other with a pocket sextant and ice horizon, justify the conclusion that the terrestrial refraction was so abnormal, that the computation of the latitude necessitated the reversal of the sign of the dip; but that this state of things was local, and that the observations of another observer only ninety miles away would not be so affected, though the temperature and general conditions were in both cases practically the same. (See comparison between the corrections for refraction in the meridian altitudes of Nansen at his Farthest North, and Hansen on board the *Fram*, April 6, 1895).

Admitting to the full the truth and justice of the remark of your reviewer in connection with the observations taken during Nansen's sledge expedition, that "the fact that observations were taken at all is the strongest possible evidence that scientific zeal is compatible with the possession of remarkable courage," it must also be admitted that a comparison of these scientific results with those which Nansen obtained from the same times and altitudes proves that scientific zeal and the power of taking observations are also compatible with the inability to comprehend the very elementary fact that if the results of two or more observations differ widely from each other neither is trustworthy, and that geographical positions and condemnations of the work of such men as Julius Payer and Wyprecht cannot, and ought not, to be based upon them.

Should any student of practical nautical astronomy go to the trouble of making this comparison, he cannot, I think, fail to perceive at every step that, however painstaking Prof. Geelmuyden and his colleagues have been in their attempt to plot Nansen's route on his celebrated sledge journey, they have been compelled to ignore his own statements and his own workings, and while straining at the scientific gnat they have freely swallowed the practical camel. Dr. Nansen had led us to believe that the scientific results would explain and justify his already published results. It can be easily shown that one or the other is hopelessly wrong. They are totally irreconcilable. If it is for one moment admitted that Nansen had the opportunity and ability to work the common observation for longitude by chronometer, then Prof. Geelmuyden's primary hypothesis is unsound. If it is maintained that that hypothesis is even approximately correct, Nansen's own recorded results become ridiculous.

Turning from matters of fact to matters of opinion, two statements of great interest to explorers in high latitudes may be noticed. On p. 14 we are informed that one of the computers employed with advantage the difference of altitude near the prime vertical to determine the latitude. Now in low latitudes, where the change of altitude is rapid, say from 10′ to 15′ per minute of time, a result within five or ten miles of the truth is perfectly possible. In latitudes from 70° to 85° N., with altitudes changing at most 5′ or 6′ per minute of time, and affected by refraction abnormal in itself, and varying rapidly according to no well-defined law, the method entirely fails. If a chance observation appears to justify its use, the altitudes must be accidentally or miraculously correct.

The remark on lunars, p. 22, will strike experienced observers as exceedingly curious. "The results," says Prof. Geelmuyden, "are not satisfactory."

Table C., p. 44, shows that eight observations were taken at various times, and from them Greenwich Mean Time was determined with errors varying from 18 seconds to 2 minutes. On the assumption that the explorers might have been dependent on them, their positions would have been affected with a maximum error of 30′ of longitude, or about four geographical miles. Let future explorers note this. It may safely be affirmed that these results will seldom be surpassed by men taking lunars under Arctic conditions. It may with equal truth be said that for the purposes of such explorers greater accuracy is unnecessary; and the submission to a practical test of Prof. Geelmuyden's opinion, that better results can be obtained by deducing the moon's right ascension from the difference of azimuth of the moon and a star, will be a task not unworthy of the scientific expert accompanying the *Discovery*. E. PLUMSTEAD.

"First on the Antarctic Continent."

SOME rather venomous criticism of my book, "First on the Antarctic Continent," has appeared in one or two periodicals. Had my book been intended to be what it is not—a scientific report upon our work in the south—the venom would to some extent be justified. There are, however, other circumstances which prevented me from producing at the time a larger and more representative account of our work in the south. Preliminarily may I state that the observations have been submitted to the Council of the Royal Society, who have accepted them, and the Society is in due time going to publish a volume on the results? This speaks for itself of the efficiency of the staff I had chosen. The Natural History Museum of South Kensington has received the bulk of the collections and I understand that the report upon them is nearly finished, and the book, written by specialists of the Museum, will probably appear within a very short time. C. E. BORCHGREVINK.

(Commander, British Antarctic Expedition, 1898–1900).

Douglas Lodge, Bromley, Kent, July 5.

The Settlement of Solid Matter in Fresh and Salt Water.

IN a letter under the above heading in your issue of June 20, Mr. W. H. Wheeler discusses the effect of dissolved salt in promoting the subsidence of alluvial matter in water. He takes exception to the conclusion of Mr. Slidell that the mixture of sea water with river water exercises a preponderating influence on the formation of deltas. The question at issue is not one that can be settled simply by a consideration of the specific gravity and viscosity of the solutions employed, and Mr. Wheeler has made it the subject of experimental investigation. There can be little doubt that it is only in the case of very finely divided solid matter in suspension that the addition of salt solution causes increased precipitation, and so far his results can scarcely be called into question. They are confirmed by the investigations on the deposition of sediment by Carl Barus and Bodländer, to whose papers references are given below.

The precipitation of such "suspensions" or "pseudo-solutions" by the addition of an electrolyte is accompanied by the coagulation or flocculation of the solid matter. Schloesing states that clay suspensions pass through a filter paper, but can easily be filtered if coagulated by a salt solution. If, however, the clay is washed free from salt, it can enter into suspension again in pure water and be precipitated afresh. These two operations can be performed in succession several times without apparent modification in the results. Picton and Linder found that the coagulum produced by the precipitation of a pseudo-solution of arsenic sulphide contained traces of the metallic ion, which could not be removed by washing.

The mud or ooze examined by Mr. Wheeler seems to have consisted entirely of matter which had already undergone precipitation, but it does not appear from his letter that any precautions were taken to remove traces of the metallic salts, so that it remains doubtful whether the sample really formed a suspension in the pure water. More satisfactory experiments could perhaps be made by collecting samples of turbid water from a river in flood and then adding sea water or a solution of salt.

I had occasion some time ago to consult the somewhat extensive literature dealing with the suspension of solid matter in a fluid and the allied one of colloidal solutions, and the following list of papers, though doubtless far from complete, may be of use to some readers of NATURE:—

Skey, *Chem. News*, xvii. p. 160; Waldie, *Chem. News*, July 24, 1874; *Journ. As. Soc. Bengal*, 1873; Th. Scheerer, *Pogg. Ann.*, lxxxii. p. 419, 1851, einige Beobachtungen über das absetzen auf geschwemmter pulverförmiger Körper in Flussigkeiten; Hunt, *Proc. Bost. Soc. Nat. Hist.*, pp. 302–4, 1874; Slidell, *Report* of Messrs. Humphreys and Abbott on the physics and hydraulics of the Mississippi, App. A, p. 11, 1861; Ch. Schloesing, *Compt. rend.*, lxx. p. 1345, 1870, sur la précipitation des limons par des solutions salines très-étendues; David Robertson, Glasgow Geol. Soc. *Trans.*, iv. pp. 257–9, 1874; W. Durham, *Chem. News*, xxx. p. 57, 1874; *Chem. News*, xxxvii. pp. 47–8, 1878; *Proc.* Roy. Phys. Soc. Edin., iv. pp. 46–50, 1874; W. H. Brewer, *Proc.* Nat. Acad. Sci., 1883; *Amer. Journ.* (3), xxix., p. 1, 1885; C. R. Stuntz, Cincinnati Soc. Nat. Hist., Feb. 1886; E. W. Hilgard, *Amer. Journ.* vi., 1873, xvii., 1879, Forschungen auf d. Geb. d. Agriculturphysik von E. Wollny, ii. pp. 57–9, 441–454, 1879, ueber die Flockung kleiner Theilchen; A. Mayer, Forschungen auf d. Geb. d. Agriculturphysik von E. Wollny, ii. pp. 251–273; Hallock, *Bull.* of the U.S. Geol. Survey, xlii. p. 137, 1887; Carl Barus, *Bull.* of the U.S. Geol.