assure him (or it), however, that the omission has been in no way connected with those "queer notions of honour, and justice, and fairness," which he conceives to be rife in our times. Why should I seek to wrong the honoured dead? And who would gain in this case by the injustice? The present Astronomer Royal? Surely no. To add this small matter to his real claims to our esteem would be

To gild refined gold, to paint the lily,
And throw a perfume on the violet.

Neither, I am sure, has any other writer who has overlooked Flamstead's claims, desired to do him injustice. On this point I would merely remark, "Rest, rest, perturbed spirit."

But now "we'll shift our ground," by the Ghost's good leave.

Our visitor from Valhalla remarks that "a stir was lately made about what was represented as a new method of investigating the motion of the solar system in space, and instead of a new there was brought forward an old acquaintance (known to Science since the times of our grandfathers)." Here the spirit of Flamstead refers obviously to the Astronomer Royal's method. I am sure that Prof. Airy would desire greatly that if his method be indeed so ancient, the fact should be made widely known. I myself am particularly anxious to be set right on this point, about which I am at this very time writing. For though I care more about explaining this and the other methods than about their history, yet it is desirable to be accurate even in historical details.

If I may say so without offence, I would remark that a ghost was not needed—certainly not the ghost of the first Astronomer Royal—to teach astronomers that the opposition of Mars in 1877 will be exceptionally important. At page 25 of my "Sun" I have already pointed this out, and I dare say others have done likewise.

I hope the "great injustice" to which our ghostly correspondent refers as endured by him in life, does not relate to his difficulties with Newton, for at the present time the opinion of Brewster on this point is in vogue—not Baily's; and the warmest admirers of Flamstead are those who least desire to moot the subject.

R. A. PROCTOR

Brighton, Nov. 4

Creators of Science

PERMIT me to do my little towards clearing up a most unfortunate confusion of thought respecting the intellectual ranks of mathematicians and metaphysicians, which is, in my experience, widely prevalent. We may safely divide the mathematicians into three orders:—(1) Inventors, (2) Experts, (3) Readers or Students, so as to discriminate from one another those who create systems, those who manipulate with them, as "ministers and interpreters of nature"—just as easily and familiarly as Professor Tait (*e.g.*) employs and applies the theory of Quaternions—and those who have merely studied into an understanding of an author or subject. It was an expedient of the late Sir William Stirling Hamilton to confound all these orders, and from the heterogeneous lump to extract—if not extort—testimonies to the worthlessness of mathematics as a mental discipline, without the least discrimination of their sources.

On the other hand, the metaphysicians cannot be trichotomised; for, even in the present advanced state of metaphysics, there is no class of philosophers corresponding to the mathematical experts, the reason of which explains why examiners in mental science do not set problems. There are, in fact, only two classes of metaphysicians: I., Creators; II., Students, more or less thoroughly versed in the systems of the leaders, and more or less accepting or rejecting, with more or less reason, those creations. Accordingly, when on May 17, 1869 (I think that was the date), Professor Tait, at a meeting of the Royal Society of Edinburgh, challenged the metaphysical world to produce a metaphysician who was also a mathematician, he not being able at the moment to call to mind a single instance, he was to be understood as asking for a person of the order I, who was also in the class I. Professor Calderwood's reply, then, was not wholly unexceptionable, for of the three names he adduced, viz., Descartes, Leibnitz, and Hegel, the last was that of a reader of mathematics, and not of a mathematical inventor. The challenger might have spared the respondent the trouble of reply, had he known what De Morgan wrote in Notes and Queries, 2nd S. vi. 293-4, where are distinguished five mathematical inventors, as *facile principes*: viz., Archimedes, Galileo, Descartes, Leibnitz, and Newton; and in which Aristotle, Plato, and D'Alembert are allowed a very high rank in mathematics. Had the inventor of Quaternions been then dead, I have little doubt that De Morgan would have

added to the five the name of Sir William Rowan Hamilton, who, besides being a mathematical inventor of the very first rank, was also a diligent and accomplished student of Plato, Kant, Reid, and the other Hamilton, and a writer on Logic; *i.e.*, as good as D'Alembert as a philosopher, and perhaps better than he as a mathematician. Now, it is not a little curious and very instructive to observe that, *pace Platonis*, the two who were creators of strictly defined metaphysical systems, viz., Descartes and Leibnitz, are the only two among the five metaphysicians adduced by De Morgan who belong to the highest rank as mathematical inventors.

It is quite incredible that a man of Professor Tait's learning (I say here nothing of his judgment) should not have been aware of the identity of Descartes (the poor dreamer!) and Cartes, the founder of the Cartesian Geometry; still more so that he should not have known that the immortal analyst, the co-inventor of the Differential Calculus, was the most eminent metaphysician native to Germany before Kant. It was, then, not "ignorance," but "ignorance," on the part of the Scotch mathematician, that was involved in his challenge; and that challenge was doubtless intended as mere *badinage*, at the expense of a science which he had taken no pains to understand.

Be that as it may, I trust I am not singular in adjudging (as De Morgan did) these two grand intellectual pursuits to be worthy of being cultivated together, and to be able to give material aid to each other. For myself, I cannot but look upon any man as the enemy of intellectual progress, who delights in setting the one class of investigators against the other, and endeavours to prolong the controversy which has raged between them since the "Principia" was promulgated.

Highgate, Nov. 8

C. M. INGLEBY

Descartes' "Animated Machines"

AS you open your valuable columns to philosophical discussions, may I request you to publish the following remarks on a passage in Mr. Lewes's popular "History of Philosophy" (Vol. ii. p. 148 of the new edition), where he confesses himself puzzled, along with other critics, to account for Descartes' theory that animals were only *animated machines*. "I am not prepared," he says, "with a satisfactory explanation." I cannot but think that a careful perusal of the "Discourse on Method" (Part 5, *sub. fin.*) and of the treatise on *les Passions de l'âme*, makes Descartes' reasons perfectly clear. In the first place, the use of the word *machine* has misled most of his critics, and if the story of Malebranche and his dog be true, even this great disciple had grievously mistaken the principles of his master. For in the last-named treatise Descartes endeavours to show that such feelings as joy, grief, fear, &c., though in us accompanied by really mental acts (*pensées*), are produced by physical causes, and produce physical effects apart from the mind. Descartes would therefore never have denied to brutes any of the bodily sensibilities which we possess; and says expressly that he calls them machines in a special sense—machines made by the Deity, and therefore infinitely more subtle and perfect than any which we can construct. He says that we could not ourselves be ranked higher in the scale of beings did we not possess the gift of *language*, the phenomena of which can only be accounted for by an internal principle different in kind from those which appear to guide the lower animals, though there are also those passions in us which we have in common with them.

But to come to the psychological reasons for the theory. Historians of philosophy before the 18th century should be particularly alive to theological *idola*, even in sceptical writers; much more so in good Catholics like Descartes. Just as Berkeley put forward prominently the theological advantages of his Idealism, so Descartes indicates plainly in his "Discourse on Method" (*loc. cit.*) that these were the chief reasons of his theory. "Next to the error of those who deny the Deity, which I have already refuted, there is none more apt to seduce feeble minds from the path of virtue than to imagine that the soul of beasts is the same as ours." But the *locus classicus* has, I think, escaped Mr. Lewis, and will be found in a letter to a Lord (supposed to be the Duke of Newcastle), the 54th of the 1st volume in the original quarto edition. Descartes there specially answers objections made to him on this point, and in the way above indicated; adding however the following passage: "Yet it may be said that although the beasts perform no action which convinces us that they think, nevertheless, as the organs of their bodies do not differ much from ours, it may be conjectured that some sort of thought is joined to these organs, such as we experience in ourselves, but much less perfect;

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 4th 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 "Stellungen" 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 linean directions determine one plane-direction; inasmuch as two linean 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 linean 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?

I. 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 suctional 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-