translated into intelligible form the various terms of one of the less formidable formulæ of Mr. Heaviside's memoir, I was surprized to find two old and very unpretending friends masquerading in one person like a pantomime Blunderbore. In one of his Avatars the monster contains, besides the enclosing brackets, no fewer than 24 letters, 12 suffixes, 3 points, and 5 signs! When he next appears he has still the brackets to hold him together, but although he has now only 18 letters, he makes up his full tale of 44 (or 46) symbols; for he has 9 suffixes, 3 indices, 3 points, 5 signs, and 3 *pairs* of parentheses! I used to know him as compounded of 14 separate marks only, viz.:— $V^2\nabla\sigma + 2S\nabla\nabla_1 S\sigma\sigma_1$:—but, unless I had required to dissect him, I should never have put him in anything resembling his new guise.

Dr. Knott's paper is, throughout, interesting and instructive:—it is a complete exposure of the pretensions and defects of the (so-called) Vector Systems. "Wer diesen Schleier hebt soll Wahrheit schauen!" I find it difficult to decide whether the impression its revelations have left on me is that of mere amused disappointment, or of mingled astonishment and pity.

P. G. TAIT.

Edinburgh, 24/12/92.

Measurement of Distances of Binary Stars.

WITH reference to Mr. C. E. Stromeyer's letter on the above subject, which appeared on p. 199, it may be of interest to point out that his plan of determining the distance of a binary star is by no means a new one.

The method was, I think, first suggested by Mr. Fox Talbot at the Edinburgh meeting of the British Association in 1871; but the mere idea was sufficiently obvious as soon as the possibility of determining velocities by the spectroscope had been demonstrated by Dr. Huggins.

The first discussion of the geometrical conditions of the problem was given by Prof. C. Niven in the *Monthly Notices*, vol. xxxiv. No. 7, where he exhibits the relation connecting the parallax, the relative velocity, and the elements of the orbit of a double star, and computes the value of the product (πV) of the parallax and velocity for a small number of binary systems.

In a paper published in the Proceedings of the Royal Irish Academy for May, 1886, I examined the same question from a slightly different point of view, being at the time unaware of Prof. Niven's paper, and was led to similar results. An epitome of this paper was published in your Astronomical Column, vol. xxxiv. p. 206. From the results obtained it appeared that, all things considered, γ-Coronæ Australis and α-Centauri were the most likely binaries to yield to this method of eliciting the secret of their parallax, while α-Geminorum, one of the stars selected by Mr. Stromeyer, was shown to be most unfavourable on account of the situation of its orbit.

In the *Monthly Notices* for March, 1890, I again drew attention to the subject in view of the accuracy of the results obtained by the photographic method in the hands of Prof. Pickering and Prof. Vogel. In this paper I gave an extended list of binaries with the usual geometrical and dynamical elements, and in addition the two elements A and B on which the relative velocity depends. I also gave the greatest value which πV can attain in each case and the velocity to be expected in the case of those stars whose parallaxes had been determined.

Again in Mr. J. E. Gore's valuable catalogue of Binary Star Orbits, published in the Proceedings of the Royal Irish Academy for June, 1890, columns 18 and 19 are devoted to the constants A and B computed from my formulæ (which I may say ought more properly to be called Prof. Niven's formulæ on account of the priority of his paper) for eighty-one different orbits.

The subject has also been discussed by Miss Clerke in "The System of the Stars," pp. 199–201, where references to most of the original publications will be found.

I may perhaps add that the inverse problem of determining the elements of the orbit from spectroscopic observations alone has also been investigated by me in the *Monthly Notices*, vol. li. No. 5, where I have deduced the principal elements of the orbit of β-Aurigæ, a spectroscopic double which no telescope can divide.

I have been disappointed that astronomers engaged on spectroscopic determinations of stellar velocities have not devoted more attention to observations of already known binaries, which appear to me to offer a promising field of work, and have often regretted that at this observatory we have not the means of undertaking the investigation, and if Mr. Stromeyer's letter has no other effect than to bring the subject once more forward it will have done good service, but I should like to point out that the second of the stars selected by him ought on no account to be taken as a test of the feasibility of the method, since the accurate discussion of the conditions shows that unless this is an exceptionally remote system the velocity must be very small indeed. For instance, assuming Johnson's parallax, viz. 0″·20, the relative velocity of the components amounted last year to only 0·6 miles per second.

In the northern hemisphere the most favourably situated binaries are 70 Ophiuchi, ξ-Ursæ Majoris, and, if Peters' orbit represents the real motion of the pair, 61 Cygni; while for the southern hemisphere special attention ought to be directed to α-Centauri and γ-Coronæ Australis.

In Mr. Gore's Catalogue, referred to above, will be found all the materials for determining when to observe any known binary most favourably in this respect, and for deducing its parallax from the measures obtained, and it ought to be borne in mind before letting the subject sink back once more into oblivion, that, other things being equal, this method is most likely to succeed in the case of the most distant systems, where the parallax is so small that the ordinary trigonometrical method necessarily fails us, and that when the micrometer, the heliometer, and the stellar photograph break down, the spectroscope will sound the further depths with ever-increasing facility.

Dunsink Observatory, co. Dublin. ARTHUR A. RAMBAUT.
December 30.

December Meteors (Geminids).

THESE meteors were moderately abundant on the night of December 12, which appears to have been a very favourable one in regard to weather. The chief radiant point was observed in the normal position very close to α Geminorum, and there was a strong contemporary shower from a centre east of β Geminorum.

At 10h. 10m. December 12, a fireball estimated to be twice as brilliant as Venus was observed by Mr. Booth at Leeds. It moved rather slowly from 150°+43° to 188°+41°, and divided into two pieces at the finish.

Mr. Wm. Burrows, of Small Lane, Ormskirk, writes to me with reference to a meteorite which he observed to fall at a later hour on the same night. He says the time was 6.52 a.m. (December 13), and refers to the phenomenon as follows:—"Seeing the meteor was coming to the earth I crossed the road to where it appeared to be falling, and it fell about two yards from me. When it struck the earth it made a noise like the report of a gun; it also went black instantly. While descending it had a tail of fire about a foot long. It is 1⅝ inch in diameter one way, and 1¼ inch another, and one inch thick."

Mr. Burrows sends drawings of the object, and it being still in his possession it is hoped the matter may be suitably investigated. Should it prove a veritable meteorite one interesting circumstance in connection with it will be that its descent took place concurrently with the shower of Geminids.

It is significant that December 9–13 constitutes a well-defined ærolitic epoch, rendered memorable by the fall at Wold Cottage, Thwing, Yorkshire, on December 13, 1795, and by many others, such as that at Mässing, Bavaria, December 13, 1803, at Weston, Connecticut, U.S.A., December 14, 1807; at Wiborg, Finland, December 13, 1813; at Ausson, France, December 9, 1858; at Baudong, Java, December 10, 1871, &c.

Bristol, January 1. W. F. DENNING.

The Earth's Age.

AS Dr. Wallace (NATURE, p. 175) trusts "that on further consideration" I shall "admit that" my "objection is invalid," it is evident that I have failed to make clear to him my argument showing that his data do not warrant his conclusion.

He overlooks the fact that a thickness of 177,200 feet of sedimentary rocks is, standing alone, a perfectly indefinite quantity; to make it definite it must have a definite area.

As he mentions no area for it we are justified in assuming that he means the land area of the globe, whereas his calculation is made as though area were not of the essence of the problem, in short, as if the formation of a pile of sediment 177,200 feet thick, of no matter what area, were the problem.

In Sir A. Geikie's calculation and all other similar ones with which I am acquainted, the thickness of the sedimentary rocks is tacitly assumed to be their thickness all over the land area of the globe.

Dr. Wallace's calculation leads to the absurd result that continents are growing nineteen times as fast as materials are produced to supply their growth.

Leaving the question of the conclusions to which Dr. Wallace's data logically lead, I may say that I am not responsible, and do not hold him to be responsible, for the absurd theory as to the thickness of sedimentary rocks on which they are based.

In order to arrive at a scientifically accurate result, what we require to know is the present actual thickness in every part of the world, plus all the thickness which has previously existed in, but since been denuded away from, every area. The existing thickness in geologically explored areas can perhaps be ascertained within certain limits of error from geological maps and memoirs. For instance where the surface consists of Torridon Sandstone overlying Archæan gneiss of igneous origin, the thickness of sedimentary rock is that of the Torridon Sandstone only, if we assume that the gneiss there is part of the metamorphosed original crust of the earth, for the existence of which Rosenbusch has recently argued.

It is easily demonstrable, first, that in many places the existing thickness of each formation, where undenuded, is far from being the maximum thickness, and, secondly, from the thinning out in some directions, or merging, near the old shoreline, into conglomerates, that some formations were never deposited over certain areas; indeed, the very existence of a sedimentary deposit necessarily implies that of land undergoing denudation and not receiving deposit, although it may well be doubted whether the land area was always nineteen times the area receiving deposit.

Reasoning from the deposits preserved as to those removed by denudation, it is highly improbable that any considerable area ever received either the complete series of deposits, or on the average anything like the maximum thickness of the deposits it actually received. In addition to this, some formations usually considered to be successive may be really contemporaneous, so that the figures representing maximum thicknesses usually taken in calculating the earth's age are probably far above the truth for the purpose in question.

The immense labour involved in calculating the existing thickness of sedimentary rocks in each area, and the thickness which there is any reasonable ground for supposing to have been at any time denuded from that area, as well as the uncertainty of the results, has probably deterred geologists from attempting the task, especially as large areas are very imperfectly known. BERNARD HOBSON.

Tapton Elms, Sheffield, December 24.

THE first part of Mr. Hobson's letter alone requires notice from me, as the latter part characterizes as absurd the views of those eminent geologists who have estimated the total thickness of the sedimentary rocks, and seems to assume that such writers as the late Dr. Croll and Sir Andrew Ramsay overlooked the very obvious considerations he sets forth.

As regards myself, he reiterates the statement that when geologists have estimated the total thickness of the sedimentary rocks at 177,200 feet, they mean that this amount of sediment has covered the whole land surface of the globe; that, for example, the coal measures, the lias, the chalk, the greensand, the London clay, &c., &c., were each deposited over the whole of the continents, since it is by adding together the thicknesses of these and all other strata that the figure 177,200 feet (equal to 33 miles) has been obtained.

Mr. Hobson concludes with what he seems to think is a *reductio ad absurdum*:—"Dr. Wallace's calculation leads to the absurd result that continents are growing nineteen times as fast as materials are produced to supply their growth."

But the apparent absurdity arises from the absence of any definition of the "growth of continents," and also from supposing that the growth of continents is the problem under discussion. The question is, as to the growth in thickness, of sedimentary deposits such as those which form the geological series. These deposits are each laid down on an area very much smaller than the whole surface of the continent from the denudation of which they are formed. They are therefore necessarily very much thicker than the average thickness of the denuded layer, and the ratio of the area of denudation to the area of deposition, which I have estimated at 19 to 1, gives their proportionate thickness. If Mr. Hobson still maintains that he is right, he can only prove it by adducing *evidence* that every component of the series of sedimentary rocks has once covered the whole land-surface of the globe; not by assuming that it has done so, and characterizing the teaching of all geologists to the contrary as absurd. ALFRED R. WALLACE.

Ancient Ice Ages.

MR. READE in his letter (NATURE, p. 174) refers to the striations on the pebbles forming the conglomerates at Abberley and the Clent Hills.

Following the late Sir Andrew Ramsay, he considers the deposits to be of glacial origin, but goes further than that distinguished geologist in citing them as proof of a former ice age.

It is but reasonable to suppose that *glaciers* existed in past ages in places where the conditions—such as high altitude and abundant precipitation—were favourable.

Before, however, the existence of a former *glacial period* can be established, we must have evidence of contemporaneous deposits of undoubtedly glacial origin, and extending over widespread areas—say half a hemisphere. J. LOMAS.

University College, Liverpool, December 31.

Printing Mathematics.

THE use of the solidus in printing fractions has been advocated by authorities of such weight that it seems almost a heresy to call it into question. Yet I venture to think that there is a good deal to be said against it. In such matters the course preferred by mathematical writers and their printers is apt to take precedence over that which is most convenient for the great body of those who will read their work. It is tacitly assumed by those who prefer this notation that the getting of mathematical formulæ into line with ordinary printing is an unmixed advantage. No doubt it is easier to set up the work in type thus, but with the consequent rapidity and cheapness of printing the advantage ends. Most people will agree that it is much pleasanter to read a mathematical book in which the letterpress is well spaced, so that the formulæ stand out clearly from the explanatory language, than one in which the two run together in an unbroken stream: just as a book divided into paragraphs is more readable than one which is not. The old style is more restful to the mind and eye, and one can more readily pick out the salient features of the demonstration.

Another aspect of the question seems to me more important. In making any calculation mentally it is much easier to visualize fractions, more especially if complicated, as written in the ordinary way than as written with the new-fashioned notation. The component parts of the mental picture are imagined as spread over a plane instead of being arranged along a line, and can be thought of separately with less confusion. From a similar point of view it will be admitted that it is inconvenient to write mathematical expressions in one form and to print them in another.

Then, again, I doubt whether the assumption that the solidus notation conduces to accuracy is justified. No doubt the printer makes fewer original errors; but whereas with the old notation his frequent glaring errors are more readily detected by the proof-reader (or, if missed by him, by the ordinary reader), with the new notation the misplacement or omission of a solidus is, from the simplicity of the error, likely to be overlooked. In general, no one will be the poorer if a little more trouble is taken with the printing, and a little more paper is used for the book.

The symbol $/$ has advantages over its equivalent $\div$, and to its restricted use, such as is made by Sir G. Stokes, one can hardly object; it matters little how such expressions as a/b or dy/dx are printed. But it is the thin end of the wedge; and one is under a debt of gratitude to Mr. Cassie for showing, in your issue of November 3, to what it may lead. May it be a long time before we have to learn to substitute for the harmless expression, $\dfrac{b^{\frac{1}{2}}}{c(d+e)^3}$ its newest equivalent, $\mid b \diagdown 1/2 \mid \;/ c \mid d + e \diagdown 3 \mid$!

I trust that no one will interpret the final note of exclamation as a factorial symbol. M. J. JACKSON.

D. I. Sind College, Karachi, November 23.