LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions ex-
pressed by his correspondents. Neither can he undertake to
return, or to correspond with the writers of, rejected manu-
scripts. 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 télé-
graphiques 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 améri-
caines sont publiées dans le numéro 59 (1880, je crois) "Hydro-
graphic Notice," et les miennes dans les "Annales hydro-
graphiques, 1866."

	Para	Pernambouco	Bahia
	h. m. s.	h. m. s.	h. m. s.
Long. télégraphique.	3 23 20·94	2 28 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 eut
jamais présenté moins d'erreur absolue ou relative que la côte
du Brésil après la publication de mes cartes et de mes observa-
tions.

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 measur-
ing of aëriform 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 expec-
tation 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. With-
out 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 state-
ments short, and, at the same time, general. Taking the axiom
about parallels for granted, it is shown that all points at an in-
finite 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 compre-
hension plays for us only the part of a single point, and accord-
ingly 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 neces-
sarily 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.