Eastern Hindu Kush, giving a brief sketch of the physiography of the region. Prof. T. G. Bonney and Miss C. A. Raisin furnish notes on the rocks collected by Mr. Conway, from which it appears that the majority much resemble those of the Alps. The most interesting specimens are a peculiar schist with secondary mica, a piedmontite-schist, and a fragment allied to pseudo-jade. Mr. W. F. Kirby identifies the butterflies, Dr. A. G. Butler the moths, and Mr. W. B. Hemsley the plants. Of the last about a dozen were obtained at or over 16,000 feet. The well-known *Saxifraga oppositifolia* was gathered at 17,000 feet, and another species (the highest habitat) at 17,320 feet. Mr. W. L. H. Duckworth writes on two skulls brought from Nagyr, and Prof. C. Roy discusses Mr. Conway's notes on mountain sickness, coming to the conclusion that the primary cause of it is asphyxia. Mr. Conway's observations agree with those of other experienced climbers, that a man in good condition begins to feel the effect of increased altitude at about 16,500 feet. The fact that he is sensible of more inconvenience when in a hollow among the peaks than when on an exposed ridge, Prof. Roy attributes to some loss of oxygen by the air when it has passed over a considerable tract of melting snow. Mr. Conway has made valuable additions to our knowledge of the geography and physical history of this remote mountain region, and the present volume supplements the more popular account of his travels, which appeared earlier in the year.

The Royal Natural History. Edited by Richard Lydekker, B.A., F.R.S. Vols. i. and ii. (London: Frederick Warne and Co., 1893-94.)

ABOUT twelve months ago (*NATURE*, vol. xlix. p. 220), in a short notice of the two first parts of this work, we heartily recommended it as worthy of the notice of our readers. On a careful perusal of the two volumes now before us, which equal one-third of the projected series, we still feel quite justified in our recommendation; the illustrations are for the most part extremely good, and the text is not only interesting, but it is also intelligently written.

The first of these volumes treats, in fifteen chapters, of the Primates, the Chiroptera, the Insectivora, and the Carnivora, as far as the dogs. We would especially notice the chapters on the cats and the dogs, as having information well up to date. Instead of the often-quoted old stories, it is refreshing to meet with accounts of the habits of these animals, taken from the writings of V. Ball, Blandford, Guillemard, Hudson, and Sterndale. Thus, in the account of the common Indian mungoose, we find mention of the results, to within the last year or two, of Mr. Espent's experiments of introducing this little carnivore to Jamaica. The sugar-planting industry in this island was threatened with destruction on account of the swarms of rats; within three or four years after the introduction of the mungoose the rat plague came to an end, and the beneficial results to the island exceeded £150,000 a year. Volume ii. commencing with the bears, finishes the Carnivora, and describes the hoofed mammals. The illustrations play so important a part in these volumes, that we would suggest that the comparative sizes of the figures should always be given, and when possible the reader should be told where the figures first appeared.

Kitchen Boiler Explosions. By R. D. Munro. Pp. 44. (London: Charles Griffin and Co., 1895.)

THE time having again arrived when domestic boilers will be a source of trouble to paterfamilias, Mr. Munro comes forward with an account of a series of experiments with red-hot kitchen boilers, apparently reprinted from the *Transactions* of some Society. Whether this be so or not we do not wish to inquire, but to us it seems that the diagrams of steam-pressure are little

suitied to the "intelligent householder" for whose edification they are intended. The chief conclusions drawn from the experiments are that (1) a dead-weight safety-valve should be fitted to every boiler; (2) water will flow into a red-hot boiler although there is no free outlet, and, also, that a steam-pressure can be attained in such circumstances sufficient to cause rupture of the strongest boilers in use; (3) whilst a very high steam-pressure may be generated in a red-hot boiler by the sudden injection of cold water, a disastrous explosion cannot thus be produced; (4) an explosion, in the true sense of the word, cannot occur unless the boiler contains water as well as steam. Probably the perusal of Mr. Munro's book will help to diminish the disasters from boiler explosions.

The Island of Madeira, for the Invalid and Naturalist.

By Surgeon-General C. A. Gordon, M.D., C.B. Pp. 110. (London: Baillière, Tindall, and Cox, 1894.)

PERSONS who are fortunate enough to be able to leave England during the dreary months of winter, and who select to sojourn in Madeira—"The Flower of the Ocean"—should take this brochure with them. The characteristics of the people and the place are concisely stated, and there is more information on the geology, meteorology, zoology, and botany of the island than is usually given in guide-books of a similar kind. It is well known that Madeira has an extensive fauna and flora, and we agree with the author that it is a matter of regret that the island has no public museum where they could be collected and investigated. Prof. Smitz, however, is gradually forming such an institution at the college in Funchal.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of *NATURE*. No notice is taken of anonymous communications.]

"Acquired Characters."

NOW that the correspondence on this subject, which you allowed me to start in your number of November 1, seems drawing towards a close, I ask leave through you to thank your correspondents for their courtesy in replying to my inquiries, and also to make a few observations by way, so far as I am concerned, of conclusion.

As none of your correspondents has found any fault with the conditions which I suggested as essential to a good definition, I conceive that I may assume them to be correct.

Furthermore, as none of these writers has adopted or defended any of the definitions which Weismann appeared to me to give or to suggest, or has said anything by way of criticism on my strictures on these definitions, I think that I may conclude that I was not far wrong in those strictures, and that Weismann's writings do not afford any good definition of the words to which he has given currency.

Mr. Poulton has suggested that a definition may be found in the statement that "whenever an organism reacts under an external force, that part of the reaction which is directly due to the force is an acquired character." But surely this is difficult of application: for in every case of a reaction on an external stimulus there are two elements—viz. first, the internal capacity to respond, and secondly, the external force or stimulus. Each of these is necessary to the result and to every part of the result, and neither is of itself sufficient to the result or to any part of the result. How then can we analyse or break up the result or the reaction into two parts, and say that the one is the direct result of the external force, and the other is either not its result at all, or its indirect result? Where there is the joint action of two causes, each necessary but neither sufficient of itself, I conceive that you cannot either logically or physically sever any part of the result from the action of both of the causes, and there is no ground for attributing directness to one part of the effect, and indirectness to another. Mr. Poulton dwells truly on the reaction having two causes—the internal and the

external ; but this does not justify the analysis of their results into two parts.

Nor does the matter become plainer to me when I follow Mr. Poulton in his application of his definition to concrete cases. The first is the case of the so-called "Exerciernochen," a reaction on an external force resulting in a certain number of changes of tissue. How is it possible to classify those changes into two categories—the one including changes directly due to the external force, the other changes not directly due to it? If the stress is to be laid on the word "direct," then one must respectfully ask what is its meaning? how is it to be ascertained and verified? one must inquire whether what is said to be an indirect result, means anything but a result which is transmitted? in which case we should find ourselves in a vicious circle. I will not follow Mr. Poulton through the other instances, but I believe the reader will find, like me, that each raises similar difficulties, and that in no case does he analyse the actual result into two distinct and separable parts.

Mr. Galton proposes, and Prof. Ray Lankester adopts another definition, viz. "Characters are said to be acquired when they are regularly found in those individuals only who have been subjected to certain special and abnormal conditions." Now I suppose that characters can be found regularly only either (1) in individuals exposed to conditions which induce them, or (2) in individuals who have inherited them. To say, then, of a character that it appears only regularly in individuals exposed to certain conditions, is to say that it does not appear in individuals by inheritance, and the proposition that acquired characters are not transmissible is thus reduced to an identical one. The possibility of inheritance is excluded by the definition, and the inquiry whether acquired characters are inherited is impossible.

I do not propose to follow your learned correspondents into many of the subjects touched on by them ; but the more their letters are read, the more apparent does it become that they are not at one, either with themselves or with Lamarck or Weismann, as to the use of the words "acquired characters ;" and for myself, I repeat my regret that an inquiry of great moment should be obscured, as I venture to think, by a premature use of classificatory words before the real classification of nature herself has been ascertained. For the question, "Are acquired characters transmissible?" I hope to see substituted the inquiry "What characters are transmitted?"

EDW. FRY.

The alleged Absoluteness of Motions of Rotation.

All motion is relative. This apparently universal statement is a particular statement about the meanings of words. The word "motion" means "relative motion," or, more precisely, "the motion of a body" means its motion relative to other bodies. We may go further and make the still more fundamental statement that *all position is relative*, or, in other words, the position of a body means its position relative to other bodies. It is easy for anyone who puts words together without reflecting sufficiently on their meaning to put together the noun "motion" or "position" and the adjective "absolute," but the expressions "absolute motion" and "absolute position" are nevertheless meaningless, just as much so as "white blackness," "retrograde progress," "the action between a rough body and a smooth body at their point of contact," or "the potential energy of non-conservative forces."

The above remarks contain the standpoint from which it is concluded that motions of rotation are no more "absolute" than motions of translation. Anyone who accepts them for translation, but rejects them for rotation, places himself in an illogical position. Unless I have misunderstood them, this is the position occupied by Maxwell and Prof. Greenhill. The standpoint of those who assert that position and motion are in all cases relative is directly opposed to that of Newton. He held that there is absolute position in immovable space, and relative position with respect to bodies, and correspondingly there are absolute and relative motions; that absolute motion of a body is its transference from one absolute place to another absolute place; that we can never determine the absolute place of a body, but only its relative place; that in cases of rotation we can distinguish absolute from relative motion by the effects of "centrifugal force." He gives no indication how to distinguish absolute and relative motions of translation. From the impossibility of determining the absolute position of a body it seems to follow that absolute motion of translation cannot be determined; this view is adopted by Maxwell.

Assuming that it is the object of the Science of Mechanics to give as simple a description as possible of the observed facts about the motions of bodies, the assumption at the outset of absolute motions or positions about which we can know nothing appears an unnecessary complication.

From a logical point of view the cardinal statement in the discussion is that all position is relative. It appears to be conceded by Newton, and has been insisted upon by Maxwell, that all knowable position is relative. To say that at any instant a body has an absolute position in space, but that we can never know where it is except by reference to other bodies, that is to say that every body has an absolute position, although we can only know its relative position, is to introduce an unnecessary complication, if it is not to talk nonsense.

What is done in practice is to determine the position of a point by reference to a Cartesian system of axes, or by an equivalent method. What is called "the velocity of a body" is its velocity relative to the axes; what is called "the acceleration of a body" is its acceleration relative to the axes; a body has a motion of rotation when the angles between lines of the body and any axis are changing with the time. It is part of the solution of a mechanical problem, as presented by any set of observed facts, to determine the system of axes with reference to which the description of the observed motion becomes as simple as possible, and there exists a calculus for transforming the expression of a motion from one system of axes to another when the relative motion of the two systems is known.

The question how the axes are to be determined has been much discussed, but no general answer appears possible. Particular answers apply in particular cases. In general any three points, not in a straight line, determine a set of coordinate axes. For one of the points may be chosen as origin, the line joining this point to another of the points may be chosen as one coordinate axis, and the plane of the three points may be chosen as one coordinate plane. For the three points any identifiable parts of bodies may be taken; but, in general, axes so chosen will be inconvenient, or, what comes to the same thing, the description of a motion by reference to them will not be simple.

The description of motion is generally made in terms of the concept *force*, that is to say, we state the acceleration relative to the axes which a free body placed in a given position relative to the axes, and moving with a given velocity relative to the axes would have, and the nature of the constraints which give rise to differences of acceleration in a constrained and a free body moving through the same position with the same velocity relative to the axes. In an actual problem the acceleration of a free body whose other circumstances (position and velocity relative to the axes) are known, must be found by experiment. It does not concern the matter now under discussion that among these circumstances position is generally predominant over velocity in determining accelerations. What is of more importance is the fact that *the force acting on a body depends on the system of axes chosen*. For the force is a vector quantity whose line of action coincides with the line of the acceleration relative to the axes, and whose magnitude is proportional to this acceleration. This point has been noted by Maxwell. The result that the field of force depends partly on the axes is frequently a guide to the choice of convenient axes of reference, namely, we choose axes with respect to which the expression of the field of force is simple, and it often happens that in this way all motional forces, other than frictional resistances, can be made to disappear. An easy and striking example of the differences introduced by changing the axes can be found by considering the motion of two particles which move in one of the planes chosen as coordinate planes with uniform velocities in different lines relative to the coordinate axes. If a new set of axes is constructed by taking one of the particles as origin, and the line joining the particles as one axis, the acceleration of the other particle relative to the new axes is directed towards the first particle, and varies inversely as the cube of the distance between the two particles. Another very interesting example is furnished by Foucault's pendulum, to be discussed presently.

These remarks will serve as a preparation for the way in which we interpret, in accordance with the principle of the relativity of motion, those experiences which have been held to favour the view that motions of rotation admit of absolute determination. The history of the discussion appears to show that little would have been heard of this doctrine apart from the desire to explain to a public unused to regard motion as relative the theory of the rotation of the earth. To one who has mastered