less definite than during the last opposition. The spot was estimated to be precisely central at 14h. 46m., and this is 14'3 minutes in advance of the time given in Mr. Marth's ephemeris (*Monthly Notices*, May, 1892). The motion of the spot has therefore shown a considerable acceleration during recent months. Between August 2, 1891, and February 2, 1892, the mean rotation period was 9h. 55m. 42'23, but between February 2 and August 19, 1892, it was only 9h. 55m. 39'3s. This is a difference of 3 seconds, and it clearly proves that the motion of the spot is affected by some remarkable variations. A very decided retardation set in at the end of August, 1891, and continued to operate until February, 1892, but since that time the spot has exhibited an expected celerity of movement.

A large number of interesting details are now visible on the planet, but the bright equatorial spots which were so conspicuous about twelve years ago have virtually disappeared. During my observation on August 19 I saw the third satellite projected on the southern limb of the planet as a bright spot.

Bristol, August 20.

W. F. DENNING.

Numbering the Hours of the Day.

WITH reference to Dr. Mill's recent interesting article on "Time Standards of Europe," I beg leave to emphatically take exception to the remark on p. 176, that "the system of numbering the hours of the day from 0 to 24 has failed to hold the popular fancy," as I maintain that the public has had no opportunity of testing the convenience of such a reckoning. The ordinary standard used in this country being railway time, so long as *Bradshaw* is printed on the old system of numbering the hours only up to 12, it is out of the question to expect the public to adopt any other. Any number of clock-faces numbered otherwise, either at Greenwich or all over the country, would not lead people to adopt the new system; the railway tables must first be altered, and as *Bradshaw* is compiled from the tables of separate companies, probably it would be necessary to approach the numerous railway companies with a view to their considering the subject and deciding upon a common plan. They would have to discuss not only the question of printing the time-tables on the new plan, but also whether it would be necessary, as well, to alter all the clock-faces at every station. I am given to understand that one railway company (in the Isle of Wight) for some time printed its tables—if it does not still—with the afternoon hours numbered from 12 to 23; though its example, because, I presume, it was of a small and unimportant line, was not copied by any other company.

A further difficulty in the way of the public making any change is that the highest authoritative book on time matters—the *Nautical Almanac*, compiled and published by the British Government—still reckons the hours from noon. As a civil day commencing at noon is not suggested, would not the railway authorities have a ready objection to urge, and decline to alter their time-tables while time bearing the name of the national observatory, and used in the national ephemeris, were reckoned on a different plan from that which it is suggested they should adopt?

As regards the existing mode of reckoning time, the art of the printer is sometimes called in to show at once, without having to refer to the tops of columns or the sides of pages, which half of the day is meant; thus in the Midland Railway's, and part of the North-Eastern Railway's time-tables thick and thin type show this, and in some other tables a short vertical line appears between the figures of the hours and those of the minutes.

If the lists of places (p. 176) at which the time of the national standard is kept and not kept are intended to be complete, may I ask if Jersey has yet adopted Greenwich time? It had not up to 1887, although Guernsey had done so.

Sunderland, August 1.

T. W. BACKHOUSE.

Propagation of Magnetic Impulses along a Bar of Iron.

IF at one end of an iron bar an alternating current be passed through a coil, will there be a wave propagation of magnetism along the bar?

Mr. Trouton (*NATURE*, vol. xlv., p. 42) hoped to find an answer to this question by searching after nodes, when two coils are placed one at each end of the bar, and the same alternating current is passed through both coils, or when one coil is

employed on a closed iron ring. The search was conducted by a secondary coil connected with a telephone. Mr. Trouton found some places of minimum, but ascertained that these were not the nodes sought for.

Mr. Trowbridge (*Phil. Mag.* [5] 33, p. 374, 1892) made use of his phasemeter; two secondary coils, each connected with a telephone, could slide along the ring on which two large primary coils were placed. Two mirrors on the diaphragms of the telephones permitted to study change of phase by Lissajous's figures.

From his experiments, Mr. Trowbridge infers that there was no wave-motion along the iron ring; he believes that the propagation of magnetic disturbances produced by forced oscillations on iron bars is closely analogous to the propagation of heat over these bars.

Though I agree with Mr. Trowbridge in his conclusions, it seems to me that neither the experiments of Mr. Trouton nor those of Mr. Trowbridge could give any but embroiled results.

In collaboration with Mr. N. G. van Huffel, *Phil. Nat. Cand.* at this University, I have made some preliminary experiments on this question. Firstly, it became obvious that care must be taken against direct induction of the primary coils on the secondary. Only when the secondary coil which was connected with the telephone was embedded in a mass of copper everywhere 2.5 cm. deep, with a narrowly closing aperture for the iron bar, these direct effects were eliminated if the distance of the secondary from the primary coil were not too small. I have found no indication that similar precautions were taken in the quoted experiments.

But secondly, the telephone proved to be not the proper instrument for conducting the research. In most cases the variation of the magnetic intensity goes too slow to be perceptible by the telephone.

At one end of an iron bar (5 meters long, 1.5 cm. diameter), a primary coil A was placed; along the bar the secondary coil B (within a mass of copper, and connected with a telephone) could slide. A magnetic impulse was given by sending a current through A. If B were near A, a single tick was heard in the telephone; if the distance between B and A were greater, nothing was perceived in the telephone. However, if the circuit composed of B and the telephone were interrupted by a tuning-fork P, a sound was heard during somewhat one second and a half of the same pitch as the tone of the tuning-fork P, every time the primary current was sent through A, or broken. The intensity of this sound diminished as the distance of B from A increased, but was still perceptible wherever B was placed on the bar.

Thereupon the primary circuit to which belongs the primary coil A was interrupted by a tuning-fork Q. If no tuning-fork were in the circuit of B and the telephone, then only when B was near A a sound was heard of the pitch of Q; at a greater distance nothing was heard in the telephone. But if now a tuning-fork P interrupted the circuit of B and the telephone, a continuous sound was heard, even at much greater distances of B from A. When the pitch of P differed slightly from that of Q, beats were perceived, also when the frequency of P was nearly half that of Q. The interpretation of these facts is so apparent, that I need not dwell upon it. But these facts illustrate the principle on which Van Rysseberghe based his method of simultaneously sending telegraphic and telephonic signals along the same line.

I believe that the propagation of magnetic impulses along a bar of iron has to be studied in an entirely different way. We intend to make an attempt in this direction ere long.

Utrecht, July, 1892.

V. A. JULIUS.

"The Limits of Animal Intelligence."

CLOSELY in connection with an observation I made the other day with respect to an argument of Prof. Pearson's, I should like to say a few words about a paper read by Prof. Lloyd Morgan before the International Congress of Experimental Psychology, on "The limits of animal intelligence." The first proposition he advanced, "That human psychology is the only key to animal psychology," and the deductions he subsequently drew, all implied that our knowledge of human psychology differed not only in degree, but in kind, from our knowledge of that of animals. Of course it is true that my knowledge of *my own* psychology does differ in kind from my knowledge of that

of animals, but it differs in exactly the same way from my knowledge of that of all other men. If in no case is "an animal activity to be interpreted as the outcome of the exercise of a higher psychical faculty, if it can be fairly interpreted as the outcome of one which stands lower on the psychological scale," the same rule should be applied to the interpretation of human activities, for the only reason for distinguishing between human and animal psychology is that their activities do, as a matter of fact, differ. Human beings are of course distinguished from animals in other ways; in the structure of their limbs, for example; but there is no *a priori* ground for inferring from such differences any, and certainly not any particular, difference in psychological powers. And so far from its being permissible to infer such a difference from greater or less complexity of brain-structure, it is only because animals which when alive displayed great activities proved, on dissection, to have possessed complex brain structure, that we can infer any connection whatever between the two phenomena. As no man has ever dissected his own brain he cannot say that any particular structure is associated with those psychological powers of which alone he has any more direct knowledge. If, for example, I say "Morality involves a perception of the relation between the actual and ideal, and is based on introspection," I say this in consequence of my personal experience. I can only infer morality, introspection, and so on, in other beings, whether animals or men, by judging from their activities. And if "most cases of so-called morality in the dog can be otherwise interpreted," so also can most cases in other men. A fundamental distinction between the psychological powers of animals and men could only be established by showing a fundamental distinction between animal and human activities, as observed from outside by a third person. And though it is easy to show that there is a difference in degree, Prof. Morgan did not adduce any cases which even tended to show that there is any difference in kind. The cases he did adduce all tended the other way; and though this was doubtless because he only adduced difficult cases in order to show that his theory was capable of explaining them away, his explanations seemed to me, for the reasons I have given, insufficient.

Note.—The quotations are from a printed *précis* of his paper distributed by Prof. Lloyd Morgan at the meeting.

EDWARD T. DIXON.

12, Barkston Mansions, South Kensington, August 5.

Tropical Cyclones.

A FEW years ago I drew up some simple mathematical rules to aid the Jamaica Weather Service when in doubt as to the indications, and thinking that these rules may be of some use to other isolated or nearly isolated stations in the Tropics, I state them here, and give an example or two as to their application.

At the time and place of observation let

p = Reading of bar in inches and decimals of an inch, corrected for instrumental error, reduced to 32° F. and sea-level, and further corrected for diurnal variation.

p_m = Mean value of p for the time of the year.

Δp = $p_m - p$ = fall of pressure below the mean.

v = Velocity of the wind in miles per hour.

r = Distance of the centre of the cyclone in miles.

$\frac{dp}{dr}$ = Bar-gradient, or the fall of p per mile towards the centre at the place of observation.

$\frac{dp}{dt}$ = Rate of fall, or the fall of p per hour.

$\frac{dr}{dt}$ = Rate of approach of the centre in miles per hour.

Now let us suppose that the centre is moving towards the place; in this case we have

$$\frac{dr}{dt} = \frac{\frac{dp}{dt}}{\frac{dp}{dr}} = \frac{\text{Rate of fall}}{\text{Gradient}} \dots \dots \dots (1)$$

In Jamaica $\frac{dp}{dr}$ is found by an exchange of telegrams between Kingston and Kempshot, these places being on the line of usual approach, and 77 miles apart.

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The next equation is based upon the results of observation:—

$$r = \frac{\Delta p}{\frac{dp}{dr}} = \frac{\text{Fall below mean}}{\text{Twice the gradient}} \dots \dots \dots (2)$$

This equation must not be pressed; it is intended to be used when the centre is still a long way off.

Thus we have

$$\text{Time of arrival of the centre} = \frac{r}{\frac{dr}{dt}} = \frac{\text{Fall below mean}}{\text{Twice the rate of fall}} \dots \dots \dots (3)$$

This is an important equation, for it shows that the direct approach of a cyclone may be ascertained by the constancy in the computed time of arrival.

As an example, let us take the cyclone which passed over Kingston, Jamaica, August 18, 1880. This was before the Weather Service was established, and the indications of the advancing cyclone were confused by the existence of a small cyclone to the north-east of Kingston; there was no wind in Kingston until 8 p.m., and rain fell quietly in showers all that afternoon from a sky covered with stratus.

Kingston, Jamaica, August 18, 1880.

	p	Δp	$\frac{dp}{dt}$	Time of arrival.
7 a.m.	29.845			
9 "	.807	0.178	0.0170	2 p.m.
11 "	.777	.208	.0142	6 "
1 p.m.	.750	.235	.0120	11 "
3 "	.729	.256	.0225	9 "
5 "	.660	.325	.0595	8 "
7 "	29.491	0.494	0.1808	8.30 "
9 "	28.937			

From the last column it appears that from 7 a.m. to 1 p.m. the cyclone was not directly approaching, and from the logs of vessels and other information it is certain that the cyclone was passing south of Jamaica on a westerly course, and that between 1 and 3 p.m. it turned on its course and advanced directly towards Kingston. The centre arrived at about 9 15 p.m., lowest $p = 28.917$.¹ A warning notice was posted at 3 p.m., but had these rules been in existence, a better notice could have been posted at 5 p.m.

Now if the gradient be not known at an isolated station, it may sometimes be deduced from the theoretical equation—

$$\frac{dp}{dr} = 0.00007v \dots \dots \dots (4)$$

This equation is only to be used for considerable distances from the centre; and if v be small, or variable, or if it be mixed up with the sea-breeze, it cannot be used at all.

If $\frac{dp}{dr}$ be known, r is known from (2), and then some idea may be formed as to the magnitude of the coming disturbance. The following rule may prove useful:—

Let Δp^c be the fall below the mean at the calm centre of the cyclone; then, roughly—

$$\Delta p^c = \frac{\Delta p \sqrt{r}}{6} (1 + \frac{3}{4} \Delta p \sqrt{r}) \dots \dots \dots (5)$$

Now $\frac{dp}{dr}$ is unknown in the example above; but if we take

$\frac{dr}{dt} = 18$ from the subsequently known circumstances, (1) gives $\frac{dp}{dr} = 0.0012$ at 3 p.m.; (2) gives $r = 106$ at the same hour which happens to be right; and (5) gives $\Delta p^c = 1.3$, which is a little too large, the observed fall at the centre being about 1.1.

As another example, let us take the great hurricane in Mauritius on April 29 this year. Some valuable notes by Dr. Meldrum are published in NATURE, June 9, but unfortun-

¹ See "Jamaica Meteor. Obs.," vol. i. (Introduction).