about 5000 metres, diffraction alone is wholly inadequate to account for the signals obtained.

If now we assume the presence of some reflecting or refracting layer, the foregoing mathematical theory can be applied at a distance large compared with H, but small relatively to the earth's radius, so that the curvature can be neglected.

In actual practice the interference between the large number of terms postulated by the theory is not present, and this may be accounted for by assuming that the layer is a good reflector only for glancing incidence; in this case it can be shown that only the zero order term remains (with $m=s$, $\beta_s=0$),

$$i.e. \qquad \frac{120\pi hI}{2H\sqrt{\lambda x}}E^{-\alpha x/\sqrt{\lambda}}$$

apart from the direct wave, which, varying inversely as the distance, is negligible at greater distances than a few hundred kilometres.

Now the experimental results show that the observed signal strength E can be expressed very well by a formula of this type in which the value of H is approximately 40 km. This must be regarded as only an equivalent value since the lower surface of the layer is probably very ill-defined. T. L. ECKERSLEY.

Marconi's Wireless Telegraph Co., Ltd.,
Chelmsford, March 20.

The Absorption of Cathode Rays in Aluminium.

IN a recent paper in the *Physical Review* (December 1924), H. M. Terrill gives some measurements of the variation in the fraction of a beam of cathode rays transmitted by an aluminium foil when the velocity of the rays is varied. These results are not in agreement with those published by me (Proc. Roy. Soc. A, vol. 104, 1923); and the author states that "Whiddington, and later Schonland, worked with rays of uniform velocity produced by magnetic sorting, but the results obtained by them are not in agreement with each other nor with those of the earlier writers. It is believed that the lack of agreement in these results may be traced to the difficulties of velocity determination."

This is scarcely correct. A variety of causes rendered the work of the earlier writers unsatisfactory from a quantitative point of view, while the experiments of Whiddington suffered from an important defect, for no precautions were taken against the disturbing effect of the emission of secondary rays from the foil. My apparatus was designed to remove this source of error, and when allowance is made for the secondary emission, Whiddington's results are in satisfactory agreement with my own.

Dr. Terrill's explanation of the difference between his results and mine would require a correction to my values of $H\rho$ amounting to about 20 per cent., while I am certain that they are not at fault by more than 2 per cent., the measurements having been repeated with a new and differently wound solenoid.

That the difference is, however, not due to this cause at all but to the experimental arrangement for measuring the fraction transmitted, is shown by the fact that our results for the velocity of those rays which are just unable to penetrate the foil (the "Range" velocity) are in close agreement. Thus, for a foil 0·00031 cm. thick he finds 19,000 volts, and I, 19,500 volts for the P.D. corresponding to this velocity. This indicates that my velocity measurements are substantially correct and that the differences between our results arise from errors in the measurement of the fraction transmitted.

I believe that Dr. Terrill's arrangement for this purpose is open to criticism, principally owing to the method adopted to prevent the emission of secondary rays from the foil, but also to the fact that so small a fraction (< 1/1000) of the cathode ray beam was employed in the actual measurements. I have had experience of both these causes of error and found them to be very serious.

B. F. J. SCHONLAND.

University of Cape Town,
February 18.

Adsorption of Acids by Purified Silica.

IN the issue of NATURE for January 31, p. 157, it has been stated that hydrated silica free from all impurities adsorbs acids. The amounts adsorbed by these samples are, however, small. We have since found that if thoroughly washed hydrated silica prepared from pure silicon tetrachloride be allowed to be partially dehydrated in air at room temperature, it shows a marked increase in its capacity to adsorb acids, as the following data will show:

Electrolyte.	Concentration.	Amount of Adsorption per 10 grams of Hydrated Silica.
Oxalic acid	N/2	46·0 c.c. of N/10 solution
	N/10	7·5 ,, ,, ,,
	N/50	1·0 ,, ,, ,,
Potassium oxalate	N/2	45·0 ,, ,, ,,
	N/10	7·5 ,, ,, ,,
	N/50	1·0 ,, ,, ,,
Sodium oxalate	N/10	7·5 ,, ,, ,,
	N/50	1·0 ,, ,, ,,
Hydrochloric acid	N/2 (F:1·08)	42·0 ,, ,, ,,

The samples are free from all impurities. At equivalent concentrations, neutral oxalates and oxalic acid show equal adsorption of the oxalate ion, which, together with the increase in the negative charge of the silica in contact with solutions of neutral salts of low concentrations, proves that we are dealing with anion adsorption as suggested by the writer to account for the latent acidity of sour soils. The large amounts of acids adsorbed leave no doubt that Joseph and Hancock were mistaken in stating that purified silica cannot adsorb acids. That they could not observe any adsorption of acids by silica was due to their using ignited silica, as we have found that on ignition the power of silica to adsorb acids greatly diminishes.

In my previous letter referred to above, a mistake occurs (page 158) in the P_H value for the acidity developed on interaction between barium chloride and potassium sulphate. The value given was 2, whereas it should have been 5.

J. N. MUKHERJEE.

University College of Science,
Calcutta.

Method of Measuring Deep Sea Tides.

IN the course of a conversation with William Beebe regarding plans for work to be done on his oceanic expedition, my attention was directed to the fact that no method had been devised up to the present time for recording the rise and fall of the tides except in comparatively shallow waters. It appeared that the Hydrographic Office was very anxious to have data regarding the tides at localities where the depth of the ocean was measured in miles.

The problem looked rather hopeless at first sight, but on thinking about it, the idea occurred to me that if we could make an artificial island, reaching up from the sea floor to within a few feet of the surface, the rest would be easy. Such an island could be made by means of a submerged buoy