This theory being accepted, for an explanation of the aberration in question we have only to suppose some slight physical alteration in the contents of the cochlea, which would cause the sound wave to strike or affect the wrong portion of the lamina spiralis, and thus a false impression would be carried to the brain. URBAN PRITCHARD

Now attention is drawn to the above allow me to give another experience.

On two separate occasions while playing the English concertina, and more particularly when single notes or simple chords were struck, I noticed that each was followed by a loud and distinct note an octave lower which appeared to be that of its fundamental tone. The musical tones of the voice of any person addressing me, also, had their deeper reverberations in a similar manner, these being numerous and of rapid succession; the confusion arising was very like that which is heard in a hall unsuitably constructed for sound.

The nuisance, for such it amounted to, I was troubled with for a couple of days each visitation, the abnormal state of hearing being peculiar to the left ear only. JOHN HARMER

Wick, near Arundel

Intellect in Brutes

THE following case will perhaps interest those who believe that the reasoning faculty in man and animals differs in degree only, and is essentially the same in kind. Some years ago a plumber told me that he had, on several occasions, been called in to examine into the cause of leakage of water-pipes under the flooring of houses, and had found that the rats had gnawed a hole in the leaden pipe to obtain water, and that great numbers of them had made it a common drinking-place, as evidenced by the quantity of dung lying about. The plumber brought me a piece of leaden pipe, about ¾ inch in diameter and ⅛ inch in thickness, penetrated in two places, taken by himself from a house on Haverstock Hill. There are the marks of the incisors on the lead, as clear as an engraving; and a few hairs and two or three of the rats' vibrissæ have been pinched into the metal in the act of gnawing it. This crucial proof of brute intelligence—a rat will not drink foul water—interested me so much, that I ventured to send an account of it to Dr. Chas. Darwin, asking his opinion on the means by which the rats ascertained the presence of water in the pipe. To this he replied: "I cannot doubt about animals reasoning in a practical fashion. The case of rats is very curious. Do not they hear the water trickling?" It may be conceded that this explanation is the most probable, and if it be the true one we have an example of an animal using his senses to obtain the data for a process of reasoning, leading to conclusions about which he is so certain that he will go to the trouble of cutting through a considerable thickness of lead. Obviously man could do *no more* under the same conditions. ARTHUR NICOLS

OUR ASTRONOMICAL COLUMN

THE COMPANION OF ALGOL.—There are grounds for suspecting that the light of the small star about 80″ distant from Algol in the S.P. quadrant is also variable. Schröter in his letter to Bode, wherein he first drew attention to this object, mentions that he detected it with a 7-feet reflector on October 12, 1787, and although small it was distinctly seen. Soon afterwards he estimated its distance from Algol at 1′ 30″. On April 9, 1788, the star was not to be found, and he therefore concluded that it must be variable. In 1792, when he was in possession of a 13-feet reflector, which he describes as the most powerful instrument then available in Germany, he re-examined the vicinity of Algol, and on March 9 saw the companion much brighter than before, and compares its distinctness in the larger telescope with its faintness in the smaller one with which he had discovered it. But on April 5, in a state of atmosphere at least as favourable as on March 9, with the same instrument and magnifying power, not the slightest trace of the companion could be perceived; on increasing the power to 370, with the utmost straining of the eye, the faintest glimmering was now and then suspected in its position. Schröter then, in this second communication to Bode, expresses himself more confidently as to the variability of the small star.

In the early part of the year 1874 the writer of these lines made several ineffectual attempts to observe the companion, using various powers on a 7-inch refractor; though the skies were favourable enough, nothing could be glimpsed in its place. It was not therefore without surprise that upon re-examining the vicinity under similar conditions on September 9 of the same year, the companion was caught at once, and seen with great distinctness. It was measured with Mr. J. G. Barclay's 10-inch refractor at Leyton, by Mr. Talmage, on October 2 following, when the angle was found to be 194°·4 and the distance 79″·02; the magnitude was estimated 11·12. An observation by Smyth in 1835 is recorded, but his distance is much too small; it is not stated whether he found the companion himself or whether his knowledge of its existence was due to Schröter's communications to Bode. It does not occur amongst the objects in the "Bedford Cycle," which were re-measured by Secchi.

While upon the subject of variable stars we may just mention that ι Andromedæ, to which attention is directed in the last number of the *Monthly Notices* of the Royal Astronomical Society as "a new variable star," is no novelty: we referred to the star as almost certainly entitled to insertion in the catalogues of such objects, four years since (NATURE, vol. xi. p. 308).

"A MISSING STAR."—From a letter addressed by Prof. C. H. F. Peters, Director of the Observatory, Clinton, New York, to the Superintendent of the Naval Observatory, Washington, which Admiral Rodgers has communicated to the *Astronomische Nachrichten* (No. 2240), it appears that he has strangely misinterpreted a note with the above heading, which was lately printed in this column. We referred to an object observed at Washington, with *Hygeia* in 1850, and afterwards sought for at that observatory and elsewhere on the assumption that it might possibly have been a trans-Neptunian planet, and in view of the failure of a careful search on this hypothesis, we remarked: "the only likely explanation appears to be that there was a variable star in this position, and that the observations in right ascension were affected with greater error than might be expected, considering that on two of the days of observation several comparisons were made." Prof. Peters, however, explains the difficulty by referring several transits to the first instead of to the second wire of the movable plate of the micrometer employed, in which case the star is identified with Lalande 36613, and Prof. Hall has found, on examining the original observing-books, that Mr. Ferguson had altered several correct observations to correspond with erroneous ones, and Admiral Rodgers accepts the explanation as satisfactory. But Prof. Peters is alarmed about the matter now that NATURE "stirs it up again," and writes to the Superintendent of the Washington Observatory "in order that nobody thereby might be induced to spend months and years upon a renewed search," and to "stop any further perpetuation of the credence, that a trans-Neptunian planet is revealed by the Washington Observations." It will be seen that our suggestion was that a variable star might exist in the observed position, and was in no way connected with a renewed search for a trans-Neptunian planet. Prof. Peters must entertain rather odd notions as to the probable knowledge of his astronomical *confrères* respecting the contents of the ecliptical region of the sky, if he believes that any one would be induced, by remarks that we might offer, to undertake in these days a search for a distant planet close to the ecliptic amongst stars of the *ninth* magnitude!

COMET 1871 V.—Dr. B. A. Gould, with his usual energy, has secured an excellent series of post-perihelion places of the comet discovered by Dr. Tempel on November 3, 1871, which in a fortnight's time sank below the European horizon. The discussion of these observations in conjunction with those made in the northern hemisphere, will lead to a much more precise knowledge of the orbit than we have at present.

OLBERS' COMET OF 1815.—In a recent note upon this comet it should have been stated that, acting upon the wish expressed by Olbers at the time, Triesnecker printed his observed differences of right ascension and declination between the comet and comparison-stars in *Zeitschrift für Astronomie*, vol. ii. The Vienna observations, therefore, admit of a new reduction, in addition to those previously named.

DIURNAL OSCILLATIONS OF THE BAROMETER

IN the "Meteorological Notes" which appeared in NATURE, vol. xviii. p. 198, some interesting results are referred to, which show marked differences in the diurnal variations of the barometer at places quite near to each other, as Greenwich, Kew, Oxford. It is remarked especially that the forenoon maximum in the months of May to July occurs near 9 A.M. at Greenwich, and near 8 A.M. at Kew; while at Falmouth and Valentia it is delayed to 11 A.M., or noon, and occurs in June as late as 2 P.M. at Helder.

Having made several investigations relatively to these questions (which I have not been able to publish as yet in detail), I think it may not be without advantage to give at present conclusions relating to the results above noticed.

It is obvious that it is of the highest importance with relation to the research as to the cause or causes of the remarkable semi-diurnal oscillations of the barometer, that we should have only real variations of atmospheric pressure to deal with, and not instrumental irregularities; and that, if there is any part of the mean diurnal variations which is due to local causes, we should be able to separate that part from any other which may be due to general or cosmic causes.

When it is remembered that the range of the mean diurnal variation with us is from two to three hundredths of an inch of mercury, and that the epochs of maximum or minimum may be shifted an hour by a difference of one or two thousandths of an inch, it will be seen how essential it is that the instruments, the observations, and the corrections shall be the best, in order to be sure that we have real variations of atmospheric pressure before us.

In order to obtain the best possible results, my investigations have been limited to observations made in first-class observatories with standard instruments. From observations made during several years at Makerstoun, Dublin, Greenwich, and Brussels, I have sought by the harmonic analysis the functions of sines which represent them most accurately. I give here the equations for the means of the three months in question—May, June, and July. The variation, v, is in ten-thousandths of an inch of mercury; the origin for each of the four stations M, D, G, and B, is mean midnight ($\theta = 0$):—

$$\text{M}, \; v = 56 \sin(\theta + 355^\circ) + 68 \sin(2\theta + 143^\circ) + 21 \sin(3\theta + 171^\circ)$$
$$\text{D}, \; v = 51 \sin(\theta + 358^\circ) + 72 \sin(2\theta + 144^\circ) + 25 \sin(3\theta + 153^\circ)$$
$$\text{G}, \; v = 63 \sin(\theta + 346^\circ) + 88 \sin(2\theta + 143^\circ) + 25 \sin(3\theta + 154^\circ)$$
$$\text{B}, \; v = 43 \sin(\theta + 354^\circ) + 92 \sin(2\theta + 140^\circ) + 24 \sin(3\theta + 170^\circ)$$

The terms on the right of each equation represent the oscillations, whose superposition completes the whole diurnal variation. We find—

From the 1st term that the epochs of the maximum and minimum were the same within a few minutes at M, D, and B (as shown by the arguments 355°, 358°, and 354°), differing at Greenwich from the others by about 40m.

From the 2nd term, that of the semi-diurnal oscillation, that the epochs were the same at all the stations within a few minutes.

From the 3rd term, that they agreed at D and G and at M and B, those for the former being about 23m. different from those for the latter.

When we consider the coefficients of the different terms, which represent half the ranges of the oscillations, slight differences are found for the 1st and 3rd terms; for the 2nd the *range diminishes regularly as the latitude increases* at the rate of 0·00101 inch for each degree of latitude.

The exact agreement in the epochs of maxima and minima and the regularity of the variation of range with latitude in the semi-diurnal oscillation show that this oscillation obeys a general law. Dr. Lamont has supposed that the 1st term, or single oscillation, is due to variation of temperature; this, I believe, is not the case. When we compare the terms for different *seasons* of the year, we find that for the *same place* the epochs of maximum and minimum may vary twelve hours in the single oscillation, while the epochs deduced from the same term for the temperature variations do not differ one hour. Not only so, I have found on the South Indian Ghats that the epochs deduced from the 1st term of the barometric equations vary seven hours in ascending 6,000 feet; while those shown by the 2nd term are absolutely constant.

For all these reasons I conclude that the semi-diurnal oscillation of the atmospheric pressure is due to a cosmic cause, independent of local influences, while the single diurnal oscillation shows that part of the solar action which is modified by atmospheric conditions yet to be determined. The results for the four stations just given are a few links in a long chain of facts which tend to prove that the semi-diurnal oscillation of the barometer is due to an action of the sun, which is repeated equally, twice in each day, like the solar oceanic tide.

It will be seen, I think, from the results obtained from the Brussels, Greenwich, Dublin, and Makerstoun observations that the differences noticed at the beginning of this article cannot be allowed to enter as data into the domain of meteorology without much greater study of all the circumstances on which they depend. The facts of atmospheric variations are very difficult of explanation, but if we begin to admit results which may be purely instrumental among these facts explanation will become impossible.

It is a fact that the true temperature of the mercurial columns has not always been obtained, and when we have to discuss observations with self-registering instruments, many sources of error, including those of temperature on the apparatus itself, have to be cared for.[1] At stations near the sea, such as Helder, Valentia, and Falmouth, we have also to remember that in the varying height of the partial base of the atmosphere, through the *solar* oceanic tide, there is a real cause of diurnal barometric oscillation whose amount and epochs should be ascertained and deducted before exact comparisons can be made with observations inland. At the same time I would remark that of the stations here considered Dublin is near the sea, while the others are more or less distant from it. JOHN ALLAN BROUN

MAROCCO AND THE ATLAS[2]

THE expedition of which an account is given in this most interesting volume was undertaken by Sir Joseph Hooker and Mr. Ball in the spring of 1871, and lasted a little over two months. Many causes com-

[1] The observations here studied at the four observatories are all made by the eye.

[2] "Journal of a Tour in Marocco and the Great Atlas." By Joseph Dalton Hooker, K.C.S.I., C.B., Pres. R.S., Director of the Royal Gardens, Kew; and John Ball, F.R.S., M.R.I.A. With an Appendix, including a Sketch of the Geology of Marocco, by George Maw, F.L.S. (London: Macmillan and Co., 1878.)