invention, and giving the whole credit to Capt. Laussedat, closing with the remark that if it should be successful the glory would belong to France. Afterwards I received a pamphlet of twenty-six pages, by Capt. Laussedat, in which, ignoring me entirely, he tried to sustain his claims against those of Faye, Foucault, and Fizeau.

In 1873, after the horizontal telescope had been in successful operation for three years, after specimens, both negatives and lithographic copies had been distributed in Europe, the French Commission, which had up to this time been making their preparations to use the method of de La Rue, adopted the horizontal telescope. It would appear from this, that whatever might have been done or said on this subject by the Frenchmen named above, it had not contributed much to a clear appreciation of the advantages of the method until after they had been demonstrated here.

Without caring anything about credit for priority of suggestion in such matters, being satisfied that no similar instrument was in use or had been used before the one at Harvard College, I was yet interested enough in the matter to look up the claims put forth by these gentlemen, and to see why they happened to be overlooked for so long a time, even by their own countrymen. I find that credit is accorded to Foucault, mainly for his perfection of the heliostat, both for the plane mirror and for the uniform motion. He published nothing in regard to its application to photography. After his death his friend, St. Claire Deville, spoke of it as one of the things that Foucault intended to do. He at the same time contemplated the use of the siderostat in all kinds of astronomical observations. M. Laussedat is unwilling to give him any share of credit for the horizontal telescope. M. Faye gives him credit only for the heliostat.

M. Faye himself took some photographs of the sun with a very long telescope of one M. Porro, of 15 meters focal length. The telescope was pointed directly at the sun. M. Faye's remarks on them before the Academy related only to the advantage of their size and their distinctness. He had nothing to say about the peculiar advantages of the long telescope, but he anticipated all succeeding inventions in the application of Photography to astronomy by predicting its early use in meridian and every other class of observations.

His next communication on this subject was on March 14, 1870, on the occasion of presenting a letter from M. Laussedat on the subject of a horizontal telescope. This was six months after my apparatus was ordered, and after some experiments had been made with it. In this communication he appears at first glance to have suggested the whole arrangement now adopted; but on closer examination he does not seem to have had any clear ideas about it. He recommends the use of a long telescope because he had seen good pictures with a long telescope; he nowhere speaks of his reasons for dispensing with the eyepiece, and in fact it does not clearly appear that he did dispense with it. In September, 1872, after it had been in use for two years and several accounts of it had been published, in his comments before the Academy on a paper of Warren De la Rue's, he seems to have understood for the first time the true theory of the long horizontal telescope.

Capt. Laussedat appears to have the most substantial claim of any that have been mentioned thus far. He used a horizontal telescope in Algeria in 1860, in observing the total eclipse of that year; but he used a very short telescope and had an eyepiece to enlarge and distort the image. His own account of what led him to this method was that he had no equatorial mounting for his little telescope and that no means were furnished him to buy one, but he had a good heliostat, and he resorted to the method as a makeshift. He fully appreciated, however, the advantages over the other method in the accuracy of orientation and in the certainty with which fixed lines of reference could be had on the plates.

M. Faye, in his communication of Sept. 1872, seriously claims that his use of the long telescope pointed to the sun in 1858—because M. Porro happened to have one, and Capt. Laussedat's use of a short one, placed horizontal, because he had no equatorial stand and clock movement—together make up the invention of the telescope as it is now used.

But, after all that has been said about the priority of suggestion, that question is settled finally by some one * in England finding that the wole arrangement was suggested by Hooke in 1676. A late communication on the subject in the *New York Times* calls it a method suggested by Hooke and perfected by Foucault.

In Hooke's day they had none but very long telescopes, but they had no heliostats. No practical application of his suggestion, however, seems to have been made.

ON THE CARDIOGRAPH TRACE

By placing the sphygmograph, as constructed by M. Marey, over that portion of the chest where the heart can be best felt beating, instead of on the wrist-pulse for which the instrument is constructed, tracings called cardiograms can be obtained which bring to light physiological facts not otherwise ascertainable. In the last-published volume of the Guy's Hospital Reports there is a paper by Dr. Galabin, on the interpretation of these tracings, which will be read with interest by physiologists on account of the considerable difficulty there is connected with all attempts to explain the numerous ups and downs which they present between any two pulsations of the heart, and also because of the comparatively slight attention which they have had paid to them.

Dr. Galabin, in the paper under consideration, limits his observations almost entirely to the vertical variations in the curves under consideration, paying but little attention to the differences in the relative lengths of systole and diastole which they so clearly indicate, and which cannot be recognised with any degree of accuracy by any other means at our disposal. From a study of the cardiograph trace, he is led to the conclusion that the two most important elevations in the systolic portion of each curve are produced by the muscular movements in the heart itself, because "the more the heart is hypertrophied (by disease) the more prominent in comparison do these two become," and under these circumstances, "the effect of any oscillations, either of the blood or of any solid structures, would become less noticeable in proportion." It is remarked that "Marey's figures (of tracings indicating intracardial pressures) prove that the first, at any rate, of the cardiac impulse is not due to any stroke against the ribs caused by locomotion of the heart as a whole, which could only commence after the opening of the semilunar valves," because "the aortic valves do not open until the ventricular pressure has nearly reached its first maximum." It must, however, be noted that other tracings, obtained by the same illustrious physiologist, demonstrate equally clearly that the maximum of intracardial pressure is reached some appreciable time before the first major systolic cardiograph rise in the trace from the chest-wall, so that it may still be reasonably argued that the rise referred to depends upon the locomotion of the heart *en masse*.

To explain the second main systolic rise, Dr. Galabin makes a statement which needs considerably more demonstration before it can be considered to be proved. He refers to "inverted tracings," by which are understood curves in which all the rises in an ordinary trace are represented by depressions, in such a way that "to see more clearly their correspondence with positive tracings

* A correspondent in NATURE.—ED.

they should be turned upside down and read from right to left," instead of from left to right. Are we to believe, on the simple dictum of Dr. Galabin that *inverted tracings*, as above explained, are developed; that every elevation in the apex cardiograph trace is the result of a movement which is represented by a fairly proportionate fall in a trace a little distance from that spot; that every apical propulsion is a lateral suction? This may possibly be the case, but it requires a considerable amount of proof before it can be accepted as true. The relative duration, or, in other words, the horizontal projections of the different undulations, is not in favour of the assumption, which seems to be based on an accidental similarity between that apex trace and the reversed one from its neighbourhood. Till Dr. Galabin introduced his view, it has been assumed that the negative trace differs from the other positive trace in the fact that in the latter some of the undulations are longer in the up than in the down stroke; whilst in the former the reverse is the case. There is need for positive disproof of this explanation before the other is even considered.

Dr. Galabin concludes that the second main systolic rise "corresponds in time to the maximum contraction of the ventricle," and that it is due to the locomotion of the heart, dependent on the consequent injection of the aorta and the propulsion of the blood. This explanation might be tenable were it not for the results obtained by the employment of the hæmodromometer of Chauveau, tracings taken with which can be found in Marey's "Circulation du Sang" (p. 273). These show that there is a regurgitant current in the carotid arteries for some appreciable period *before* the closure of the aortic valve, which can only exist in connection with a similar one in the ventricular cavity. It is the hæmodromometer trace which has led the writer of this article to lay more than usual stress on the interval between the termination of the cardiac systole and the moment of closure of the aortic valves, termed by him the diaspasis.

Dr. Galabin remarks, "Mr. Garrod attributes the elevation *d* (the first main systolic rise), solely to the locomotion of the heart caused by the lengthening of the aorta. The rise *f* (the second main rise) he considers to intervene between the end of systole and the closure of the aortic valves, and to be due to the initial relaxation of the ventricle. It appears to be impossible that the relaxation of the ventricle, apart from its repletion, could produce an elevation in the curve except in those cases in which its hardening produces a depression either at the commencement or towards the conclusion of systole." In the explanation here referred to the elevation under consideration is, however, not supposed to be the result of the relaxation of the muscular walls of the ventricles, or to have anything to do with that phenomenon, but to be caused by the reflux of blood from the aorta and pulmonary artery into the ventricles which, when it has attained a sufficient velocity, closes the semilunar valves.

Dr. Galabin, by employing the stethoscope in conjunction with the cardiograph, watching the development of the trace whilst listening to the heart-sounds, has been able to satisfactorily verify the observation that the first sound occurs during the primary up-stroke, and that the instant at which the second sound is heard corresponds to a point on the principal down-stroke, and before the succeeding small and constant rise. This is further verified by the superposition of the sphygmograph trace on the cardiograph trace taken at the same time, a method which has elsewhere been shown to lead to particularly important theoretical results.

No particular stress is laid by Dr. Galabin on the peculiarities of the cardiograph trace associated with different rapidity of pulse and nothing else. The thorough study of the subject necessitates this point being taken into consideration, as is demonstrated by the great differences there are always found in the curves derived from the same individual when the heart beats at say 45 and 125 a minute.

Most of the paper under consideration is devoted to pathological points, especially mitral stenosis or contraction. With this we cannot here deal. One particularly interesting tracing proves that in some extremely slow pulses (*e.g.* twenty-five a minute) there may be an abortive attempt towards an intermediate contraction, perceptible in the cardiograph tracing, but not seen in that from the arterial pulse.

Whilst on this subject it may be mentioned that Dr. C. Hanfield Jones has recently read a paper before the Royal Society on reversed sphygmograph tracings, or tracings in which the systole is represented by a fall instead of a rise. These he explains on the assumption that they are produced by the brass end-pad of Dr. Sanderson's modified instrument resting on the artery instead of the spring-pad. This is no doubt the true cause in many cases; these tracings are, however, in our experience sometimes produced when Marey's unmodified instrument is employed. They may sometimes result from the fact that a curved artery is, during systole, rendered part of a larger curve, and so slips from under the spring-pad at that time.

A. H. GARROD

SIR JAMES KAY-SHUTTLEWORTH ON SCIENTIFIC TRAINING

ON the occasion of presenting the prizes to the successful students at the Giggleswick Grammar School, near Settle, on July 28, Sir J. Kay-Shuttleworth made some forcible remarks on the above subject. Sir James points out with so much wisdom the relative position which science and literature ought to hold in the training of youth, that his remarks deserve the serious attention of all interested in education. Our columns constantly bear witness to the increasing prominence given to science in education, both at the higher schools and universities. Sir James, after noticing this and other features in the progress of the Giggleswick School, and referring to some of the results of the training of the school, went on to say:—

"You will perceive that among them are proofs of the influence of the practical teaching in natural science in opening a career to our pupils in the universities. In the growth of any institution on a new basis, time must be allowed for its development. Difficulties will be encountered in discipline, in domestic management, and in the attainment of the ideal to which its course of studies is expected to rise. Yet it is well to keep that ideal closely in view as the goal of all efforts; to retain a firm hold on the principles of action, and while confessing the length and the arduous character of the way, to press forward, undismayed by any partial failure, towards the summit of our hopes. I find in the examination papers a continually higher standard. They embrace a wide range of studies. But it must not be supposed that we are so presumptuous as to expect that even the *élite* of the school could attain a high degree in the whole range of these studies. No error could be more fatal than that they should be obligatory on all our pupils. Indeed, we must, in the first place, point out that in consideration of the prominence given to modern languages and to practical instruction in natural science, Greek is not among the subjects comprised in the scheme of the school, though it will be taught to all boys preparing for the universities, or for any of the public examinations. To determine how best the faculties of those not gifted with average energy and capacity can be developed requires a delicate and thoughtful discrimination. But the curriculum is open to boys in proportion to the mental and physical vigour which they bring to the task. I have said that Greek is not one of the subjects of instruction made obligatory by the scheme, and the reasons for this will become more apparent as I proceed, but among these reasons is