proportional to $\sqrt{H}$ for the former branch, and to H^2 for the latter, showing that the singular case discovered by Gmelin is not confined to the satellites of the yellow line 5790. The same remark applies to the satellite -242 of the green line; the $(-)$ branch becomes fainter with increase of the field, and is parabolic in the sense above mentioned, the $(+)$ branch increases in brightness with the field, and the wave-length goes on increasing until it reaches a maximum, whence it gradually returns to the initial value in $H=24,000$, and decreases farther at a constant rate. The $(+)$ branch becomes ultimately parallel to the principal line P_{-1}. The direction taken by this Branch ultimately coincides with that of the $(-)$ branch of the satellite, -74; on approaching the $(+)$ branch of -242, this $(-)$ branch of -74 becomes fainter, and is finally lost to view; the other branch of -74 runs probably parallel to P_{+1}, and increases in intensity.

The satellites -26 and $+78$ have both a curved branch towards the negative side, and a straight branch on the positive side, both being parallel to P_0. Thus in these lines the different branches to which the satellites are divided ultimately run parallel to the principal lines, whether the vibration takes place parallel or perpendicular to the direction of the magnetic force. This stage is reached earlier in the latter than in the former, as an inspection of the figures will show. The same holds good also for the strong satellites of the mercury line 4359. On reaching this stage, the change in wave-length takes place proportional to the corresponding change in magnetic field, and the separation becomes ultimately normal.

It is in the transition from zero field to this final stage that the separation of the satellite takes place in a singularly anomalous manner, that we seldom meet with in the separation of the principal lines.

This fact will have an important bearing on the elucidation of the nature of the satellite, and probably may have an intimate connection with the recent experiments of Paschen and Back. Before entering into theoretical speculation as to the probable origin of the anomalous mode of separation, we think it advisable to extend the investigation to see if such an effect is common to satellites of lines of other elements.

H. NAGAOKA.
T. TAKAMINE.
Physical Institute, Imperial University, Tokyo,
July 31.

The Piltdown Horse "Grinder."

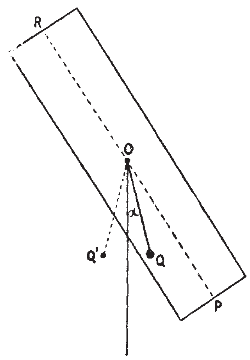
IN the Dawson-Woodward paper on the Piltdown skull of a "hominid" (Q.J.G.S., vol. lxix.) mention is made of a tooth of *Equus*, and an accurate description (so far as it goes) is given. After handling it again at Kensington, and comparing it by measurements with recent finds from this Stort Valley, also with one recently placed in the Sedgwick Museum, and another in the Saffron Walden Museum, I have found that the tooth in question appears to be the *fourth premolar* (p.m. 4) of *Equus robustus*, which Prof. Cossar Ewart has recognised as the true "Solutré Horse" ("Restoration of an Ancient Race of British Horses," Proc. Roy. Soc., Edin., vol. xxx., part 4). The importance of this identification (if it is confirmed by experts) is too obvious to need further comment to those who are familiar with recent advances in our knowledge of the prehistoric horse. It remains to determine the exact horizon in the gravel-deposit at which this tooth was found before we can appraise its precise value as a time-index (see NATURE, July 8, 1909, paper to the Royal Soc. by Prof. J. C. Ewart, F.R.S.). But one may venture to assert that the *stratum* of Piltdown gravel, from which this tooth of Equus came, is of far later date than anything belonging to the Pleiocene.

A. IRVING.
Bishop's Stortford, August 16.

Automatic Stability in Aëroplanes.

PROF. BRYAN's explanation of his model illustrating instability due to friction is somewhat obscure, but in any case it is difficult to see how there is not a violation of the principle of conservation of energy in his conclusion.

If θ and ϕ are the angles made with the vertical at any instant by OQ and OP respectively, the potential energy of the controlling mechanism is $k(\theta-\phi)^2$, where k is some constant. When the system starts to move from the position depicted in the figure, its energy is $C+k(\beta-\alpha)^2$, and when it reaches the position of rest on the other side its energy is $C'+k(\gamma-\alpha)^2$, where C and C′ depend on the position of Q and Q′ relative to O, and therefore are equal, and β and γ denote the angles which OP makes with vertical in the first and last positions. Now Prof. Bryan states that γ is greater than β, in spite of the fact that some energy has been degraded by friction in passing from one position to the other. Where is his concealed source of energy?



J. B. DALE.

THE system contemplated in my letter assumes the existence of an external source of energy, and perhaps it might have saved misunderstanding if this fact had been stated at the expense of brevity. If we imagine an aëroplane performing purely lateral oscillations, and suppose it furnished with a pendulum so arranged as to operate on a pair of ailerons, we have a system the action of which might be represented to a first approximation by the model assumed by me. In this case the necessary energy is being supplied by the wind, which, by its action on the ailerons, causes the aëroplane to rotate like a windmill during the interval that the pendulum rotates with the aëroplane, while the inclinations of the ailerons remain constant. The work done in a small displacement is of the form $k(\theta-\phi)d\theta$, but this does not integrate into an expression representing potential energy.

G. H. BRYAN.

Physiological Factors of Consciousness.

I WISH to ascertain the opinion of physiologists and psycho-physicists on the following point, and I hope some readers of NATURE will be good enough to supply me with the information required.

My query is this: What is the true explanation of the fact that stimuli sufficiently strong to arouse vivid sensations in a subject while he is wide awake apparently fail to arouse any sensation at all in a state of unconsciousness? Four explanations appear to be possible, namely:—

(1) The afferent nervous current does not penetrate at all along the conducting paths into the central nervous system.

(2) It penetrates into it, but only up to a little way, and does not reach the highest nervous centres.