The late Prof. Tacchini.

As a tribute to the memory of the late distinguished Italian astronomer, of whom an obituary notice appeared in the columns of NATURE last week, may I be permitted to add a few personal reminiscences? Prof. Tacchini took part in the eclipse expedition of 1875 to the Nicobar Islands. He joined our party from India, where he had been staying from the previous year, having been commissioned by his Government to make observations on the transit of Venus of 1874. The Italian Government sanctioned his remaining in India until the following year in order that he might make use of the opportunity with the instruments in his charge for the observation of the forthcoming total solar eclipse. Of the little band of observers who assembled on the Island of Camorta in April, 1875, most are happily still with us. Vogel, the introducer of "orthochromatic" photography, has passed away, but Pedler, Waterhouse, and others will remember the pleasant *camaraderie* which existed between ourselves and our Italian colleague. The expedition failed in its object through a cloudy sky, and we were all more or less the victims of intermittent malarial fever; but we made the best of adverse circumstances, and under conditions which, to many a party of observers similarly placed, would have been extremely trying, the good understanding which the members had arrived at among themselves helped to lighten the burden of our disappointment. Not the least weighty factor in the formation of this good fellowship among the representatives of different nations was the geniality of Tacchini, with whom we parted on the P. and O. steamer *Baroda* on the homeward voyage with every regret.

April 15. R. MELDOLA.

Propagation of Earthquake Waves.

MR. RUDZKI, in his letter to NATURE of April 6, observes that "it is only for perfectly elastic and isotropic bodies that the separation of the dilatational (normal) from the tortional (transverse) wave takes place with certainty"; and his conclusion is that "it is more than highly improbable that the effect of internal friction would neutralise the effect of æolotropism." If the term "internal friction" is intended to refer to the effect of pressure, this objection was forestalled by Major Dutton by the remark that "towards this more compact and continuous condition (of a compact mineral substance with a feeble pronounced cleavage), the pressure of great depths in the earth should, it may seem, tend to bring the material subject to it."

To me it is refreshing to learn that any objection can be raised to the view that the two speeds of earthquake waves are respectively condensational and tortional, the latter being held to prove a high degree of rigidity for the interior of the earth.

To examine the question whether the interior is to a considerable depth liquid or solid formed one subject of my "Physics of the Earth's Crust," and I came to the conclusion that it is liquid; and, so far as I am aware, my arguments have never been refuted. On this question Sir A. Geikie writes (NATURE, February 9), "the geological belief rests upon a large body of evidence from the structure of the terrestrial crust, which it is difficult or impossible to explain except on the supposition of an internal mass which, at least in its outer parts, is sufficiently liquid to emerge at the surface as molten lava."

To produce arguments on the opposite side of the question is another matter, and that derived from the two speeds of earthquake propagation is perhaps the strongest. I was consequently led to inquire whether the same result could not be obtained on the hypothesis of a liquid magma holding water gas in solution, subject to Henry's law that the same volume of gas can be absorbed by a given volume of the liquid at all pressures. The result which I obtained was that two waves would be propagated with different velocities, the one a condensational wave depending on the elasticity of the liquid, and the other a wave depending upon the pressure and the volume of the gas which could be held in solution by a given volume of the liquid.

If e be the elasticity of the liquid and D its density, then $\sqrt{e/D}$ will be the velocity of the condensational wave. And if P be the pressure and rV the volume of gas which can be held in solution by the volume V of the liquid, then $\sqrt{P/rD}$ will be the velocity of the gaseous wave. If we accept Laplace's law of density, P/D will increase with the depth, and r will probably decrease, hence the velocity of the gaseous wave will increase (*Proc.* Cambridge Phil. Soc., vol. xii., part v., 1903).

Harlton, Cambridge, April 10. O. FISHER.

The Ancient Races of the Thebaid.

ON my return to Oxford I saw Prof. Pearson's letter in your issue of March 30.

Since Prof. Pearson admits that he is not an anatomist, it would serve no useful purpose to discuss with him the anatomical value of the criteria which Mr. MacIver and I employed in our analysis of the skulls of the ancient inhabitants of the Theban province of Egypt.

The letter may be regarded as an interesting record of a method of interpreting percentage values adopted by a professed statistician. ARTHUR THOMSON.

Oxford, April 8.

THERE is an old saying that all good science is shorthand common sense. I am sorry that Prof. Arthur Thomson does not think it worth his while in the case of his just published far-reaching negroid cranial criterion to convert the esoteric methods of the anatomist into simple language for the benefit of other readers of NATURE, if not for that of the "professed statistician." I hope he will meet me later when I ask him to discuss, as I propose shortly to do, the mathematico-statistical treatment of his volume, which is of a somewhat remarkable character. Meanwhile, in order to expedite those further investigations by professed craniologists which his discovery is exciting, it would be of great value if he would tell us to what negro series he, *a priori*, applied his criteria, and what percentages of pure negroid, non-negroid, and intermediate crania he found in that series. KARL PEARSON.

Inversions of Temperature on Ben Nevis.

THE recent letters of Mr. Dines and Mr. Rotch (NATURE, February 16 and March 30) have suggested that a note as to the occurrence of temperature inversions on Ben Nevis may be of interest.

During the thirteen years 1891–1903, occasions were not infrequent when the temperature at the top of the mountain (4406 feet) was higher than that at the base. These inversions have been grouped according as the summit temperature was the higher, (1) at one hour at least of the day; (2) at each of the twenty-four hours of the day; (3) on the mean of the twenty-four hours of the day.

The total number of cases in the thirteen years was as follows:—

	Class I.	Class II.	Class III.
January	7	—	3
February	18	1	5
March	11	—	1
April	9	—	—
May	7	—	—
June	8	—	—
July	4	—	—
August	4	—	—
September	22	—	3
October	15	—	5
November	29	3	8
December	24	5	8
Year	158	9	33

Thus inversions occurred at all seasons, but inversions continued throughout the twenty-four hours of the civil day only in February, November, and December, and those of Class III. only between September and March. The average difference of temperature between Ben Nevis and Fort William ranged from 16°·8 F. in April to 14°·4 in December, the mean for the whole year being 15°·4. Hence inversions were at all seasons large departures from the usual conditions.