scheme will be then any more productive of real gain. I demand for them, however, a share in the advantages.

I also pointed out the danger of private workers in the provinces being swamped by London committees—*experto crede*. I ran that danger, but was not swamped. Some years ago, I was working at the subject of hydrophobia; for some years, I had been collecting material, and, fortunately, had my MSS. in order, but not quite ready for the printer. I had published a few pages of matter. A publisher informed me that there was a Committee appointed in London to investigate this subject. This Committee had on it Drs. Burdon Sanderson, Dr. Lauder Brunton, Mr. Callender, Dr. Greenfield, Mr. Ernest Hart, etc. I saw clearly that, in the face of such a Committee, any words of mine would not have a chance of a hearing, and that my only hope of safety lay in the speedy publication of anything I had to say. I rushed my MSS. into book-form, at a considerable sacrifice of time, with imperfections which would not have occurred had I not been thus hurried, for I reckoned on a couple of years' more time to complete my observations. The success of my book I need only briefly allude to, thanks especially to Sir Joseph Fayrer, who reviewed it in *Brain*. The book became to me not only a financial success, but I also won what we are all striving for—some *kudos*. Had I waited until this Committee published their report I should probably not have had a reader. At the end of my book, I said, as I was compelled to do, "I feel a pleasure in leaving further investigation in the hands of such competent experimenters as Drs. Burdon Sanderson and Lauder Brunton, to the publication of whose researches I look forward with interest and hope." I naturally felt that I must give way before such names, and that I was virtually effaced. What publisher would look at the MS. of an obscure provincial practitioner in the face of such a Committee, even though my facts were as good? He would decline it; I know this from experience. I got over the difficulty by publishing my work at my own expense.

5. What, after all, must we expect to obtain from collective investigation, supposing that it realised the expectations of its promoters? Individual opinions, which will only have their value either from the personal bias of the secretary or of the framer of the card.

The Committee, in their first report, stated: "That the success will much depend upon the energy, perseverance, and judgment of the secretary." I said, in one of my letters, "that I had no doubt the largest share of work would fall upon him; and that he ought to be a histologist, physiologist, physicist, therapist, and hygienist."

The Committee were fortunate in securing the services of Dr. Mahomed, but naturally they could not expect to retain his services long. Any man of ability would simply use this as a stepping-stone, so that we have now a new secretary, who in turn will be succeeded by another; thus we shall have ever changing opinion in the interpretation of the facts submitted. But, it will be answered, the opinions of the secretary will always be checked by the Committee or the framer of the card. Very true, but even then we shall be liable to individual opinions and crotchets, in the absence of the philosophical faculty which makes discovery rare; but, admitting this power of veto possessed by the Committee, we shall still be largely at the mercy of the secretary, who will first sift the cards and draw up the report.

6. The Association is now spending in collective investigation a very large sum annually, and I think it is time the members should look carefully into it, and ask themselves the questions: Is such an outlay required? Will scientific medicine be benefited to a proportionate extent by such a disposition of the funds? Could we not devote our surplus money to better purposes? I have endeavoured to show that, up to the present, the outlay has not been productive of proportionate results.

I trust that before the Belfast meeting the subject will be fully ventilated in the columns of the JOURNAL, and that my observations may lead to healthy criticism of the scheme. I would ask the members to leave on one side the man who has said these things, and simply to consider "what has been said," and to discard altogether the bugbear of authority.

We may be told that, in several countries, this scheme has been copied. Man is an imitative animal, and *primum facie* there is a glamour about collective investigation which appeals to some of our instincts. It has called forth some eloquent speeches, and I could myself write a very readable paper in favour of it, for the subject is very broad. I have taken, however, a side of the picture which has not been presented to the members.

I am as desirous of scientific advancement as any man in the profession, and I am also desirous of the general good of the Association; but I hold that scientific medicine will advance without this subsidy, and that the greatest good to the greatest number of the Association is not secured by this scheme.

Admirable scientific work and scientific research have been done in England by private workers, without any pecuniary assistance from funds. Stimulated by ambition or love of science, numbers of men have contributed to the elucidation of the problems of disease at their own expense, and have published the result of their researches on the usual marketable terms.

In the future, as in the past, there will always be found men able and willing to throw themselves into scientific work, in order to clear up some of the problems which disease opens out. Such subjects as micro-organisms, and the properties of iodoform, the nature of phthisis, diphtheria, chorea, rheumatism, acute gout, are now occupying the attention of scientific workers in all parts of the civilised world; and, when that inspiration comes—which is of the nature of the poetic faculty, rare—we shall receive that light for which we are longing.

The various medical societies in London and in the provinces are also engaged in the same class of work, so that there is no fear of scientific medicine being left in a neglected condition. In conclusion, then, I hold it to be unnecessary to devote from the funds of the Association—

1. £200 a year, with £100 a year for travelling-expenses, for a Secretary to the Collective Investigation Committee.

2. £200, in two endowments of £100 each, to two men selected by the Committee for some special researches. The class of work undertaken by the men thus endowed will be worked out as well by private workers, and there is danger of private workers being suppressed by this expenditure.

3. A sum varying from £300 to £500 a year on cards, memoranda, etc., relating to the special subjects selected by the Investigation Committee.

The reason of my objection is well expressed in the phrase, *Le jeu ne vaut pas la chandelle*. The funds of the Association might be devoted to more useful and universal objects, whereby science would be equally advanced, and the general bulk of the members benefited.

FIVE CASES OF EXTRA-UTERINE PREGNANCY OPERATED UPON AT THE TIME OF RUPTURE.

By LAWSON TAIT, F.R.C.S. Ed. & Eng.,

Surgeon to the Birmingham and Midland Hospital for Women.

PENDING the discussion on the pathology and treatment of extra-uterine pregnancy, which is to take place at Belfast, I desire to place on record this, the first series, as I believe, of cases of extra-uterine pregnancy operated upon at the time of rupture; that is, from the tenth to the thirteenth weeks. Most of us are familiar with such dramatic incidents as that of the actress in the Bois de Boulogne, where sudden or very rapid death has occurred from hæmorrhage due to the tear of a venous sinus in the rupture of an early Fallopian pregnancy. I have been unfortunate enough to see a large number of them, five or six and twenty, and of late I have been encouraged by my success in other abdominal diseases to try what surgery could do in these cases.

For this treatment, of course the difficulty was the diagnosis, but as I have now completely adopted the principle of always opening the abdomen when I find a patient in danger with abdominal symptoms, this barrier no longer exists. The diagnosis is, however, not so very difficult after all, for in many cases the existence of pregnancy has been suspected before the rupture occurred. It may be in the majority, however, that this misleading feature is present; the patient has never been pregnant, or has not been so for many years, and then the arrest of menstruation attracts no particular attention. If, however, it be found that the patient has been eight weeks or more without a period, that there is a pelvic mass fixing the uterus and on one side of it, and that sudden and severe symptoms of pelvic trouble and hæmorrhage came on, the rupture of a tubal pregnancy may be at once suspected, and if an operation is to be done—and it clearly ought to be done—it must be done without delay. Early interference is clearly a chief element of success in modern abdominal surgery.

The first case to which I was called after I had made up my mind as to the line to be adopted in such cases, occurred on January 17th, 1883, in the practice of Mr. Spackman of Wolverhampton. He had already made the diagnosis, and I was of opinion that he was perfectly correct. The patient had not had a child for many years; menstruation had been arrested for eleven weeks, and symptoms of rupture had occurred. When I saw her she was blanched from hæmorrhage, and her skin had the peculiar staining which is characteristic of the extravasation of blood into the peritoneum. The contents of the

pelvis were fixed, and there was a distinct mass on the left side. The abdomen was distended, and the patient in a good deal of pain.

Though I feared that interference might have come too late, still I advised operation, and immediately proceeded to carry it out. I opened the abdomen, and found a quantity of clot derived from a ruptured Fallopian pregnancy on the left side. As well as I could, I stitched the edges of the rent to the abdominal wound, but every touch caused hæmorrhage, so that I had to desist without doing much save removing the fetus and some of the placenta. The patient never regained consciousness, and died shortly after being removed from the operating table.

On March 1st, 1883, I saw, with Dr. Page of Solihull, a patient who had not been pregnant for many years, in whom there was a fixed mass in the pelvis, and whose menstruation had been arrested for about three months. She had a high pulse and an exalted temperature and great pain. I advised abdominal section, and found the abdomen full of clot. The right Fallopian tube was ruptured, and from it a placenta was protruding. I tied the tube and removed it. I searched for, but could not find the fetus, and I suppose it got amongst the folds of intestine and there was absorbed. Certainly it has not since been seen. The patient made a very protracted convalescence, but she is now perfectly well.

On April 9th, 1884, I opened the abdomen in the case of a patient whom I had seen a few days previously with Dr. J. W. Taylor, of Moseley, Birmingham. She had symptoms of acute pelvic inflammation, the organs being fixed, and there was a mass behind and to the right of the uterus. She also had not been pregnant for many years. I admitted her to my private hospital; and during the removal, doubtless, the rupture occurred. At the operation, I found the abdomen filled by a quantity of blood-serum and free clot. The left tube was distended by a placenta, which protruded through a rupture. I tied and removed the tube, but I could not find the fetus. She made an easy recovery, and now has almost completed her convalescence.

On May 25th, I operated upon a patient in the Hospital for Women, aged 27, who had had two children, the last having been born three years ago. She had menstruated regularly till three months previously. Pelvic pain had been in existence for some weeks, and had become steadily worse. I found the uterus fixed, and a larger tender mass on the left side; and I diagnosed the case as possibly one of pyosalpinx. At the operation, I found the left Fallopian tube had burst, and that there was a quantity of loose clot in the pelvis. The fetus was lying in the pelvis, attached to the placenta, which remained in the tube. The tube was adherent to surrounding structures, but was easily detached, tied, and removed. She made an easy recovery; the wound has completely healed, and she has left the hospital.

The fifth case has just occurred, an illustration of the curious sequence of exceptional cases often seen in practice.

A. M., aged 34, married fifteen years, has had four children, the last having been born six years and a half ago. Her last menstruation was early in April. Late in May she had an attack of intense abdominal pain, which increased in intensity till June 3rd. I found the uterus fixed, and a boggy swelling to the left. The abdomen was swollen, and peritonitis was clearly beginning, the pulse and temperature being both high, intense pain occurring occasionally, and the patient's expression being anxious. I opened the abdomen on June 5th, and found the pelvic contents matted together with recent lymph and blood-clot. I found the left tube torn almost in two, and occupied by a placenta, apparently of about the tenth week. I could not find the fetus. I tied and removed the tube, drained the pelvis, and the patient has made an easy recovery.

These cases all confirm the view of the pathology of extra-uterine pregnancy, which I advanced many years ago, that in origin it is always tubal, and that its varieties depend merely on the direction in which rupture occurs. These results also confirm the soundness of the policy of interfering early in such cases, for four out of the five have been easily and completely cured of one of the most formidable conditions of pregnancy. The first and only fatal case might have had a better ending if Mr. Spackman had seen her sooner, for it is only just to him that I should say he recognised the nature and gravity of the case at once, and sent for me immediately.

SOCIETY FOR THE STUDY AND CURE OF INEBRIETY.—An adjourned discussion on the papers of Dr. W. B. Carpenter, F.R.S., and Mr. Axel Gustafson, will be opened by Mr. Jabez Hogg, on Tuesday, July 1st, 1884, in the rooms of the Medical Society of London, 11, Chandos Street. The President, Dr. Norman Kerr, will take the chair at 4 o'clock. Dr. H. Rayner, Surgeon-Major G. K. Poole, Dr. C. R. Francis, Mr. Harrison Branthwaite, Dr. H. W. Williams, Dr. Joseph Rogers, and others, hope to take part in the discussion.

A CASE OF STRYCHNIA POISONING.

By LESLIE OGILVIE, M.B., B.Sc.,

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On May 12th, at a quarter past 9 o'clock p.m., while seeing my patients at the North-West London Hospital, I received a summons to visit a man named Thomas Cross, aged 72, who lived near the hospital. I was told that both he and his wife were seriously ill, and that he was on the point of death. It was explained to me that both had taken a powder about two hours previously, that the woman felt ill about ten minutes afterwards, and that an interval of about an hour had elapsed before the man was seized with any symptoms. In the interval, he had been able to visit a daughter who lived in the neighbourhood, and he became suddenly ill on his return home.

On my arrival, I found the old man near the open window *in articulo mortis*, the buttocks resting on a chair, his body and legs extended, the heels resting on the floor, and his head supported by a woman.

On inquiry, I was told that he had been convulsed, and, knowing that a powder had been administered, the position of the body suggested to me death from strychnia poisoning. I was told, however, that he had been subject to fainting fits, in which he struggled for breath, and for which he was under treatment at the hospital. Both he and his friends thought at first that this attack was of a similar nature, because he had great difficulty of breathing.

The symptoms presented by the wife strengthened the suspicion of strychnia poisoning. She had an anxious expression, and was seated on the bed, with her chin elevated and her jaws closed, so that she could with difficulty articulate. Her mind was clear, and she described her symptoms with intelligence. She complained of a "crampy feeling in the chest," rigidity of the muscles of the legs, and complete inability to walk or assist her dying husband, stiffness of the jaws and neck, and great thirst. She stated that the symptoms had been more severe an hour previously. She was with difficulty removed to another room, as she trembled when moved, and begged to be left alone. I did not think it necessary to administer apomorphia, for which, however, I had sent to the hospital, because the symptoms were subsiding, and nearly three hours had elapsed since their onset. I gave her sixty grains of bromide of potassium, and repeated the dose almost immediately. Owing to the partial closure of the jaws and the difficulty of swallowing, she was only able to drink small quantities at a time. She gradually improved, and ultimately recovered. In neither case had there been any vomiting.

I was informed that one of the daughters had arrived on a visit that afternoon, and had brought the powders with her. She stated that she had given the powders in beer, and that they contained calomel. She had been in the habit of taking them for years, and thought that her parents might derive benefit similar to that which she had experienced. She had no other powder with her, and the cups in which the doses were given had been washed.

I had little doubt that strychnia had somehow been administered, and the circumstances being such as to excite suspicion, I deemed it necessary to communicate with the police, in the meantime collecting the bottles, and requesting the daughter and other persons not to leave the house.

On viewing the body of the man about one hour after death, with Dr. Murrell of the Westminster Hospital, whom I had called to my assistance, we found the rigor mortis well marked, the legs somewhat separated, the knee-joints stiff, the skin dry and still warm, the face pale, the mouth closed, and the pupils moderately contracted.

The following is an account of the *post mortem* examination, performed forty-four hours after death, in warm and damp weather. I was fortunate in procuring the advice and assistance of my colleagues, Dr. Allechin and Dr. Murrell, of the Westminster Hospital, who verified the appearances observed. The body was well nourished. It showed considerable signs of *post mortem* decomposition, as indicated by the green discoloration and distended condition of the abdomen, and extreme straining of the superficial veins over the root of the neck, shoulders, upper arms, and the inside of the thighs and legs. There was considerable *post mortem* congestion of the posterior aspect of the entire trunk, limbs, head, neck, and ears; and gaseous distension of the subcutaneous tissue at the root of the neck (*post mortem* emphysema). There was extreme and equal arcus senilis. Some sanguineous fluid was escaping from the mouth. The *post mortem* rigidity was passing off, but was still distinctly perceptible in the muscles of the calf and thighs. The usual cadaveric rigidity of the masseter muscles was noticed. There were no marks of injury of any kind on the head or body. A