markedly tricrotic pulses, say from the femoral and also from the dorsalis pedis arteries. According to the view of Dr. Burdon Sanderson and most other writers, the interval between the primary and tidal waves ought to be more than doubled in the dorsalis pedis; according to the view of Mr. Garrod, on the contrary, that between the tidal and dicrotic waves. It will be found that there is no such considerable and constant variation as would be required by either theory, although the tidal wave does not maintain its relative position so closely as does the dicrotic wave. The kind of pulse best of all suited for this experiment is fortunately * rather scarce; it is that of a young person who has a granular kidney, but is free from dropsy.

The theory of Mr. Garrod may appear at first sight suitable to one of the forms of healthy pulse, in which the tidal wave appears as a slight elevation preceding the dicrotic wave; but I do not think that it will be accepted by anyone who has watched its variation in a large number of diseased pulses, and has seen it pass through every gradation, from a separate and distinct wave to a mere convexity in the descending curve, which may commence immediately from the top of the primary upstroke. In the pulse of rigid arteries this latter form is often taken when the heart is quiet, but when it acts more vigorously the tidal wave becomes separated, owing to the development of the so-called "percussion element," which is really the effect of acquired velocity in the sphygmograph. The case which should afford the most crucial test is perhaps that very rare one in which the aortic orifice is closely obstructed, and scarcely any valves remain to produce a wave by their closure. The tidal wave should then, according to Mr. Garrod's theory, be at least greatly diminished, but, in point of fact, it is then more greatly developed than under any other circumstances whatever. Evidence to the same effect may be derived from the use of an artificial heart with experimental elastic tubes, for it is found that, under suitable conditions, the tidal wave may be greatly prolonged by a protracted contraction of the heart. This was first shown by Mr. Mahomed in the *Medical Times*, and although I believe his theory to be erroneous as to the relation between the primary and tidal waves, yet, with regard to the practical associations of the tidal wave, my experiments have led me to conclusions which are quite in agreement with his, namely, that three things contribute to the development of the tidal wave—increase of pressure, diminution of elasticity, and prolongation of the heart's contraction.

Mr. Garrod argues that the tidal wave cannot have anything to do with the inertia of the long lever, because it is shown in the reflecting sphygmoscope, in which that is absent. I do not, however, consider that the result is due solely, and possibly not even chiefly, to the inertia of the lever, but to that of the instrument altogether, and inertia is possessed likewise by the sphygmoscope. Moreover, since the latter does not record its indications, it would be difficult to ascertain whether the tidal wave shown by it corresponds precisely to that of the sphygmographic tracing. Another instrument has also been called a sphygmoscope, in which the motion of the pulse is shown by the variation of a gas flame. In this there appears indeed the counterpart of the tidal wave, but not in the form of a single wave; instead of this a series of small waves is shown. These may appear only as a slight quivering motion, and are evidently due to the oscillation of the elastic diaphragm upon which the pressure of the pulse is received.

Mr. Garrod maintains his own theory especially on the ground of observations with his cardio-sphygmograph, showing the commencement of the tidal wave in the radial pulse to be synchronous with the closure of the aortic valves. But the determination of the moment of that closure depends on the correctness of his interpretation of the minor elevations in the cardiographic tracing. These are numerous, and his interpretation of them all is most ingenious, but to accept it requires an implicit faith that the instrument itself has no part in producing any of the minor features of the curve. Now, that curve was drawn by a lever, moving on a pivot, and balanced between two springs, which would seem a contrivance peculiarly liable to oscillate. When therefore it is further found that in cardiac tracings published by other observers, or those obtained by applying the sphygmograph directly to the heart, there is no close correspondence either in the number or the position of the elevations, the conclusion can hardly be resisted that some of them are due to such oscillation. My own opinion is that neither in the cardiographic nor in the radial pulse tracing can the point corresponding to the end of systole be precisely determined.

The whole subject is one which it is difficult even to state intelligibly without a constant reference to diagrams of tracings, and therefore, for a fuller account of my views as to the theory of the pulse, particularly in reference to the complete explanation of the dicrotic wave, I must refer to a paper to be published in the next number of the *Journal of Anatomy and Physiology*.

While I consider that the construction of the sphygmograph has some influence on the tracing produced, yet I believe that, by a fortunate chance, the result is more practically useful than if the pulse-wave were recorded with perfect accuracy, for I think that slight differences in it, which would then perhaps escape notice, are, as it were, magnified and made manifest to the eye.

I may say in conclusion that I do not quite agree in the view that we must wait for the practical application of the sphygmograph until physiologists are agreed about the theory of the pulse, for, according to present appearances, that consummation is distant indeed. There is, however, among sphygmographers an agreement about practical inferences which is almost as notable as the confusion which prevails as to mechanical causes. It is possible therefore for a person to use the sphygmograph for diagnosis and prognosis, who does not even attempt to understand the cause of the waves seen in its tracings. But it must be allowed that the settling of the mechanical question is much to be desired, and that, without it, the sphygmograph cannot afford that service, which otherwise it would be capable of doing, to the solving of all general physiological problems relating to the vascular system. And, from a practical point of view, these may perhaps be regarded as among the most important in physiology, for it is probably through the agency of the vascular system that many of the greatest effects of remedies are produced.

A. L. GALABIN

* [We have omitted the prefix *un*- from this word: we hope Mr. Galabin will forgive us.—ED.]

On the Origin of Nerve-Force

IN a paper on this subject, by Mr. A. H. Garrod, in NATURE, vol. viii. p. 265, the author states that in cold-blooded animals, nerve-force must be generated by the difference between their own temperatures and that of the medium by which they are surrounded. Now, to take the case of a frog as a common example of a "so-called" cold-blooded animal: A few days ago, when the thermometer was standing at 71°, I took the temperature of two frogs, one was 69°, and the other 67°; the difference between their temperature and that of the surrounding air was practically *nil*. Now, on a day of this sort of temperature, it would seem that the pervious integument of the frog is continually exhaling moisture, and that in consequence the temperature falls, and would continue to fall below that of the surrounding air, were it not that it was raised by the heat generated "by the destruction of tissue that is continually going on within the body of the animal;" so between these two contending forces a state of equilibrium results, and the temperature of the animal and the surrounding air are the same. But, if this be true, it follows that the whole of the heat from the animal is used up in keeping up its temperature, and therefore none can be spared for conversion into nerve-force. Therefore, a frog at rest on a summer's day ought to have no nervous energy. Now, suppose our frog takes to leaping vigorously, he will develop a certain amount of heat, and then he ought to have a great deal of nerve-force; but it is not found that an active frog is more "nervous" than a quiescent one.

Again, the nervous irritability of a frog, though perhaps not acting with the instantaneous energy with which it acts in a mammal, still persists far longer than in other vertebrates, and will continue much longer after the somatic death of the animal, when it is quite clear that the temperature of the body and the surrounding medium will be the same. Now in this case the nerves may be so irritated as to lose all irritability, and yet, after a period of rest, this irritability will be regained, clearly, to my mind, showing that nervous energy must be generated after the death of the animal, when all differences of temperature have ceased.

Finally, it must be admitted, without the aid of any hypothesis, that the difference between the temperature of a frog and the surrounding air is, at any time, very slight; and yet this animal possesses what we call an extremely "persistent" form of nerve-force.

R. LYDEKKER