Q may well be different: or the immediate past history of the air: or P and Q may differ in height from the ground by perhaps 5-10 metres in undulating country. Any of these effects will cause a difference between the values of w'^2 at P and Q. But a difference of 0·001 mb. in pressure over a distance of 100 metres in latitude 55° requires an air-flow of about 8 m./sec. for stability. So the only way to avoid, maybe violent, readjustments is for the horizontal components of the turbulent energy to differ between P and Q by the same amount as the vertical components. This gives us at once that, for a steady state, the horizontal contributions to the eddy-energy at any point must differ from the vertical by an amount which is constant over a wide horizontal area.

When the mean wind velocity or vertical temperature gradients are varying every few minutes it will take time for the turbulent energy over some distance to readjust itself; this may account for the components of turbulent velocity measured by the standard autographic instruments being about three times as great, for the same mean wind-velocity, as those obtained from a light sensitive vane (Scrase, loc. cit.). On a larger scale this readjustment is very likely a cause of the 'wind-swinging' described in a letter by Johnson [4] some years ago; he says that it is accompanied by microbarograph oscillations of the order of 0·05 mb, with a period of 6-8 minutes.

This also has some bearing on the turbulent motion of liquid between two coaxial vertical rotating cylinders. Taylor [5] has studied this problem. The work of earlier experimenters showed him that the turbulence could not be two-dimensional; he calculated the disturbance, if restricted to symmetry about the axis of rotation, and verified his calculations by experiment. But why should there never be two-dimensional turbulence? Ertel's result may show us. With the usual cylindrical co-ordinates, r, θ, z, two-dimensional disturbances would be in r and θ only; this would reduce the mean static pressure at the level, z_0, considered; but as the average depth of the liquid is not changed, vertical, or z-, disturbances must occur at the same time as the r- and θ-disturbances in order to support the liquid above z_0. Two-dimensional turbulence is thus not possible in the motion of liquid between a pair of vertical coaxial rotating cylinders.

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Nov. 27.

[1] *Met. Zeit.*, **47**, p. 22; 1930.
[2] *M.N.R.A.S.*, **89**, p. 721; 1929.
[3] *M.O. Geophysical Memoirs*, No. 52.
[4] NATURE, **115**, 263; 1925.
[5] *Phil. Trans. Roy. Soc.*, A, **223**, p. 289; 1922.

Deinotherium in the Pleistocene.

THE discovery made by the East African Archæological Expedition on Nov. 17 last, announced in the *Times* of Dec. 3 (see also NATURE, Dec. 12, p. 995, and Dec. 26, p. 1075), of *Deinotherium* remains in implementiferous Pleistocene deposits is extremely important. It is, however, not quite the first of its kind, for M. Delpierre, a Belgian geologist who had been working on the sediments of the Albertine rift near Ngeti, told me, a year or two ago, that he had discovered *Deinotherium* teeth and bones in beds which, on other evidence, he was convinced are of Pleistocene date. He affirmed, too, that these most unexpected fossils were not derived.

Now, at Kurungu in South Kavirondo, Kenya Colony, on Lake Victoria, there is a series of fossiliferous sediments, made known to geologists by the researches of Dr. Felix Oswald, which largely, though not entirely, because they yield *Deinotherium*, are regarded as Miocene, in spite of the fact that the contained Mollusca are all living species. In January 1930, I discovered large developments of these beds farther north, on Rusinga Island and on the mainland near it. The strata are there interbedded with lavas and volcanic tuffs which might well prove valuable for dating purposes over a limited area. Lying on these sediments I found a crude *coup de poing* which, judging from the appearance of its surface, seemed to have been derived from the underlying beds; but as these were to be dated as Miocene, I considered any such derivation impossible, and so regarded the artefact as a purely surface find and of little consequence. The Oldoway discovery, however, would seem to render the alternative explanation possible.

A study of the map is sufficient to show, apart from indisputable geological evidence, that Lake Victoria, with its drowned coastline and reversed feeders, is younger than the main river channels, the upper courses of which are submerged beneath it. Yet on the Kavirondo evidence, part of it at least dates back to the Miocene. Clearly, then, a difficulty would be solved if the deposits at Kurungu and Rusinga could be regarded as, say, Plio-Pleistocene, or lower to middle Pleistocene, and laid down, in part at any rate, during the first pluvial period—I long ago pointed out that if anybody wanted a pluvial period in the Miocene, the Kurungu could provide the necessary evidence—and from what has been said above, this interpretation is seen to be no longer out of the question. But there is more. Controversy has arisen between Prof. J. W. Gregory and the East African Archæological Expedition (1926–27 and 1928–29) concerning the age of the Kamasian beds in the Eastern Rift valley; Gregory correlates these with the Karungu deposits, and therefore regards them as Miocene; but Leakey, on the sound evidence of the discovery of stone tools therein, regards them as Pleistocene and laid down during the first (Kamasian) pluvial period. It would appear then, in the light of the discovery of *Deinotherium* in Pleistocene beds at Oldoway, and the possibility that my *coup de poing* was derived from beds of Karungu date, that Gregory's correlation is not unsound, as Leakey thought, and that the artefacts were in the Kamasian beds, which Gregory disputed. (In this connexion see NATURE, Dec. 19, p. 1019.) Moreover, if this contention is admitted, another riddle in the history of Lake Victoria is satisfactorily solved.

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Ether-Drift and Gravity.

IT is well known that, by means of electrodynamic experiments, no one has yet succeeded in ascertaining the cosmic movement of the earth, which, particularly in relation to the spiral nebulæ, amounts to very high velocities. Apart from the still incompletely explained results obtained by Miller, which, from the work of Joos, should undoubtedly be considered as perturbations, Courvoisier at Potsdam and Esclangon at Strasbourg recently got positive results. These are interpreted by Courvoisier as indicating a Lorentz-contraction of the earth, in consequence of a movement of the earth towards the light-ether with a speed of 750 km./sec. and with an apex at $A = 60°$; $D = 40°$. In 1926 one of us (R. T.) demonstrated—by means of a very much refined repetition of Trouton's and Noble's experiments—that such a Lorentz-contraction is not obtainable electrodynamically; although 1/2,000,000 of the effect asserted by Courvoisier must have been detected in this way.