

tinum or silver, mounted on handles of convenient length. These probes may be curved to follow the course of the uterine canal. This is far the best way of applying nitrate of silver to the os and cervix uteri; and it is the only safe way of applying it to the interior of the uterine cavity. The armed end of a probe may be passed into the uterus without the speculum, although the aid of this instrument is sometimes convenient. For example, unless the armed probe be protected by a cannula, the caustic will first touch the vulva and vagina in its passage, which is apt to leave unpleasant effects, and the guiding finger of the operator will be stained.

One of the most widely useful topical applications to the mucous membrane of the cervix and body of the uterus is sulphate of zinc. The value of this agent, when applied to the relaxed or morbid mucous membrane of the vagina in the form of injections, is familiarly known: how to apply it to the uterine mucous membrane is, therefore, a matter of great interest. This has been accomplished by Messrs. Johnson, the well-known assayers, on the suggestion of Dr. Braxton Hicks, who prepared small cylindrical sticks of fused sulphate of zinc weighing three and five grains. These can be carried quite into the uterus without having touched the vagina by the way, by means of a cannula, first made on my design by Messrs. Weiss, and now generally sold by instrument-makers. It consists of a silver cannula of the size of a No. 8 or 9 catheter, gently curved, open at the end, and supplied with a stilet or piston. The stick of sulphate of zinc or other material is placed in the uterine end of the cannula; the instrument is then passed into the uterus just as the uterine-sound is passed, the patient lying on her left side; and the operator's finger, placed on the os uteri, guides the instrument. It is a great advantage of this contrivance that the use of the speculum is quite unnecessary after it has aided in establishing the diagnosis which supplies the indication in treatment. When the instrument has gone the proper depth, the piston pushes out the stick, and the instrument is withdrawn, leaving the stick to dissolve. This it soon begins to do, and, by its speedy effect in constricting the mucous membrane, it keeps itself *in situ* until it is completely dissolved.

Nitrate of silver, reduced by admixture with nitrate of potash, may be used in the same way; so may persulphate of iron, but this should be considerably reduced. When used nearly pure, I have known it cause severe colic and bleeding.

A most precious way of applying astringents, caustics, solvents, or alteratives to the interior of the uterus, is in the form of ointment or pasma. In this way almost any substance can be applied. Where grease is objectionable as a vehicle, a pasma of suitable consistence may be made by aid of glycerine or other matters. In this form we may use substances which cannot easily be applied in any other way. For example, we can hardly use bromine, or iodine, or mercury, in a solid shape; and to use them in liquid form is open to the objections already discussed. Almost anything can be made into an ointment or pasma, and thus we get a complete practical command over a large range of useful agents.

To introduce ointment into the cavity of the uterus, Messrs. Weiss have made from my design a very convenient instrument, also capable of being used like a sound without the speculum. The instrument is easily charged by dipping it into the ointment—a sufficient quantity of which is carried into the uterus, and, by pushing up the piston, is deposited there.

If it be desired to apply a powerful liquid caustic, as chromic acid or strong bromine, to the interior of the uterus, this can be done by the ointment-carrier. A few shreds of asbestos may be packed in the space between the eyelet-holes and charged with the fluid. On ramming down the piston, the fluid will be squeezed out.

In discussing the action of powerful styptic injections in arresting flooding after labour, the conditions under which the practice I have recommended is indicated have not always been accurately appreciated. The great agent, of course, in stopping hæmorrhage, is the constriction of the uterine vessels by the muscular wall in which these vessels run. All the ordinary means of arresting hæmorrhage are aimed at producing muscular contraction. But muscular contraction depends on nervous power. Thus cold, grasping the uterus, introducing the hand, galvanism, all depend for their efficacy upon the spinal cord being able to respond to the peripheral call. When, therefore, these means prove sufficient, the inference is generally warranted that the case, although serious, is not desperate. The condition is very different when the excito-motor function is suspended; when neither by peripheral excitation, nor by centric stimulus, the nerve-force can be drawn or sent from the spinal cord to the uterus in sufficient intensity to cause contraction. At this point, unless the bleeding is arrested by syncope, or by temporary enfeeblement of the circulation, the patient is in the most imminent danger of death. The slightest shock or disturbance will extin-

guish the flickering spark of life. Under such circumstances I have known death follow, to all appearance immediately caused by the injection of cold water, or passing the hand into the uterus. If, instead of cold water, we inject a solution of perchloride of iron, the same catastrophe may ensue. Is it more likely to ensue? Very careful observations are required before this question can be answered in the affirmative. People are apt to think that cold water is so simple a thing that it cannot do any harm. But if it cannot do any harm, is it not probable that it is, under the conditions discussed, equally powerless to do any good? Harmless remedies, as a rule, fail in great emergencies. Now, cold water fails not because it is harmless, for the shock and depression which it causes are extremely dangerous; but it fails because, nervous power being exhausted, it cannot excite uterine contraction, and it has no other virtue in arresting hæmorrhage.

Here, then, it is that styptics come to the rescue. The emergency is extreme, and would be desperate, but for the new power invoked. If blood be still running, it is instantly seized at the mouths of the vessels, which become sealed up by coagula. It also constricts the inner surface of the uterus, and thus further closes the vessels. The system then has time and opportunity to rally, and by and by the contractile power returns. In estimating the relative value, then, of cold water and perchloride of iron, we must reflect that iron acts and saves life when water is inert or injurious. If occasionally death follows, and is apparently accelerated by, the iron injection, we have, on the other hand, to remember that it was used as a last resource, when the patient was likely to die even if nothing were done, and that even under these unpromising conditions *many lives, to all appearance doomed, have been saved.*

The great lesson to learn is to take courage to use the styptic in time; that is, before the vital power has sunk too low. It was not to be expected that a remedy powerful enough to save under the last extremity should be altogether free from danger. But I have seen so many women bleed to death, and have seen so many saved by the timely use of the iron injection, that I am much more afraid of the bleeding than of the remedy.

In some cases, there is reason to believe that the iron enters the uterine vessels. I have known intense pain in the uterus follow immediately on the injection. How is this explained? If blood were present in the vessels, it is a chemical necessity that contact with the iron would cause coagulation. I infer, then, that in some cases the vessels are for a time nearly empty; and that there is a certain amount of suction-action induced by the relaxed state of the uterus, and by the lateral or semiprone position of the patient. I would therefore urge that the patient be placed on her back, and that the uterus be grasped firmly between the two hands of an assistant during the injection.

In some cases, it is easy to carry a swab of sponge soaked in the iron solution into the uterus. In this way probably some of the risk attaching to injection is avoided. The persulphate of iron, which is preferred by our American brethren, may have its advantages. Its styptic force is probably greater. It may be used in the form of one part of the liquor ferri persulphatis of the *British Pharmacopœia* to six or eight of water. The proper strength of the perchloride solution is one in ten.

ARTERIO-CAPILLARY FIBROSIS.

By LIONEL S. BEALE, M.B., F.R.S.,

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WITH reference to the observations of Dr. Johnson in the last number of the JOURNAL under the above heading, I beg permission to remark that my colleague is altogether mistaken in the inference which he has drawn concerning my opinion as regards the results of his labours, for nowhere have I even hinted that Dr. Johnson had "mistaken amyloid disease or other forms of degeneration for true muscular hypertrophy". My words are: "It has, I think, been too hastily assumed that the thickening is really hypertrophy of the muscular coats of the arteries. If this thickening is to be regarded as hypertrophy, it is unquestionably associated with great change and degeneration of the normal tissue." And then follows the paragraph quoted by Sir William Gull in the JOURNAL of the week ending December 28th, 1872. These extracts are taken from the last edition of my book on *Kidney-Diseases, Urinary Deposits, and Calculous Disorders*, pp. 71-72, and were written nearly five years ago. The book was published in October 1868.

The difficulties under which I then laboured have not been removed; and Dr. Johnson must pardon me for remarking that, as far as I am able to judge, there is, so to say, still plenty of room for differences of opinion upon the question at issue. My friend speaks of "true mus-

cular hypertrophy" of the arterial walls; but how is one to feel sure that the appearance one sees is really due to "true muscular hypertrophy"? Is this not, in fact, a question of interpretation?

Dr. Johnson feels convinced that the drawing he gives (fig. 2, p. 4, BRIT. MED. JOURNAL, January 4th, 1873) has been copied from an artery the muscular tissue of which was "hypertrophied"; but I venture to think that some who have studied the small arteries in health and in disease with great care will not acquiesce in that opinion: at any rate, the appearances delineated may be accounted for in more ways than one.

If by "true muscular hypertrophy" is to be understood an increase in the amount of muscular tissue, associated with a corresponding increase in contractile power of the vascular coats, I think it will be very difficult for any one to feel perfectly convinced that he has seen a true example of such a change. To decide such a question affirmatively, it seems to me, would be no easy matter. It is an unquestionable fact, that a *healthy artery* may be so contracted in part of its course as to be indistinguishable from an artery of less diameter, but with thicker coats. If such a specimen were examined only in this contracted part, the observer might reasonably infer that the vessel was a small artery, the contractile coat of which had become considerably hypertrophied; while, if he followed the vessel a little further, he might find that the tube expanded to three or four times the diameter of the contracted portion, its walls being reduced proportionately in thickness. I am speaking now from actual observation: indeed, one or two specimens illustrating this fact were shown by me at the *soirée* of the Royal Medical and Chirurgical Society.

I have not succeeded in obtaining a single specimen which seemed to me conclusive in favour of the view that the muscular tissue of the artery was really hypertrophied: nor do I feel satisfied, looking at the question either from the physiological or from the pathological side, that such a change ought to be regarded as one likely to occur under any supposable circumstances. These are, of course, but conclusions and opinions of an individual—arrived at, however, only after a good deal of careful investigation and thought. They may be altogether erroneous; still it appears to me that the questions discussed can be determined only by careful individual research, and repeated observation and discussion.

I am sorry to learn that the questions in dispute are to be referred to a "scientific committee", which will, I suppose, have to decide or come to a conclusion of some sort upon the evidence presented to it. I venture to think that, if the Fellows of the Society who are interested in these inquiries would work in their own way and in their individual capacities, and publish the results of their labour, they would add much to our knowledge. But science can gain nothing by opinions authoritatively expressed by committees, while the interests of individual observers may be unfairly promoted or damaged by the decision arrived at, without the cause of truth being in any way advanced. Why cannot those who have formed opinions express themselves freely and openly, and accept the responsibility of their statements? Scientific committees might, and I believe would, act very injuriously, by repressing individuality and discouraging work which happened to be contrary to the "tendencies of thought" at the time; while, on the other hand, it is difficult to see what good could possibly result from their deliberations. Some seem to think that opinions of little value in themselves acquire authority, and perhaps infallibility, by being expressed in a formal way, and endorsed as the opinion of the majority of a committee. The tendency of scientific committees would be to repress liberty of opinion and liberty of thought. What is now much to be desired in every branch of science is the encouragement of individual work and thought in every possible way—not the promulgation, by authority, of opinions concerning the work done by individual workers.

CASE OF HÆMORRHAGE INTO THE PERITONEAL CAVITY.*

By A. WELLESLEY TOMKINS, M.D. Dubl., Leamington.

A YOUNG woman, aged 24, eight months married, retired to rest on the night of September 18th, apparently in good health. She had not menstruated for three periods, and believed herself to be pregnant. In an hour afterwards she awoke her husband, complaining of sharp pain principally referred to the left iliac region. Her usual medical man saw her soon afterwards, and diagnosed internal hæmorrhage. She continued to sink; the surface of the body rapidly blanched; the pain was general over the entire belly. She was conscious to the

* Read before the Birmingham and Midland Counties Branch.

last, and expired about thirteen hours after the commencement of the attack.

Sixty hours after death we made an examination of the body, the results of which, being totally negative, may be summed up in a few words. The entire abdomen was distended with blood, fluid and in immense clots. Each viscus was separately and carefully examined; the intestines throughout their entire extent; and finally, and especially, as being the most probable source of the mischief, the uterus, ovaries, and Fallopian tubes. The uterus itself was found to be normal, unimpregnated, and free from any sign of congestion or inflammation. The left ovary was slightly larger than its fellow; but neither in them nor in the Fallopian tubes could the slightest lesion be discovered to account for the hæmorrhage, which in a few hours had drained the entire system. The thoracic viscera were healthy, with the exception of a slight adhesion towards the apex of the right lung.

The following facts have since come to my knowledge, and seem to throw some light on the subject. The deceased was the second of a family of four. The eldest, a young man, had been from a child subject to epistaxis, and is now hemiplegic from an apoplectic attack at the age of seventeen. The deceased commenced to menstruate at thirteen years, and the discharge was almost invariably so profuse as to necessitate medical advice. Her two sisters present the same tendency to hæmorrhagia, and she and they had been subject to violent epistaxis upon little or no provocation.

The following queries present themselves.

1. Had the hæmorrhage any connection with the tendency to lose blood, as shown by the hæmorrhagia and frequent epistaxis?
2. What degree of influence had the amenorrhœa of the past three periods upon this tendency?
3. What was the source of the hæmorrhage?

TRICÆLIAN HUMAN HEART.*

By S. MESSENGER BRADLEY, F.R.C.S., Lecturer on Comparative Anatomy in Owen's College, Manchester.

AMONGST the rarer cardiac abnormalities, are those in which there is a deficiency in the usual number of cavities: thus, instances have been recorded by Wilson,† Standert,‡ Farre,§ and Ramsbotham,|| of hearts composed of but two chambers, an auricle, and a ventricle—a form similar to the condition of the organ in fishes, and known as *dicealous* heart (Hunter). The instances of three-chambered hearts (*tricælious*) are still rarer: their occurrence is even doubted by Dr. Todd (in his article on the Abnormal Conditions of the Human Heart, in the *Cyclopaedia of Anatomy and Physiology*), the cases related by Breschet¶ and by Wolff** being considered by him to belong rather to the former category of *dicealous* hearts, as the auricular septum was in each case present, although imperfect. The specimen I have the pleasure of now showing to the Society, is a good illustration of a *tricælious* heart, a condition which is persistent among the Batrachia.††

The history of the case—for which, together with the specimen, I am indebted to Mr. W. O. Jones of Bowdon—is as follows. N. F., born at 8 A.M. on April 22nd, 1872, appeared to be perfectly healthy and well nourished; his breathing was, however, noticed from the first to be hurried. He took the breast, and the nurse gave him some weak gruel occasionally in consequence of the scantiness of the mammary secretion. About eleven o'clock, on the morning of the 23rd, his breathing was more hurried, and he was unable to suck; the bowels acted naturally, and there was a copious excretion of urine. About two o'clock, some gruel was given; and at three he had an attack of vomiting, immediately after which the skin assumed the characteristic hue of cyanosis, and symptoms of suffocation showed themselves. He lingered till mid-day on the 24th, when he died. The *post mortem* examination was made hurriedly, and unfortunately the heart alone was removed, the vessels being cut off close to their attachment. The

* Read before the Medical Section at the Annual Meeting of the British Medical Association in Birmingham, August 1872.

† *Phil. Trans.*, 1798. In this case, the dicealous heart was situated on the convex surface of the liver. The child lived for seven days.

‡ *Phil. Trans.*, 1805.

§ *On the Malformations of the Human Heart*. London: 1814. Dr. Farre relates a case very similar to mine, in which the child lived seventy-nine hours, and thirty after respiration was effected.

|| *London Medical and Physical Journal*.

¶ *Rep. Gén. d'Anat.*, tom. ii.

** In Kreyzig's *Die Krankheiten*. Herz., B. iii.

†† Life, for a considerable period, appears to be compatible with a very similar cardiac condition. Dr. Elliot of Carlisle relates a case (in *Med.-Chir. Trans.*, 1868), in which there was an absence of the ventricular septum, where life was prolonged to the nineteenth year. There are other cases of a like character on record.