

insufficient optical power and bad definition, which disappear in a fair atmosphere with a good telescope well adjusted to focus." With respect to facts, I must be allowed to observe that I believe the facts are entirely the other way. This is a point which can only be tested by appealing to the facts themselves.

Wales and Dymond observed the Transit of Venus in 1769 in the Hudson's Bay. This is their account of what they observed:—"We took for the instant of first internal contact the time when the least visible thread of light appeared behind the subsequent limb of Venus, but before that time Venus's limb seemed within that of the sun, and his limb appeared behind hers in two very oblique points, seeming as if they would run together in a broad stream, like two drops of oil, but which, nevertheless, did not happen, but joined in a very fine thread at some distance from the exterior limb of Venus. This appearance was much more considerable at the egress than at the ingress, owing, as we apprehend, to the bad state of the air at the time. We took for the instant of internal contact at the egress, the time when the thread of light disappeared before the preceding limb of the planet, from which time W. W. took notice that he had told about 24^s, when the limbs of the sun and Venus were apparently in contact; a circumstance which he did not venture to attend to at the ingress."

The observers evidently saw these phenomena both at ingress and egress. From the detailed account at ingress the definition must have been very good. I have printed the whole passage, including the part which may be turned against my argument, that "the appearance was more considerable at egress, owing, as we apprehend, to the bad state of the air." That the appearance of such a ligament seen under great atmospheric tremor may have been more striking, I can well believe.

Again, Chappe writes:—

"A l'entrée totale de Vénus, j'observai très distinctement le second phénomène que avait été remarqué par la plus grande partie des astronomes en 1761. Le bord du disque de Vénus s'allongea comme s'il étoit attiré par le bord du Soleil. Je n'observai point pour l'instant de l'entrée totale celui où le bord de Vénus commençoit à s'allonger; mais ne pouvant pas douter que ce point noir ne fût partie du corps opaque de Vénus, j'observai le moment où il étoit à sa fin; de façon que l'entrée totale ne peut être arrivée plutôt, mais peut-être plus tard de deux ou trois secondes. Le point noir étoit un peu moins obscur que le reste de Vénus. Je crois que c'est le même phénomène que celui que j'observai à Tobolsk en 1761." I might quote other extracts. The phenomena are noted at Wardhus at the egress. It is expressly stated by Cook and Green at Otaheite that the extinction of the thread of light between Venus and the sun was gradual, and that at Otaheite the observers did not note the end at the ingress and the commencement at the egress. Now, as a practical man, I would ask Mr. Newcomb are not these appearances observational facts? They appear to me so real, that, to admit their non-reality, would be the same thing as if we were to argue that if Wales, Dymond, and Chappe had put down in their observing books the times when the limbs of Venus first appeared in contact, instead of waiting until they could not see the slightest trace of any connection between the limbs of Venus and the Sun, they would not have given earlier times than those which now appear in their journals; or that if Cook and Green at Otaheite had given the times corresponding to the last appearance of any connecting ligament at the ingress, the times given would not have been later than those which now appear in their journals. It was from these considerations, which appear clear enough, that I have treated the Otaheite observations as referring to a different phase from those of the Hudson's Bay observers, and Chappe's ingress observations. Similar remarks apply to the egress observations. You cannot talk of such appearances being simple products of insufficient optical power and bad atmospheric circumstances. The appearances presented to and described by Wales and Dymond, even at the egress, took place according to their own estimation, which is the largest, within about a second of arc. Such appearances could not be discriminated amongst with insufficient optical power, and under very bad circumstances of observing. Chappe particularly uses the phrase "très-distinctement." This point appears to have been overlooked by many who have written much upon the subject. With respect to experimental facts, I should, indeed, esteem it a great favour, and I am sure that it would be important as bearing on our preparations for the Transit of 1874, if Mr. Newcomb can refer us to any experiments bearing upon this point. It will, however, be necessary to understand clearly the positions taken up.

First, I assume the existence under sufficient illumination of irradiation. Secondly, I assume that the illumination of the sun is so great that under the ordinary circumstances of telescopic observation the optical enlargement of the sun's disc due to this phenomenon is about 3", the exact quantity will vary under different circumstances. The data upon which these assumptions are grounded are, amongst others, the experiments of Dr. Robinson, vol. v. Mem. Royal Astron. Society, and the eclipse discussions made from observations with the great equatorial of the Greenwich Observatory. Can Mr. Newcomb refer us to any experiments which have been made with a disc sufficiently illuminated to present under the circumstances of examination an optical enlargement of 2", and in which sufficiently powerful optical means have been employed to discriminate between the changes presented within 1" of arc, as a small portion of the illuminated surface near the limb has been cut off by an opaque body?

I know of no such experiments. I do know that experiments were made at Paris by Wolf, in which the illumination of the disc was such that no sensible optical enlargement was exhibited. The results obtained had, therefore, no bearing on the question of irradiation; they were simply experiments on the disappearance of a small portion of a feebly illuminated disc. The results are such as any one conversant with the subject would have predicted with such a disc. The diameter of Mercury is so small that the appearance presented in a transit would not be so clearly marked as in a transit of Venus. Of the reality of the appearance of a connecting ligament in the transit of Mercury of 1868 I have no doubt, for I saw it. I would, with all due diffidence, give here a word of caution respecting discussions of these results. The phenomena under discussion, whether real or supposed, are presented only within a second of arc from the sun's limb. It is perfectly useless, therefore, to appeal to upon this question any observations which have been made with insufficient optical means to subdivide a second of arc.

The optical enlargement by irradiation is a function of the brightness, and can be made insensible by sufficiently diminishing that brightness. Unfortunately, however, when this diminution of brightness is carried to a very great extent errors in an exactly opposite direction to those of irradiation will come into play, similar, in fact, to the results of Wolf's experiments. The observations of Mercury on the sun's disc in 1868 were made with very different optical means, and some very different methods were adopted for diminishing the sun's glare. If the observations are put together without any discrimination upon these points some curious results will appear. I am afraid that some gentlemen have been much misled by want of attention to these simple points.

E. J. STONE

Royal Observatory, Cape of Good Hope, July 19

On the Age of the Earth as Determined from Tidal Retardation

CONSIDERABLE discussions have taken place in the Geological Society and elsewhere in regard to Sir Wm. Thomson's conclusion that had the earth solidified several hundred millions of years ago, when it must have been rotating at a much greater rate than at present, its form ought to be different from what it actually is. That is to say, there ought to be a much greater difference than there is between the equatorial and polar diameters. I observe that the discussion on this point has lately been renewed at the meetings of the British Association.

Although I regard all the other arguments advanced by this eminent physicist in regard to the age of the globe, so far as I have been able to follow his reasoning, as unassailable, yet I never could agree to this conclusion deduced from tidal retardation. But, so far as I remember, I have nowhere seen stated what appears to me to be the real objection to the argument. The objection is as follows:—

As the rate of rotation decreases under tidal retardation, centrifugal force must decrease also. The consequence, therefore, is that the sea must be slowly sinking at the equator and rising at the poles (see *Phil. Mag.* for May 1868, p. 382). But denudation is also lowering the level of the land at the equator. Now the whole question concentrates itself into this, viz., will denudation lower the level of the land at the equator as rapidly as the sea sinks? This question, happily, can be answered. The method lately discovered of measuring the rate of sub-aerial denudation enables us to determine the rate at which the land at the equator is being lowered. We are enabled from

the principles of mechanics to determine the rate at which the sea is sinking at the equator. By this means it can be shown that the land is being lowered by denudation as rapidly as the sea is sinking, and that consequently, in so far as this part of the argument is concerned, we cannot infer from the present form of the earth what was its form at the time when solidification took place.

But it must be borne in mind that four years ago when Sir William read his paper on the subject before the Glasgow Geological Society, the method referred to of determining the rate of subaerial denudation was then accepted by scarcely any geologists. Taking the ideas which at that time prevailed regarding the slow rate of denudation, his conclusions were perfectly legitimate.

JAMES CROLL

Edinburgh, August 21

Neologisms

THE word *prolificness*, though not a model, is not a monster. It is a hybrid; but so is vindictive-ness. The chief objection lies in the fact of the *ic* being not the ordinary adjectival formative, but the *c* in the *fac* of *facio* = *I make*.

The true compounds of this root *change* the vowel, where, as in *satisfaction*, *malefactor*, &c., we have no change. Here, the original combination was no compound, but merely a pair of words in contact with each other.

Now, if we lay aside the hybrid forms, and use the word *abstract* with a certain amount of latitude, we get the following real or possible series of analogies:—

1. *Prolifaction*, like *satisfaction*.
2. 3. *Prolificacy* and *prolificacy*; the former like *capacity* from *capax*, the latter like *efficacy* from *efficax*.
4. *Prolificality*. This implies an adjective in *alis*, from a substantive like *beneficium*, whence *beneficial*.
5. *Prolificity* suggests *prolifex*, *prolificis*, *prolificalis*; like *pontifex*, *pontificalis*.
6. *Prolificence*. Here we must look at the same time to *adjectives* like *maleficus*, and to *participles* like *sufficiens*, *-entis*, *-entia*; the rule being that, formally, the *adjectives* have no abstract of their own; but, instead of it, the participial form in *-entia*. Hence the numerous words like *benevolence*, *grandiloquence*, &c.

To this class the form under notice belongs; and it will, probably, be admitted that *prolificence*, along with its predecessor *prolificity*, is the least exceptionable, of the list.

Prolificity is the best abstract: *prolificence*, perhaps, the better word. None of them, however, are forms which need only be known to be adopted. There is something to demur to in all them. What this is would require a longer discussion than is here practicable.

Of the present short notice the result is that it is easier to either impugn or to excuse such a word as *prolificness* than to find a substitute for it.

R. G. LATHAM

NOTES

WE are glad to be able to state that Her Majesty's Government has been pleased to accede to the request of the British Association with respect to the proposed Eclipse Expedition. We may therefore hope for a most important series of observations along a line extending from the Neigherry Hills in India to Cape York in Australia. The observation in India will be entrusted to Mr. Pogson, Colonel Tennant, and Captain Herschel. Mr. Lockyer has been requested to observe in Ceylon. The observing stations in Java will be occupied by the Dutch Government, and possibly also by M. Janssen, while a strong expedition has been formed from Sydney and Melbourne. The necessary instruments will be sent out to Australia by the next mail, and those for India will follow shortly. As before, the Government not only help in money but in transport, camping, and the like. The handsome way in which the Government has at once responded to this appeal justifies all we have said regarding its good intentions towards science when the requirements of science are properly represented by responsible bodies. We may add that the Government have also agreed to undertake photographic observations of the approaching Transit of Venus.

OUR thanks are due to the *Times* for the article (reprinted in another column) in which it exposed the injustice which Mr. Cardwell attempted to perpetrate in the case of Prof. Sylvester's retirement. Prof. Sylvester being only a scientific man, was, of course, fair game for a placeman, but it is none the less amusing to see how the whole pleading of "precedent" and the regulations of the service was allowed to go for nothing the moment there was a question of a hostile vote, thus showing the injustice of Mr. Cardwell's appeal to justice. An Account-General with a taste for income-tax, to judge from the amount of retirement awarded in a recent notorious case, is a much more valuable public servant in the present most satisfactory condition of army matters than a professor of European reputation, who is emphatically the man to infuse that scientific method into our officers of which they are so much in need.

THE introductory addresses at the winter session of the London Medical Schools, which commences on the 2nd of October, will be delivered by the following gentlemen:—At Charing Cross Hospital, by Dr. T. H. Green; Guy's Hospital, by Dr. Oldham; King's College, by Dr. Rutherford; London Hospital, by Dr. W. J. Little; the Middlesex Hospital, by Dr. John Murray; St. George's Hospital, by Dr. John Clarke; St. Mary's Hospital, by Dr. Alfred Meadows; St. Thomas's Hospital, by Mr. Le Gros Clark; Westminster Hospital, by Dr. Basham. No introductory address will be given at St. Bartholomew's Hospital, The Lecturer at University College has not yet been appointed.

THE British Archaeological Association has been holding its annual sitting at Weymouth. On Monday night, after the return of the congress from their tour of inspection in the villages of Preston and Osmington, the inaugural dinner took place at the Royal Hotel, under the presidency of Sir William Medlicott, Bart. On Tuesday the members and friends of the Association visited Maiden Castle, an immense earthwork fortification three miles from Dorchester, which was described by the Rev. Mr. Barnes. At the evening meeting of the Association the following papers were read: "On the Origin and Titling of English Laws;" "Report on the Municipal Archives of Dorset;" and on "The Cerne Giant."

WE learn from a correspondent in New Zealand that footprints of the Moa have recently been detected in a new district in the province of Auckland. The locality is at the mouth of the Waikenei Creek, near the settlement of Gisborne, Poverty Bay, near the Taruhera River. The slabs in which the impressions were found were about five feet below a deposit of silt and alluvium of different kinds which had been washed away by the action of the water, leaving the stone in which the footprints were found visible, very plainly indented and following each other in succession. On either side of this track were dents here and there, as though made by the bird's short beak in picking up food as he walked—the closeness of the stride favouring this belief. Hard by this spot Mr. Worgan picked up an old stone hatchet, which, from the signs of traces it bears, is doubtless as ancient as the tracks of the Moa. Casts of these footprints have been presented to the museum of the Auckland Institute. The length of the footmark from the heel to the tip of the centre toe was seven and seven-eighths inches; from the heel to the tips of the inner and outer toes, six inches; the distance of tips of the outer and inner toes was seven inches; the length of the stride was twenty inches from heel to heel, and there were eight impressions altogether.

THE account of the whirlwind at Chilton, Buckinghamshire, on July 30, is worth careful study. The correspondent, J. B., who writes to the *Times*, sends the following facts:—"The storm began about five o'clock in the morning, accompanied by terrific thunder, large hailstones, and a most violent and terrific wind. The piece of country devastated by this wind is about