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

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?

TRICELIAN 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 *dicalious* heart (Hunter). The instances of three-chambered hearts (*tricalious*) are still rarer: their occurrence is even doubted by Dr. Todd (in his article on the Abnormal Conditions of the Human Heart, in the *Cyclopædia of Anatomy and Physiology*), the cases related by Breschet¶ and by Wolff** being considered by him to belong rather to the former category of *dicalious* 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 *tricalious* 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 *dicalious* 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 Kreyssig'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.

* Read before the Birmingham and Midland Counties Branch.