

suppose that Prof. Pearson would actually accuse Major Darwin and myself of such dishonesty, but he has not guarded his words against the possibility of this interpretation being put on them, and so I meet the charge—in the only way possible—by a flat denial.

It is interesting to inquire how the mistake under discussion could have arisen. It seems probable that the words "last few months" conveyed the idea of some indeterminate period of time, and that this idea and not the actual words were held in the memory, afterwards to be retranslated into words as "last years." This would probably not have occurred if Prof. Pearson had himself been a little more precise in the first instance. The interview which he refers to during which Sir Francis Galton expressed doubts concerning the policy of the Eugenics Education Society took place about three weeks before Galton's death. Is three weeks the precise period which Prof. Pearson describes as a few months? The last letter quoted in *The Eugenics Review* in answer to Prof. Pearson's original letter was written, not in 1909, but in October, 1910, about three months before Galton's death.

Finally, when Prof. Pearson wrote, "I have no other effective means except through the courtesy of your columns to correct a wholly erroneous statement, which the editor of that society's journal has put into my mouth," had not he already received a letter from Major Darwin apologising for the mistake, and assuring him that it would be corrected in the next number of *The Eugenics Review*?

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Origin of Argentine Wild Horses.

ANENT the recent discussion as to the origin of the wild (or feral) horses of the Argentine Republic, there is one line of evidence to which I venture to direct attention. That is the question of infertility.

Assuming, as I suppose most reasonable people do, that the South American horses were derived originally from the north—whether in the northern part of North America or in north-eastern Asia is immaterial—and that the South African horses are similarly derived, it would seem that the Argentine species would be at least as remote geographically from the wild ancestors of the domestic horse as are the modern zebras and asses, and could not be any more nearly related genetically. The species native to the Argentine, if they continued to exist down to modern times, would have evolved in complete isolation from any northern species since the early Pleistocene at least, and probably longer as regards any Old World species. Now the infertility of crosses between zebras or asses and domestic horses is based upon a separation that does not appear to date earlier than the late Pliocene. Beyond that they must be derived from a common stock. The autochthonic Argentine horses were therefore not any more nearly related to *Equus caballus* than are the zebra or the ass. They should therefore be equally infertile when crossed with the domestic stock. (The degree of infertility of distinct species varies in different families of mammals; but the known facts regarding the horse, asses, and zebras afford a measure of its degree in this family.) So far as I know there is no record of infertility in such crosses, and since, as I am informed, the wild horses are caught and domesticated on the pampas just as they were in the western United States, any such infertility could scarcely escape notice. This would seem to me to be a decisive argument against the theory that the existing wild horses of South America

are descended either wholly or partly from any surviving native stocks. The argument would apply with less force to the wild horses of the western United States and Mexico, yet even with these it would appear to be a strong point. But the geologic evidence against the survival to modern times of any native horses in North America is very nearly conclusive in itself.

Even if we admit that some of the native horses may have survived in the Argentine until the time of the Spanish settlement—and I think that the evidence for that contention is strong, and that it is quite in conformity with some other features in the faunal history of South America—the native stock would presumably be no more able to interbreed with domesticated or feral stock of *Equus caballus* than could the quagga in South Africa. It would remain separate and immiscible until exterminated. No strain of it could survive in the modern feral horses.

W. D. MATTHEW.

American Museum of Natural History,
New York, January 15.

Specific Heats and the Periodic Law.

AT his last Friday evening lecture at the Royal Institution Sir James Dewar announced his somewhat startling discovery that at temperatures of about 20° absolute the specific heats of the elements are periodic functions of the atomic weights, and are therefore not in accordance with Dulong and Petit's law (established at ordinary and higher temperatures). May I venture to point out that a simple consideration of the difference of conditions in the experiments of Sir James from those of Dulong and Petit may ultimately harmonise the two sets of results?

From Guldberg and Wage's "mass law" it follows that the velocity increases with the mass (atomic weight), but this increase of velocity takes place at higher temperatures at a very much greater rate, with the result that at higher temperatures the atomic mass becomes relatively less important, *i.e.* the special atomic properties will be less emphasised. The velocity factor becoming so predominant, a proportionately smaller additional increase of (heat) energy will be required to raise the mass to a higher temperature, *i.e.* the specific heat will be inversely proportional to the mass (Dulong and Petit's law). At very low temperatures—say at about 20° absolute—when the velocity is very small—almost negligible—the mass of the atom is the predominant factor, and hence we find a periodic function of the atomic weight as the expression of the specific heat as well as of the other (physical and chemical) properties. The above suggestion might be tested by experiments to find a temperature at which neither the Dulong and Petit nor the Dewar law would be strictly obeyed.

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January 31.

The End-product of Thorium.

IN continuation of our letter published in *NATURE* of February 5, containing a suggestion as to the nature of the end-product of thorium, we would point out that, of course, our view involves atomic weights for the various disintegration products of thorium higher than is ordinarily assigned to them, and that therefore the determination of the atomic weight of any one of them would afford a test of the truth of the hypothesis.

J. JOLY.

J. R. COTTER.

Iveagh Geological Laboratory, Trinity College,
Dublin, February 7.