and bark round the stump of a branch sawn off, is a reaction of the organism on the action of an external influence. It seems, therefore, fair to suppose that when our author speaks of a mutilation as an acquired character, he means the growth of the organism consequent upon the mutilation. But if so, a difficulty appears to arise; for the tendency to repair a wound is heritable, and therefore it seems difficult to suppose that the Professor would treat of it as not heritable. It may be said that the parent has the actuality of the repair, the child only the possibility of repairing; but this is, I suppose, all that can ever be expected of inheritance as applied to contingent reactions—i.e. reactions which only arise under peculiar circumstances. There can, I suppose, be no more characteristic heritable reaction than that of the pollen on the ovule, and of the ovule on the pollen; these reactions have taken place in the parent plant, but in the offspring they are originally potentialities, tendencies, contingencies, and they are converted into facts in the event, and in the event only, of the meeting of the pollen and the ovule.

Or take, again, the secretion of the gastric juice in response to the presence of food in the stomach. The parent has taken food, and the reaction has taken place; but the infant inherits, I suppose, the capacity to secrete and not the counterpart of the actual secretion of the parent.

It would seem that there is no essential difference between these three cases. The parent transmits the power to repair a wound, but not the actual reparation of a wound: it transmits the power of fertilisation, but not the fertilised ovules: it transmits the power to digest, but not the already secreted gastric juice.

The emphasis which Weismann has laid on the case of wounds and mutilations would suggest that his doctrine might be thus paraphrased: when an organism is endowed with a capacity, or, to use his word, a predisposition, to react in response to given stimuli, and has so reacted—then what the organism transmits to its progeny is the capacity or predisposition, and not the actual result of the reaction.

It is impossible to doubt that some characters of an organism are hereditary; that others are not, and that the ascertainment of the dividing line between the two classes is of the highest moment to the study of biology; and to Weismann we owe a debt of gratitude for having called pointed attention to this matter. I have at times been tempted to wish that men of science had applied themselves to ascertain the two categories of characters, and then by a careful induction had proceeded to learn the law of heredity without regard to any hypothesis or theory—without reference to germ or soma. But this is not the course which in fact has been taken; and therefore it seems highly necessary to inquire what is the precise meaning of the terms of the proposition affirmed by the one side, and denied by the other.

I conclude this, I fear, too lengthy paper with two questions: (1) Are the conditions which I have suggested as essential to a good definition correct ones; if not, in what are they erroneous? (2) What is the true definition of the words "acquired characters" in the present controversy which satisfies these conditions?

EDW. FRY.

Discontinuous Motion.

THE old theory of the motion of solid bodies through a frictionless liquid supposed that the liquid flowed according to the electrical law of flow. This theory was found to be unsatisfactory, because it makes the pressure negative when the velocity of the solid exceeds a certain critical value.

The theory of discontinuous motion removes the above objection, but is open to others of a different kind. Assuming for the sake of argument that the two theories give correct results when the velocity of the moving solid is respectively less and greater than the critical value, the theory of discontinuous motion ought to be capable of explaining the transition from one kind of motion to the other, and how and why it is possible for a vortex sheet to be called into existence when the critical value of the velocity is exceeded.

Although vortex sheets and other motions involving molecular rotation cannot be generated in a frictionless liquid by a conservative system of forces or by operations performed on the boundary, yet it is easy enough to produce such motions by ordinary mechanical agencies. If a mixture of ice and water be stirred up and the ice allowed to melt, the liquid will

acquire molecular rotation owing to the presence of the particles of melted ice, even though it is absolutely devoid of viscosity. So also if liquid at rest were separated by an indefinitely thin horizontal plate, and the upper liquid were set in motion with horizontal velocity V and the plate were removed, the surface of separation would be a vortex sheet. But the production of motions of this kind requires methods of a somewhat artificial character, and it is difficult to see how they could be set up by a solid whose velocity is allowed to increase gradually from zero to some magnitude greater than the critical value. In fact, I entertain very little doubt that the final motion of the liquid would be quite different from what the theory of discontinuous motion would indicate.

There is, however, a further point, for there are strong grounds for believing that vortex sheets are unstable. No general proof of this proposition appears as yet to have been given, but in every case that has been examined the theorem has been found to be true, (i.) when the liquids on either side of the sheet are identical, (ii.) when the densities of the two liquids are different, but no bodily forces such as gravity and the like are in action. If, therefore, steady discontinuous motion existed at any particular instant, the probabilities are that the motion would be unstable, and the region of dead water in the rear of the moving solid would break up and be changed into a region of turbulent motion. The pressure in the rear of the solid due to this turbulent motion would be different from that of the dead water, and it is therefore not surprising that the theory of discontinuous motion should furnish results which do not agree very well with experiment.

We must also recollect that a frictionless liquid is an ideal substance which does not exist in nature. All fluids are more or less viscous; and it is just at the point where the pressure would otherwise tend to vanish and change sign that we should anticipate the effect of viscosity would appear, and prevent this state of things from taking place; and I believe that many of the difficulties which have arisen in connection with this subject are due to the fact that the effect of viscosity has been overlooked. A vortex sheet cannot exist in a viscous liquid; and if by any artificial means one were produced, it would immediately disappear, and molecular rotation would be propagated into the surrounding liquid. On the other hand, in a viscous liquid, molecular rotation requires no artificial means for its production; for a viscous liquid cannot move without molecular rotation, except in the single case in which the liquid moves like a rigid body having a motion of translation alone. In all other cases, if irrotational motion existed at any particular instant, the motion would immediately cease to be so, and molecular rotation would instantaneously be generated.

Unfortunately the equations of motion of a viscous liquid are so intractable that very little progress has been made in applying them to the solution of hydrodynamical problems. By means of Oberbeck's solution (*Borchardt's Journal*, vol. lxxx.) for the steady motion of translation of an ellipsoid in a viscous liquid, it can be shown that the above difficulties do not arise when viscosity is taken into account; but since the integration of the equations of motion proceeds upon the assumption that the squares and products of the velocities may be neglected, the solution is inapplicable except in the case of slow motions like those produced by the small oscillations of pendulums. The solution gives no information as to what will happen when a disc is moving through a liquid with a velocity of several feet per second.

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Capacity for Heat.

IN the course of some writing upon which I am now engaged, I have constantly to refer to the *capacity for heat of certain substances as compared with the capacity for heat of an equal volume of water*.

The phrase given in italics is a most clumsy one, but I know not how (accurately) to convey the same idea in a shorter way. "*Capacity for heat of unit volume*" has been suggested; but I think that a little reflection will show that it does not express accurately the exact meaning.

Specific Heat $\times$ *Specific Gravity* gives the numerical value required, but cannot be regarded as a definition. There can be no doubt but that a concise expression is wanted. In calorimetry it is often of greater importance to the experimenter to consider the capacity for heat of volumes rather than the capacity

of masses. In the course of a communication which I made to the Physical Society of London, at their last meeting, I appealed to the Fellows present to supply me with the missing phrase. In the discussion which followed the paper, this matter was only incidentally referred to; but, although I think that there was a general agreement as to the want, unfortunately the meeting closed without coming to any conclusion as to the best method of supplying the deficiency.

Will some of your readers help me in this matter?

12 Park Side, Cambridge. E. H. GRIFFITHS.

The Swallowing of One Snake by Another.

THE snake incident, described in NATURE, October 25 last, page 620, as having occurred in the reptile-house in the Zoological Society's menagerie, recalls to my mind two similar cases, recorded in the same periodical, vol. xxx. July 3, 1884 ("A Cannibal Snake," by E. H. Evans), and July 31, 1884 ("The Swallowing of One Snake by Another," by C. R. Osten-Sacken). The first case was observed in Java, the other was witnessed by me in Washington, D.C. In the latter case one of the snakes, although three-quarters of its length had already been engulfed in the other, succeeded in getting out, apparently unharmed, as it remained alive and well in the cage a long time afterwards.

In the *Figaro*, July 26, 1894, I found still another instance of the same kind, which happened in the Jardin d'Acclimatation in Paris. A large snake, while attempting to swallow a rabbit, was interfered with by another one, and passed with the rabbit into the body of its comrade of captivity ("L'un des deux passa à la suite du lapin dans le corps de son camarade de captivité").

C. R. OSTEN-SACKEN.

Heidelberg, Germany, October 28.

ON RECENT RESEARCHES IN THE INFRA-RED SPECTRUM.¹

I PRESENTED to the Association in 1882, at Southampton, an account of some researches made by means of the bolometer, in the infra-red spectrum, formed by a glass prism; but though these labours have continued with occasional intermission during the past twelve years, it is for reasons, which will be explained later, only within the past three years that any notable advance has been made, and only within the past twelvemonth that such a measure of success has been attained as justifies the present communication.

This is not the time to give any historical account of discovery in the infra-red, but all those interested in the subject know that the first investigator here was Sir William Herschel, whose observations consisted essentially in finding that there was something which the eye could not see in a region which he proposed to call the "thermometric spectrum." His distinguished son, Sir John, made a curious anticipation of later discovery by indicating, though crudely, that this invisible heat was not uniformly distributed, and a similar conclusion was reached in an entirely different manner, through the thermopile, by the too early lost Melloni. So ignorant, in spite of these investigations, of those of the elder Draper and of the elder Becquerel, were we till lately, that when, quite within my own recollection and that of most of you, Lamansky in 1871 published, from his observations with the thermopile, a crude little illustration showing three inequalities in the energy curve, universal attention was excited by it among those interested in the subject.

Among other minds my own then received a stimulus which turned it in this direction, and having, as it seemed to me, exhausted the capacities of the thermopile, I invented an instrument for continuing the research, which was afterwards called the bolometer, and with which, in 1881, at an altitude of 13,000 feet upon Mount Whitney, I found spectral regions hitherto unreachd, and whose existence had not been suspected.

I returned with a strong impression of the prospective importance of this discovery, and laboured at the Allegheny Observatory in improving all portions of the new method of research, especially of the bolometer and its adjuncts, with the twofold object of obtaining greater sensitiveness to heat, and greater precision in fixing the exact point in the spectrum where the change of heat originated. With the former object such a degree of sensitiveness was at that time reached, that the bolometer indicated a change of temperature of $\frac{1}{100000}$ of a degree Centigrade, and with the latter, such precision that it was possible to fix the relative position of a line, not merely with a possible error of a considerable fraction of a degree, such as Lamansky's determination is evidently subject to, but with a certainty that the error would be within a minute of arc. The range of the apparatus in wave-lengths was almost unlimited as compared with any other process, and both its sensitiveness and its possible precision seemed to be at that time notable as compared with previous methods, for a great advance was made on anything done before with the thermopile, when the presence of the well-known "D" line of sodium was rendered sensible (though barely sensible), even as a *single* line, by the change of temperature. The sensitiveness was also, as has been said, accompanied with the possibility of unusual precision. The results of this labour were laid before the British Association in the communication already alluded to, and which exhibited ten or twelve inflections of the curve in the portion till then almost unknown, which extends from a wave-length of 1μ to a wave-length of nearly 3μ , at which point the glass prism then used became wholly opaque to radiation. The positions of these inflections were fixed with a precision quite impossible to the thermopile, but this exactness was only obtained in practice by a process so slow as to be almost prohibitory; and with this apparatus the writer personally made in those earlier years such a number of observations as he hardly likes to recall, so disproportionate did the labour inherent in this method seem to the final result.

The justification of this labour seemed to lie in the fact that it does not appear that photography has ever rendered anything much below a wave-length of 1μ —anything, at least, which has been reproduced for publication in a way which gives confidence that we are in touch with the original. The processes which involve the use of phosphorescent substances have given some indications of lines considerably below 1μ , but it is safe to state that the work which has just been referred to as communicated to this Association in 1882, presents almost the only indications which we have possessed, even up to the present time, about the lower infra-red solar spectrum.

Now the curve which was given, even in the later Allegheny observations made with the rock-salt prism, contained but a dozen inflections below the wave-length of 1.5μ , and these inflections, with their correct prismatic and wave-length positions, represent, I think, most of our present knowledge in these regions even to-day.

To understand the method by which there were attained, but only at this great cost of labour, results till then unreachd, it may be repeated that the bolometer had been rendered more sensitive than the thermopile, but that it was capable of being pointed and its position in the spectrum being measured, only by a tedious process which has been exclusively used till lately (but which that presently to be described advantageously replaces). Whichever process is used, when the bolometer thread touches a cold line in the spectrum (since what is black to the eye is cold to it), a larger current flows through the galvanometer, and the spot of light marking the needle's motion is deflected through a certain number of degrees.

From this point forward, the new process, whose results I am about to have the pleasure of bringing before

¹ A paper read to Section A of the British Association, at the Oxford meeting on August 11, by S. P. Langley.