

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Longitudes in Brazil

LE numéro du 18 novembre de NATURE publie un article du professeur Young sur les progrès de l'astronomie depuis dix ans, dans lequel il est dit que les observations de longitude télégraphiques des officiers américains ont corrigé une erreur de 8'54s. sur Lisbonne, et une bien plus étonnante encore de 35s. sur Rio.

Il y a là une grosse erreur inexplicable de la part du professeur Young, contre laquelle je dois protester comme auteur des cartes hydrographiques du Brésil encore employés aujourd'hui, et auteur de toutes les déterminations géographiques relatives et absolues faites douze ou quinze ans avant la mission américaine de MM. Green et Davis pour les longitudes télégraphiques entre le Brésil et l'Europe.

Sur les mille lieues de côte du Brésil la mission américaine a déterminé six longitudes entre le Para et Buenos Ayres. Voici la comparaison des résultats obtenus par MM. Davis et Green, à l'aide du télégraphe, et par moi, à l'aide de chronomètres et d'observations astronomiques directes. Les observations américaines sont publiées dans le numéro 59 (1880, je crois) "Hydrographic Notice," et les miennes dans les "Annales hydrographiques, 1866."

	Para			Pernambouco			Bahia		
	h.	m.	s.	h.	m.	s.	h.	m.	s.
Long. télégraphique.	3	23	20'94	2	24	48'6	2	43	29'6
Long. Mouchez ...	3	23	18'67	2	28	47'5	2	43	26'9
Erreur ...			- 2'27s.			- 1'1s.			- 2'7s.

	Rio			Montevideo			Buenos Ayres		
	h.	m.	s.	h.	m.	s.	h.	m.	s.
Long. télégraphique.	3	2	2'3	3	54	9'9	4	2	49'9
Long. Mouchez ...	3	2	0'1	3	54	9'4	4	2	49'9
Erreur ...			- 2'2s.			- 0'5s.			0'0s.

Il résulte de ce tableau que la plus grande erreur que j'ai commise est -2'7s. sur Bahia. A Rio l'erreur est de -2'2s., et non de 35s. comme le prétend M. Young. Dans le Rio de la Plata l'erreur a été trouvée nulle.

Je ne crois pas qu'aucune étendue de côte de mille lieues eût jamais présenté moins d'erreur absolue ou relative que la côte du Brésil après la publication de mes cartes et de mes observations.

Quant à l'erreur sur Lisbonne je l'avais signalée depuis plus de trente ans, elle était connue.

Je vous serais très obligé de vouloir bien publier au moins le tableau comparatif des longitudes que j'ai l'honneur de vous envoyer aujourd'hui, pour protester contre l'erreur qui m'est indirectement imputée.

Veuillez agréer l'assurance de ma parfaite considération.

E. MOUCHEZ

Cooke's "Chemical Physics"

I AM told that I have been the object of severe strictures in your journal for republishing my old "Chemical Physics" as if it were a new book. It is a sufficient answer to say that the book was stereotyped when first issued in 1860, and that there has never been any pretence on my part that it has been revised since. I find, on inquiry, that the American publishers have made, since the first edition, three reprints from the plates, and have called these reprints second, third, and fourth editions, changing, with each issue, the date on the title-page; a usage which I regard myself as reprehensible, but which must be sanctioned by the trade since it is so universally followed. All this time, however, the date accompanying my signature after the preface, and the date of the copyright, have remained unaltered. I had supposed the book entirely out of print; and the last reprint of a very few copies to meet a small demand still existing, chiefly in England, was made entirely without my knowledge or consent. On its very face the whole aspect of the

book is antiquated; but in it there was brought together certain positive knowledge in connection with the weighing and measuring of aëriiform matter, derived chiefly from the classical researches of Regnault, which is still of great importance and not readily found elsewhere; and this is, unquestionably, the reason of the continued demand for a compilation made more than twenty-five years ago. I have, until within a few years, had the expectation of revising the book and presenting the old facts in their new dress, but the failure of my sight has obliged me to give up the plan, and younger men must do the work.

JOSIAH PARSONS COOKE

Cambridge, U.S.A., November 16

Note on Mr. Budden's Proof that only One Parallel can be drawn from a Given Point to a Given Straight Line

MR. BUDDEN's paper in the last number of NATURE (p. 92) is full of inaccuracies of a more or less serious character. Without pointing out these, I wish to show that the essential idea which underlies his reasoning is altogether wrong, as it is based on the "infinite," which he introduces in the most innocent manner by letting his figure grow without limit, and about which he then calmly reasons as if he still dealt with a finite figure. If we let a quantity "increase without limit," we get a quantity which has increased beyond our comprehension, and no one in his senses will wittingly and seriously draw conclusions from what he does not comprehend. Here we might stop, were it not that the constant use in modern mathematics of the infinite (both the small and the great) has made us so familiar with it that an attempt to base an elementary proof on it might seem to many a very natural thing.

In algebra, the infinite number is shown to have one property which we can comprehend, viz. that its reciprocal is zero; and with this property alone we work safely.

In modern geometry, on the other hand, the infinite is used as a kind of shorthand, which enables us to make long statements short, and, at the same time, general. Taking the axiom about parallels for granted, it is shown that all points at an infinite distance in a line may be taken to be one point as far as constructions at a finite distance are concerned. For all lines joining a fixed point, P, to any point at infinity in a line may be taken as parallel to this line, and therefore as coincident. To express this more shortly, it is said that the whole indefinite and infinite part of a line which is out of the reach of our comprehension plays for us only the part of a single point, and accordingly it is called a "point," viz. *the point at infinity* of the line. Similarly it is shown that all points in a plane which are at an infinite distance may be considered as lying in one line, which is then spoken of as *the line at infinity* in the plane, and which is freely and safely used in deducing theorems and solving problems.

If, then, a line in a plane be moved to infinity, making always a given angle with a fixed line, it will ultimately become coincident with—which here means indistinguishable from—the line at infinity. The latter then makes with the fixed line a given angle. But this angle may be anything. Hence the "line at infinity" makes any angle we like with any given finite line; in other words, it makes no definite angle at all with it.

It follows, if we take a property of a figure which depends upon the magnitude of an angle, that this property will not necessarily any longer hold if one of the limits of the angle be moved to an infinite distance; for then this angle has not any longer a definite magnitude. To base any reasoning on that property after the figure has been indefinitely increased must therefore necessarily be fallacious. But this is exactly what Mr. Budden does. His proof is based on the implied assumption that if a figure in a plane be increased indefinitely, we can still reason upon it as if it were finite. He may take this as an *axiom*, but then he has replaced Euclid's axiom by another, and has not proved it; and the question would arise, Which form of the axiom is preferable? I prefer Euclid's.

O. HENRICI

Lunar Glaciation

I TRUST you will allow me a small space to explain regarding this theory of lunar glaciation, referred to by Mr. Darwin in NATURE (vol. xxxiv. p. 264).

First, I must thank him for the remarks made, and say that I certainly was not aware that Capt. Ericsson had been at work in the same direction some ten years or more before me.