

theory as well as on experiment, we are forced to conclude that the diameter of a molecule cannot be larger than  $0.003\mu$ . On the other hand, the specific gravity of a molecule of air, for instance, appears to be very great, viz. five times that of water (see Meyer, "Die kinetische Theorie der Gase"). This maximum value of the diameter of a molecule is so enormously smaller than the values demanded by Prof. Arrhenius' theory that the latter appears to be incompatible with any assumption which regards the cometary matter as being of a gaseous constituency. In order to explain the repulsion of matter in the tails of comets by the pressure of the sunlight, this matter must be assumed to consist of small drops, each of a bulk sufficiently large to harbour at least one million molecules within its bounding surface. Whether such an assumption can be justified appears to me very doubtful. At any rate, Prof. Schwarzschild's profound mathematical investigation makes it absolutely clear that the idea of minute electrically-charged corpuscles—of about one-thousandth the size of a hydrogen atom (see *Observatory*, February 1902, p. 103)—being propelled by the sun's light towards the earth and causing the various phenomena of auroræ, Gegenschein, &c., receives no support from the mathematical point of view. But, even apart from these difficulties, it can hardly be said that the ingenious theory of Arrhenius settles the question as to the nature of the force acting on the cometary matter. So far it offers no explanation of the remarkable phenomenon of the contraction of the coma with the approach towards the sun. Doubtless this *contractile* force is also of solar origin. But can it be identified with the force which repels the cometary matter in the direction of the radius vector? It appears to me that the theory in its present form only removes one difficulty by introducing several others.

J. HALM.

Royal Observatory, Edinburgh, February, 20.

#### Experimental Geometry in Secondary Schools.

In the report, in your issue of February 27 (p. 404), of a meeting of the Mathematical Association, it is stated to have been the opinion of most speakers that the study of demonstrative geometry should be preceded by a course of work with ruler, compasses and protractor, in which simple measurements and constructions form the chief part. The note continues with the statement that such a course has been adopted in Scottish schools, and in English elementary schools and kindergartens.

May I venture to point out that there are a considerable number of secondary schools where this kind of work forms a regular part of the curriculum in the lower mathematical classes (this has, for instance, been the case here for the past three years), and that it has been recognised as a valuable medium by which a boy's interest may be aroused in geometry before he is introduced to the very irksome and difficult task of assimilating Euclid's phraseology?

C. A. RUMSEY.

Dulwich College, March 5.

#### The Zodiacal Light.

THE zodiacal light was visible here last night at 7h. 30m. p.m. The base at the horizon was about  $16^\circ$  wide; the axis of the cone pointed towards the Pleiades, but the apex did not reach much beyond  $\alpha$  Arietis. At 7h. 45m. it was brightest; at 8h. it had faded out, possibly on account of mist in the air, as a fog set in about an hour afterwards.

J. P. MACLEAR.

Chiddingfold, Surrey, March 4.

#### Contributions to Anatomical Journals.

PROF. HUNTINGTON, of Columbia University, has called my attention to a paragraph in an article on "A New Journal of Anatomy" in your issue of January 9 to which he, naturally, takes exception.

In justice to Prof. Huntington, I beg leave to state that he had no part whatever in the recommendation, acceptance or production of either of the papers on which your critic adverts.

The first of these was accepted by one of my British co-editors and sent to me for publication, the author supplying the plates. For the second I, solely, am responsible.

The method of the higher criticism is not always trustworthy when its results can be checked by contemporary history.

ALEX. MACALISTER.

New Museums, Cambridge, February 14.

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I REGRET to say that my statement concerning Prof. Huntington and the origin of the paper which I described was based on hearsay, and in face of Prof. Macalister's letter I can but tender Prof. Huntington my sincere apologies.

I am relieved to find that the author provided the plates.

February 26.

THE WRITER OF THE REVIEW.

#### FURTHER DEVELOPMENTS IN WIRELESS TELEGRAPHY.

SINCE the article which appeared in NATURE last week was written, some further details concerning Mr. Marconi's Transatlantic signalling have been published, and also another success attained by the inventor has been announced. The information with reference to the latter point is contained in a telegram from the New York correspondent of the *Daily Telegraph*, which appeared in that paper on Monday last. Messages, it seems, were transmitted from Poldhu to the *Philadelphia*, whilst that ship, on board of which was Mr. Marconi, was on her way from this country to New York. Five messages in all were received, the first when the *Philadelphia* was at a distance of 250 miles from the Lizard, and the last on February 25, when the distance had been increased to 1551 miles. In addition, a signal of the much-talked-of letter "S" was received at a distance of 2099 miles. The news is confirmed by the following telegram from Mr. Marconi, which is contained in a letter from the Wireless Telegraph Co. to Wednesday's *Times* :—

"Health good. Received messages 1551 miles. Test letter at 2099. All on tape receiver. Records duly attested by ship's officers.—MARCONI."

The chief interest of these results lies in the fact that the received messages, including the signal "S," were recorded on the tape of the receiving apparatus, and not merely heard in the telephone, as was the case with the Transatlantic signal. Mr. Marconi must feel greatly gratified at thus having visible record of the success of his experiments in this instance, as it removes the possibility of the suggestion that he was deceived by the wish to hear being father to the thought that he heard. Messages were naturally only transmitted in the one direction, as the transmitting apparatus on board the *Philadelphia* was not so powerful as that at the Cornwall station.

The further details as to the Transatlantic signalling to which we referred above are published in an article on Mr. Marconi in this month's *Century Magazine*. This article, the proofs of which have been read by Mr. Marconi, contains an account of the development of wireless telegraphy, and is illustrated by a number of interesting pictures. We reproduce here a photograph of the transmitting station at Poldhu from which all the long-distance signals have been transmitted, which shows very clearly the group of twenty masts, each of which is 210 feet high. The power is obtained from an alternate-current generator of 38 horse-power. There is thus more than two hundred times as much power used as in the signalling apparatus installed on board ships using the Marconi system. It will be noticed that in the later work development has taken place rather along the lines of increasing the output of power than of using higher masts. Mr. Marconi, it is said, considers that a mast about 200 feet high is the most suitable from all points of view, and in some remarks, which we quote at the end of this article, states that he thinks that any desired distance could be bridged given sufficient power.

A good deal of objection is still raised on the question of syntony, and it is pointed out that although tuning has been obtained with sufficient accuracy to prevent interference, this does not prevent the picking up of messages by an outsider who should experiment with a