

But how seldom can we find a case so favourable even as this as an experiment. So seldom that, according to Mr. Mill, the experimental methods of investigation are but little worthy of attention in this line of research. We are thus compelled to fall back on the deductive method. Even this is not available, except to a very limited extent. For every deduction must start from a previous one, or some induction. If we wish to find out under this method whether any particular medicine is useful in a certain malady, we must discover first of all the laws of all the causes implicated in the result. We must clearly determine the properties of the drug. These, of course, include its physiological action. This part of the process is beset with a thousand fallacies. We must then determine the pathological laws which govern the said malady; and who could assert that he thoroughly understood the natural history of any malady at all? Take croupous pneumonia. The physical signs and peculiar pulmonary changes are well known. But, why should the disease commence sometimes with acute gastro-enteric catarrh? What is the significance of the little crop of labial herpes so often accompanying it? Why should the general symptoms abate so remarkably just when the lung becomes solidified? Why should the chlorides desert the urine. These are but a few of the queries which must be answered before we know what pneumonia really means. And when all this is done, and we have become fully acquainted with all the particulars essential to the elucidation of the subject, and are in a position to draw our deduction as to the value of our drug, we must verify it by past experience or by some other evidence. Mr. Mill ably discusses this matter, and justly points out the small amount of success likely to be obtained in such intricate investigations. Suppose we took a case of chorea and made a series of inductions with regard to the causes of the disease, we should find some cases induced by fright, etc., some in connection with rheumatism, and many other apparent causes. But how difficult to connect the causation with the disease. If rheumatism be concerned, some will say it is the rheumatic element; some that it is an embolic state of certain parts of the brain, from detached valvular vegetations; some that the rheumatism is a mere coincidence. These are only hypotheses, of but little value till proved true. Take the various drugs used in this disease, we know little more about them than that they are so-called nerve tonics mostly. Bring them to the test, they fail as often as they succeed. One vaunts arsenic, another praises zinc, a third juice of conium. Some cases resist all. I remember a boy, on whom bark and ammonia acted like magic, when all other means had been tried in vain. Another case of very severe unilateral chorea in a girl, with anæmia and valvular mischief, resisted iron and arsenic, with chloral at bedtime. The patient was very sleepless; and, at the end of three or four days, was quite worn out. Three or four one-scruple doses of chloral at two hours intervals sent her to sleep, and she awoke well. The next case I had derived no benefit from chloral. Can we generalise from such facts as these. The results, perhaps, depend on the varying causes or the different conditions of the patients. But the general conclusion must be that none of the drugs mentioned are *boni fide* curers of, or at all events specifics against, chorea. Want of success depends in many cases, no doubt, upon our ignorance of certain essential conditions in them, and thus our want of skill in combating these. Time would fail if I were to touch upon general principles of treatment, such as the use of bleeding, counterirritation, and a host of others; and I must pass by many minor impediments, such as the inaccuracy of names and classifications, and the fallacies of statistics and averages as guides to treatment.

I will allude very briefly to two other points. 1. There can be no doubt that we are too prone to slight the labours of those who have gone before us. We despise their antiquated notions, in which lie concealed from our superficial notice truths of the greatest value. Our brethren in all ages have performed their part in leaving us rich legacies of knowledge. But we are not willing to claim or make use of them, because they are encumbered with much that is absurd, useless, and untrue. Each generation, instead of adding to the great fabric whose foundations were laid thousands of years ago, prefers to pull the edifice to pieces and rebuild it after its own fashion. There is thus a perpetual change in theory and practice by no means conducive to progress. 2. Valuable stores of knowledge, accumulated by individual thought and experience, often perish with those who possessed them. How many have amassed most useful materials during their years of practice, and never given them the publicity they deserved. The consequence is that hundreds of able cultivators of our art leave the world without contributing in the least degree to the advancement of their science. Certainly much more is done than heretofore in this direction. But a great Association like ours ought to develop some plan, by which this most important means of spreading medical knowledge could be more successfully carried out than at present.

Having sketched very roughly and imperfectly the main causes of

the backwardness of medicine, I will say one word in conclusion on her future. It were idle to imagine, so long as the state of the world and men's passions and frailties exist, as at present, that diseases will cease from off the earth. Some may disappear; but their place will probably be taken by others. The pangs and terrors of most will very likely be mitigated. But there will ever be the need of the labours of men like ourselves. The dreams of the optimist are worse than useless. Let us, then, note down every fact, however trivial; sift every piece of evidence, however insignificant; and test every theory by the most scrutinising experience; thus furnishing broad and sure bases for greater deductions than even the brilliant discoveries of Jenner, Marshall Hall, and other worthies; deductions which will throw clearer floods of light upon the nature of disease, and therefore upon the treatment of it. It falls to the lot of but few

"To leave a name,
A light, a landmark on the hills of fame";

but we can, at least, hand down to another generation the torch, dimmed by no want of care or zeal, but burning more strongly and brightly in the cause of humanity.

ON INTRAUTERINE INJECTIONS. ✓

By ROBERT BARNES, M.D.,
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THE cases of injection of perchloride of iron to restrain *post partum* flooding, recorded in recent numbers of the BRITISH MEDICAL JOURNAL by Mr. Boddy and Mr. J. C. Clark, together with the comments of Dr. Swayne and Mr. G. F. Hodgson, induce me to offer a few observations upon the subject.

In Mr. Boddy's case, the injection was followed by death. The severity of the hæmorrhage and the reduced state of the patient are attested by the fact that "several large doses of ergot and two injections of cold water into the uterus" produced only a "temporary good effect". Under such conditions, reflex action failing, the resort to styptic injection is, in my experience, generally indicated. I cannot doubt that Mr. Boddy was justified in using it in this case. But the mode in which he used it seems to be open to some objection. The instrument employed was an "ordinary enema-syringe"; the styptic consisted of "four ounces of liquor ferri perchloridi fortior to twelve of water. The nurse worked the syringe. . . . The hæmorrhage ceased immediately on the injection of the solution; and, during the first part of the process, the patient made no complaint, and the uterus began to firmly contract. But, during the latter part of the process she began to be uneasy, complained of pains in the belly; then severe cramps of the flexors of the legs occurred; next she gave a shriek, crying out 'Oh my belly'; a general convulsion took place, the skin became blue, the pulse ceased, and she expired. All this took place during the last two or three strokes of the enema-syringe. The uterus was firmly contracted round the injection-tube, some little force being required for its withdrawal."

I do not question the correctness of Mr. Boddy's conclusion "that the injection of the perchloride of iron was the immediate exciting cause of death". But his opinion that "the hæmorrhage could not be the cause, because the injection was used before the loss of blood had begun to affect the nervous system" is at variance with his previous statement "that ergot and injections of cold water failed to induce active uterine contraction". When the reflex function is impaired to the degree indicated by this test, a very serious effect has been produced upon the nervous system, under which it is peculiarly susceptible to the influence of shock. Shock was caused by the iron injection; and from the narrative it may be concluded that it was mainly due to the mode in which the injection was made. Mr. Boddy misunderstood my directions, if he thought that the whole quantity of sixteen ounces must be injected; and the use of an enema-syringe worked by a nurse is no part of my instructions.

At page 472 (*Obstetrical Operations*, second edition) are the following directions: "You have the Higginson's syringe, adapted with an uterine-tube, eight or nine inches long. Into a deep basin or shallow jug pour a mixture of four ounces of the liquor ferri perchloridi fortior of the *British Pharmacopœia* and twelve ounces of water. Pumped through the delivery-tube two or three times to expel air, and insure the filling of the apparatus with the fluid before passing the uterine-tube into the uterus. This, guided by the finger of the left hand in the os uteri, should be passed quite up to the fundus. *Then inject slowly and steadily.*"

Without insisting unduly upon the probability that a nurse under exciting circumstances cannot be trusted to pump slowly and steadily,

I must insist that this part of the operation should be done by the surgeon himself, and that an enema-syringe is calculated to pump the fluid into the uterus with unnecessary and even dangerous force. From this risk the Higginson's syringe is comparatively free. The propelling force, even when the ball is firmly compressed, is small.

Further, if the finger guiding the injection-tube be kept in the os uteri, information is given as to the effect produced in checking the bleeding, and the closure of the os upon the tube is obviated. As I have explained in my work on the *Diseases of Women*, when the os uteri is closed, the uterus, contracting concentrically upon fluid contents, tends to drive the fluid along the Fallopian tubes. Dr. Swayne's comment to this effect is perfectly in accordance with observation. It is an imperative condition to preserve patency of the cervix for the outflow of the injected fluid. In the work referred to, and in the third edition of the *Obstetrical Operations*, which will shortly be published, I have discussed with some care the subject of intrauterine injections in the puerperal and non-pregnant woman. In the first work, I have given an example similar to that of Mr. Hodgson of severe collapse, following an ordinary vaginal injection of sulphate of zinc in a non-pregnant woman suffering from prolapse, with patency of the cervix uteri.

Mr. Clark's case is an excellent illustration of the beneficial use of iron-injection in *post partum* flooding. Here again, as in many other cases, it acted after the failure of grasping, ergot, firm pressure on the abdomen, and the injection of cold water. It is not unworthy of remark, that Mr. Clark had previously injected a solution of permanganate of potash. It did no good; although, had the uterus been excitable, the cold might have been expected to act. I have recently been informed by an excellent provincial practitioner that, in a case where previous experience had led him to anticipate flooding, when he had consequently given ergot and used all other means of prophylaxis, hæmorrhage occurring, he injected permanganate solution with apparent advantage. But the uterus relaxed again, filled with blood, and death ensued. He has since trusted to the iron-injection in similar cases, and always successfully. He believes that, had he not been deterred from using it in the case described, the woman would have been saved.

ON THE CAUSE OF DEATH AFTER USING PERCHLORIDE UTERINE INJECTIONS FOR *POST PARTUM* HÆMORRHAGE.

By EDWARD JOHN TILT, M.D.

UNDER the head of "Obstetrical Memoranda", in last week's JOURNAL, Mr. Hodgson of Brighton relates how, while making a vaginal injection, a patient of his passed the tube into the cervix, and thereby caused a solution of zinc to flow from the womb through the Fallopian tubes into the peritoneum, thereby producing what he calls "short-lived peritonitis". I doubt this explanation, unless Mr. Hodgson ascertained that, in this patient, the cervical canal was sufficiently patulous to freely admit the tube of the instrument.

I should not presume to doubt the validity of his explanation, had I not met with half a dozen cases during the last thirty years, in which the use of a vaginal injection was suddenly followed by pelvic symptoms sufficiently acute to suggest peritonitis; symptoms yielding to appropriate treatment in one, two, or three days. In these cases, I had found more or less trouble to introduce the uterine sound, so I cannot suppose that my patients could have possibly passed into the cervix an elastic tube ten times the size of the uterine sound. I should add that, in these cases, the liquid injected was a weak solution of acetate of lead, and that the women had been in the habit of using injections. I submit that a better way of accounting for such cases is to admit that the patients have been awkward, and roughly jammed the extremity of the tube against the sore extremity of the cervix, the concussion being sometimes intensified by the patient, in her fright, pressing the instrument farther in, instead of withdrawing it. Those who so frequently explain the sudden advent of formidable symptoms in disease of the unimpregnated womb, by the passing of a fluid through the Fallopian tubes into the peritoneum, should remember that any surgical treatment of the womb, or even the rough use of the uterine sound, may cause the most formidable pelvic symptoms, and leave traces of pelvic peritonitis even if these symptoms soon abate. They should also bear in mind that Dr. Fontanes has published an account of various experiments in which he found it impossible, by distending the dead womb, to make fluids pass through the Fallopian tubes into the peritoneum. Neither should it be forgotten that the unimpregnated womb has often been injected with various fluids; that the patients have sometimes died and been examined; and that, as far as I am aware, in no case has the fluid in-

jected into the womb been found outside the fimbriated extremities of the Fallopian tubes, except in two cases mentioned in the discussion of my paper on Pelvic Lymphangitis, at the Obstetrical Society of London; one by Mr. Ross Jordan of Birmingham, the other by Dr. Palfrey—cases which have not been published, I believe. It would be singularly illogical to let these occasional untoward results of using injections weigh against their great value in the treatment of diseases of the sexual organs; the more so as these accidents can be prevented by telling the patients to use a flexible tube, and by showing them how to bend it, and how to introduce it into the vagina so that the nozzle of the tube may not run any risk of touching the os uteri.

Dr. Swayne of Bristol has lately explained a death that occurred after the injection of a solution of perchloride of iron into the womb to check *post partum* hæmorrhage by supposing that the solution had passed into the peritoneum. I am not aware that this explanation is warranted by the finding of the perchloride outside the fimbria in similar cases when the bodies have been opened. During the last few years, in the Paris hospitals, a solution of carbolic acid has been often injected into the womb soon after parturition. Many of these patients have died and been examined; but it was not found that the carbolic acid had passed through the Fallopian tubes into the peritoneum. I do not, therefore, think the fear of such an occurrence should bar the use of Dr. Barnes's method of checking incoercible *post partum* hæmorrhage; but I think it would be advisable to use tincture of iodine instead of the perchloride of iron, for reasons that I have given elsewhere.

A CASE OF GENERAL EXFOLIATIVE DERMATITIS (PITYRIASIS RUBRA), ACCOMPANIED WITH FEVER AND GENERAL PROSTRATION.

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THE report in the BRITISH MEDICAL JOURNAL of March 13th, 1875, page 359, of a case of pityriasis rubra exhibited to the Glasgow Pathological and Clinical Society by Dr. Gairdner, has induced me to publish an account of a case in which the temperature was also elevated, which was under my care in 1873, and to which I had previously been unable to find a parallel. The history is as follows.

M. A. D., aged 48, was admitted into Charing Cross Hospital, under my care, on August 9th, 1873. She was brought in a cab, and was too ill to walk. All that could be discovered about her previous history was, that she had for some years been subject to rheumatic attacks. She was in Guy's Hospital twelve years ago with rheumatic fever, and, five years ago, was laid up with another attack for nearly five months. She had had eight living children, the youngest now four years old. Her husband was a strong healthy man. She had lived at Woolwich several years; but, two months ago (June 13th), she moved to New Cross, and had not been well since. She had ailed all the year, and had pains in her elbows and knees, and her joints had been swollen sometimes, but not so much as to prevent her from getting about. Her skin had been for some time rough and dry, but without scalliness, until about two months ago, when it became somewhat redder than natural, and inclined to peel in the bends of the elbows and in the axillæ. About a month ago, her hands were rough and scaly, and she believed that the redness and roughness of her body became much worse about a week before her admission. For some time, she had slept badly, and been thirsty and scarcely able to satisfy her thirst. Her bowels had been much confined; her appetite had been bad for some time. The remainder of the history will be given *verbatim* from the daily notes.

State on Admission, August 9th.—She complains of pains in her knees and shoulders on movement; but the joints are not swollen nor tender. Tongue very white on the dorsum, with red edges. Pulse, 120; temperature, 102.5 deg. Fahr. (4 P.M.). The most striking thing about her is the condition of her skin. There is a diffused redness of the chest and arms; but the legs are less affected, except the inner parts of the thighs, which are of a very deep red colour, and at first sight suggest the idea of scarlet fever. The skin is at the same time considerably thickened and dry, especially below the clavicles and over the front of the chest, and there is a large amount of scaly desquamation about the popliteal spaces, outer sides of the hips, both shoulders, and the front of the chest. The face is unaffected, except that there is a slight patch of redness inclined to scale on the left of the nose. The hands, to the tips of the fingers, are red, dry, and shiny, and there is desquamation in the flexures of the finger-joints. There are three or four flat pustules, of the size of small peas, over the lower end of the sternum. The patient is very weak, and unable to sit up-