

most of these the hardness is of little practical importance, but the table does show how capricious this quality is and how apparently unrelated either to density, atomic weight, or position in the periodic classification.

Much depends on the condition of each specimen and on the treatment to which it has been subjected, as, for example, whether it is cast, forged, rolled, or drawn. In the cast state some of the metals consist of an aggregate of small crystals which separate comparatively easily, and in these cases the value found for their hardness relates to the junctions of the crystals and not to the crystals themselves.

Only those metals which are, to some extent at least, malleable, give a true measure of hardness in this form of test. (These are marked *M* in the table.) The others show the apparent hardness of the particular specimen used, depending, in part, on the closeness of the aggregation of small crystals, or on the relation of the crystallographic axes to the direction of the crushing force.

Many alloys have been tested. Various kinds of steel range from 700 tons/square inch for hardened carbon steel down to 56 tons/square inch for pure iron, and it is worth while to notice that none of the reputedly hard metals such as iridium approach the former figure. A. MALLOCK.

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Is Darwinism Dead?

THE review in NATURE of Jan. 15 of my criticism on Mr. Wells's somewhat antiquated biology has only just been shown to me, hence the delay in my sending this letter. I will make it as brief as possible, for I am only concerned with showing that the distinguished reviewer, Sir Arthur Keith, though he has doubtless been given a few sentences from my book for purposes of quotation, has not read the book itself.

(1) He says: "So adroitly does Mr. Belloc cover his verbal tracks with a smoke screen" that he cannot determine whether I am a 'fundamentalist' or a 'Darwinian.' As a fact, I cannot conceive myself being either, but the point is that no one who had read my book could have imagined that 'Fundamentalism' was the issue. The only issue was whether natural selection were the process whereby the differentiation of species came about.

(2) He says that I give 'with approbation' St. Thomas's conclusion that the creation of man was (in scholastic language) 'immediate': that is, special and direct. Had the reviewer read my book he could never have sincerely written that. I quoted this *exceptional* conclusion on immediate human creation to show that St. Thomas probably thought the creation of animate beings other than man to be 'mediate': that is, evolutionary.

(3) The reviewer is "forced to the conclusion" that I have never read "The Origin of Species." If he had read my book he could not possibly have been 'forced' to so foolish a conclusion. All I say in it on this matter is written in direct relevance to that work—with its only original (and erroneous) thesis of natural selection as the machinery of differentiation.

(4) He says that "Mr. Belloc resuscitates this ancient misrepresentation of 'accidental' and 'single' variations," and follows the sentence up with a good deal of irrelevant abuse. Had he read my book he would have found that I know all about Darwin's retreat in this matter, and am careful to point out that it was a muddled retreat. For the mathematical argument against natural selection applies just as much to a thousand cases out of a million as to one out of a thousand.

(5) He so completely misunderstands the example I take from the growth of horns that he clearly has not read the original passage but is judging from a chance sentence put before him, and even that he fails to grasp. My point—clearly stated, emphasised, reiterated—was that multiple adaptation is mathematically incompatible with the blind mechanical action of natural selection. Multiple adaptation presupposes design. The citation of the hormone as a disproof of God is wildly off my point. One might as well say that the presence of glue in a piece of woodwork disproved the presence of a carpenter.

(6) I have kept to the last the most damning count in this indictment. The reviewer sets me down as owing my remarks entirely to Mivart, as having merely copied Mivart's work of more than half a century ago: implying my ignorance of all since. Had he read my book he would have seen that I quoted from authority after authority among the highest names in modern biology from the beginning of the discussion to works which appeared so recently as three years ago. I give their actual words, which prove with what increasing force the old-fashioned Darwinian theory of natural selection has been beaten down. I end by a list of no less than forty such names—I might easily have made it a hundred. No one who had read my book could possibly have missed this continued and repeated citation of authority from every side, which is the principal feature of this section.

I conclude, therefore, that the reviewer has not read my book; for I hope that not even the most violent religious animosity could lead him to deliberate misrepresentation. H. BELLOC.

NOT only did I read Mr. Belloc's book with great care, but I also took the trouble of turning up the works of some of the authorities he cites. On p. 12 he mentions, with bated breath as it were, "the great work of Vialleton." This "great work" is a very good elementary treatise on embryology which Prof. L. Vialleton, of the University of Montpellier, wrote for his students, and it stands in much the same relationship to the works of Charles Darwin as do those of Mr. Belloc to Shakespeare's.

Mr. Belloc cites Vialleton as his authority for denying the possibility of birds having been evolved from reptiles. On searching Prof. Vialleton's "Éléments de morphologie des vertébrés"—published in 1911—I found on p. 611 that after citing what Huxley, Owen, Seely, Mivart, and Gadow had to say about the matter, Prof. Vialleton concludes thus: "L'origine des oiseaux reste donc dans le plus complet mystère," which is a very different thing from denying their origin from reptiles. I have collected many other errors of a like kind, enough to convince me that Mr. Belloc's references are untrustworthy. Many of the authorities he cites, such as my friend the late Prof. Dwight, of Harvard, belonged to the generation which never succeeded in assimilating the teachings of Darwin.

ARTHUR KEITH.

Radioactivity and the Heat of the Earth.

IN his presidential address to the Geological Society, abridged in NATURE of Jan. 1, Dr. J. W. Evans refers (page 15) to the above topic in the following words: "although the whole of the energy given out by radioactive elements, when isolated, is converted into heat, it is probable that a considerable proportion of the energy liberated by such elements, when they occur as rock-constituents, is used up in effecting physical, chemical, or atomic changes in the surrounding minerals.