

tuists," and is "irrational." A more baseless charge was never yet made in controversy, nor a more obvious attempt to alter the terms of discussion so as to give some appearance of plausibility to a lost cause. The Duke, in fact, now at length tells us that *he* does not mean by "acquired characters" what *we* mean. Why then did he "interpolate" his remarks on the subject and make use of the term?

If the meaning which the phrase has for the scientific world generally be insisted upon, we are now, it appears, to understand that the Duke of Argyll agrees with us: what *we* mean by "acquired characters" are not, he admits, shown to be transmitted.

"Fortuitists," the Duke says, "have invented a new verbal definition of what they mean by 'acquired.'" I have shown at the commencement of this letter that the term "acquired" is used to-day as it was by Lamarck. To the Duke this meaning is "new"—because he has either never read or has forgotten his Lamarck. If this be so, the Duke has been writing very freely about a subject with which his acquaintance is very small. The alternatives are as clear as possible: either the Duke of Argyll knew the significance of the term "acquired characters" as employed by Lamarck, in which case it would have been impossible that he should charge those whom he calls "fortuitists" with having invented a new verbal definition of what they mean by "acquired"; or he did not know Lamarck's use of the phrase, and was therefore not qualified to offer an opinion in the discussion, nor to press his "beliefs" and "position" upon public attention.

I have no time and you have no space to devote to a full exposure of the character of other assertions made in the Duke of Argyll's "statement of his position" which are as reckless and demonstrably erroneous as that concerning the meaning of the term "acquired."

Perhaps the most flagrant of these is the assertion that "the theory of Darwin is essentially unphilosophical in so far as it ascribes the phenomena of variation to pure accident or fortuity" (paragraph 4). Of course the Duke cannot be acquainted with the following passage from the "Origin of Species," sixth edition, p. 106; but if he has to plead ignorance of the writings not only of Lamarck, but also of Darwin, what is the value of his opinions and beliefs on Lamarckism and Darwinism? The words of Mr. Darwin referred to are these:—"I have hitherto sometimes spoken as if the variations, so common and multiform with organic beings under domestication, and in a lesser degree with those under nature, were due to chance. This, of course, is a wholly incorrect expression, but it serves to acknowledge plainly our ignorance of the cause of each particular variation."

Whatever meaning the Duke may attach to the word "fortuity," it is mere empty abuse on his part to call the later Darwinians "fortuitists," and still less justifiable to insinuate that their investigations and conclusions are not guided by a simple desire to arrive at truth, but by the intention of propping up a worship of Fortuity. It is natural for the Duke to suppose it impossible to write on Darwinism without some kind of theological bias.

In conclusion, I venture to point out that the Duke of Argyll has (1) failed to cite facts in support of his assertions of belief in "prophetic germs," and "transmission of acquired characters" when challenged to do so; (2) that he displays ignorance of two of the most important passages in the works of Lamarck and of Darwin, whom he nevertheless criticizes, and in consequence of his ignorance completely, though unintentionally, misrepresents; and (3) that he has introduced into these columns a method of treating the opinions of scientific men, viz. by insinuation of motive and by rhetorical abuse, which, though possibly congenial to a politician, are highly objectionable in the arena of scientific discussion.

February 22.

E. RAY LANKESTER.

Physical Properties of Water.

As you inform me that my anonymous critic (*ante*, p. 361) does not intend to avail himself of the opportunity I gave him (through you) of correcting his misstatements about my *Challenger* Report, I must ask to be permitted to correct them myself.

(1) There is nothing whatever in my Report to justify the critic's statement that I "had never heard of Van der Waals' work . . . till the end of the year 1888." Yet this is made the basis of an elaborate attack on me!

What I did say was to the effect that I was not aware, till Dr.

Du Bois told me, that Van der Waals had given numerical estimates of the value of Laplace's K . I had long known, from the papers of Clerk-Maxwell and Clausius, the main features of Van der Waals' investigation. But I also knew that Maxwell had shown it to be theoretically unsound; and that Clausius had taken the liberty of treating its chief formula as a mere empirical expression, by modifying its terms so as to make it better fit Andrews' data. This paper of Clausius is apparently unknown to my critic, as is also my own attempt to establish (on defensible grounds) a formula somewhat similar to that of Van der Waals.

(2) I said nothing whatever about the "Volume of Matter in unit volume of Water." Hence the critic's statement, "Prof. Tait's value is 0.717," is simply without foundation.

I merely said that the empirical formula

$$p(v - a) = \text{constant},$$

if assumed to hold for all pressures, shows that a is the volume when the pressure is infinite. I still believe that to be the case. If not, Algebra must have changed considerably since I learned it.

My critic speaks of a totally different thing (with which I was not concerned), which may be $a/4$ or $a/4\sqrt{2}$, or (as I think is more plausible) $a/8$. But he says that liquids can be compressed to 0.2 or 0.3 of their bulk at ordinary temperatures and pressures. I was, and remain, under the impression that this could be done *only at absolute zero*, and then no compression is required.

There are other misrepresentations of my statements, quite as grave as those cited. But it would be tedious to examine them all. I have no objection to a savage review, anonymous or not; on the essential condition, however, that it be *fair*. It is clear from what I have shown that this essential condition is absent.

But my critic, when his statements are accurate, finds fault with the form of my work. I will take two examples of this kind, and examine them.

(3) He blames me for not using C.G.S. units. The *Challenger* Reports are, as a rule, written in terms "understood of" nautical men. I wonder what such men would have said of me, in their simple but emphatic vernacular, if I had spoken of a pressure of 154,432,200 C.G.S. units, when I meant what they call a "ton"; or, say, of 185,230 C.G.S. units, when I meant a "naut."

(4) I am next blamed for "mixing units."

I should think that if we could find a formula expressing, in terms of a man's age, the average rate at which he can run, say for instance

$$v = \frac{Ax(B-x)}{x^2 + C},$$

even my critic would express A in feet per second, and take x as the mere number denoting the age in years. Would he, alone in all the world, insist on expressing x as denoting the age in seconds in order to prevent what he calls the mixing of units? This is a case precisely parallel to the one in question.

Generally, I would remark that my critic seems to have written much more for the purpose of displaying his own knowledge than of telling the reader what my Report contains. For at least three of the most important things in my Report are not even alluded to:—the compressibility of mercury, the nature of Amagat's grand improvement of the *Manometre Desgoffes*, and (most particularly) the discussion of the wonderful formula for the compressibility of water given in the splendid publications of the *Bureau International*.

P. G. TAIT.

THE last volume of the *Challenger* Reports contains papers on various branches of science. The review which appeared in *NATURE* was not the work of one writer, and was therefore not signed, but I have no desire to avoid taking full responsibility for the part of which I am the author.

It will be convenient to reply to Prof. Tait in paragraphs numbered to correspond with his own.

(1) Of course I fully accept Prof. Tait's account of his knowledge of Van der Waals' theory at the time when his *Challenger* Report was written, but I entirely dissent from his statement that what he said about it in the Addendum referred to in the review was "to the effect" described above.

It is hardly possible to do justice to my own case without quoting freely, but I will compress as much as possible. He