of Mammoth-tusks in the frozen mud of Siberia, and by the wonderful aggregation of Hippopotamus-bones revealed to us by Dr. Falconer's explorations in the Palermo caves—be also taken into account, we can scarcely, as it seems to me, avoid the conclusion, that the period in the later stages of which we get the first indubitable evidence of Man's existence (to say nothing of any anterior to it) was much more distinguished than the present for the aggregate bulk and wide distribution of the largest members of its fauna. WILLIAM B. CARPENTER

CAN Mr. Wallace throw any light on Mr. Allen's somewhat extraordinary sentence: "I feel a genuine respect for every donkey I meet, *when I remember that it was the mere accidental possession of an opposable thumb* that gave my ancestors a start over his in the race for the inheritance of the earth towards the very close of the tertiary period." I take Mr. Allen to be an evolutionist, but there is no place for accident in evolution, or in any other scientific theory. The "opposable thumb" must be the result of some conditioning factor, and this being so the word accident is quite out of place. ΦΠ

February 27

Moths Attracted by Falling Water

WHILST watching the great horse-shoe falls of the Skjálfandafljót near Ljósavatn in Iceland, I saw moth after moth fly deliberately into the falling water and disappear. Some which I noticed arriving from a distance, fluttered at first deviously, but as they neared the water flew straight in. The gleaming falls seemed at least as attractive as artificial light, and if the fact has not been observed in this country I should suppose it is because the moths likely to be attracted, fly by night, whilst in Northern Iceland there is no night during the summer. The preference trout show for pools near falls is more likely to arise from the extra food they find there, than the more aërated state of the water. The latter supposition, seeing the number of species of lake trout, always seemed to me a lame one, invented for want of a better, whilst the former explains why broken water is always inhabited by insectivorous fishes. The instinct of self-destruction in moths must be older than the introduction of artificial light, and cannot be of use exclusively to collectors, but though its benefits to salmon and trout are obvious enough, its advantages to the moths are not so apparent, unless this self-devotion checks an increase that otherwise would be disadvantageous. J. STARKIE GARDNER

Hypothetical High Tides

I HAVE no desire to constitute myself a champion of Mr. Ball's high tides, but I do not think that the testimony of the Coal-Measures, to which Mr. S. V. Wood calls attention, will decide much. These deposits are mainly of non-marine origin, the plants being terrestrial, and the prevailing mollusc, *Anthracosia*, closely resembling *Unio*. Marine strata do indeed occur, but in almost inappreciable proportion. If it be objected that, in these marine episodes, the hypothetical tidal wave must have wrought fearful havoc; I would suggest that there is no proof that in the Carboniferous epoch the speed of the wave was enormously greater than at present. When we reflect that by that time nearly, if not quite all the classes of the animal kingdom had come into existence; we can hardly avoid the conclusion that the Coal-Measures were formed in a period which, in comparison with the age of the globe, must be regarded as comparatively recent. Considering how slight is the denuding power of modern tides, I doubt if even a treble velocity would materially increase the effect.

Mr. Elsden's suggestion that the accelerated tidal wave may account for the absence of estuarine deposits before the Carboniferous epoch, takes for granted what remains to be proved. How do we know that there were no pre-Carboniferous deltas? We recognise estuarine strata by the intermixture of terrestrial or fresh-water fossils with marine organisms. The Old Red Sandstone of Britain, being a lacustrine deposit, does not bear upon the question; but I see no reason why the Devonian strata of Russia, in which, according to Murchison, fresh-water fishes are associated with marine shells, may not be in part of estuarine origin. Below the Devonian, the evidence of terrestrial life is very meagre; and to infer from its absence in a set of beds that they must be marine, would be hazardous reasoning.

I do not make these observations in the interests of any theory, but simply to evoke discussion on a very interesting question.

Wellington, Salop, March 3 C. CALLAWAY

Rime Cloud observed in a Balloon

I SEE in NATURE, vol. xxv. p. 385, an interesting letter from a German physicist, who comments on the recital of my last balloon ascent (January 25, 1882) as published in your columns. I am very grateful for the numerous instances of *frost-rime* that he quotes as having been observed on former occasions, but I cannot possibly admit his theory of the liquidity of minute water-drops suspended in the air at a low temperature. The reason why I object to this view was explained more than a century ago by the celebrated Bouguer, when describing in 1744, to the French Academy of Sciences the coronæ he observed in the Andes on the occasion of his ascending the Pichincha. I beg leave to quote this interesting account of a quite forgotten exploration:—

"On voit presque tous les jours sur le sommet de ces montagnes un phénomène extraordinaire qui doit être aussi ancien que le monde, et dont il y a bien de l'apparence que personne n'est été temoin avant nous. Chacun de nous vit son ombre projetée sur un nuage qui n'était point à trente pas. Le peu de distance permettant de distinguer toutes les parties de l'ombre—on voyait le bras, les jambes, la tête; mais ce que nous étonne c'est que cette dérnière partie était ornée d'une gloire on d'une aureole formée de trois ou quatres petites couronnes concentriques d'une couleur tres vive, chacune avec le mieux nuance que l'arc-en-ciel primaire, c'est à dire le rouge en dehors.

After having insisted on the description of the phenomenon (*Mémoires de l'Académie* pour 1744, p. 264 and 265), Bouguer says:—"Le phénomène ne se trace que sur les nuages formés de gouttes de vapeur et même sur ceux dont les portraits sont glacées, mais non sur les gouttes de pluie comme l'arc-en-ciel." Having seen the *corona* for more than an hour, almost without interruption, and nothing resembling a rainbow, I cannot possibly admit any liquid water in the cloud, and I am obliged to oppose the surfusion theory as advocated by M. Jamin, to explain the crushing by ice-crystals of the loftiest trees of the Forest de Fontainebleau. W. DE FONVIELLE

Paris, February 26

The Markings on Jupiter

MR. DENNING'S interesting communications in NATURE (vol. xxv. pp. 223, 265) led me to consult my notes of observations of Jupiter made in the summer of 1878. I used a telescope of only $3\frac{3}{8}$ inches aperture, but of exquisite definition, made by John Byrne, of New York. Under date of July 7, 1878, I find this entry:—"10 p.m.—There is a remarkable light spot near the centre of the light equatorial zone of Jupiter."

On July 27 I wrote:—"I saw on the bright equatorial belt of Jupiter a spot of obviously greater brightness than any other part of the disk. Just above and to the west of it was a dark spot on the southern belt. The bright spot grew more distinct as it approached the centre, and caught the eye the instant it was placed at the eyepiece. The bright spot was equal in diameter to about two-thirds of the width of the south belt."

Again on July 31:—"Saw a white spot on the light equatorial belt, probably the same seen on the 27th."

I have also sketches of Jupiter made in the fall of 1879, from which I see that on September 4, at 10 p.m., there was a distinct white spot indenting the northern border of the great south belt, and opposite the forward end of the red spot. On September 6 this white spot had advanced, so that it was ahead of the red spot. Other fainter white spots are shown in my sketches. These rude observations may be of some use in assisting Mr. Denning to trace back the history of the remarkable markings that for three or four years have attracted so much attention to Jupiter. G. P. SERVISS

New York, February 9

The Level of the Mediterranean

AMONG the "Notes" in NATURE, vol. xxv. p. 395, I read Prof. Naudin's opinion on the apparent lowering of the level of the Mediterranean along the whole Riviera during the months of January and February; but I think there is a far more simple explanation of the phenomenon. In Genoa we had for many days as much as 43 centimetres below the standard level, but that was