58, 544-545

X

inions ex-
undertake
, rejected
NATURE.

ndeb.

by one of
the ac-
partment,
sis of the
e of your
ARTON.

ANDEB.

rdanelles
ea to the
ain height

any large
a have a

originally devised by Lieutenant Pilsbury, U.S.N., and considerably altered after a series of experiments by Captain Usborne Moore in the English and Færoe Channels, seemed to offer a chance of more success.

Lieutenant and Commander Gedge, commanding H.M. surveying ship *Stork*, was therefore directed to endeavour to get further observations in Bab-el-Mandeb by means of this instrument, and has admirably and most successfully carried them out.

On January 19, 1898, the *Stork* was anchored in 118 fathoms about seven miles S.W. by W. from Perim Island, and remained constantly observing, during daylight, for four days, when the parting of the cable brought the series to a close. Had not the wind been unusually light, varying from force 3 to 6, it is probable that the observations could not have been continued so long.

The observations are appended (in publication quoted), but the broad result may be briefly stated.

There was a permanent current on the surface setting into the Red Sea of about 1½ knots per hour.

There was at 105 fathoms depth a permanent current setting outwards of probably the same velocity.

The tidal stream was about 1½ knots at its maximum, and flowed for about twelve hours each way, as might be expected from the fact that in this locality there is practically only one tide in the day.

arge Strait of Bab-el-Mandeb by H.M.S. Stork *in January* 1898.

At 25 fms.		At 50 fms.		At 75 fms.		At 105 fms.	
Direction.	Rate.	Direction.	Rate.	Direction.	Rate.	Direction.	Rate.
N.W.	3	Slack	—	—	—	—	—
—	—	S. by E.	¼	Variable	—	—	—
N.W.	2	N.W. by W.	⅔	—	—	—	—
—	—	N.W. by N.	¼	N.N.W. ½ W.	½	—	—
N.N.W. ½ W.	2	N.W.	1⅔	N. ½ E.	1	S. by W.	¼
—	—	N.N.E.	½	N. by E. ½ E.	¾	S. by W.	1¼
N.W. ¾ W.	1	E. by S.	½	S.S.E.	⅔	South	1⅓
N.N.W.	½	West	1	—	—	S. E. ½ S.	1½
—	—	South	1½	S.E. by E.	1⅓	S.S.E. ½ E.	3
Slack.	—	S.S.E.	1	S.E.	1	S.S.E. ½ E.	2½
E. by N.	½	—	—	S.S.E. ½ E.	1½	E.S.E.	1¾
S.E.	½	S.E.	1	E. by S.	2¼	—	—
—	—	—	—	—	—	—	—
N.W. by N.	¾	—	—	E.S.E.	2	S.E. by E.	1¾

by which
cate that
E. wind
ack Sea,

he strait
ation on
ny years

s strong
into the
surface
nflow of
on the
n of the

is deep,
a tidal
whether
vel the
moother

ected to
s, while
surface,

meter,

This tidal stream prevails to the bottom, with variations of strength.

Somewhere about 75 fathoms is the dividing line between the two permanent currents, but it would require a longer series of observations to determine this point with any precision.

Fourier's Series.

IN all expositions of Fourier's series which have come to my notice, it is expressly stated that the series can represent a discontinuous function.

The idea that a real discontinuity can replace a sum of continuous curves is so utterly at variance with the physicists' notions of quantity, that it seems to me to be worth while giving a very elementary statement of the problem in such simple form that the mathematicians can at once point to the inconsistency if any there be.

Consider the series

$$y = 2\,[\sin x - \tfrac{1}{2}\sin 2x + \tfrac{1}{3}\sin 3x - \ldots]$$

In the language of the text-books (Byerly's "Fourier's Series and Spherical Harmonics") this series "coincides with $y = x$ from $x = -\pi$ to $x = \pi$ . . . Moreover the series in addition to the continuous portions of the locus . . . gives the isolated points $(-\pi, 0)$ $(\pi, 0)$ $(3\pi, 0)$, &c."

S. A. Tilling

If for x in the given series we substitute $\pi + \epsilon$ we have, omitting the factor 2,

$$-y = \sin\epsilon + \tfrac{1}{2}\sin 2\epsilon + \tfrac{1}{3}\sin 3\epsilon + \quad + \frac{1}{n}\sin n\epsilon + \ldots$$

This series increases with n until $n\epsilon = \pi$. Suppose, therefore, $\epsilon = k\dfrac{\pi}{n}$, where k is a small fraction. The series will now be nearly equal to $n\epsilon = k\pi$, a finite quantity even if $n = \infty$.

Hence the value of y in the immediate vicinity of $x = \pi$ is not an isolated point $y = 0$, but a straight line $-y = nx$. $-\pi$

The same result is obtained by differentiation, which gives

$$\frac{dy}{dx} = \cos x - \cos 2x + \cos 3x - \ldots$$

putting $x = \pi + \epsilon$ this becomes

$$-\frac{dy}{dx} = \cos\epsilon + \cos 2\epsilon + \cos 3\epsilon + \ldots + \cos n\epsilon +$$

which is nearly equal to n for values of $n\epsilon$ less than $k\pi$.

It is difficult to see the meaning of the tangent if y were an isolated point.

ALBERT A. MICHELSON.
The University of Chicago Ryerson Physical Laboratory,
September 6.

Helium in the Atmosphere.

C. FRIEDLÄNDER and H. Kayser have independently claimed to have found helium in the atmosphere. On examination of some photographs of the spectrum of neon I have identified six of the principal lines of helium, which thus establishes beyond question the presence of this gas in the air. The amount present in the neon it is, of course, impossible to estimate, but the green line (wave-length 5016) is the brightest, as would be expected from the low pressure of the helium in the neon.

E. C. C. BALY.
University College, London, Gower Street, W.C.,
September 28.

THE discovery of helium lines in the spectrum of neon, by Mr. E. C. C. Baly, will necessitate a modification of the views we have expressed in our communication to the British Association at Bristol. We there estimated the density of neon at 9·6, allowing for the presence of a certain proportion of argon unavoidably left in the neon. As it contains helium, however, this is probably an under-estimate. It is unfortunately not possible to form any estimate of the amount of helium mixed with the neon from the relative intensity of spectrum lines, as has been already shown by Dr. Collie and one of us; we do not despair, however, of removing a large part, if not all of this helium, by taking advantage of the greater solubility of neon than helium in liquid oxygen.

The presence of helium, however, in no way alters our view as to the position of neon in the periodic table. The number 9·6 implies an atomic weight of 19·2; and a somewhat higher atomic weight would even better suit a position between fluorine, 19, and sodium, 23.

WILLIAM RAMSAY.
MORRIS W. TRAVERS.
University College, London,
Gower-street, W.C., September 28.

Chance or Vitalism?

I AM glad to see that Prof. Karl Pearson has called attention to Prof. Japp's address at Bristol. Only that one does not like to criticise adversely a presidential address, I would at the time have pointed out the weakness in the argument that Prof. Pearson criticises. He does not go nearly so far in this criticism as the circumstances warrant. It is conceded that right- and left-handed crystals of quite sensible size are produced sufficiently separated to be seen and handled as separate crystals. Now assuming, what there is every reason otherwise to think quite probable, that life started from some few centres, the chances are, not that it was equally divided between right- and left-handed forms, but that one or other of these forms preponderated. In fact, if life started from a single centre, it *must* have been either right- or left-handed. Hence the fact adduced only shows, what was otherwise very probable, that life started from a small number of origins, possibly only one.

Another reason for ei
living organisms on the
force of the foregoing arg
that it probably started
hemisphere, and in eith
heavens may be a suffici
structure in an organism

Trinity College, Dubli

IN his presidential a
sociation, Prof. Japp a
"directive force," or in
of the first asymmetric
time was supposed to
mechanics of organised
the foreground, till, in
point in the vast distance
disappear altogether?
A sensible quantity o
an enormous number
relative proportion pre
Each molecule, having
forces, has an even cha
the improbability of the
of one form over the oth
that an optically activ
contention of Prof. Ja
Pearson, in NATURE
however improbable,
allowed. He postula
May we not, however
stead of molecules, and
Let us consider a so
left-handed molecules a
is consequently opticall
the solvent, the right-
substance crystallises,
Their number will be
of the molecules, and,
it is not so highly imp
the crystals will be un
place and a partial re-
tribution of the two va
event—and we have
here acted the part pl
of M. Pasteur, by sele
crystals.
Is it yet possible t
rotatory protein could
substance, separated
play of chance upon t

17 Denning Road,

T

MAY I refer Sir
written autobiography
mechanician and astro
"Soon afterwards"
to me, that although
the sun is far on the
motion must be in a
sun: and upon maki
path in the heavens—
simple machine for de
a long paper laid on
delineation to the lat
Royal Society, on a
satisfaction at seeing
that evening to the
lineation and the me
the Society was over,
with him the next
name was Ellicott, a
ingly went and was
showed me the very

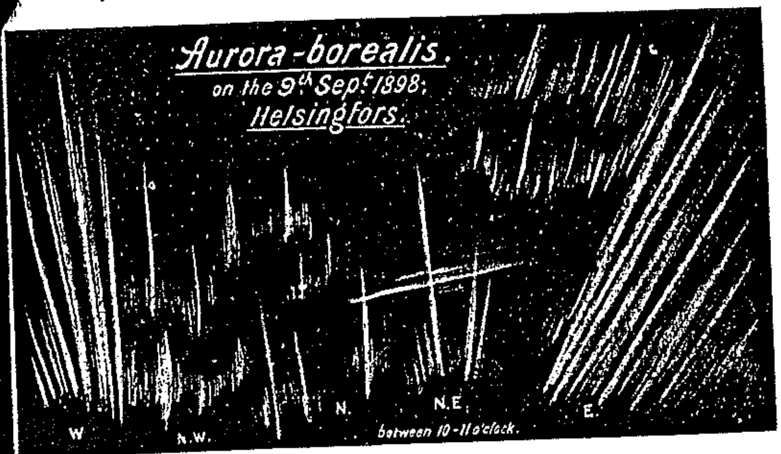
175

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 intended for this or any other part of NATURE. *No notice is taken of anonymous communications.*]

The Aurora Borealis of September 9.

I HAVE read, with much interest, in NATURE of September 15, the article concerning the aurora borealis of September 9, and it may be of interest to your readers to know that this



beautiful phenomenon displayed its splendours the same evening in all parts of Finland territory.

On that day I had the good fortune to see it in Helsingfors, from its earliest beginning to its end, in a clear, perfectly cloudless sky, and a calm and transparent air. These favourable conditions enabled me to sketch the principal movements of it, and I send you herewith a copy of the drawing I made.

The aurora was not only one of the most splendid that has been seen, but also that has appeared in our latitude for a long series of years. It began a little before 9 o'clock, and at 10 arrived at its maximum brilliancy, a state in which it, ever changing, remained till 11 o'clock, displaying the whole time an exceedingly beautiful brightness in all its parts.

The display began with a very bright arc in the north, but this very soon disappeared, while at the same moment exceedingly brilliant streamers extended at once up from the western and eastern horizons, sending immense columns to the zenith, and taking the shape of a colossal arc arching the whole sky from horizon to horizon. Masses of light flowed from both sides to the zenith, where they seemed to disappear. At 10 o'clock the great arc was interrupted on both sides by a dark region, the bright streamers remaining only on opposite horizons; but in the same moment a corona of the highest splendour appeared in the zenith, consisting of three nearly parallel streamers, stretching from west to east, and ending towards the west in the dark space, and towards the east in a beautiful fan of light.

Half an hour later the corona took the shape of an immense dome, the ribs and columns of which stood around all parts of the horizon. The whole visible sky at that moment presented one single enormous dome of indescribable beauty. The brightest columns of this dome were to the west and to the east, those to the north were much less bright, and the columns to the south were scarcely visible. From every part of this dome streamers of light, without interruption, flowed up to the zenith.

At 11 o'clock, when the dome suddenly disappeared, the corona took the shape of a luminous spiral-ring, sending short but very bright streamers in all directions, especially to the east. This latter formation was surrounded by quite black spaces of sky, which made the luminous phenomena look more beautiful.

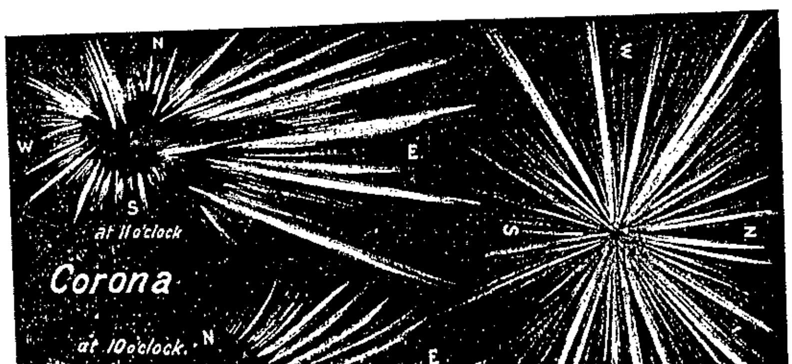
Meanwhile, in the northern part of the sky, the aurora took the shape of ever-changing columns, and long, sometimes spiral and undulating bands, which twice, in the north-west and in the north-east, doubled, resembling curtains hanging one over the other.

A little after eleven I saw in the north a very strange formation of aurora; three vertical columns in their upper part were crossed by a bright horizontal streamer, extending nearly from north-west to north-east.

Soon after 11.30 the aurora began to vanish everywhere, and, in a very marked manner, took more and more the aspect of some luminous shapeless cloud. After 12 o'clock all traces of columns and streamers disappeared, and at 1 o'clock nothing more of the phenomenon was to be seen.

N. KAULBARS.

Helsingfors, September 28.



Fourier's Series.

IN a letter to NATURE of October 6, Prof. Michelson, referring to the statement that a Fourier's series can represent a discontinuous function, describes "the idea that a real discontinuity can replace a sum of continuous curves" as "utterly at variance with the physicists' notions of quantity." If, as this seems to imply, there are physicists who hold "notions of quantity" opposed to the mathematical result that the sum of an infinite series of continuous functions may itself be discontinuous, they would be likely to profit by reading some standard treatise dealing with the theory of infinite series, such, for example, as Hobson's "Trigonometry," and the paper by Sir G. Stokes quoted on p. 251 of that work.

Prof. Michelson takes a particular case. He appears to find a difficulty in the result that the sum of the series

$$y = 2[\sin x - \tfrac{1}{2}\sin 2x + \tfrac{1}{3}\sin 3x - \ldots]$$

is equal to x when x lies between $-\pi$ and π, is equal to $-2\pi + x$ when x lies between π and 3π, and so on, and further is equal to zero when x is $-\pi$, or π, or 3π, and so on.

With the view of stating his difficulty simply, he has tried to sum this series, and the series obtained from it by differentiating its terms, for values of x of the form $\pi + \epsilon$, where it appears to be meant that ϵ is positive and less than 2π.

The series (thus obtained) for y and dy/dx are given by the equations

$$-\tfrac{1}{2}y = \sin\epsilon + \tfrac{1}{2}\sin 2\epsilon + \tfrac{1}{3}\sin 3\epsilon + \ldots + \frac{1}{n}\sin n\epsilon + \ldots$$

$$-\tfrac{1}{2}\frac{dy}{dx} = \cos\epsilon + \cos 2\epsilon + \cos 3\epsilon + \ldots + \cos n\epsilon + \ldots$$

176

Of the first series Prof. Michelson says: "This series increases with n until $n\epsilon = \pi$. Suppose therefore $\epsilon = k\pi/n$, where k is a small fraction. The series will now be nearly equal to $n\epsilon = k\pi$, a finite quantity even if $n = \infty$.

"Hence the value of y in the immediate vicinity of $x = \pi$ is not an isolated point $y = 0$, but a straight line $-y = nx$."

Of the second series he says that it "is nearly equal to n for values of $n\epsilon$ less than $k\pi$."

Neither of these statements is correct. The sum of the first series can be proved to be $\frac{1}{2}(\pi - \epsilon)$ when ϵ lies between 0 and 2π, and $-\frac{1}{2}(\pi + \epsilon)$ when ϵ lies between 0 and -2π, and it is zero when $\epsilon = 0$. The sum of n terms of the second series does not approach to any definite limit, as n is increased indefinitely; nor does the difference between the sum of this second series to n terms and the number n tend to zero or any finite limit, but the ratio of the sum to n terms and the number n tends to the definite limit zero as n is increased indefinitely.

The processes employed are invalid. It is not the case that the sum of an infinite series is the same as the sum of its first n terms, however great n is taken. It is not legitimate to sum an infinite series by stopping at some convenient nth term. It is not legitimate to evaluate an expression for a particular value of x, *e.g.* $x = \pi$, by putting $x = \pi + \epsilon$ and passing to a limit; to do so is to assume that the expression represents a continuous function. It is not legitimate to equate the differential coefficient of the sum of an infinite series to the sum of the differential coefficients of its terms; in particular the series given as representing dy/dx in the example is not convergent.

Lastly, Prof. Michelson says "it is difficult to see the meaning of the tangent if y were an isolated point." The tangent, at a point, to a curve, representing a function, has of course no meaning, unless the function has a differential coefficient, for the value corresponding to the point; and a function which has a differential coefficient, for any value of a variable, is continuous in the neighbourhood of that value.

St. John's College, Cambridge, October 7. A. E. H. LOVE.

Helium in the Atmosphere.

THE letter of Mr. Baly in your issue of last week, corroborating the statement of Friedländer and Kayser that helium is a constituent of the atmosphere, induces me to put on record a further confirmation of the accuracy of this observation. Having had the opportunity, on June 20 last, of examining samples of the more volatile portions from liquid air, which had been handed to me by Prof. Dewar, I had no difficulty in seeing the lines of helium in them. Further, a sample of the helium separated by Prof. Dewar from Bath gas (following the discovery of Lord Rayleigh) undoubtedly contained the substance called neon.

In giving these facts I am only confirming the observations of Prof. Dewar given to me in letters accompanying the samples of gas. WILLIAM CROOKES.

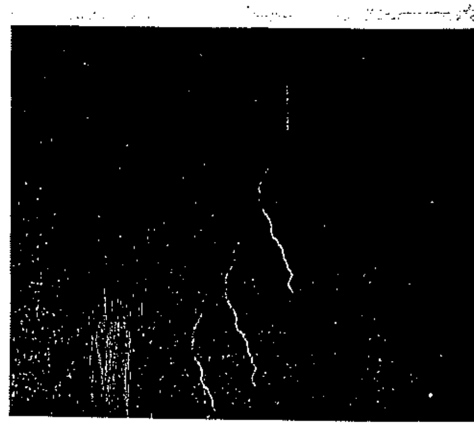
October 11.

Triplet Lightning Flash.

AT the suggestion of Lord Kelvin, I send you the enclosed photograph of a triplet lightning flash which was taken during a recent thunderstorm at Whitby, and under the following conditions.

The flash must have been about two miles distant (out at sea). The focus of the camera lens was 8 inches; the aperture, $f/64$; the plate, Ilford Empress. The camera was not stationary, but was purposely oscillated by hand. It was intended that its axis should describe a circular cone, but from the photograph the path appears to have been rather elliptical. Each revolution occupied about 1/80 minute. From these rough data I estimate that the three flashes followed each other with a frequency of about 30 to 35 per second. They are identical in shape, but the top part of the lowest (left-hand) one is missing, and the bottom is screened. On the negative the centre flash is rather weaker than the other two. Each flash is sharply defined on the left edge and somewhat hazy on the right edge, due probably to the gradual cooling of the glowing gases, and showing that the lowest (left-hand) flash is the first of the three. The photograph also contains a faint image of a single flash. During this thunderstorm two other plates were exposed under the same conditions as the above, but no images were found on them. Possibly the lightning was too far off, and the aperture too small.

In view of the importance of obtaining more definite information about lightning, I would suggest that in the presence of a thunderstorm photographs similar to the above should be taken. Greater accuracy than was possible under the above conditions could be attained by rigging up the following simple contrivance. An ordinary bedroom looking-glass should be placed on a table in front of an open window facing the storm. The mirror should be inclined at any angle of 45°. The camera tripod, with its legs spread as wide apart as possible, should be placed on the table so that its centre is over the looking-glass. The camera, with its objective downwards, should be suspended from this centre by means of three strings, and should be made to swing in a circle by a gentle finger pressure close to the point of suspension. The period of revolution should be noted. Should any multiple flash imprint itself on the negative, it will now be possible to accurately measure the intervals of time, except



under the following conditions. If there are only two flashes, the radius of the circle described by the camera can only be guessed at. If the camera has described an ellipse, at least four lightning images are required to find its elements. A camera revolving on an axis passing through the objective would in some respects be more convenient to work with, but unless it is revolved by clock-work the time measurements would not be trustworthy. The aperture used by me, $f/64$, is probably too small except for very brilliant flashes; but if it is intended to allow several discharges to imprint themselves on one negative, a very large aperture will be found inconvenient because of the illumination of the landscape. The size of the aperture, rapidity of plate, and distance of each lightning flash should be noted to assist at forming some idea as to the heat generated.

C. E. STROMEYER.

Lancefield, West Didsbury, October 3.

The Centipede-Whale.

THE "Scolopendrous Millipede," which forms the subject for the epigrams of Theodoridas and Antipater, and to which Mr. W. F. Sinclair kindly called my attention (NATURE, vol. lvi. p. 470), seems to mean a being quite different from the "Centipede-Whale" which Ælian and Kaibara describe (see my letter, *ibid.*, p. 445); for the former apparently points to a huge skeleton of some marine animal, while the latter is an erroneous but vivid portrait of an animal actively swimming with numerous fins.

Major R. G. Macgregor, in his translation of the Greek Anthology (1864, p. 265), remarks upon the "Scolopendrous Millipede" that the "word *millipede* must be understood rather in reference to the extreme length of the monster than to the number of its feet." However, it would appear more likely that, in this similitude of the animal remains to the Myriapod, the numerous articulations of the vertebral column as well as its length played a principal part, should we take for comparison the following description of an analogous case from a Chinese work (Li Shih, "Süh-poh-wuh-chi," written thirteenth century, Japan. ed., 1683, tom. x. fol. 6, *b.*):—"Li Mien, a high officer (eighth century), during his stay in Pien-Chau, came in possession of one joint of a monstrous bone, capable of the use as [illegible]

177

(see next page)

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 intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Fourier's Series.

IN reply to Mr. Love's remarks in NATURE of October 13, I would say that in the series

$$y = \sin x + \tfrac{1}{2}\sin 2x + \ .\ .\ .\ + \frac{1}{n-1}\sin (n-1)x + \frac{1}{n}\sin nx,$$

in which $\frac{1}{n}\sin nx$ is the last term considered, x must be taken smaller than π/n in order to find the values of y in the immediate vicinity of $x = 0$.

If it is inadmissible to stop at "any convenient nth term," it is quite as illogical to stop at the equally "convenient" value π/n.

ALBERT A. MICHELSON.

The University of Chicago Ryerson Physical Laboratory, Chicago, December 1.

I SHOULD like to add a few words concerning the subject of Prof. Michelson's letter in NATURE of October 6. In the only reply which I have seen (NATURE, October 13), the point of view of Prof. Michelson is hardly considered.

Let us write $f_n(x)$ for the sum of the first n terms of the series

$$\sin x - \tfrac{1}{2}\sin 2x + \tfrac{1}{3}\sin 3x - \tfrac{1}{4}\sin 4x + \&c.$$

I suppose that there is no question concerning the form of the curve defined by any equation of the form

$$y = 2f_n(x).$$

Let us call such a curve C_n. As n increases without limit, the curve approaches a limiting form, which may be thus described. Let a point move from the origin in a straight line at an angle of 45° with the axis of X to the point (π, π), thence vertically in a straight line to the point $(\pi, -\pi)$, thence obliquely in a straight line to the point $(3\pi, \pi)$, &c. The broken line thus described (continued indefinitely forwards and backwards) is the limiting form of the curve as the number of terms increases indefinitely. That is, if any small distance d be first specified, a number n' may be then specified, such that for every value of n greater than n', the distance of any point in C_n from the broken line, and of any point in the broken line from C_n, will be less than the specified distance d.

But this limiting line is not the same as that expressed by the equation

$$y = \lim_{n=\infty} 2f_n(x).$$

The vertical portions of the broken line described above are wanting in the locus expressed by this equation, except the points in which they intersect the axis of X. The process indicated in the last equation is virtually to consider the intersections of C_n with fixed vertical transversals, and seek the limiting positions when n is increased without limit. It is not surprising that this process does not give the vertical portions of the limiting curve. If we should consider the intersections of C_n with horizontal transversals, and seek the limits which they approach when n is increased indefinitely, we should obtain the vertical portions of the limiting curve as well as the oblique portions.

It should be observed that if we take the equation

$$y = 2f_n(x),$$

and proceed to the limit for $n = \infty$, we do not necessarily get $y = 0$ for $x = \pi$. We may get that ratio by first setting $x = \pi$, and then passing to the limit. We may also get $y = 1$, $x = \pi$, by first setting $y = 1$, and then passing to the limit. Now the limit represented by the equation of the broken line described above is not a special or partial limit relating solely to some special method of passing to the limit, but it is the complete limit embracing all sets of values of x and y which can be obtained by any process of passing to the limit.

J. WILLARD GIBBS.

New Haven, Conn., November 29.

FOURIER'S series arises in the attempt to express, by an infinite series of sines (and cosines) of multiples of x, a function of x which has given values in an interval, say from $x = -\pi$ to $x = \pi$. There is no "curve" in the problem. Curve in the solution of the problem, and there they occur b of illustration. There are two sorts of curves which occ the first place, taking $\phi(x)$ as the function to be expre the series, and $f(x)$ as the sum of the series, we have the $y = \phi(x)$ and $y = f(x)$, the graphs of the two functions. coincide wherever the series expresses the function; but function $\phi(x)$ is one which cannot be expressed by a F series for all values of x in the interval, the curves d incide throughout the interval. In the second place, $f_n(x)$ as the sum of the first n terms of the series, we h family of curves $y = f_n(x)$, the graphs of $f_n(x)$ for d values of n. As n increases the graphs of $f(x)$ and f proach to coincidence in the sense that, if any particular of x is taken, and any small distance d is specified, a n' may then be specified such that for every n greater th the difference of the ordinates of the two curves is less But this is not the same thing as saying that the curve to coincide geometrically, and they do not in fact l each other in the neighbourhood of a finite disconti $\phi(x)$. It is usual to illustrate the tendency to disconti $f(x)$ by noting the form of the curve $y = f_n(x)$ for large of n, but the shape of this curve always fails to give an cation of the sum of the series for the particular values of which $\phi(x)$ and $f(x)$ are discontinuous. This is the ca the example cited by Prof. Willard Gibbs, where all par values between $-\pi$ and π are equally indicated by the $y = f_n(x)$, but the sum of the series is precisely zero.

May I point out that there is some ambiguity in the pression "the limiting form of the curve" used by Willard Gibbs? Taking his example, it is quite true can be taken so great that, for every n greater than n', a point of C_n within the given distance d of any point broken line, but this statement is not quite complete. also true that a number n can be taken great enough to the point of C_n on *any assigned* ordinate within the give tance d of its *ultimate position* on the broken line, but further essential to observe that no number n can be great enough to bring *every* point of C_n within the give tance d of its ultimate position on the broken line. The n which succeeds for any one ordinate always fails for other ordinate. Suppose, to fix ideas, that we take a point C_n for which $y = 1$, and x is nearly π, so that $\pi - x$ is les d, and keeping x fixed, observe how y changes when n incr it will be found that, for values of m very much greater n, the ordinate of C_m, for this x is very nearly π, and w in fact take m great enough to make this ordinate lie betw and $\pi - d$. In words, the representative point, which begi nearly coinciding with a point on a vertical part of the broke creeps along the line, and ends by coinciding with a point o oblique part of the broken line. This will be the case for value of x, near $x = \pi$, with the single exception of the v Thus, in the passage to the limit, every point near the v part of the broken line disappears from the graph, excep points on the axis of x. This peculiarity is always prese a series whose sum is discontinuous; in the neighbourho the discontinuity the series does not converge uniformly, o graph of the sum of the first n terms is always appre different from the graph of the limit of the sum.

In this way the graph of the sum of the first n terms indicate the behaviour of the function expressed by the l this sum, and we may illustrate the distinction between the as Prof. Willard Gibbs does, by considering the intersect the graph with lines parallel to the axis of x. Keeping say $y = 1$, we may find, in his example, a number n, there is a corresponding value of x differing from π by le d, and then, allowing n to increase indefinitely, we shall series of values of x, having π as limiting value. But this ing value is *not attained.* In Prof. Willard Gibbs's no the equation $2f_n(x) = 1$ has a root near to π when n is and n can be taken so great that the root differs from π b than any assigned fraction; but the equation

$$\lim_{n=\infty} 2f_n(x) = 1$$

has no real root. In fact Prof. Willard Gibbs's "limiting of the curve" corresponds to limits which are not attained the limiting form in which the vertical portions of the b line are replaced by the points where they cut the axi corresponds to limits which are effectively attained. It

O THE EDITOR.

imself responsible for opinions ex-
dents. Neither can he undertake
ond with the writers of, rejected
this or any other part of NATURE.
nymous communications.]

er's Series.

narks in NATURE of October 13, I

$$\ldots + \frac{1}{n-1}\sin(n-1)x + \frac{1}{n}\sin nx,$$

erm considered, x must be taken
nd the values of y in the immediate

at "any convenient nth term,"
op at the equally "convenient"

ALBERT A. MICHELSON.

Ryerson Physical Laboratory,
December 1.

' words concerning the subject of
TURE of October 6. In the only
TURE, October 13), the point of
dly considered.
sum of the first n terms of the

sin $3x - \frac{1}{4}\sin 4x$ + &c.

stion concerning the form of the
of the form

$2f_n(x)$.

As n increases without limit,
ing form, which may be thus
from the origin in a straight line
of X to the point (π, π), thence
e point $(\pi, -\pi)$, thence obliquely
$(3\pi, \pi)$, &c. The broken line
nitely forwards and backwards)
urve as the number of terms
if any small distance d be first
e then specified, such that for
n', the distance of any point
of any point in the broken line
ecified distance d.
e same as that expressed by the

$$\infty \quad 2f_n(x).$$

roken line described above are
by this equation, except the
axis of X. The process indi-
lly to consider the intersections
ersals, and seek the limiting
hout limit. It is not surprising
vertical portions of the limit-
er the intersections of C_n with
he limits which they approach
we should obtain the vertical
vell as the oblique portions.
take the equation

x),

o, we do not necessarily get
it ratio by first setting $x = \pi$,
e may also get $y = 1$, $x = \pi$,
ssing to the limit. Now the
of the broken line described
imit relating solely to some
imit, but it is the complete
s of x and y which can be
to the limit.

J. WILLARD GIBBS.

29.

ttempt to express, by an in-
of multiples of x, a function
interval, say from $x = -\pi$ to $x = \pi$. There is no "curve" in the problem. Curves occur in the solution of the problem, and there they occur by way of illustration. There are two sorts of curves which occur. In the first place, taking $\phi(x)$ as the function to be expressed by the series, and $f(x)$ as the sum of the series, we have the curves $y = \phi(x)$ and $y = f(x)$, the graphs of the two functions. These coincide wherever the series expresses the function; but, if the function $\phi(x)$ is one which cannot be expressed by a Fourier's series for all values of x in the interval, the curves do not coincide throughout the interval. In the second place, taking $f_n(x)$ as the sum of the first n terms of the series, we have the family of curves $y = f_n(x)$, the graphs of $f_n(x)$ for different values of n. As n increases the graphs of $f(x)$ and $f_n(x)$ approach to coincidence in the sense that, if any *particular value* of x is taken, and any small distance d is specified, a number n' may then be specified such that for every n greater than n', the difference of the ordinates of the two curves is less than d. But this is not the same thing as saying that the curves tend to coincide geometrically, and they do not in fact lie near each other in the neighbourhood of a finite discontinuity of $\phi(x)$. It is usual to illustrate the tendency to discontinuity of $f(x)$ by noting the form of the curve $y = f_n(x)$ for large values of n, but the shape of this curve always fails to give an indication of the sum of the series for the particular values of x for which $\phi(x)$ and $f(x)$ are discontinuous. This is the case in the example cited by Prof. Willard Gibbs, where all particular values between $-\pi$ and π are equally indicated by the curve $y = f_n(x)$, but the sum of the series is precisely zero.

May I point out that there is some ambiguity in the expression "the limiting form of the curve" used by Prof. Willard Gibbs? Taking his example, it is quite true that n' can be taken so great that, for every n greater than n', there is a point of C_n within the given distance d of any point on the broken line, but this statement is not quite complete. It is also true that a number n can be taken great enough to bring the point of C_n on *any assigned* ordinate within the given distance d of its *ultimate position* on the broken line, but it is further essential to observe that no number n can be taken great enough to bring *every* point of C_n within the given distance d of its ultimate position on the broken line. The number n which succeeds for any one ordinate always fails for some other ordinate. Suppose, to fix ideas, that we take a point on C_n for which $y = 1$, and x is nearly π, so that $\pi - x$ is less than d, and keeping x fixed, observe how y changes when n increases; it will be found that, for values of m very much greater than n, the ordinate of C_m, for this x is very nearly π, and we can in fact take m great enough to make this ordinate lie between π and $\pi - d$. In words, the representative point, which begins by nearly coinciding with a point on a vertical part of the broken line, creeps along the line, and ends by coinciding with a point on the oblique part of the broken line. This will be the case for every value of x, near $x = \pi$, with the single exception of the value π. Thus, in the passage to the limit, every point near the vertical part of the broken line disappears from the graph, except the points on the axis of x. This peculiarity is always presented by a series whose sum is discontinuous; in the neighbourhood of the discontinuity the series does not converge uniformly, or the graph of the sum of the first n terms is always appreciably different from the graph of the limit of the sum.

In this way the graph of the sum of the first n terms fails to indicate the behaviour of the function expressed by the limit of this sum, and we may illustrate the distinction between the two, as Prof. Willard Gibbs does, by considering the intersections of the graph with lines parallel to the axis of x. Keeping y fixed, say $y = 1$, we may find, in his example, a number n, so that there is a corresponding value of x differing from π by less than d, and then, allowing n to increase indefinitely, we shall get a series of values of x, having π as limiting value. But this limiting value is *not attained*. In Prof. Willard Gibbs's notation, the equation $2f_n(x) = 1$ has a root near to π when n is great, and n can be taken so great that the root differs from π by less than any assigned fraction; but the equation

$$\lim_{n=\infty} 2f_n(x) = 1$$

has no real root. In fact Prof. Willard Gibbs's "limiting form of the curve" corresponds to limits which are not attained; but the limiting form in which the vertical portions of the broken line are replaced by the points where they cut the axis of x corresponds to limits which are effectively attained. It is the latter limiting form, and not the former, which is the graph of the sum of the Fourier's series.

The matter here discussed is perhaps that referred to by Prof. Michelson in NATURE of October 6, but I did not understand his letter so. In regard to his present communication, I agree with him if he means that it is just as necessary, in tracing the part of the curve C_n near the vertical part of the broken line, to take a particular value of n, as it is to keep x within a narrow range of values corresponding to n. But this admission is not equivalent to admitting that an infinite series may be summed by stopping at any particular term. Rather it confirms the conclusion, explained above, that the graph of the sum of the infinite series contains no vertical line.

December 22. A. E. H. LOVE.

The Schmidt-Dickert Relief Model of the Moon.

THE present location of the Schmidt-Dickert relief model of the moon is probably not generally known in Europe. Webb's "Celestial Objects for Common Telescopes" (edition of 1896) states that the model is in Bonn, and this impression probably generally prevails. As a matter of fact the model has been for about twenty years in America. It has been on exhibition only at rare intervals during the time, however, and hence has been lost sight of. By a disposition recently made of it, it has fortunately become available to students of science and the public generally. Through the generosity of Mr. Lewis Reese, of Chicago, it has been presented to the Field Columbian Museum, and is now installed in this institution.

The model is in the form of a hemisphere about nineteen feet in diameter, and upon its surface are shown, in proportional relief, over 20,000 distinct localities. In his original description, Dr. Schmidt, the eminent selenographer, states that the details were based on the chart of Beer and Madler, but many features were added from his own observations. He also states that he carefully guided and watched over the work of construction, and with his own hand tested its correctness in all essential particulars. These statements give sufficient assurance of the accuracy of the model, and the confidence with which it may be studied. It is probably the best substitute extant for a trip to the moon.

OLIVER C. FARRINGTON.

Field Columbian Museum, Chicago, December 12.

Maxwell's Logic.

IN a paper on the experimental verification of Ohm's law (Brit. Assoc. Report, 1876), Maxwell makes the following statement.

"Assume that the resistance of a given conductor, at a given temperature, is a function of the strength of the current. Since the resistance of a conductor is the same for the same current, in whichever direction the current flows, the expression for the resistance can contain only even powers of the current."

It seems to me that such an argument is not applicable to a case of this kind.

Consider, for example, the flow of a liquid along a capillary tube. We might define the resistance of any portion A B of such a tube to be the ratio of the difference of pressure between A and B to the quantity of liquid flowing across any section in unit time.

Now would it not be equally legitimate to apply the above reasoning to this case, and prove that the resistance of a capillary tube could not vary as the first power of the velocity? Although of course, there may be no physical analogy between flow of liquid and electric current. Again, imagine a uniform wire A B along which a current of electricity is flowing, the ends A and B dipping into mercury cups (say). Now, instead of reversing the direction of the current, let the wire be turned end for end. Surely there is no difference between this and the previous case, and yet the current in the wire is reversed.

JOHN LISTER.

Royal College of Science, London, South Kensington,
S.W., December 12.

LORD IVEAGH'S GIFT.

THE announcement, made in the daily papers last week, of Lord Iveagh's intention to devote the princely sum of 250,000*l*. to the endowment and promotion of bacteriological research in England, has arrested the attention of the country and of every class

This, with the angular velocity of the earth inductor, is all we need for determining the absolute measure of the resistance, since we know by calculation the coefficient of mutual induction between the primary and secondary of the transformer.

The method has some advantages. The value of the earth's field need not be constant. Thermo-currents make no difference, as we are using A.C. voltages, and these may be taken very large compared with any possible thermo-effect in the primary. The same coils would be used for determining both the ohm and ampere, so that any error in calculating the coefficients for them would affect both units. Modifications will readily suggest themselves; as, for instance, two sets of such coils, one on each arm of a balance, and the movable coils acting both as secondary and as the movable coil of a Kelvin balance.

REGINALD A. FESSENDEN.

Western University of Pennsylvania, April 3.

Fourier's Series.

I SHOULD like to correct a careless error which I made (NATURE, December 29, 1898) in describing the limiting form of the family of curves represented by the equation

$$y = 2 (\sin x - \tfrac{1}{2} \sin 2x \;\ldots\; \pm \frac{1}{n} \sin nx) \;\ldots \quad (1)$$

as a zigzag line consisting of alternate inclined and vertical portions. The inclined portions were correctly given, but the vertical portions, which are bisected by the axis of X, extend beyond the points where they meet the inclined portions, their total lengths being expressed by four times the definite integral

$$\int_0^{\pi} \frac{\sin u}{u} du.$$

If we call this combination of inclined and vertical lines C, and the graph of equation (1) C_n, and if any finite distance d be specified, and we take for n any number greater than $100/d^2$, the distance of every point in C_n from C is less than d, and the distance of every point in C from C_n is also less than d. We may therefore call C the limit (or limiting form) of the sequence of curves of which C_n is the general designation.

But this limiting form of the graphs of the functions expressed by the sum (1) is different from the graph of the function expressed by the limit of that sum. In the latter the vertical portions are wanting, except their middle points.

I think this distinction important; for (with exception of what relates to my unfortunate blunder described above), whatever differences of opinion have been expressed on this subject seem due, for the most part, to the fact that some writers have had in mind the *limit of the graphs*, and others the *graph of the limit* of the sum. A misunderstanding on this point is a natural consequence of the usage which allows us to omit the word *limit* in certain connections, as when we speak of the sum of an infinite series. In terms thus abbreviated, either of the things which I have sought to distinguish may be called the graph of the sum of the infinite series.

J. WILLARD GIBBS.

New Haven, April 12.

Tasmanian Firesticks.

WHILE preparing for a second edition of the "Aborigines of Tasmania, I received from Mr. Jas. Backhouse Walker, of Hobart, two separate accounts of fire-making by the aborigines, which differ materially from those already known. The accounts come from two very old colonists, Mr. Rayner and Mr. Cotton, and describe fire as being obtained by means of the stick and groove process. Mr. Rayner's account runs thus: "A piece of flat wood was obtained, and a groove was made the full length in the centre. Another piece of wood about a foot in length, with a point like a blunt chisel, was worked with nearly lightning rapidity up and down the groove till it caught in a flame. As soon as the stick caught in a blaze, a piece of burnt fungus, or *punk*, as it is generally termed, was applied, which would keep alight. I cannot say what kind of wood it was. My father has seen them light it. The piece with the groove, he said, was hard, the other soft. The blacks in Australia get fire by the same method. I have seen that done. I think it almost impossible for a white man to do it, for I have seen it tried, and always prove a failure." Cotton's account agrees in the main with Rayner's. We are thus in possession of accounts of three distinct methods of fire production, viz.: (1) flint and tinder; (2) fire drill and socket; (3) stick and groove. At first sight it may appear incredible that a race so low in culture could have known and used these methods; nevertheless such a supposition might occur, for some neighbouring tribes in Australia are known to have at least two methods. As regards the Tasmanians, we may, I think, leave out of consideration the flint process, as both Furneaux and La Billardière seem to have mistaken so-called flint implements for fire flints. We may also eliminate indefinite accounts which simply refer to the process used as one of rubbing two sticks together, although rubbing describes rather the stick and groove method than the drill process. We may also omit the statement about the fire-drill supplied by Bomirck's bushranger as being untrustworthy. We are thus left with the two specimens of fire-drill (in the Pitt-Rivers Museum, Oxford, and in the possession of Sir John Lubbock, respectively) supplied by Dr Milligan and Protector Robinson, with Melville's description and with A. H. Davies' description. When Melville published his V. D. Almanac in 1833, he gave a short account of the aborigines, but to fire-making he made no reference at all; when he wrote his "Present State of Australia" (mostly an account of Tasmania), printed in London in 1850, he described the drill method of making fire as having been used by the Tasmanians. But, in the meanwhile, Davies, writing in 1845 in the *Tasm. Journ. of Sci.*, says he is "informed" that the Tasmanians raised fire by the drill process. But this statement, on hearsay, was made long after the aborigines had been deported to Flinders Island (1837), and after they had long been familiar with Australian aborigines imported into Tasmania; so that, although his statements may in general be relied on, this one wants confirmatory support, especially as his statement is the first one describing the drill process as being a Tasmanian method. Melville's account appears to me to be taken from Davies. Milligan knew nothing of the aborigines until 1847, when he was put in charge of them at Oyster Cove after their return from Flinders Island, and at a time when it was not likely that, in close proximity to European settlements, they would have continued to produce fire by native methods. Although we are much indebted to Milligan for the vocabularies, on the other hand there is considerable carelessness in his translation of the native sentences, and it is well known locally he was not interested in his charge. Hence his presentation to Barnard Davies of a fire drill as a Tasmanian instrument does not prove the drill to have been Tasmanian. Robinson, in spite of his intimate intercourse with the aborigines, and his voluminous reports of his doings while capturing the wretched remnants, has left us such a comparatively small amount of information concerning them, that I have for a long time past come to the conclusion that he was a very unobservant man, an opinion largely confirmed by his presentation to Barnard Davies of ground Australian stone implements as Tasmanian, but the real origin of which was settled as Australian by Prof. Tylor's paper on the subject read at the Oxford meeting of the British Association. As Robinson was afterwards Protector of Aborigines in Victoria, it is not at all unlikely that he confused his specimens, and called them Tasmanian instead of Australian. On the other hand, we have circumstantial accounts of stick and groove fire-making apparatus by two settlers, well advanced in years, who carry us back to the early part of the century when the natives were still roaming about the country before they were wholly robbed of it, and to a time when they had been little in touch with Australians or Europeans. Either there were two methods of fire-production used by the natives, or the stick and groove was the only one.

H. LING ROTH.

Halifax, England, April 13.

WIRELESS TELEGRAPHY.

ALTHOUGH at the present moment there is not a single commercial line of the so-called wireless telegraphy at work, and probably not a single penny has yet been earned by those exploiting it, the one pound shares of the Company have been quoted at six pounds and perhaps more. At the same time the shares of many of the Submarine Cable Companies have fallen considerably owing to the popular delusion that wireless telegraphy is going to displace wires. Thus a popular scare—the outcome of ignorance—has appreciated the one property and depreciated the other to the value of about

180