought always to be preferred.

2. Abdominal section is more likely to succeed than vaginal, after tubal rupture with hemorrhage threatening the life.

3. After primary rupture, when hemorrhage is followed by fever, the indications for vaginal incision are just the same as in hæmatocele from any other cause. (Tait agrees with this.)

4. At or soon after term, before suppuration has occurred, there may be conditions which indicate vaginal delivery as preferable to abdominal section. These are :—

5. *When the child presents by the head, breech, or feet, so that it may be extracted without turning ; and,*

6. *When it is quite certain, from the thinness of the structures separating the presentation from the vagina, that the placenta is not in the septum, and when it is not certain that the placenta is not on the anterior abdominal wall.*

7. If the child cannot be delivered without turning, prefer abdominal section.

8. No attempt should be made to remove the placenta.

9. Frequent washing out of the sac should constitute the after treatment.

10. After suppuration has occurred, the natural opening of the sac into the vagina is one of the more favorable probable terminations.

These are not Herman's exact words, but they contain the substance and spirit of his conclusions. Herman's only ground for preferring vaginal section in any case, seems to be the fear of meeting with the placenta on the anterior abdominal wall. Now, I think I have shown that the pelvis is the seat of the placenta in the vast majority of cases ; that the abdominal wall is a very rare site, and that it is not impossible to diagnose its presence there before operation. Dr. Braithwaite, of Leeds (*Brit. Med. Jour.*, Jan. 3d, 1885), met with a case of abdominal placenta, but he is of the opinion that it is rare and easily diagnosed. And in his case the incision (in the median line) was only two inches from the edge of the placenta, and it gave no trouble.

"Careful separation of the placenta in this direction did not produce hemorrhage." Lusk estimates that the placenta is on the anterior abdominal wall in one case out of six; but this is probably a mistake, made by raking up old statistics, as it is decidedly contrary to Tait's opinion, who has more personal experience than any living man. At the debate on Herman's conclusions, Mr. Knowsley Thornton made several weighty objections to Dr. Herman's conclusions. With conclusions 1 and 2 he agreed. He thought 3 was hardly precise enough. As to vaginal extraction, he doubted whether, in most cases, the diagnosis could be as easily made as Dr. Herman indicated, and he did not think the risk of *injuring a coil of intestine* was a small one, though this was not mentioned by Dr. Herman. He thought, with the present means of stopping hemorrhage, the possibility of the placenta being situated on the abdominal wall *need not be such an alarming complication of abdominal section as to influence the choice of operation*. The whole question of primary laparotomy hinged on perfect diagnosis, and knowing how often there was something wrong with the child in these cases he would disregard it altogether, and simply consider the mother, and urge that *operation should follow at once on certain diagnosis*. Lawson Tait said he quite agreed with Mr. Thornton as to the difficulty of diagnosis, but he never allowed any uncertainty to stand in the way of trying to serve his patient, and in doubtful cases he opened the abdomen. Dr. Herman in reply touched on two very important points: 1. The fact that in some cases it is extremely easy to make the diagnosis from the vagina, when the placenta is not between the presentation and the examining fingers. He said we could often feel the sutures of the skull; in that case he thought the intestines safe. 2. The condition of the placenta; Dr. Herman pointed out that there are two distinct kinds of placenta in extra-uterine gestation at an advanced stage. The first was the common variety, where it was thin and spread out very much, and where it was both difficult and dangerous to remove it in either operation. In the second variety the placenta was a thick, solid mass, with very narrow attachment. These are the placentæ of which we hear occasionally, which are removed in both forms of operation so easily. He believed the spreading out often occurred by growth and sprouting of the placenta *after foetal death*. If Dr. Herman is proved to be right on this point, it would be an additional argument for primary laparotomy.

But it is Lawson Tait who is the great opponent of vaginal section, and in dealing with Herman's conclusions, he says that, admitting them all to be perfectly correct, in Nos. 5, 6, and 7 Herman lays down objections which will exclude the vaginal operation; for, after foetal death (and the great majority of cases only present themselves when their supposed pregnancy has not been followed by delivery), it is *absolutely impossible to tell where the placenta is*, nor is it always certain even while the child is living. Tait declares he has twice failed to discover its seat, even with his hands in the foetal sac ("Lectures," p. 84). Tait also believes that the most expert obstetricians could not accurately ascertain the presentation of an extra-uterine foetus until the sac had been opened—at least, he once saw a very skillful man fail. Tait says that Dr. Herman has collected a series of twenty-three cases of elytrotomy (the *Brit. Med. Jour.* reports Herman as saying thirty-three), with *fourteen* mothers and only *one child saved*. Tait has searched out several others, but he says this kind of research is really without value, for there are so many points of discrepancy that it is utter folly to argue from such tables to any general, and much more any particular, conclusion. That elytrotomy is very unsatisfactory as to the child is proved by the constant difficulties experienced in removing the foetus, and by the fact that there are only two cases where it survived for any length of time. Tait says the mortality of all the collected cases is over 60 per cent.; but he says this is not half so good an argument against the operation as the reports of the tearing of the parts which was revealed at the post-mortem examinations, and the concealed hemorrhage which was almost always the real cause of death. He says he has done it once, but shall never, under any circumstances whatever, operate on an extra-peritoneal pregnancy from the vagina. In his case, reported at length in *Med. Times and Gazette*, 1873, the patient had been allowed to run down—the great cause of the high mortality of all these operations; and although he easily delivered an eight months' foetus which had been dead some time, and even removed the placenta without the least hemorrhage, she died in a few hours. He says that, in this instance, the temptation to resort to elytrotomy was great, for the foetus felt as if only separated from the finger by mucous membrane. It seemed as if a scratch of the nail would free the child, and he thinks this was the temptation to most who have done the operation, but he says it is clearly wrong for two reasons: 1. The placental relations are

always chiefly pelvic, *generally wholly so*, the child must, therefore, be dragged through tissues in which large sinuses have been abnormally developed, tearing and damaging them, and with every probability of killing both mother and child. And if, as so often happens, there be bleeding from the torn maternal sinuses, it is quite hopeless to find them, or secure the bleeding points. Tait concludes that opening the peritoneal cavity, where it must be opened, is a "clumsy and risky procedure under *any* circumstances," and has no advantage over the abdominal method, while it has many disadvantages. But unless people will persist in making the incision in the middle line of the abdomen, there is really no necessity for opening the peritoneum in many cases, at least. This fact is exemplified in a case reported in the (*Nouv. Arch. d' Obstet. et de Gyn.*, Feb. 25th, 1887) practice of Dr. Busch, who did the right thing without probably knowing anything about the rearrangement of the peritoneum which occurs in broad ligament pregnancies, as proved by Tait, Berry Hart and Carter, and which makes the foetus so easy to get at unless we go to the middle line of the abdomen, where the peritoneum is never peeled off. In the case of Dr. Busch, the patient was aged 27, and had already been delivered of a child. The present gestation had gone on normally until between the second and third month. She had then a good deal of abdominal pain. At the onset of false labor extra-uterine gestation was diagnosed. It was only eight months later, however, that an operation was decided on. An incision four centimètres in length was made above and *parallel* to Poupart's ligament, and as soon as the *skin was cut through* the foetal head was seen. Craniotomy was performed and the foetus extracted in bits, so that it might be unnecessary to enlarge the incision. The cyst was washed out with boracic acid solution, and recovery was rapid and complete.

Owing to the author's residence abroad, it has not been possible for him to either revise this essay, or to correct the proof sheets.

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	PAGE
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INDEX.

- Abdominal and uterine walls, thinning of, 44.
collapse, 36.
section, country doctors could and should perform, 46.
puncture more dangerous than, 51.
- Abortion, supposed, should suggest extra-uterine gestation, 13.
ordinary, diagnosis from, 13.
- Abscess, pelvic, diagnosis from, 65.
suppurating sac from, diagnosis of, 54.
- Absorption of foetus, 85.
- Advance in diagnosis, from knowledge of the absence of symptoms, 30.
- After rupture cases should be nursed to viable period, 101.
- Albuminous urine without kidney disease, 15.
- Albuminuria no bar to operation in pregnancy, 16.
- Amenorrhœa, pregnancy falsely excluded because of absence of, 15.
- Aspirator never to be used in diagnosis, 51.
- Barnes' views on symptoms, 17.
objections to, 18.
- Berry Hart and Carter confirms Tait's views, 5.
- Best treatment of extra-uterine pregnancy, 90.
- Bland, Sutton and Arthur Johnstone confirm Tait's views, 12.
- Bleeding, intra-peritoneal, symptoms of, 36.
- Bi-manual examination replaces sound, 19.
- Braun passed sound through fundus uteri, 19.
- Cancer of pelvis, diagnosis from, 65.
of peritoneum, diagnosis from, 65.
- Carter and Berry Hart confirm Tait's views, 5.
- Case where diagnosis was difficult, 21, 22.
where diagnosis was easy, 20.
- Cases of extra-uterine pregnancy nearly all tubal, 5.
- Cause of extra-uterine pregnancy, 12.
- Cellulitis, pelvic, diagnosis from, 65.
- Cervix patulous in extra-uterine pregnancy, 13.
- Character of hæmatocele dependent on direction of rupture, 5.
- Collapse, abdominal, 36.
- Collective investigation almost worthless, 6, 7.
- Communication of sac with small intestine and uterus, 58.
- Conclusions as to diagnosis, 23.
- Current, electrolytic power of, 84.
- Cysts of round ligaments, diagnosis from, 63.
ovarian, mistaken for foetal hands and feet, 47.
- Dangers of retained foetus, 56.
in using sound, 19.
- Death, foetal, at period, diagnosis after, 48.
- Decidua never forms in tubes, Virchow, 14.
thrown off in particles or *en masse*, 14.
- Dezeimeris first recognized extra-peritoneal rupture, 5.
- Diagnosis about viable period, 43.
after foetal death at period, 48.
after rupture, 28.
after shock of rupture has passed, 42.
aspirator never to be used in, 51.
at time of rupture, 34.
a substitute for the uterine sound in, 10.
conclusions as to, 23.
difficulties of, illustrated, 20.
difficult when foetus passes through uterus, 59.
evident if foetal bones have appeared, 54.

- Diagnosis, false and true, principles of, 27.
 false, baneful to medicine, 6.
 from cancer of pelvis, 65.
 of peritoneum, 65.
 cysts of round ligament, 63.
 distention of Fallopian tube, 63.
 normal pregnancy, 60.
 pelvic abscess, 65.
 cellulitis, 65.
 retroversion of gravid uterus, 47.
 small ovarian tumors, 47.
 simple hæmatocele, 64.
 sub-peritoneal tumors, 63.
 uterine tumors, 63.
 knowledge of absence of symptoms
 the principal advance in, 30.
 missed on history; extra-uterine preg-
 nancy began when patient was
 suckling, 21.
 of extra-uterine character of preg-
 nancy, 7.
 normal pregnancy with displaced
 uterus, 44.
 pregnancy in one horn of bi-lobed
 uterus, 45.
 suppurating sac from pelvic abscess,
 54.
 practically impossible, 7.
 previous to rupture, Parry on diffi-
 culties of, 17.
 rare, previous to rupture, 11.
 source of false; failure of doctor to
 recognize the possibility of extra-
 uterine pregnancy, 15.
 use of sound in, 18.
 Difficulties of diagnosis illustrated, 21.
 Does foetal death benefit the mother, 76.
 Duverney's case of extra-uterine preg-
 nancy, 98.
- Electricity, fatal objections to, 88.
 previous to rupture, 82.
 Electrolytic power of current, 85.
 Elytrotomy previous to rupture, 81.
 Encystment, how it happens, 37.
 Evidence, doubtful, a hindrance to medi-
 cine, 6.
 Examination, bi-manual, the sound re-
 placed by, 19.
 Exploratory incision, 51.
 Extra-peritoneal rupture, first recog-
 nized by Dezeimeris, 5.
 hæmatocele, 5.
 hemorrhage not fatal, 14, 35.
 Extra-uterine pregnancy, diagnosis of, 7.
 began when patient was suckling;
 diagnosis missed on history, 21.
- Extra-uterine pregnancy, best treatment
 of, 90.
 beyond four months, rupture occurs in
 broad ligament, 6.
 cause of, 12.
 cervix patulous in, 13.
 condition of menses misleading in, 11.
 detachment of placenta in, 15.
 diagnosis of, from ordinary abortion,
 13.
 Duverney's case of, 98.
 evolution of uterus in, 13.
 failure of doctor to recognize the pos-
 sibility of, source of false diag-
 nosis, 15.
 Gervis' case of, 58.
 Jessop's case of, 98.
 Macdonald's case of, 57.
 Matthews Duncan's case of, 77.
 metrorrhagia in, 11.
 modern pathology of, 1.
 nearly all cases tubal, 5.
 Nonat's case of, 98.
 normal pregnancy mistaken for, 30.
 retention of urine in, 47.
 treatment of, 68.
 Veit's opinion of, 103.
- Fæces, recovery from operation on foetal
 sac containing, 57.
 Fallopian tube, diagnosis from disten-
 tion of, 63.
 Fallopian tubes, not found in lower ani-
 mals, 3.
 Feet and hands, foetal, small ovarian
 cysts mistaken for, 51.
 found above pelvic brim, 51.
 Fluctuation in the uterus, Rasch's sign
 of pregnancy, 10.
 Foetal heart heard by vagina at eleventh
 week, 8.
 Foetus only dealt with at first operation,
 112.
 does killing the, cause tubal rupture?
 72.
 growth of placenta after killing, 75.
 morality of killing, before tubal rup-
 ture, 75.
 nature's powers of absorbing, 85.
 objections to killing, 74.
 passes through uterus, 59.
 removed without opening peritoneum,
 6.
 retained, dangers of, 56.
 uncertainty and danger of killing, 77.
 Fry's sign, 9.
 Fundus uteri, sound passed through, by
 Braun, 19.

- Gervis' case of extra-uterine pregnancy, 58.
- Gestation, extra-uterine suspected in supposed abortion, 13.
 modern pathology of, 1.
 operation for ruptured tubal, 96.
 site of tubal rupture determines further course of, 14.
 treatment at full period of, 105.
- Growth of placenta after killing foetus, 75.
- Hæmatocele, character of, dependent on direction of rupture, 5.
 extra-peritoneal, 5.
 intra-peritoneal, 5.
 simple, diagnosis from, 64.
- Hands and feet, foetal, small ovarian cysts mistaken for, 51.
 found above pelvic brim, 51.
- Health, leucomaines and ptomaines in, 113.
- Heart, foetal, heard by vagina at eleventh week, 8.
- Hegar's sign of pregnancy, 9.
- Hemorrhage at false labor and decrease in size of tumor constant and important symptoms, 49.
- Hemorrhage, extra-peritoneal, not fatal, 14, 35.
 intra-peritoneal, fatal, if left alone, 14, 35.
- Herman's opinion on diagnosis, 25.
- History, diagnosis missed on; extra-uterine pregnancy began when patient was suckling, 21.
 of operation for tubal rupture, 91.
 previous to impregnation, 12.
- Hypodermic injections of nitroglycerine in syncope, 95.
- Impregnation, history previous to, 12.
- Incision, exploratory, 51.
- Injection of poisons into sac, 80.
- Interstitial pregnancy, rarity of, 6.
 rupture of into peritoneum, 6.
 time of rupture of, 6.
 treatment of, 117.
- Intestine, gangrenous, recovery from operation on, 57.
 small, communication of sac with, 58.
- Intra- and extra-peritoneal hemorrhage, difference in fatality discussed, 14, 35.
- Intra-ligamentous extra-uterine pregnancies after tubal rupture, 5.
- Intra-peritoneal hæmatocele, 5.
- Intra-peritoneal bleeding, symptoms of, 36.
 hemorrhage, fatal, if left alone, 14, 35.
 pregnancy, primary, denied by Berry Hart and Carter, 6.
- Intra-venous saline injection, 93.
- Investigation, collective, almost worthless, 6, 7.
- Jessop's case of extra-uterine pregnancy, 98.
- Johnstone and Sutton confirm Tait's views, 12.
- Kiwisch found that true decidua never forms in tubes, 14.
- Labor, hemorrhage at false, an important symptom, 49.
 spurious, 47, 48.
- Laparotomy, Tait's results in primary, 107.
- Leucomaines and ptomaines, in health, 113.
 removed by oxidation, 113.
- Ligament, broad, extra-uterine pregnancies beyond four months rupture in, 6.
 cysts of round, diagnosis from, 63.
- Lower animals have no Fallopian tubes, 3.
- Macdonald's case of extra-uterine pregnancy, 57.
- Matthews Duncan's case of extra-uterine pregnancy, 77.
- Maury confirm's Tait's pathology, 52.
- Medicine, doubtful evidence a hindrance to, 6.
 false diagnosis the bane of, 6.
- Menses, condition of misleading in extra-uterine pregnancy, 11.
- Metrorrhagia in extra-uterine pregnancy, 11.
- Micturition, frequent, especially at night, a sign of pregnancy, 10.
- Middle line, in incision, avoid, 111.
- Morality of killing foetus before tubal rupture, 75.
- Mortality, Tait's, at section for tubal rupture, 46.
- Thomas' opinion as to percentage of, in Tait's operations, 46.

- Mortality, Thorburn's opinion as to percentage of, in Tait's operations, 46.
- Nature's power of absorbing foetus, 85.
- Necrosis of placenta, no necessity to wantonly induce, 112.
- Nitroglycerine, hypodermic injections of, in syncope, 95.
- Normal pregnancy mistaken for extra-uterine, 30.
- Nonat's case of extra-uterine pregnancy, 98.
- Objections to Barnes' views, 17.
killing foetus, 74.
- Observation, imperfect, the shame of medicine, 6.
- Operation, foetus only dealt with at first, 112.
for ruptured tubal gestation, 96.
history of, 91.
in pregnancy, albuminuria no bar to, 16.
gangrenous intestine, recovery from, 57.
primary as successful as secondary, 107.
new details of, 107.
resuscitation of patient for successful, 93.
- Thomas', from vagina, 81.
- Operations, Tait's, Thomas' opinion as to percentage of mortality in, 46.
Thorburn's opinion as to percentage of mortality in, 46.
- Ovarian cysts mistaken for foetal hands and feet, 51.
pregnancy denied by Berry Hart and Carter, 5.
tumors, small, diagnosis from, 47.
- Ovum may not burst at tubal rupture, 14.
- Parry on comparative danger of puncture and abdominal section, 51.
on difficulties of diagnosis previous to rupture, 17.
- Pathology, modern, of extra-uterine gestation, 1.
Tait's, confirmed by Maury, 52.
- Patulous uterus constant, 50.
- Pelvic abscess, diagnosis from, 65.
brim. hands and feet found above, 51.
cellulitis, diagnosis from, 65.
- Pelvis, cancer of, diagnosis from, 65.
- Peritoneum, avoid opening, 111.
cancer of, diagnosis from, 65.
foetus removed without opening, 6.
rupture of interstitial pregnancy into, 6.
tubo-ovarian pregnancy must rupture into, 32.
- Peritonitis rare after rupture, 37.
- Petit's opinions retarded knowledge for 175 years, 11.
- Placenta, detachment of, in extra-uterine pregnancy, 15.
growth of, after killing foetus, 75.
necrosis of, no necessity to wantonly induce, 112.
situation of, determines site of rupture of tube, 14.
Tait condemns removal of, 112.
- Poisons, injections of, into sac, 80.
- Pregnancies, extra-uterine, beyond four months rupture in broad ligament, 5.
- Pregnancy, extra-uterine, diagnosis of, 7.
cause of, 12.
cervix patulous in, 13.
condition of menses misleading in, 11.
detachment of placenta in, 15.
diagnosis of, from supposed abortion, 13.
Duverney's case of, 98.
evolution of uterus in, 13.
failure of doctor to recognize the possibility of, source of false diagnosis, 15.
falsely excluded because of absence of amenorrhœa, 15.
Gervis' case of, 58.
in one horn of bi-lobed uterus, 45.
Jessop's case of, 98.
Macdonald's case of, 57.
Matthews Duncan's case of, 77.
metrorrhagia in, 11.
most women have no suspicions of, until rupture occurs, 11.
Nonat's case of, 93.
normal, mistaken for, 30.
occurred when patient was suckling; diagnosis missed on history, 21.
retention of urine in, 47.
rupture occurs before certainty of, 8.
treatment of, 68.
Veit's opinion of, 103.
with displaced uterus, diagnosis of, 44.
interstitial, rarity of, 6.
rupture into peritoneum, 6.
time of rupture of, 6.
treatment of, 117.
normal, diagnosis from, 59.

- Pregnancy, normal, elevation of temperature in vagina as a sign of, 9.
 frequent micturition, especially at night, a sign of, 10.
 Hegar's sign of, 9.
 operation in, albuminuria no bar to, 16.
 pulse of, 8.
 Rasch's sign of, 10.
 sphygmographic tracings as a sign of, 8.
 uterine souffle due to, distinguished from souffle due to other disorders, 18.
 ovarian and intra-peritoneal, denied by Carter and Berry Hart, 6.
 sub-peritoneo-abdominal, 6.
 tubal, not rare, 71.
 twice occurred in unsuspecting patient, 12.
 Primary operation, new details of, 107.
 Principles of false and true diagnosis, 27.
 Ptomaines and leucomaines in health, 113.
 removed by oxidation, 113.
 Puerperal women are sensitive to sepsis, why, 113.
 Pulse of pregnancy, 8.
 Puncture more dangerous than abdominal section, 51.
- Rasch's sign of pregnancy, 10.
 Registrar General's returns incorrect, 7.
 Removal of foetus without opening peritoneum, 6.
 Resuscitation of patient for successful operation, 93.
 Retroversion of gravid uterus, diagnosis from, 47.
 Rupture, a case where the doctor was consulted before, 15.
 treatment between time of, and viability, 104.
 diagnosis after, 28.
 at time of, 34.
 electricity previous to, 82.
 elytrotomy, previous to, 81.
 diagnosis after shock has passed, 42.
 no opportunity for diagnosis previous to, 11.
 occurrence of, before certainty of pregnancy, 8.
 of interstitial pregnancy into peritoneum, 6.
 of tube, situation of placenta determines site of, 14.
- Rupture, Parry on difficulties of diagnosis previous to, 17.
 period of, 32.
 peritonitis rare after, 37.
 secondary, 48.
 treatment, at time of, 90.
 between time of and full period, 98.
 prior to, 72.
 tubal, does killing foetus cause? 72.
 history of operation for, 91.
 ovum may not burst at, 14.
 site of, determines further course of gestation, 14.
 Tait's mortality at section for, 46.
- Sac, communication of, with small intestine and uterus, 58.
 foetal, containing faeces, recovery from operation on, 57.
 injection of poison into, 80.
 suppurating, from pelvic abscess, diagnosis of, 54.
 Secondary operation, primary operation as successful as, 106.
 rupture, 48.
 Section, abdominal, puncture more dangerous than, 51.
 vaginal, 118.
 Sepsis, why puerperal women are sensitive to, 113.
 Sign, Fry's, 9.
 frequent micturition, especially at night, a, 10.
 Hegar's, 9.
 Rasch's, 10.
 sphygmographic tracings as a, 8.
 Souffle, uterine, due to pregnancy distinguished from souffle due to other disorders, 18.
 period of cessation of, 48.
 Sound, passed by Braun through fundus uteri, 19.
 use of the, replaced by bi-manual examination, 19.
 Spurious labor, 47.
 "Sub-peritoneo-abdominal pregnancy," 6.
 Sutton and Johnstone confirm Tait's views, 12.
 Symptoms, Barnes' views on, 17.
 hemorrhage at false labor and decrease in size of tumor important, 49.
 of intra-peritoneal bleeding, 36.
 Syncope, hypodermic injections of nitroglycerine in, 95.

- Tait's law, 1878, 70.
 pathology confirmed by Maury, 52.
 views confirmed by Bland, Sutton and Arthur Johnstone, 12.
 confirmed by Carter and Berry Hart, 5.
- Temperature, elevation of, in vagina as a sign of pregnancy, 9.
- Thomas' operations from vagina, 81.
 opinion as to percentage of mortality in Tait's operations, 46.
- Thorburn's opinion as to percentage of mortality in Tait's operations, 46.
- Treatment at full period of gestation, 105.
 at time of rupture, 90.
 between time of rupture and full period, 98.
 and viability, 104.
 of extra-uterine pregnancy, 68.
 interstitial pregnancy, 117.
 prior to rupture, 72.
- Tubal, nearly all cases of extra-uterine pregnancy, 4.
 pregnancy twice occurred in unsuspecting patient, 12.
 rupture, diagnosis previous to, 7.
- Tube, Fallopian, diagnosis from distention of, 63.
- Tubes, Fallopian, not found in lower animals, 3.
- Tubo-ovarian pregnancy always ruptures into peritoneum, 32.
- Tumor, decrease in size of, constant and important symptom, 49.
 relation of uterus to, constant, 46.
- Tumors, small ovarian, diagnosis from, 47.
 sub-peritoneal, diagnosis from, 63.
- Urine, retention of, in extra-uterine pregnancy, 47.
- Uterine and abdominal walls, thinning of, 44.
 souffle heard by vagina before third month, 10.
 sound, substitute for, in diagnosis, 10.
- Uterus, communication of small intestine with, 58.
 evolution of, in extra-uterine pregnancy, 13.
 fluctuation in, Rasch's sign of pregnancy, 10.
 normal pregnancy in one horn of bi-lobed, diagnosis of, 45.
 pregnancy with displaced, diagnosis of, 44.
 patulous constant, 50.
 relation of, to tumor constant, 50.
 retroversion of gravid, diagnosis of, 47.
- Vagina, elevation of temperature in, as a sign of pregnancy, 9.
 foetal heart heard by, 8.
 Thomas' operation from, 81.
 uterine souffle heard by, before third month, 10.
- Vaginal section, 118.
- Veit's opinion of extra-uterine pregnancy, 103.
- Viability, treatment between time of rupture and, 104.
- Virchow found that true decidua never forms in tube, 14.

