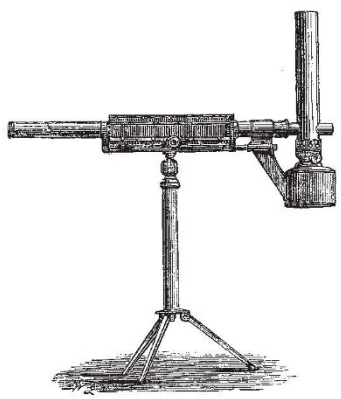
advantages in supplementing the photographic records by direct eye-observations. I regret that the very few occasions on which it has been possible to observe the sun has put it out of my power to make further experiments in these and some other obvious directions.

P.S.—[I have Capt. Abney's permission to add the following letter this day received from him;—"A careful examination of your series of sun-photographs, taken with absorbing media, convinces me that your claim to having secured photographs of the corona with an uneclipsed sun is fully established. A comparison of your photographs with those obtained during the eclipse which took place in May last, shows not only that the general features are the same, but also that details, such as rifts and streamers, have the same position and form. If in your case, the coronal appearances be due to instrumental causes, I take it that the eclipse photographs are equally untrustworthy, and that my lens and your reflector have the same optical defects. I think that evidence by means of photography of the existence of a corona at all is as clearly shown in the one case as in the other."—*December* 15, 1882.]

A WEDGE AND DIAPHRAGM PHOTOMETER

A NEW photometer, shown in perspective in the figure, has lately been constructed by Mr. Sabine. The stand supports a straight horizontal tube, at one end of which is a paraffin lamp, and at the other an eyepiece. The middle portion of the tube is cut away, and has, slipped over it, a collar to which a frame is attached, carrying a wedge of neutral-tinted glass, adjustable by means of a rack and pinion. Inside the collar is fixed a transverse disc of ground opal glass, which the paraffin lamp illuminates to a definite degree. This disc constitutes the field of comparison, the illumination of which is



adjustable by means of a series of diaphragms of known aperture at the end near to the paraffin lamp. At the side, between the wedge and the collar which carries it, is a narrow pane of ground opal glass, just behind which a small mirror is fixed at an angle of 45° to the axis of the tube. This mirror is supported from the centre of the transverse opal disc in such a way, that the support is hidden from the observer by the mirror itself, an arrangement which insures the apparent juxtaposition of the illuminated surfaces which have to be compared. The light to be measured is placed on the right-hand side of the photometer; and the collar is turned so that the light falls normally upon the face of the wedge, passes through the wedge, through the pane of opal glass, and is incident upon the mirror, which reflects a portion of it to the eye of the observer. The wedge is then shifted, if necessary, to interpose a greater or less thickness of absorbing medium, until a balance is obtained, that is until the apparent illumination of the mirror is equal to that of the field of comparison, in the middle of which it is seen. If the range of the wedge is insufficient to admit of this, the degree of illumination of the field is altered, by means of the diaphragms, and the wedge is then adjusted.

The employment of glass wedges for photometric comparisons is not new, having been already used by both Xavier de Maistre and Quetelet; but no practical photometer based upon this method has hitherto been constructed. The employment of diaphragms for extending the range of the wedge is found to work well and to enable the operator to adjust the illumination of the field with exactitude, the bright part of the paraffin flame being of course, kept opposite to, and so as to well cover the diaphragm aperture. A table is constructed giving for each position of the wedge and for each diaphragm, the value, in standard candles, of any light placed at a distance of one metre from the instrument; and if the light be placed at any other distance, the number in the table has simply to be multiplied by the square of the actual distance in metres. For ascertaining approximately the amount of light which passes through any given coloured glass, for example, orange glass, the eyepiece is furnished with a rotary disc containing small panes of white and different coloured glasses, either of which can be interposed at pleasure.

This photometer is being made by Messrs. Elliott Bros., in two forms, one for use as a portable photometer, as shown in the figure, and the other on a more solid stand, for laboratory purposes.

ON THE OCCURRENCE OF GREAT TIDES SINCE THE COMMENCEMENT OF THE GEOLOGICAL EPOCH[1]

IT will I daresay be within the recollection of many of those who are now present that I was honoured by the invitation to deliver the opening lecture in this hall last year. In response to that invitation I addressed to you a discourse which I ventured to call "A Glimpse through the Corridors of Time." Accounts of it have appeared in very many quarters, both at home and abroad. I am myself responsible for the account which appeared in the columns of NATURE, as well as for the pamphlet form in which the lecture has since been issued. The chief reason why I now recur to the subject remains to be stated. Among the various comments which have been made upon that address, some are by no means favourable to the views I ventured to put forward, and they have been the theme of considerable discussion. Up to the present I have not made any reply to the criticisms which have appeared; I postponed doing so until a suitable opportunity should have arisen for a review of the whole subject. Your kindness in inviting me once again to address this great Institute has afforded such an opportunity, and with your permission I propose to preface the subject of my lecture this evening by a reply to those critics who have honoured me with their attention.

Let me recall to you very briefly the subject of that lecture, so as to enunciate clearly the point as to which an issue has been raised. You will perhaps recollect that the lecture treated principally of the tidal relations between the earth and the moon, of the influence of the tides during ages past, and of the future which awaits the earth-moon system during ages to come. I pointed out that at the present moment the orbit of the moon must be gradually growing in size, that this gradual increase of the distance from the earth to the moon is essentially non-periodic, and thus is totally different to the ordinary lunar irregularities which are recognised in rigid-body astronomy. As a consequence of this incessant growth in the moon's distance we see that in past ages the moon must have been appreciably nearer to the earth than it is

[1] Extract from a lecture delivered at the Midland Institute, Birmingham, on November 20, 1882, by Prof. Robert S. Ball, LL.D., F.R.S. Communicated by the Author.

at present, and that if we look far enough back we must inevitably come to an epoch when the moon would seem to have been quite close to the earth; indeed looking earlier still we are not without reasons for believing that in primeval times the earth and the moon formed but a single body.

Wondrous as this narrative may seem, yet on a due consideration of the mathematical evidence in its favour we are constrained to admit that it must be substantially correct. Unless some notable agency at present unknown to us has intervened in past time, the course of events must have run along the lines we have indicated. We make but little pretence to give the date when the moon seems to have commenced its independent existence, nor to indicate the chronology of the epochs when its distance increased by one thousand miles after another. All that seems certain is that the events we are at present discussing must have occurred millions, many millions of years before man placed his foot on this planet.

The cause of these mighty series of changes is still in hourly operation. The ebb and flow of the tides around our coasts is only the survival of greater tides with which in earlier days the ocean must have throbbed. The earlier we look back the mightier must have been the daily ebb and flow. I even invited you to look back to an excessively remote epoch when the moon was only at a fraction of its present distance, and when the daily rise and fall, instead of being counted in tens of feet, must have been reckoned by hundreds. Even up to this point there has been little or no controversy, there can be none. I do not know that any one has attempted to deny that the earth must once have experienced these mighty tides either in the actual body of the earth, or in the ocean on its surface. The controversy has arisen on the question as to whether these great tides had subsided before the commencement of the geological epoch, or whether they were contemporaneous therewith.

In my lecture in this hall last year, I made the suggestion that the reign of the mighty tides did perhaps extend into the commencement of the geological epoch. I further ventured to suggest that these great tides had left their traces on the solid crust of the earth. I quoted eminent geological authority to show that the rocks at the base of our stratified system are of the most stupendous volume and thickness. It has always been a difficulty to determine how the present geological agents could have manufactured so mighty a mass, and I appealed to the great tides as a grinding engine competent to aid in this work. At this point issue has been taken, and it is now my duty to review the arguments which have been adduced as bearing on this question. But let me here make a single remark which disposes of many of the objections that have been raised. Several of my critics do not seem to have observed that I postulated the mighty tides for the manufacture of the earliest primæval rocks, and *for these alone*. "Take, for example," I said (p. 25), "that earliest and most interesting epoch when life perhaps commenced on the earth, and when stratified rocks were deposited five or ten miles thick, which seem to have contained no living form higher than the eozoon, if even that were an organised being." Again and again I stated that I merely referred to these primitive strata. Yet this is a point that many of my critics have ignored. They have been at pains to prove that colossal tides did not exist in the comparatively modern geological epochs, and many interesting facts have been adduced. But such considerations have only an infinitesimal bearing on the position I adopted. Even the coal-measures are a modern formation when compared with the primeval rocks, for the manufacture of which I suggested the mighty tides.

The controversy as to the great tides has principally ranged around the question as to whether the primitive rocks present any indications of great tidal action. I am not a practical geologist, and am most anxious to obtain the views of those that are. Now these opinions are to be had. They are to be found in the correspondence in NATURE at the commencement of this year; yet there is, as might have been anticipated, considerable differences of opinion. I first refer to Prof. Hull's letter (NATURE, vol. xxv. p. 177), and find that this most competent authority adduces direct evidence of tremendous denudation in the Palæozoic ages, such as might have been produced by mighty tides. On the other hand, Prof. Newberry (vol. xxv. p. 357) says that there is no direct evidence whatever to show that the denuding agencies were greater in former times than now. In the following number of NATURE we have letters from Dr. Callaway and Mr. Hale (p. 385) to show that Prof. Newberry's conclusions are not necessarily valid. Mr. S. V. Wood and Mr. J. Vincent Elsden bring forward facts which go to support Prof. Newberry's view, while Dr. Callaway, though carefully declining to commit himself to the high tides, controverts Mr. Wood and Mr. Elsden (p. 409). Now all these gentlemen speak with special knowledge, but it is not easy to deduce from this correspondence as to which side the balance of skilled opinion really inclines. It would almost seem as if a very fundamental point had escaped attention. Prof. Geikie, in his great work just published, tells us that these great tides could not have existed in geological times, because, if they had done so they must have left certain traces, and we do not find these traces. The fundamental question is, What traces of great tides ought we to expect to find if those great tides had really existed? It would seem that unless this question be first answered, it is impossible to dispose of the question with the brevity which Prof. Geikie has adopted. I apprehend it cannot be doubted that by the great tides the materials of stratified rocks would be rapidly formed, and that in suitable localities these materials would be deposited to form rocks. I can see no necessary difference between a ton of mud ground up by colossal agents in former days, and a ton of mud ground up by the more prosaic agents of modern days. But for each ton of mud now made there would then have been a great many tons. The strata would thus have grown more rapidly in early times, and thus the exceptional thickness of the earliest stratified rocks could be accounted for. It seems useless to assert that vestiges of the great tides do not exist, unless we can form some idea of the sort of vestiges that should be expected if the great tides had existed. I believed at the time I gave my lecture, and I believe still, that we do see vestiges of vast primæval tides. I can even count these vestiges. They are five, or perhaps ten in number; they are the five or ten miles of vertical thickness of stratified rocks which were deposited at the bottom of the ocean during the earliest stages of the geological epoch.

I have derived so much pleasure and so much instruction from the study of Mr. G. H. Darwin's writings, that on this ground alone I would be reluctant to have any difference of opinion with him. Indeed, seeing that the earth-moon history is one which he has made peculiarly his own, and illuminated by discoveries which I believe to be the most important contributions made to physical astronomy in modern times it would seem presumption in me to venture to differ from him. Mr. Darwin, writing in NATURE, vol xxv. p. 213, has asked me to reconsider the views I had set forth as to the probability of the great tides being contemporaneous with geological phenomena. Mr. Darwin shows that at the time when the great tides supposed in my calculation existed, the earth must have been spinning round once in seven hours, and that this would involve trade-winds of $3\frac{2}{3}$ times their present velocity and vertical storms of prodigious violence, and then he adds:—

"Now if this state of things existed in geological history, we should expect to find the earlier sedimentary

rocks of much coarser grain than the modern ones. But I am not aware that this is the case. Again, to understand such blasts, the earliest trees should have trunks of enormous thickness and their leaves must have been very tough, or they would have been torn to shreds. There seems to be no reason to suppose that the trees of the Carboniferous period present marked peculiarities in these respects."

"It is on these grounds that I venture to dissent from Mr. Ball in the geological interpretation to be placed on the tidal theory, and I think we must put these violent phenomena in pre-geological periods."

But is it necessarily true that the prodigious tides must have produced a coarser material as the result of their grinding than is found in the later rocks? I can imagine it to be contended that the more powerful mill would produce the finer flour, but in truth I really do not see that we have any *a priori* grounds for deciding whether the *débris* produced by mighty tides should be fine or coarse. Have we not illustrious authority for invoking our "Domestic Productions" to throw light on obscure questions removed from actual observation. Let us look at the biggest tides we know of, and see whether they are associated with fine mud or with coarse. I appeal to every one who has stood on the Clifton Suspension Bridge or walked on the Beach at Weston-super-Mare to answer this question. In both cases they will see mud of a fineness and a stickiness that is proverbial; yet that mud is washed twice every day by the mightiest tides in the British Islands. I do not say, nor do I believe, that the fineness of the mud in the Avon is the consequence of the great tides; but I think the illustration is a fair reply to an argument which says the tides in ancient days cannot have been of great size, because the mud with which those tides are associated is not coarse.

In the second place, Mr. Darwin urges against me the trees of the Carboniferous epoch, and his inference that the tremendous tides cannot have existed in the Carboniferous epoch is probably well founded. But I have not said that these tides did exist in the Carboniferous epoch. I can only again repeat that my argument supposed that the mighty tides may have existed in the times when the very earliest stratified rocks were deposited. In the course of ages, as the moon receded, so the tides gradually dwindled down until in the comparatively modern time indicated by the Carboniferous epoch, they may have been small enough to be connected with the wonderful coal vegetation.

I had, as I was bound to do, most carefully weighed the words in which I addressed you from this place last year. I was aware that the opinion I advanced would meet with opposition. This was a reason why I should consider the subject most carefully before I spoke, but it was not a reason why I should withhold the views at which I had arrived. I have again considered the matter with the results now set forth, and I have seen no reason to depart in the slightest degree from the position which I had previously adopted.

MARS[1]

THE similarity which has long been thought to exist between our own globe and the planet Mars would naturally commend itself to careful examination at the hands of such observers as possess instruments adequate to the inquiry. The shadowing of large portions of its surface with patches which easily lend themselves to the supposition of being collections of water, the occasional indistinctness of their outlines, so strongly indicative of atmospheric obscuration, the clothing of either pole with the semblance of a snowy mantle obedient in its extent to solar action, all this would bespeak of itself a critical investigation. And the challenge has been taken up from an early period, and to an extent which would probably surprise those who are unfamiliar with the subject. Already in 1873 the number of drawings collected by Dr. Terby of Louvain, than whom no man is more intimately conversant with areography, amounted to 1092, and the nine subsequent years, which have included among others the celebrated representations of Green and Schiaparelli, have greatly augmented that imposing number. We should be mistaken, however, if we were to estimate the progress of our knowledge by the multiplication of designs. In this case the ancient saying *πλέον ἥμισυ παντός* would probably express too large a proportion. The increase, if in some respects not to be regretted, brings with it additional elements of uncertainty, if not of error. Many representations might be discarded with positive advantage to the final conclusion: like numerical observations whose unworthiness is detected by their wide deviation from the mean of the rest, the result is all the surer for their exclusion. An unpleasant experience proves that the most careful observer is not always the most successful draughtsman, nor in such matters is zeal any pledge of excellence. Comparison of the results obtained by different astronomers leads to the conclusion that, after due allowance has been made for instrumental and atmospheric differences, all men do not see alike, or interpret in the same way what they see, or transfer the image to paper with equal success. Here it is that photography, though not exempt from defects and hindrances of its own, is now beginning to render invaluable aid. But such an object as the disc of Mars would not lend itself very readily at present to the camera, and the pencil and the brush must do the best they can till some further advance is made to supersede them.

But however improved may be our future representations, and whatever may be the result—on every supposition most interesting—of the keen scrutiny that is in store for the next opposition of the planet, it would undoubtedly be an injudicious course to discard as unworthy of study and comparison the delineations of earlier days. Less valuable, if standing alone, they may attain considerable importance in the elucidation of some otherwise unexplained difficulty; and evidence which, unsupported, might be of little weight, may acquire especial consequence from its collateral bearing on more direct testimony. The comparatively rude and defective sketches of a long-passed era, contained in the publication before us, executed in a spirit of unwearied industry and unimpeachable fidelity, but under the influence of a mistaken impression, form a striking illustration of the previous remarks.

The history of the "Areographische Beiträge" is connected with a very lamentable occurrence in the life of the worthy old Hanoverian observer, Dr. Johann Hieronymus Schröter. He had long been settled in a Government office at Lilienthal, not far from Bremen, where his almost innumerable observations on sun, moon, and planets (with stars he did little) had been carried on with reflectors of various sizes—two by Sir W. Herschel of 4 and 7 feet focal length, others by Schrader, of Kiel, of 7, 11, 15, and 27 (26 English) feet, and a 4-inch object-glass by Dollond, equatorially mounted. His passion for observation would never have allowed so interesting an object as Mars to escape him, and accordingly we find that between the years 1785 and 1803 he had accumulated 217 designs, with a corresponding description marked by all the minute preciseness of detail and inference which characterise his other labours. The work had been promised for publication at Easter, 1812, but had been somehow delayed, when an event occurred on the night of April 20, 1813, in connection with the occu-

[1] "Areographische Beiträge zur genauern Kenntniss und Beurtheilung des Planeten Mars." Von Dr. J. H. Schroeter; herausgegeben von H. G van de Sande Bakhuyzen, Director der Leidener Sternwarte. 8vo, 447 pp., with Atlas. Leiden: E. J. Brill.