*to which I have no reply to make, except that if they thought as we do, they must have an immortal soul as we have,* which is not likely, as we should apply the argument to *all* animals, such as sponges, oysters," &c. I am sure these ideas are not unfrequently repeated in his correspondence, as for example, in one of his replies to Morus (vol. i. No. 67 of the 4to edition, in Cousin's Edition, x. p. 204 *et seq.*). He there even talks of two souls, an *âme corporelle* which is the cause of passions and affections, and an incorporeal principle of thought, which he elsewhere says was infused by the Deity into man at the first moment of his existence. He also observes, I think logically enough, that as no boundary line can be drawn elsewhere, we have no choice between conceding a soul to oysters or refusing it to all animals save man. I am not however concerned to defend the validity of his reasons, but rather to contribute this information as an historical point of interest.

Trin. Coll., Dublin, Nov. 11 J. P. MAHAFFY

### Plane-Direction

I THINK "plane-direction" is the best of the competing names. The planes of cleavage in a crystal are the "plane-directions" in which it is most easily split. They cannot be called either "aspects" or "positions." The opposite faces of a cube certainly cannot be said to have the same "aspect."

If a rigid body receives a movement of translation, it retains something unchanged. What is this something to be called? It might be called "lie" or "set," but both names are equivocal. Two equal and similar figures possessing this something in common might be very well described as "similarly laid," "similarly set," or "similarly placed." We may say that they have "similar positions," but we can scarcely say that they have "the same position;" for change of position is commonly held to include movements of translation as well as of rotation, and a point is usually defined as having position but not magnitude. I think it is worth while to consider whether "position" cannot be restricted to the more limited sense, "place" being employed in the wider sense.

I wonder that no one has yet raised a murmur against the proposition itself, which your correspondents are so anxious to render literally into English. It appears to me that the plain English form in which Mr. Wilson first stated it is clearer and more precise than the German abridgement. In the strictest sense of "determine," one "Richtung" determines one "Stellung" and one "Stellung" determines one "Richtung," inasmuch as to one plane-direction there corresponds one normal direction.

In a special sense it is true that two "Richtungs" determine a third (perpendicular to them both), and that two "Stellungs" determine a third (also perpendicular to both); just as two points may be said to determine one plane (bisecting their joining line at right angles). In all these instances the fact is that not one only but many are "determined," but all except one come out in pairs or multiples of two. It is this one, which has no fellow, that is in a special sense "determined."

I think it is paradoxical and misleading to state, without qualifying words, that two linear directions determine one plane-direction; inasmuch as two linear directions really serve to define as many different pairs or multiple pairs of plane-directions as we please, and if we are permitted to distinguish the two linear directions by different names, three plane directions can be separately defined by them without any ambiguity. Similar remarks, of course, apply to the other half of the proposition.

J. D. EVERETT

Rushmere, Malone Road, Belfast, Nov. 11

### "Wormell's Mechanics"

WILL you do me the favour of inserting a brief reply to the few remarks made concerning the above text-book in last week's NATURE?

1. On page 8 of the book occurs an explanation of what is usually termed the transmissibility of force, and a statement of the axiomatic principle that we may imagine a force to be applied at any point in the line of its direction, *provided this point be rigidly connected with the first point of application.* On page 14 a deduction from this principle is made and employed to prove the rule for finding the directions of the resultant of two forces acting on a point. The reviewer says that this deduction, "if true, would assert that the attraction of the sun and the earth upon the moon might be transferred to any heavenly body in space which happened to be in the line of direction of the resultant of the forces." If the restriction laid down with emphasis in the book, and printed in italics as quoted above, be not ignored, this is a legitimate inference, and if the point to which the forces are transferred parallel to themselves be rigidly connected with the moon, any conclusion having reference to the magnitude or direction of the resultant action on the moon derived as a consequence of the imaginary transposition of the point of application of the forces will be correct.

2. In finding the direction of the resultant of two parallel forces, the same transposition of the point of application is employed, and, of course, it is understood with the same proviso. This proof your reviewer qualifies as "meaningless," whereas I feel sure that, taken in connection with the original axiom and the deduction above referred to, it would be accepted by any mathematician as both intelligible and correct.

3. The next statement is that the definition of a rigid body is given as a property of forces. This is not so, but the whole theory of statics, when developed independently of dynamics, rests on the properties of a force and the properties of a rigid body jointly.

4. The reviewer next dwells upon a curious error which unfortunately escaped my notice until it was pointed out but a short time ago by a schoolboy, and which forms one of three corrections on a slip of errata. Any student would, however, have been able to make the correction for himself by the help of the preceding pages and the applications to the following exercises, a circumstance which I think an unprejudiced critic should not have overlooked.

5. Your reviewer next remarks that a student who tries an experiment with a block and tackle would naturally be surprised at finding that the result of experiment does not agree with that of the theory, and adds, "nor can we find a single word in the book which would enlighten his difficulty." The reviewer cannot have read section 71.

6. The subjects included in the book are such as comprise the course described in the curriculum and examination papers of the University of London, and if occasionally the discussion of unpractical arrangements of mechanical powers is required, I am not answerable. Indeed, I hope to see the day when a reform of this part of the curriculum will necessitate my rewriting the work on an entirely different plan, namely, one according to which kinematics forms the first part, kinetics the second, and statics the third, the propositions of the third part being special cases of those of the second. But that at present it answers the purpose for which it is intended, is proved by the fact that all the questions set this year can be answered from it.

So far as most of the facts and illustrations are concerned, "I am but a gatherer and disposer of other men's stuff," and a writer of an elementary text-book to suit the requirements of a particular examination could not easily be more.

The tone of depreciation with which the writer of the article has been pleased to refer to the work, so directly opposed to a previous notice of the same book in the same journal, seemed to me to call for some reply, and I should wish to describe more fully the objects I have aimed at in compiling the work, but that I know I have already taken up enough of your valuable space.

RICHARD WORMELL

## ONE OF THE GREATEST DIFFICULTIES OF THE DARWINIAN THEORY

SIR JOHN LUBBOCK has done good service to science in directing attention to the metamorphoses of insects, by admitting freely the great difficulty in conceiving "by what natural process an insect with a suctorial mouth, like that of a gnat or butterfly, could be developed from a powerful mandibulate type like the Orthoptera, or even from that of the Neuroptera" (NATURE for Nov. 9, page 28). Such "difficulties" have struck many from the first, and it is in no small degree encouraging to those who love the liberty of science, to find that the time is ap-

proaching when difficulties may be brought under consideration and discussion.

"There are," Sir John Lubbock remarks, "peculiar difficulties in those cases in which, as among the Lepidoptera, the same species is mandibulate as a larva, and suctorial as an imago." The power of mastication during the first period of life being an advantage, on account of a certain kind of food being abundant, and that of suction during the second, when another kind of food prevailed, or *vice versâ*, is suggested as a possible explanation of the origin of species which are mandibulate during one period of life and not during another. In such cases it is said we have "two forces acting successively on each individual, and tending to modify the organisation of the mouth in different directions." It is suggested that the change from one condition to the other would take place "contemporaneously" with a change of skin. Then it is urged that even when there is no change of form, the softness of the organs precludes the insect from feeding for a time, and when any considerable change was involved, "this period of fasting, it is remarked, would be prolonged, and would lead to the existence of a third condition, that of pupa, intermediate between the other two."

There is much that is assumed in this reasoning; but I shall now venture to call the attention of naturalists to one point only, namely, the analogy between the period of fasting caused by the temporary softness of the organs while the caterpillar changes its skin, and the more prolonged fasting period when the organs undergo that more considerable (!) modification involved in the change from the mandibulate to the suctorial type of mouth. The change from a small mandibular apparatus to a larger one seems to be compared with the change from a mandibular to a suctorial apparatus—the change of skin of the caterpillar with the change of skin when the caterpillar becomes the pupa, and the latter the imago—the temporary softness which prevails when the little mandibles grow into bigger mandibles, with the temporary softness which prevails while the mandibles become converted (!) into the suctorial mouth. But these changes are surely of different orders, and the operations of a different nature. The mandibles do not change. The one type of mouth does not pass through gradations of any kind into the other kind of mouth. But one abruptly ceases, its work having been discharged, while the other is developed anew. As compared with the change of skin of the caterpillar, the change of skin from chrysalis to butterfly is indeed a "*considerable change*." It would require an amazing intelligence to premise from the study of a caterpillar that from it, after certain changes of skin and periods of rest, would emanate a butterfly.

It is very well to suggest that "in reality the necessity for rest is much more intimately connected with the change in the constitution of the mouth"; but what, I would ask, is the evidence of the connection implied? Between the *change* from the small mandibles to the large, and the *change* from the latter to the suctorial apparatus, there can be no comparison—no analogy, for the suctorial mouth is developed anew during the pupa state, and its formation is not commenced until all traces of the mandibles are gone. Nay, every tissue of the caterpillar disappears before the development of the new tissues of the imago is commenced. The muscular and nervous systems of the latter are as different from those of the former as are the digestive apparatus, the oral mechanism, and the external covering. These organs do not change from one into the other; but one, having performed its work, dies, and is removed entirely. Not a vestige of it remains. Its place is occupied by formless living matter, like that of which the embryo in its early stages of development is composed; and from this *formless matter* are developed all the new organs so marvellously unlike those that preceded them; and others unrepresented at all in the larval stage, make their appearance. To explain, according to Mr. Darwin's theory, the "period of change and quiescence" intermediate between the caterpillar and imago states of existence, is likely to remain for some time a very difficult task. If the difficulty cannot be resolved until the period of quiescence during which the imago is formed, is proved to be analogous to the periods of quiescence during the change of skin of the larva, the life history of a butterfly will remain for a long time a puzzle to Mr. Darwin and those who believe in the universal application of his views.

LIONEL S. BEALE

## ON THE RECURRENCE OF GLACIAL PHENOMENA DURING GREAT CONTINENTAL EPOCHS

IN the August number of the Geological Society of London I published two papers "On the Physical Relations of the New Red Marl, Rhætic Beds, and Lower Lias," and "On the Red Rocks of England of older date than the Trias." In the latter I attempted to prove that for the north of Europe and some other parts of the world, a great Continental epoch prevailed between the close of the upper Silurian times and the end of the Trias or commencement of the deposition of the Rhætic beds; in other words, that the Old Red sandstone, Carboniferous strata, Permian beds, and New Red series were chiefly formed under terrestrial conditions, all, with the exception of the Carboniferous series, in great lakes and inland seas, salt or fresh.

The Permian strata, in particular, appear to have been deposited under conditions to which the salt lakes in the great area of inland drainage of Central Asia afford the nearest modern parallel.

While brooding over the whole of this subject for several years past, I have often been led to consider its bearing on those recurrent phenomena of glacial epochs which now begin to be received by many geologists.

The phenomena of moraine-matter, scratched stones, and erratic boulders, whether deposited on land by the agency of glaciers, or in the sea and lakes by help of floating ice, are evidently intimately connected with the contemporary occurrence of large areas of land, much of which may, or probably must, have been mountainous.

The late Mr. Cumming, in his History of the Isle of Man, "hints at the glacial origin of certain Old Red conglomerates in that island, conceiving that the bony external skeletons of some of the fish of the period may have been provided to enable them to battle with floating ice. In lectures and in print I have frequently stated my belief that the brecciated subangular conglomerates and boulder beds of the Old Red sandstone of Scotland and the north of England are of glacial origin, so distinct, indeed, that when these masses and our recent boulder clay come together, there is often actual difficulty in drawing a line of demarcation between them. I frequently felt this difficulty years ago, when, commencing the Geological Survey of Scotland, I mapped the strata in the country south of Dunbar, and the same difficulty was occasionally felt by others in the valley of the Lune, near Kirkby Lonsdale.

If, as I believe, the Old Red sandstone was deposited in inland Continental waters, the Grampians, as a mountain tract, bordered these waters, and they must have been much higher then than now; not only because of the probably greater elevation of the whole continent, but also because the Grampians formed land during the whole of the Upper Silurian epoch, and suffered great waste by denudation, then and ever since. The glaciers of these mountains marked an episode in Old Red sandstone times, and yielded much of the material of the boulder beds of the Old Red sandstone.

In these regions and in North America, the Carboniferous