Local Features of Global Expansions

presented by

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Spectral Accuracy

Let $S_N f(x)$ **denote the Fourier projection of a** 2π **periodic function:**

$$\begin{aligned} S_N f(x) &= \sum_{|k|\leq N} \hat{f}(k) e^{ikx} \\ \hat{f}(k) &= \frac{1}{2\pi}\int_{-\pi}^{\pi} f(x) e^{-ikx} dx \end{aligned}$$

Then we know (Tadmor, Acta Numerica 2007)

- **The error depends only on the global smoothness of** $f(x)$

$$|S_N f(x) - f(x)| \leq ||f||_{C^s} \frac{1}{N^{s-1}}$$

  Where $||f||_{C^s} = \max_{|k|\leq s} ||f^{(k)}||_{L^\infty}$

- **The Fourier coefficients decay fast**

$$\hat{f}(k) \leq A||f||_{C^s} \frac{1}{1+|k|^s}$$

For functions in *Gevrey class* G_α

$$||f||_{C^s} \sim \frac{(s!)^\alpha}{\eta^\alpha}$$

We have

- $$|S_N f(x) - f(x)| \leq ANe^{-\alpha(\eta N)^{\frac{1}{\alpha}}}$$

- $$\hat{f}(k) \sim e^{-\alpha\eta|k|^{\frac{1}{\alpha}}}$$

In particular if $\alpha = 1$, $f(x)$ is analytic and

- $$\hat{f}(k) \sim e^{-\eta|k|}$$
  $$|S_N f(x) - f(x)| \sim Ne^{-\eta N}$$

Spectral Accuracy - the General Case

Consider an orthonormal family $\{\Psi_k(x)\}$, under a scalar product $(\cdot, \cdot)$. Denote

$$f_N(x) \;=\; \sum_{k=0}^{N}(f, \Psi_k)\Psi_k(x)$$

We have spectral accuracy if

- $$||f - f_N|| \le A||f||_{C^s}\frac{1}{N^{s-1}}$$

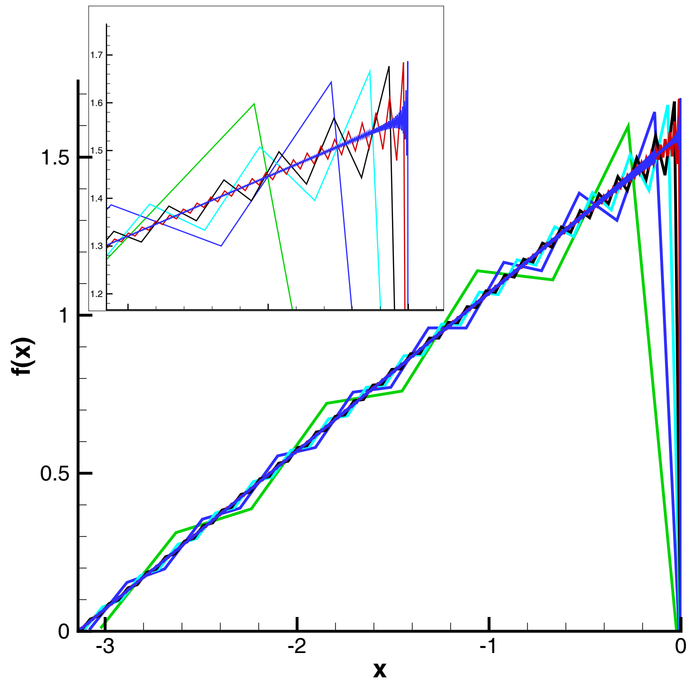
- $$(f, \psi_k) \sim ||f||_{C^s}\frac{1}{N^s}$$

In particular for an analytic function f_N converges exponentially to f.

- **Examples are Chebyshev and Legendre polynomials, also general Jacoby polynomials.**

Gibbs Phenomenon

If the solutions are discontinuous (Shock), we have the *Gibbs* Phenomenon.



Remark :

The order of accuracy for the high order schemes are reduced to $O(1)$.

We have to recover a density function $f(x, y)$ from its Radon Transform $p(r, \theta)$,

$$p(r, \theta) = \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} f(x, y)\delta(x \cos(\theta) + y \sin(\theta) - r) \, dx \, dy$$

The Slice Theorem:

$$\hat{p}(\rho, \theta) = \hat{f}(\rho \cos \theta, \rho \sin \theta)$$

where

- **$\hat{p}$ is the Fourier Transform (in r) of p,**
- **$\hat{f}$ is the two dimensional Fourier Transform of f.**

The DFM

$$p \longrightarrow \hat{p} \longrightarrow \hat{f} \longrightarrow f$$

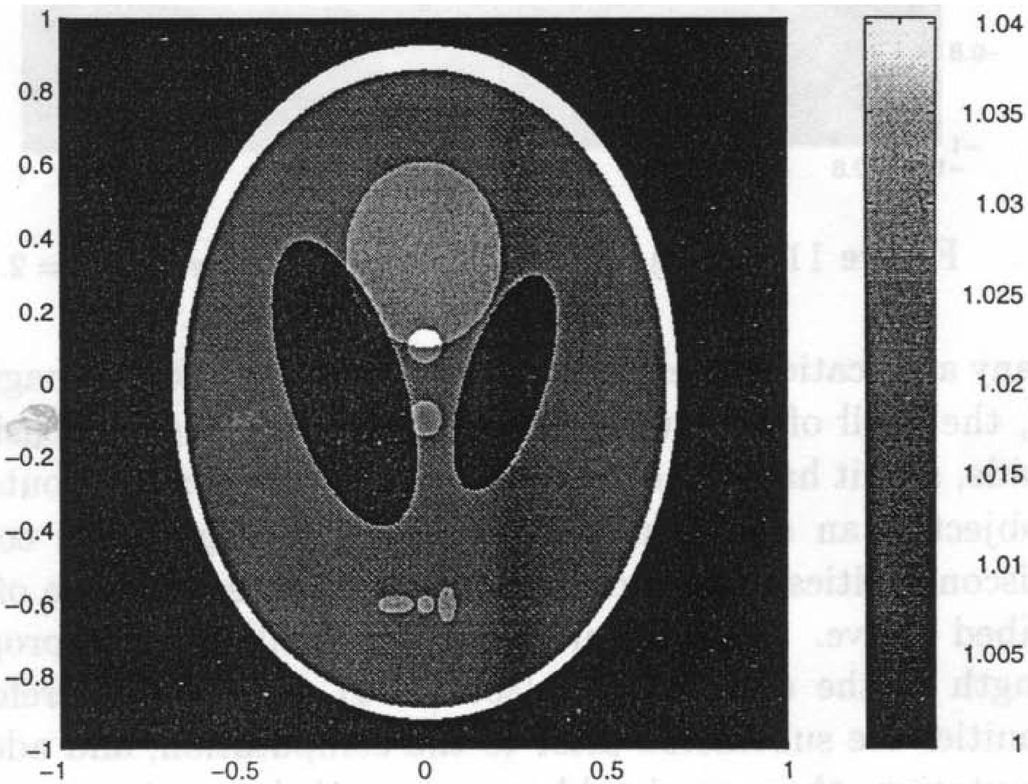


Figure 12: The Shepp-Logan phantom.

The domain $\{-1 \leq r \leq 1\,,\ 0 \leq \theta < \pi\}$ is discretized using a 512×256 grid, and the corresponding line integrals $p(r, \theta)$ are computed by using the trapezoidal rule with 2048 points along each line.

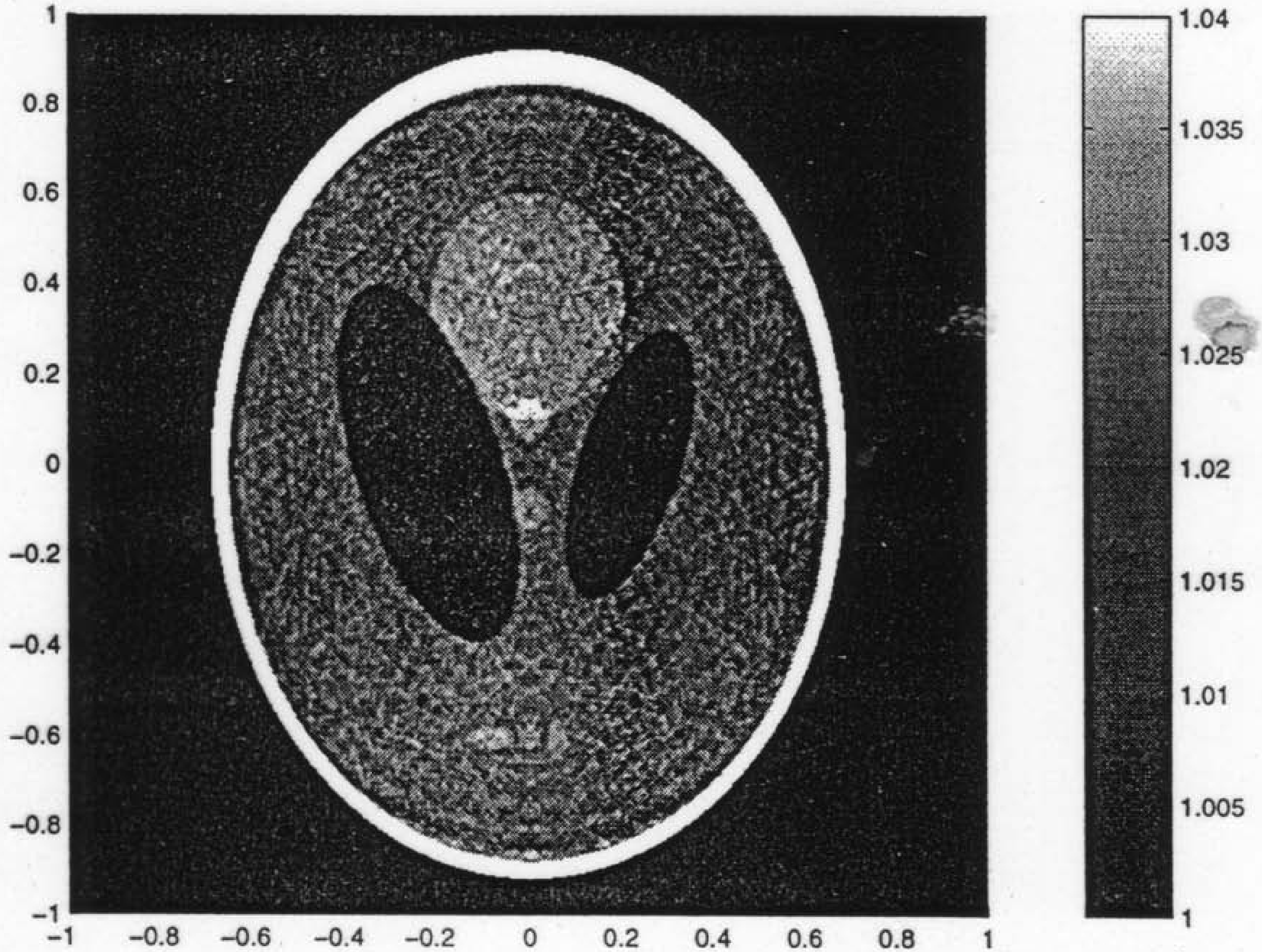


Figure 17: Shepp-Logan, $N = 512$, $m_1 = 19$, $m_2 = 13$, $\alpha = 5$, $\beta = 2$, l_2-error=0.0384.

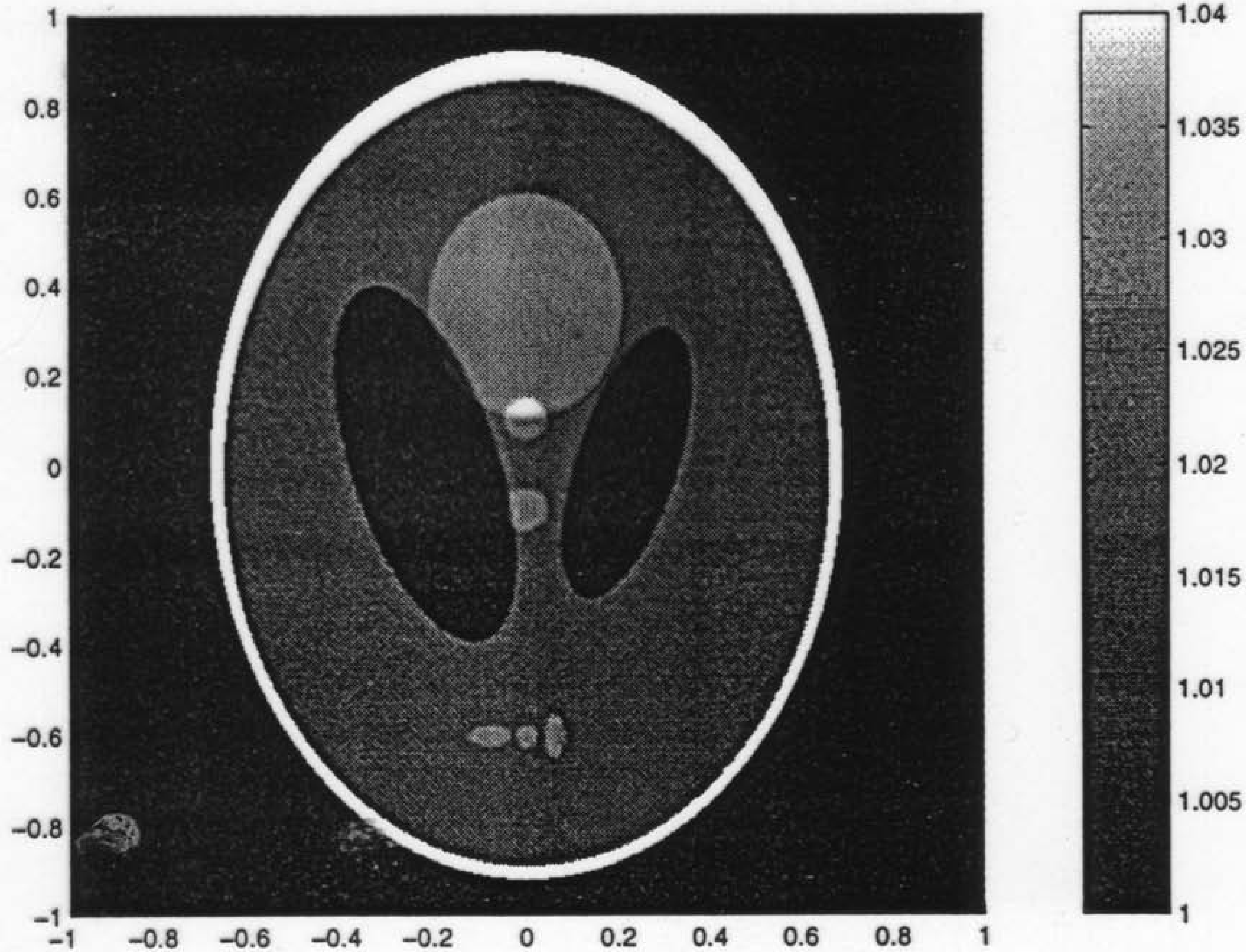


Figure 18: Shepp-Logan, $N = 512$, $m_1 = 19$, $m_2 = 13$, $\alpha = 5$, $\beta = 2$, l_2-error=0.0005.

cylinder H, shown in fig. 24 in end view. It can turn about its axis, being supported on knife-edges O. To it springs are attached at the prolongation of a horizontal diameter; to the left a series of n small springs s, all alike, side by side at equal intervals at a distance a from the axis of the knife-edges; to the right a single spring S at distance b. These springs are supposed to follow Hooke's law. If the elongation beyond the natural length of a spring is λ, the force asserted by it is $p = k\lambda$. Let for the position of equilibrium l, L be respectively the elongation of a small and the large spring, k, K their constants, then

Michelson and Stratton analyser.

$$nkla = \mathrm{K}\mathrm{L}b.$$

The position now obtained will be called the *normal* one. Now let the top ends C of the small springs be raised through distances $y_1, y_2, \ldots y_n$. Then the body H will turn; B will move down through a distance z and A up through a distance $\frac{a}{b}z$. The new forces thus introduced will be in equilibrium if

$$ak\left(\Sigma y - n\frac{a}{b}z\right) = b\mathrm{K}z.$$

Or

$$z = \frac{\Sigma y}{n\frac{a}{b} + \frac{b}{a}\frac{\mathrm{K}}{k}} = \frac{\Sigma y}{n\left(\frac{a}{b} + \frac{l}{\mathrm{L}}\right)}.$$

This shows that the displacement z of B is proportional to the sum of the displacements y of the tops of the small springs. The arrangement can therefore be used for the addition of a number of displacements. The instrument made has eighty small springs, and the authors state that from the experience gained there is no impossibility of increasing their number even to a thousand. The displacement z, which necessarily must be small, can be enlarged by aid of a lever OT′. To regulate the displacements y of the points C (fig. 24) each spring is attached to a lever EC, fulcrum E. To this again a long rod FG is fixed by aid of a joint at F. The lower end of this rod rests on another lever GP, fulcrum N, at a changeable distance $y'' = $ NG from N. The elongation y of any spring s can thus be produced by a motion of P. If P be raised through a distance y', then the displacement y of C will be proportional to $y'y''$; it is, say, equal to $\mu y'y''$ where μ is the same for all springs. Now let the points C, and with it the springs s, the levers, &c., be numbered $C_0, C_1, C_2 \ldots$ There will be a zero-position for the points P all in a straight horizontal line. When in this position the points C will also be in a line, and this we take as axis of x. On it the points $C_0, C_1, C_2 \ldots$ follow at equal distances, say each equal to h. The point C_k lies at the distance kh which gives the x of this point. Suppose now that the rods FG are all set at unit distance NG from N, and that the points P be raised so as to form points in a continuous curve $y' = \phi(x)$, then the points C will lie in a curve $y = \mu\phi(x)$. The area of this curve is

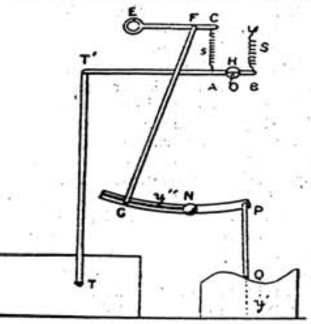


FIG. 24.

$$\mu\int_0^c \phi(x)dx.$$

Approximately this equals $\Sigma hy = h\Sigma y$. Hence we have

$$\int_0^c \phi(x)dx = \frac{h}{\mu}\Sigma y = \frac{\lambda h}{\mu}z,$$

where z is the displacement of the point B which can be measured. The curve $y' = \phi(x)$ may be supposed cut out as a templet. By putting this under the points P the area of the curve is thus determined—the instrument is a simple integrator.

The integral can be made more general by varying the distances NG $= y''$. These can be set to form another curve $y'' = f(x)$. We have now $y = \mu y'y'' = \mu f(x)\phi(x)$, and get as before

$$\int_0^c f(x)\phi(x)dx = \frac{\lambda h}{\mu}z.$$

These integrals are obtained by the addition of ordinates, and therefore by an approximate method. But the ordinates are numerous, there being 79 of them, and the results are in consequence very accurate. The displacement z of B is small, but it can be magnified by taking the reading of a point T′ on the lever AB. The actual reading is done at point T connected with T′ by a long vertical rod. At T either a scale can be placed or a drawing-board, on which a pen at T marks the displacement.

If the points G are set so that the distances NG on the different levers are proportional to the terms of a numerical series

$$u_0 + u_1 + u_2 + \ldots$$

and if all P be moved through the same distance, then z will be proportional to the sum of this series up to 80 terms. We get an *Addition Machine.*

The use of the machine can, however, be still further extended. Let a templet with a curve $y' = \phi(\xi)$ be set under each point P at right angles to the axis of x hence parallel to the plane of the figure. Let these templets form sections of a continuous surface, then each section parallel to the axis of x will form a curve like the old $y' = \phi(x)$, but with a variable parameter ξ, or $y' = \phi(\xi, x)$. For each value of ξ the displacement of T will give the integral

$$\mathrm{Y} = \int_0^c f(x)\phi(\xi x)dx = \mathrm{F}(\xi), \quad . \quad . \quad . \quad (1)$$

where Y equals the displacement of T to some scale dependent on the constants of the instrument.

If the whole block of templets be now pushed under the points P and if the drawing-board be moved at the same rate, then the pen T will draw the curve $\mathrm{Y} = \mathrm{F}(\xi)$. The instrument now is an *integraph* giving the value of a definite integral as function of a *variable parameter.*

Having thus shown how the lever with its springs can be made to serve a variety of purposes, we return to the description of the actual instrument constructed. The machine serves first of all to sum up a series of harmonic motions or to draw the curve

$$\mathrm{Y} = a_1 \cos x + a_2 \cos 2x + a_3 \cos 3x + \quad . \quad . \quad . \quad (2)$$

The motion of the points $P_1P_2 \ldots$ is here made harmonic by aid of a series of excentric disks arranged so that for one revolution of the first the other disks complete 2, 3, . . . revolutions. They are all driven by one handle. These disks take the place of the templets described before. The distances NG are made equal to the amplitudes $a_1, a_2, a_3, \ldots$ The drawing-board, moved forward by the turning of the handle, now receives a curve of which (2) is the equation. If all excentrics are turned through a right angle a sine-series can be added up.

It is a remarkable fact that the same machine can be used as a harmonic analyser of a given curve. Let the curve to be analysed be set off along the levers NG so that in the old notation it is

$$y'' = f(x),$$

whilst the curves $y' = \phi(x\xi)$ are replaced by the excentrics, hence ξ by the angle θ through which the first excentric is turned, so that $y'_k = \cos k\theta$. But $kh = x$ and $nh = \pi$, n being the number of springs s, and π taking the place of c. This makes

$$k\theta = \frac{n}{\pi}\theta . x.$$

Hence our instrument draws a curve which gives the integral (1) in the form

$$y = \frac{2}{\pi}\int_0^\pi f(x)\cos\left(\frac{n}{\pi}\theta x\right)dx$$

as a function of θ. But this integral becomes the coefficient a_m in the cosine expansion if we make

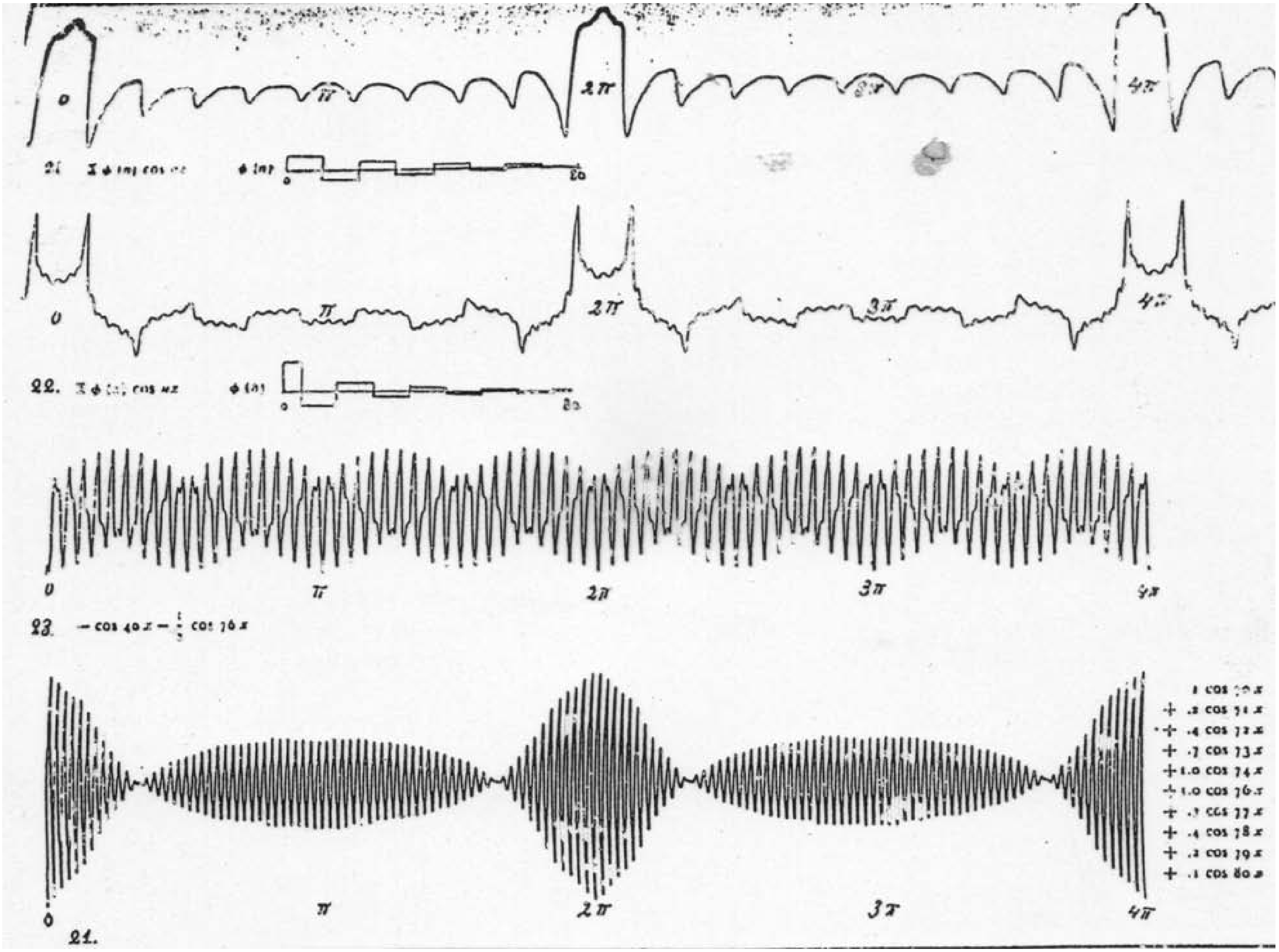
$$\theta n/\pi = m \text{ or } \theta = m\pi/n.$$

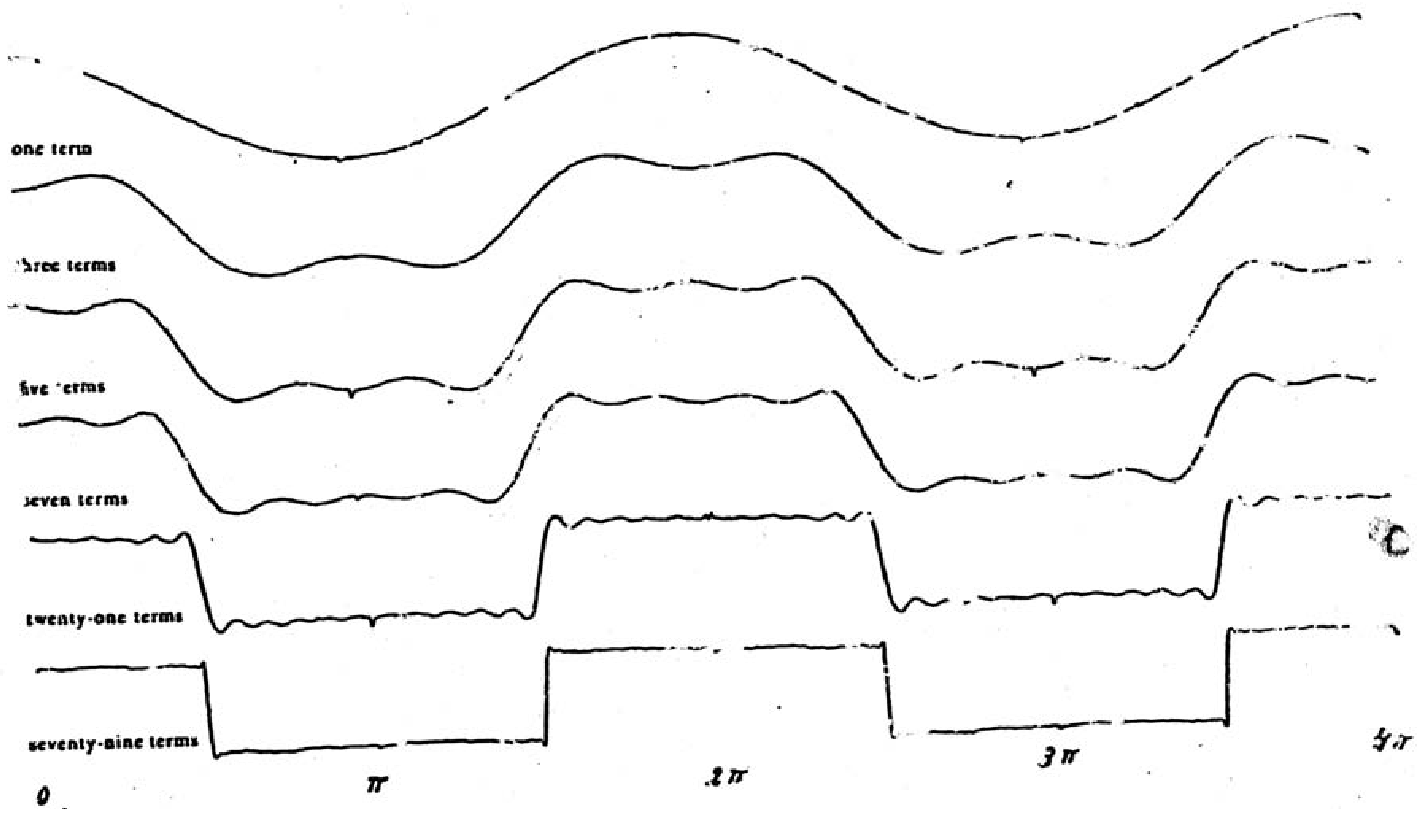
The ordinates of the curve at the values $\theta = \pi/n$, $2\pi/n \ldots$ give therefore all coefficients up to $m = 80$. The curve shows at a glance which and how many of the coefficients are of importance.

The instrument is described in *Phil. Mag.*, vol. xlv., 1898. A number of curves drawn by it are given, and also examples of the analysis of curves for which the coefficients a_m are known. These indicate that a remarkable accuracy is obtained. (O. H.)

CALCUTTA, the capital of British India and also of the province of Bengal. It is situated in 22° 34′ N. and 88° 24′ E., on the left or east bank of the Hugli, about 80 m. from the sea. Including its suburbs it covers an area of 27,267 acres, and contains a population (1901) of 949,144. Calcutta and Bombay have long contested the position of the premier city of India in population and trade; but during the decade 1891–1901 the prevalence of plague in Bombay gave a considerable advantage to Calcutta, which was comparatively free from that disease. Calcutta lies only some 20 ft. above sea-level, and extends about 6 m. along the Hugli, and is bounded elsewhere by the Circular Canal and the Salt Lakes, and by suburbs which form separate municipalities. Fort William stands in its centre.

Public Buildings.—Though Calcutta was called by Macaulay "the city of palaces," its modern public buildings cannot compare with those of Bombay. Its chief glory is the Maidan or park, which is large enough to embrace the area of Fort William and a racecourse. Many monuments find a place on the Maidan, among them being modern equestrian statues of Lord Roberts and Lord Lansdowne, which face one another on each side of the Red Road, where the rank and


0
π
2π
3π
4π
22. Σ φ(n) cos nx
φ(n)
23. − cos 40x − ½ cos 76x
1 cos 70x
+ .2 cos 71x
+ .4 cos 72x
+ .7 cos 73x
+ 1.0 cos 74x
+ 1.0 cos 76x
+ .7 cos 77x
+ .4 cos 78x
+ .2 cos 79x
+ .1 cos 80x
21.



one term
three terms
five terms
seven terms
twenty-one terms
seventy-nine terms
0
π
2π
3π
4π

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.]*

Undercurrents in the Strait of Bab-el-Mandeb.

AN interesting observation has recently been made by one of H.M. surveying vessels, and I forward the Preface to the account of the details published by the Hydrographic Department, which contains the principal facts, and also the Analysis of the observations, both of which may be of interest to some of your readers.

W. J. L. WHARTON.

Hydrographic Department, Admiralty, Whitehall, London, S.W., September 27.

UNDERCURRENTS IN THE STRAIT OF BAB-EL-MANDEB.

IT has long been known that in the Bosporus and Dardanelles when the surface water sets strongly from the Black Sea to the Mediterranean, the lower strata of the water for a certain height from the bottom sets strongly in the opposite direction.

While in this instance it is probable that the many large rivers which discharge their waters into the Black Sea have a share in producing the surface current, the observations by which the undercurrent was revealed appeared to plainly indicate that the surface drift, caused by the generally prevailing N.E. wind heaping the water up in the south-western part of the Black Sea, was the main factor.

The somewhat similar conditions which occur in the strait of Bab-el-Mandeb offered another opportunity of observation on this interesting form of oceanic circulation, and for many years such observations have been a desideratum.

In this strait for nearly half the year a more or less strong easterly wind prevails, driving much water before it into the Red Sea, and, great as is the evaporation from the surface of that sea, which must be made up wholly by an inflow of water through the strait of Bab-el-Mandeb, it appeared on the whole probable that during this season the phenomenon of the Dardanelles would be repeated.

The observation is, however, difficult. The water is deep, over 100 fathoms; the sea generally heavy; there is a tidal current to complicate matters; and it seemed doubtful whether the somewhat crude apparatus which served to unravel the movement of the lower strata in the shallower and smoother Dardanelles would give good results in this locality.

Nevertheless, Captain W. Usborne Moore was directed to attempt it in H.M.S. *Penguin* in 1890, but the results, while showing that the under strata were not running with the surface, were two ambiguous to afford much definite information.

The possession, however, of a deep-sea current meter, 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.

Analysis of Tidal Streams observed in the Large Strait of Bab-el-Mandeb by H.M.S. Stork *in January* 1898.

Time of tide at Perim.	At surface.		At 5 fms.		At 25 fms.		At 50 fms.		At 75 fms.		At 105 fms.	
	Direction.	Rate.	Direction.	Rate.	Direction.	Rate.	Direction.	Rate.	Direction.	Rate.	Direction.	Rate.
High water ...	N.W. ½ W.	2¼	N.W. by W.	3¼	N.W.	3	Slack	—	—	—	—	—
1h. after ...	N.W. ½ W.	2¾	N. ½ W.	3⅓	—	—	S. by E.	¼	Variable	—	—	—
2 „ ...	N.W.	2¾	N.W.	4	N.W.	2	N.W. by W.	⅓	—	—	—	—
3 „ ...	N.W. ½ W.	2⅓	N.W. by N.	3	—	—	N.W. by N.	¼	N.N.W. ½ W.	½	—	—
4 „ ...	N.W.	2	N.W. by N.	2½	N.N.W. ½ W.	2	N.W.	1⅓	N. ½ E.	1	S. by W.	¼
5 „ ...	N.W.	1¾	N. by W.	2	—	—	N.N.E.	½	N. by E. ½ E.	¾	S. by W.	1¼
6 „ ...	N.W. ½ W.	1½	N.W. ½ N.	¾	N.W. ¾ W.	1	E. by S.	½	S.S.E.	⅔	South	1⅓
7 „ ...	N.W. ½ W.	¾	N.W.	½	N.N.W.	½	West	1	—	—	S. E. ½ S.	1½
8 „ ...	W.N.W.	½	S.W. ½ W.	½	—	—	South	1½	S.E. by E.	1⅓	S.S.E. ½ E.	3
9 „ ...	W.N.W.	¼	W.N.W.	½	Slack.	—	S.S.E.	1	S.E.	1	S.S.E. ½ E.	2½
10 „ ...	N.N.W.	¼	N.W.	¼	E. by N.	½	—	—	S.S.E. ½ E.	1½	E.S.E.	1¾
11 „ ...	North	¼	N.N.W.	½	S.E.	½	S.E.	1	E. by S.	2¼	—	—
12 „ ...	N.W.	¾	N. by E.	½	—	—	—	—	—	—	—	—
13 „ ...	N.W. by N.	1¾	—	—	N.W. by N.	¾	—	—	E.S.E.	2	S.E. by E.	1¾

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

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

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."

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

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

This series increases with n until $n\epsilon = \pi$. Suppose, therefore, $\epsilon = k\frac{\pi}{n}$, 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 $-y = nx$.

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 either a right- or left-handed structure in living organisms on the earth, and one which diminishes the force of the foregoing argument for a small number of origins, is that it probably started either in the northern or in the southern hemisphere, and in either case the rotation of the sun in the heavens may be a sufficient cause for a right- or left-handed structure in an organism growing under its influence.

GEO. FRAS. FITZGERALD.

Trinity College, Dublin, September 27.

IN his presidential address to Section B of the British Association, Prof. Japp argues the necessity of supposing a "directive force," or intelligence, to have guided the formation of the first asymmetric substance. "Vitalism," which at one time was supposed to regulate the physiology and even the mechanics of organised beings, has passed more and more from the foreground, till, in the vision of some it remains only as a point in the vast distance of time at the origin of life. Is it to disappear altogether?

A sensible quantity of a mixture of enantiomorphs contains an enormous number of molecules. Chance determines the relative proportion present of right- and left-handed forms. Each molecule, having resulted from the action of symmetric forces, has an even chance of being of one or the other. Hence, the improbability of there being present a great preponderance of one form over the other is so great, that it is inconceivable that an optically active solution could result. To the above contention of Prof. Japp, the reply is made by Prof. Karl Pearson, in NATURE of September 22, that a chance result, however improbable, will occur, if sufficient opportunity be allowed. He postulates the vast ages of the earth's history. May we not, however, invoke chance to deal with masses instead of molecules, and thus perhaps substitute weeks for ages?

Let us consider a solution, in which the numbers of right- and left-handed molecules are very approximately equal, and which is consequently optically inactive. In the slow evaporation of the solvent, the right- and left-handed nuclei, about which the substance crystallises, will *most probably* be evenly distributed. Their number will be extremely small in comparison with that of the molecules, and, as chance determines their distribution, it is not so highly improbable—it is at least conceivable—that the crystals will be unevenly grouped. Suppose such to take place and a partial re-solution, roughly in the lines of the distribution of the two varieties of crystals—a not very improbable event—and we have an optically active solution. Chance has here acted the part played by organised matter in the person of M. Pasteur, by selecting and rejecting the oppositely formed crystals.

Is it yet possible to deny that the first ancestor of lævo-rotatory protein could have been built up from an asymmetric substance, separated in some such way as the above, by the play of chance upon the natural working of symmetric forces?

CLEMENT O. BARTRUM.

17 Denning Road, Hampstead, N.W., September 24.

The Moon's Course.

MAY I refer Sir S. Wilks to the simple and beautifully written autobiography of James Ferguson, F.R.S., self-taught mechanician and astronomer? I will quote a passage.

"Soon afterwards" (the previous date was 1743) "it appeared to me, that although the moon goes round the earth, and that the sun is far on the outside of the moon's orbit—yet the moon's motion must be in a line—that is, always concave toward the sun: and upon making a delineation representing her absolute path in the heavens—I found it to be really so. I then made a simple machine for delineating both her path, and the earth's, on a long paper laid on the floor. I carried the machine and the delineation to the late Martin Folkes, Esquire, President of the Royal Society, on a Thursday afternoon. He expressed great satisfaction at seeing it, as it was a new discovery, and took me that evening to the Royal Society, where I showed the delineation and the method of doing it. When the business of the Society was over, one of the members desired me to dine with him the next Saturday at Hackney, telling me that his name was Ellicott, and that he was a watchmaker. I accordingly went and was kindly received by Mr. Ellicott, who then showed me the very same kind of delineation and part of the

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."

LETTERS TO THE EDITOR

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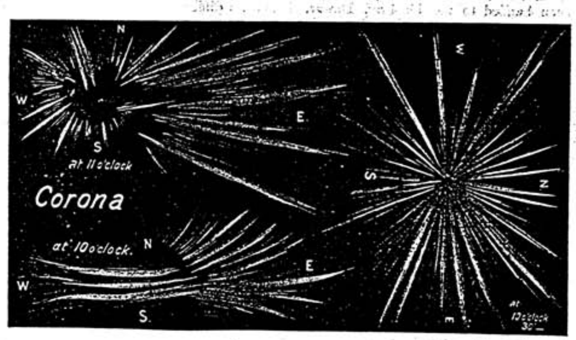
The Aurora Borealis of September 9.

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

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

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

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. Jap. ed., 1683, tom. x. fol. 6, *b.*):—"Li Mien, a high officer (ninth century), during his stay in Pien-Chau, came in possession of one joint of a monstrous bone, capable of the use as ink

Fourier's Series.

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

LETTERS TO THE EDITOR.

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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 at 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. 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

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

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

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 on 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

LETTERS TO THE EDITOR.

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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.

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.

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.

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

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

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.

being caused to move in a larger orbit than that described by it while still a part of the sun's mass," and the author suggests the action of comets carrying off portions of the nebulous border of a sun, as they struck it in the direction of its motion at a suitable moment.

The fifth and last article, in the results of which Dr. Smith expresses confidence other than he shows in respect of his earlier excursions into heterodox and quasi-heterodox physics, is devoted to "the laws of river-flow." Residence on the banks of the Mississippi enabled him to discover the formula of a double spiral action, by which to explain the elevation of the middle of a stream, the drift of floating material from the sides and of sunken material to the sides, the shape and depth of the eroded channels, the different speed of diverse portions of the current. This piece of at any rate unborrowed speculation appears not unworthy of consideration. H. W. B.

Das Heidelberger Schloss und Seine Gärten in alter und neuer Zeit und der Schlossgarten zu Schwetzingen. By H. R. Jung and W. Schröder. Pp. 74. (Berlin: G. Schmidt, 1898.)

IN this work we have an historical account of the gardens and castles of Heidelberg—the famous German university town, and its less well-known neighbour Schwetzingen. The authors are both gardeners, and, although the book is written chiefly from a garden point of view, a good deal of space is given to purely historical matter. Judging from the photographs, the gardens at Schwetzingen seem to be far more beautiful and natural than those of Heidelberg, where grottoes, shrines, and various other architectural devices, appear to be the leading features, and not always ornamental ones either. To those interested in the history of very old and famous gardens, this treatise may be of use; and it will not take up much space on the library shelf, being only about a quarter of an inch in thickness. It is well printed and illustrated, and is practically free from misprints; the only one of any importance being at p. 47, where Azalea appears as Aralea. Were it not that there is a genus Aralia, this slip would not be worth mention.

JOHN WEATHERS.

Graduated Test papers in Elementary Mathematics. By Walter J. Wood, B.A. Pp. 71. (London: Macmillan and Co., Ltd., 1899.)

THERE are forty test-papers in this collection, each containing questions in arithmetic, Euclid, and algebra. At the head of each test are notes stating the parts of the subjects required in order to solve the questions. The papers are primarily intended to test the progress of students preparing themselves for the examination in first stage mathematics of the Department of Science and Art, and Departmental teachers will find them of real value for that purpose. In the lower mathematical forms of secondary schools, also, the papers should be of service, as many of the questions have been selected from the papers of public examining bodies mostly favoured by such schools. Care appears to have been taken in selecting and arranging the questions, and answers are given to all the questions in arithmetic and algebra.

The Story of the British Race. By John Munro. Pp. 242. (London: George Newnes, Ltd., 1899.)

SOME time ago Mr. Munro wrote "The Story of Electricity" for this library of useful stories. In this volume he transfers his attentions to the science of anthropology, and expresses in his preface the hope that his book will "tend to destroy some errors regarding the origin and pedigree of the nation which have infected life and literature for ages." The volume should be the means of creating an interest in the study of mankind, in addition to imparting a knowledge of the nature of the races in the British Islands.

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Fourier's Series.

I HAVE M. Poincaré's authority to publish the accompanying note regarding the applicability of Fourier's series to discontinuous functions, and send it accordingly for publication in NATURE.

A. A. MICHELSON.

MON CHER COLLÈGUE,—Comme je l'avais prévenu vous avez, tout à fait raison. Prenons d'abord l'integrale $\int_0^y \frac{\sin xz}{x} dx$, dont la limite pour $y=\infty$ est $\pi/4$, 0, $-\pi/4$ selon que z est positif, nul ou négatif.

Faisons maintenant tendre simultanément z vers o et y vers l'infini de telle façon que zy tende vers a. La limite sera $\int_0^a \frac{\sin x}{x} dx$ qui peut prendre toutes les valeurs possibles depuis o jusqu'à $\int_0^\pi \frac{\sin x}{x} dx$.

Si nous prenons maintenant n termes dans la série $\Sigma \frac{\sin kz}{z}$ en faisant tendre simultanément z vers o et n vers l'infini de telle façon que le produit nz tende vers a, cela sera évidemment la même chose ; et la différence entre la somme et l'integrale sera d'autant plus petite que z sera plus petit. Cela se voit aisément.

Tout à vous,

(Signed) POINCARÉ.

A Note upon Phosphorescent Earthworms.

IT has been long known that earthworms may be phosphorescent. So long ago as 1836 Prof. Dugès described, under the name of *Lumbricus phosphoreus*, a worm which showed this peculiarity. In 1887 Prof. Giard showed that a worm probably identical with this, and, if so, not a *Lumbricus* at all, was marked luminous, especially when the soil was disturbed in the vicinity. Giard named the species *Photodrilus phosphoreus*. It has been met with and noticed to be luminous by two other observers. Quite recently (*Zoolog. Jahrbücher*, xii., 1899, p. 216) Dr. Michaelsen, of Hamburg, ascertained that this species of Giard is really identical with *Microscolex modestus* of Rosa. The multiplication of names is hardly the fault of Prof. Giard, since the genus *Microscolex* had only been instituted a few months before his genus *Photodrilus*. This species, unlike the majority of its congeners, which are chiefly congregated in Patagonia, and there very abundant, is not only European, but also occurs in England. It seems also to be, at least usually, phosphorescent. I received some time since, through the kindness of Mr. Carleton Rea, a few small earthworms from the neighbourhood of Worcester, which were undoubtedly a *Microscolex*, and at least not much different from *M. modestus*. Mr. Rea informed me that they were phosphorescent, with a "light emitted exactly similar to that of the glow-worm." They could be stimulated to show this light by "stamping the lawn." It has been suggested that this phosphorescence in earthworms is really due to photogenic bacteria entangled in the slime upon the skin. Possibly such an explanation may account for the occasional phosphorescence of *Allolobophora foetida* (the "Brandling"), observed by Vejdovsky. But the regularity, and the mode of excitation, of the luminosity seems to show that *Microscolex* is phosphorescent in its own right.

FRANK E. BEDDARD.

ON THE CHEMICAL CLASSIFICATION OF THE STARS.[1]

IN the attempts made to classify the stars by means of their spectra, from Rutherford's time to quite recently, the various criteria selected were necessarily for the most part of unknown origin; with the exception of hydrogen, calcium, iron, and carbon, in the main chemical origins could not be assigned with certainty to

[1] By Sir Norman Lockyer, K.C.B., F.R.S. A paper read at the Royal Society, May 4.

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Tout à vous,

(Signed) POINCARÉ.

blood to the cells which is required." In many places the sense is seriously interfered with by faulty punctuation, and we note a rather plentiful crop of misprints, especially towards the end of the book. Such are "centre nervous system," "tircuspid," "vertebræ," (for "vertebrata"), "cauda equinæ," "straining" (for "staining"), "Weber-Feehner law," "fenestra rotundis," (several times repeated), "scala tampani," "selerotic," "viteous humour." Nor do we care for the form "oculimotor." It is to be hoped that a future edition will be more carefully revised. The author has been fortunate in securing the use of the well-known and admirable figures from Quain's "Anatomy" and Schäfer's "Essentials of Histology." They add materially to the value of the work.

The Dawn of Reason. By James Weir, jun., M.D. Pp. xiii + 234. (New York: The Macmillan Company, London: Macmillan and Co., Ltd., 1899.)

THIS book on the mental processes of animals is the fruit of much original observation, and in many cases this observation has been supplemented by experiment; but, unfortunately, all the author's results are vitiated by his uncritical and biased attitude in favour of an extreme view of the mental life of animals, and there are few of his facts which the comparative psychologist would be justified in using without ample corroboration by other observers. Instinct is regarded as the great bane of psychology, and it almost seems as if the author believed it to be a special invention of those whom he calls "creationists." He poses as an ardent evolutionist, but is so blind to the most elementary principles of the evolution of mind that when a water-louse frightens some rhizopods, he can only conclude either that the latter have eyes and ears so small that lenses of the highest power cannot make them visible, or that these creatures are the possessors of senses utterly unknown to and incapable of being appreciated by man. He makes observations on spiders which show that they are differently affected by loud and soft vibrations of an organ—observations which do not even demonstrate the existence of hearing—and concludes that these animals have attained a very high degree of æsthetic musical discrimination. He has also seen a spider "intentionally beautifying" its web with flakes of logwood, and he has watched rhizopods employing their time in "simple amusement" resembling a game of tag. Nevertheless, among these extravagances, one meets with observations which would be of distinct value and interest if one had confidence in the observer.

The Arithmetic of Chemistry. By John Waddell, B.Sc., D.Sc. Pp. viii + 133. (New York: The Macmillan Company. London: Macmillan and Co., Ltd., 1899.)

THE volume does not differ essentially from other books on chemical arithmetic. Every teacher has his own method of presenting an arithmetical problem, which he often feels impelled to share with others. The author's methods seem thoroughly sound and logical, and no exception can be taken to them. There is a good deal to be said, too, for the plan of treating the calculations on a purely experimental basis independently of theories; but it is not always advisable to hold to it too rigidly. A good illustration is offered by the following example.

The author begins by showing that the combining weight of oxygen taken as 8 is thoroughly satisfactory, not only in its relation to hydrogen (1) in water, but to carbon (6) in its two oxides. It then becomes necessary to explain that this number for oxygen does not fulfil the expectations which it first raised, and that the formula for water HO(9) must be discarded in favour of H_2O(18). "It is found that while by electrolysis of water all of the hydrogen that is in the water is set free as a gas, and $\frac{1}{9}$ of the water decomposed is hydrogen; on the other hand, when sodium acts on water, only one-half as much hydrogen is set free, that is $\frac{1}{18}$ of the weight of water acted upon." It is questionable whether this explanation would carry conviction to the beginner. A plain dogmatic statement would surely serve the purpose better, until the student had advanced to a stage when he could grasp the whole question involved. The author has collected together an excellent set of examples from a variety of sources, which should be useful to teachers in elementary classes.

J. B. C.

The Flora of Cheshire. By the late Lord de Tabley (Hon. J. Byrne Leicester Warren), edited by Spencer Moore; with a Biographical Notice of the Author by Sir Mountstuart Grant Duff. Pp. cxiv + 399, with a portrait of the author and a map of the county. (London: Longmans, Green, and Co., 1899.)

THE manuscript of this "Flora," we are told, was completed a quarter of a century ago. Those who knew the sensitive, retiring disposition of the late Lord de Tabley will not be surprised that he laid it aside as not ready for press; nor will they be surprised at the excellence of what was done. There is little beyond an enumeration of the plants of the county, but made with extreme care and with conscientious acknowledgment of doubts and difficulties in dealing with critical plants.

Two classes of vegetation seem particularly to have attracted the author's notice, and both in a decidedly historical aspect. The one class is that of the alien plants, whose spread from ballast-heaps, &c., is traced; the other is the shore vegetation of a coast which has been much changed both by man and by tidal denudation. There probably exists no "Flora" of any county in Britain which approaches it in interest in either respect, unless it be that of Middlesex by Trimen and Thiselton-Dyer, published in 1869 at the time when Lord de Tabley was at work on what has just been printed.

To the matter which was put into his hands, the editor has wisely added enough to bring the work into line with our present knowledge of Cheshire botany. The biographical notice in its want of facts is a little disappointing; and the attempt to give each plant a binomial English name leads one to a curious and not altogether happy result. These, however, are small matters.

I. H. B.

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Fourier's Series.

THE statement of Fourier's theorem for the special case which has intermittently for some months past been a subject of discussion in NATURE, is as follows:—The function whose value is $\frac{1}{2}(\pi - x)$, when x lies between 0 and π, and $-\frac{1}{2}(\pi + x)$, when x lies between 0 and $-\pi$, can be expressed by the series $\sum_{k=1}^{k=\infty} \frac{\sin kx}{k}$ for values of x which lie between π and $-\pi$.

The proof of the theorem, whether in this special case or in more general cases, consists in summing the series; and the result obtained in this special case is that the sum of the series is

$$\begin{array}{ll} \frac{1}{2}(\pi - x), & \text{when } x \text{ lies between 0 and } \pi, \\ -\frac{1}{2}(\pi + x), & \text{when } x \text{ lies between 0 and } -\pi, \\ 0 & , \text{when } x = 0. \end{array}$$

Prof. Michelson has found a difficulty in this result in that, whereas the sum of any number of terms of the series is a continuous function of x, the sum of the series is a discontinuous function of x. If I have not misunderstood him, he contends that for extremely small positive values of x the sum of the series should be regarded as indeterminate and as having any value between 0 and $\frac{1}{2}\pi$, and I understand him to support this contention by the consideration that when n terms of the series are taken, so that x being extremely small nx is finite, such an indeterminateness is found.

Such a position involves a misconception of the meaning of the "sum of an infinite series." When $u_1 + u_2 + \ldots$ is the series, the terms being uniform functions of x, the sum of the series for any value of x is the limit of the sequence of numbers u_1, $u_1 + u_2$, $u_1 + u_2 + u_3$, ... in each of which x has the given value; the limit of the sum of the series when $x=0$, is the result obtained by *first* summing the series for a finite value of x, and *afterwards* diminishing x without limit; the sum of the series when $x=0$ is the result obtained by *first* substituting 0 for x in the functions u_1, u_2, ... and *afterwards* forming the limit of the sequence u_1, $u_1 + u_2$, ... In the example in question, the results thus obtained are $\frac{1}{2}\pi$ and 0 respectively. The results that can be obtained by summing the series to n terms, diminishing x indefinitely, increasing n indefinitely and keeping nx finite, generally do not coincide either with the sum for $x=0$ or with the limit of the sum for $x=0$, when these are different. Such results may, as I have pointed out in a previous letter, be useful for purposes of illustration, but they are quite beside the mark when it is a question either of the statement of Fourier's theorem or of the sum of Fourier's series.

M. Poincaré, in his letter printed in NATURE for May 18, does not assert that the sum of the series can be obtained by allowing x to approach zero and n to increase at the same time, in such a way that nx remains finite; but he states that Prof. Michelson is perfectly right in contending that the result of this process is indeterminate. So far as I am aware this contention has not been called in question in the course of the discussion.

Oxford, May 19. A. E. H. LOVE.

Bessel's Functions.

THE remarks of "C. G. K." (p. 74) concerning the defects of style which are frequently observed in the writings of scientific men, lead me to point out a grammatical error which is creeping into mathematical literature. I allude to the use of the incorrect phrase "Bessel Functions" in the place of "Bessel's Functions."

In certain cases the name of a person may be converted into an adjective by the addition of an appropriate termination, of which such words as *Elizabethan* and *Victorian* are examples; but to use the name itself (which is a noun) as an adjective, is a violation of one of the most elementary rules of grammar.

When the conversion of a proper noun into an adjective would be cumbrous or inelegant, the only correct mode of expression is to use the *genitive* case. If, therefore, we reject such an adjective as "Bessellian" on the ground of its inelegance, we must use the phrase "Bessel's Functions," that is functions of Bessel. The absurdity and incorrectness of the phrase "Bessel Functions" is at once seen by comparing it with such phrases as "*Green* Theorem," "*Chrystal* Algebra," "*Love* Elasticity."

The correct use of the genitive case is a subject upon which considerable misapprehension has existed. Thus we find in the Prayer Book the phrase "For Jesus Christ His sake," instead of "For Jesus Christ's sake." The error arose from the fact that the compilers of the Prayer Book were ignorant that the *'s* is not a conception of the pronoun *his*, but is the old Teutonic genitive which still exists in most German languages.

Fledborough Hall, Holyport, May 28. A. B. BASSET.

"The Art of Topography."

IN your issue of March 23 (No. 1534, vol. lix.) appears a review of "Recherches sur les Instruments, les Méthodes et le dessin Topographiques, par le Colonel A. Laussedat," signed by "T. H. H." The review brought to my attention several points of interest upon which I beg leave to comment.

Regarding planetable instruments, the reviewer says "that 'Russians and Americans' use very complicated instruments." Of the Russian instruments I have no knowledge, but this is certainly not true of the American.

The U.S. Geological Survey makes use of the planetable to a greater extent than any and all other organisations in America, fully two hundred of these instruments being constantly in use.

The instruments used are remarkable in simplicity and efficiency, are reasonably light, portable and accurate. The instruments are of a model designed by Mr. Willard D. Johnson, of the Survey, and are fully described on pages 79 to 89 of *Monograph* xxii. of the U.S. Geological Survey, entitled "Manual of Topographic Methods," by Mr. Henry Gannett.

This work also treats of the methods of accomplishing topographic mapping by the Geological Survey. Mr. Gannett explains the use made of the planetable, and shows that all work is controlled by points, located by triangulation or other means dependent upon numerical measurements and carefully computed. The triangulation is carried on with eight-inch theodolites reading, by micrometer microscopes, to two seconds.

The instructions to triangulators include the order that points must be selected and arranged so as to best control the area under survey, and that *three* points at least should be located on each atlas sheet of the map. Since these sheets differ in area in different parts of the country, ranging from 1/16 of a square degree to a square degree, the distance between triangulation stations necessarily varies considerably.

After the primary triangulation points are located in an area, dependence upon the planetable is absolute for the "secondary" triangulation within that area, the control, both horizontal and vertical, is carried on by use of this instrument. If the surveyor using a planetable for graphic work starts from accurately located points with check point available, he very soon discovers any "accumulation of error," in that it is impossible to make the several locations check one with another.

In regard to the use of "continuous contours" to express relief, the "Commission of 1826" seems to have drawn the remarkable conclusion that for scales less than 1 : 10,000 this system is insufficient.

The Geological Survey publishes topographic maps which vary in scale between 1 : 9600 and 1 : 250,000 (1 inch to 800 feet and 1 inch to 4 miles about, respectively), and on these maps the contour interval varies between 5 feet and 200 feet. The expression of relief is, I think, in these cases satisfactory, at least so far as giving accurate information is concerned; the artistic effect is very good also, especially when the topographic features are large and the slopes steep, cliffs appearing as broad heavy lines where differentiation of the individual contours is impossible.

About 1890, the use of mercurial barometers was abandoned by the Geological Survey, and trigonometric methods for obtaining heights were adopted. At the present time the primary heights are determined by spirit-levelling, from which elevations are carried in connection with the triangulation or by lines run with vertical angle readings and carefully measured distances. The use of the aneroid barometer is only allowed in inaccessible areas between the known elevations, and must be frequently checked. The experience of the writer in widely separated regions in the United States, in obtaining differences of elevation with the aneroid, leads him to the conclusion that, as a rule, the instrument fails to record differences as accurately when carried from a higher to a lower region as it does when the change of elevation is in the opposite direction. Also, that an aneroid which has been used in a region of elevation of given range must be given time to accommodate itself, if it be required to do good work in a region of greater or less elevation than that in which it has been used. The principle and construction of the aneroid is such that it never can be accepted as an instrument of precision except within well-defined limits, with frequent comparison with known elevations. The Survey has in use several hundred aneroid barometers, but no confidence may be had in any one of them unless frequently checked, as stated. It will be seen that the methods now in use in America agree more closely with those practised by the British Government, at least so far as the Colonial surveys are concerned, than with any other of the European surveys. R. H. C.

The Heating of the Anti-Kathode in X-Ray Work.

SINCE the beginning of X-ray work the heating of the anti-kathode has caused great difficulty, and with the introduction of the Wehnelt interrupter it is even more important that this should be prevented. In other words, we all along have had more energy from the coil than could be utilised in the Crookes' tube. Many workers like myself have tried to remedy this, and various plans have been adopted to keep the anti-kathode cool. It occurred to me that if we could get a piece of platinum, fused into the glass tube itself, to act as the anti-kathode, and placed opposite the kathode, this object might be attained. Such a tube, after many attempts, has at last been made; and although the first experiments have only been successful in making small tubes, others of a larger size are at present being attempted. The advantage of this method will easily be seen, because the heating of the piece of platinum can be prevented by placing the whole tube in a fluid cooling mixture or otherwise. These tubes are difficult to make at present, but I possess one which has retained its vacuum for some weeks.

179 Bath Street, Glasgow, May 28. J. MACINTYRE.

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.

THE statement of Fourier's theorem for the special case which has intermittently for some months past been a subject of discussion in NATURE, is as follows:—The function whose value is $\frac{1}{2}(\pi - x)$, when x lies between 0 and π, and $-\frac{1}{2}(\pi + x)$, when x lies between 0 and $-\pi$, can be expressed by the series $\sum_{k=1}^{k=\infty} \frac{\sin kx}{k}$ for values of x which lie between π and $-\pi$.

The proof of the theorem, whether in this special case or in more general cases, consists in summing the series; and the result obtained in this special case is that the sum of the series is

$$\begin{aligned} &\tfrac{1}{2}(\pi - x), \text{ when } x \text{ lies between 0 and } \pi, \\ -&\tfrac{1}{2}(\pi + x), \text{ when } x \text{ lies between 0 and } -\pi, \\ &0 \quad, \text{ when } x = 0. \end{aligned}$$

Prof. Michelson has found a difficulty in this result in that, whereas the sum of any number of terms of the series is a continuous function of x, the sum of the series is a discontinuous function of x. If I have not misunderstood him, he contends that for extremely small positive values of x the sum of the series should be regarded as indeterminate and as having any value between 0 and $\frac{1}{2}\pi$, and I understand him to support this contention by the consideration that when n terms of the series are taken, so that x being extremely small nx is finite, such an indeterminateness is found.

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Oxford, May 19. A. E. H. LOVE.

Reprojection - General Theory

- **We assume that**
  - $f(x)$ **is in** $L^2[-1,1]$**;**
  - **there is a subinterval** $[a,b] \subset [-1,1]$ **in which** $f(x)$ **is analytic;**
  - **there exists an orthonormal family** $\{\Psi_k(x)\}$**, under a scalar product** $(\cdot\,,\cdot)$**.**
- **Denote**

$$f_N(x) = \sum_{k=0}^{N} (f, \Psi_k)\Psi_k(x)$$

- $$\lim_{N\to\infty} |f(x) - f_N(x)| = 0$$

  almost everywhere in $x \in [-1,1]$**.**
- **Denote** $\xi = -1 + 2\frac{x-a}{b-a}$ **such that if** $a \le x \le b$ **then** $-1 \le \xi \le 1$**.**

Definition:

The two parameters family $\{\Phi_k^\lambda(\xi)\}$ is called a Gibbs complementary to the family $\{\Psi_k(x)\}$ if

(a) Orthogonality

$$< \Phi_k^\lambda(\xi), \Phi_l^\lambda(\xi) >_\lambda = \delta_{kl}.$$

(b) Spectral Convergence
The expansion of an analytic function $g(\xi)$ in the basis $\Phi_k^\lambda(\xi)$ converges exponentially fast, i.e.

$$\max_{-1\le\xi\le1} \left| g(\xi) - \sum_{k=0}^{\lambda} < g, \Phi_k^\lambda) >_\lambda \Phi_k^\lambda(\xi) \right| \le e^{-q_1\lambda}, \qquad q_1 > 0.$$

(c) The Gibbs Condition
There exists a number $\beta < 1$ such that if $\lambda = \beta N$ then

$$\left| < \Phi_l^\lambda(\xi), \Psi_k(x(\xi)) >_\lambda \right| \max_{-1\le\xi\le1} \left| \Phi_l^\lambda(\xi) \right| \le \left(\frac{\alpha N}{k} \right)^\lambda, \qquad k > N,\ l \le \lambda,\ \alpha < 1.$$

Comments:

- **Condition (b) implies that the expansion of a function g in the basis $\{\Phi_l^\lambda(\xi)\}$ converges exponentially fast if g is analytic in $-1 \leq \xi \leq 1$ (corresponding to $a \leq x \leq b$).**

- **Condition (c) states that the projection of $\{\Psi_k\}$ for large k on the low modes in $\{\Phi\}$, ($\Phi_l^\lambda(\xi)$ with small l) is exponentially small *in the interval* $-1 \leq \xi \leq 1$.**

Theorem

- $f(x) \in L^2[-1,1]$ **and analytic in** $[a,b] \subset [-1,1]$**.**
- $\{\Psi_k(x)\}$ **is an orthonormal family with the inner product** $(\cdot\,,\,\cdot)$**.**
- $\{\Phi_k^\lambda(\xi)\}$ **is a Gibbs complementary to the family** $\{\Psi_k(x)\}$ **as defined in (a)-(c), with** $\lambda = \beta N$**.**

Then

$$\max_{a \le x \le b} \left| f(x) - \sum_{l=0}^{\lambda} < f_N, \Phi_l^\lambda >_\lambda \Phi_l^\lambda(\xi(x)) \right| \le e^{-qN}, \qquad q > 0.$$

Comment:

- **Even if we have a slowly converging series**

$$\sum_{k=0}^{N}(f,\Psi_k)\Psi_k(x)$$

it is still possible to get a rapidly converging approximation to $f(x)$ if one can find another basis function $\{\Phi\}$ that yields a rapidly converging series to f as long as *the projection of the high modes in the old basis* $\{\Psi\}$ *on the low modes in the new basis is exponentially small.*

- **In the following theorem we will use**

$$\Phi_k^\lambda(\xi) = \frac{1}{\sqrt{h_k^\lambda}} C_k^\lambda(\xi)$$

where $C_k^\lambda(\xi)$ **is the Gegenbauer polynomial and** h_k^λ **is the normalization factor. The** $< \cdot, \cdot >_\lambda$ **inner product is defined by**

$$< f, g >_\lambda = \int_{-1}^{1} (1-\xi^2)^{\lambda-\frac{1}{2}} f(\xi) g(\xi)\, d\xi$$

If $g(\xi)$ **be a function with** p **continuous derivatives then**

- $$|g(\xi) - \sum_{k=0}^{\beta N} < g(\xi), C_k^{\beta N} > C_k^{\beta N}(\xi)| \leq ||g||_{C^p} \frac{1}{N^{p-1}}$$

- **If** $g(\xi)$ **is analytic then**

$$|g(\xi) - \sum_{k=0}^{\beta N} < g(\xi), C_k^{\beta N} > C_k^{\beta N}(\xi)| \leq C e^{-\eta N}$$

Spectral Accuracy

Suppose now that the family $\Psi_k(x)$ provides spectral accuracy that is

- **If $f(x)$ has p continuous derivatives then**

$$|(f,\Psi_k)| \leq C(p)\frac{1}{k^p}\max_{-1\leq x\leq 1}||f||_{C^s}$$

  Where

$$C(p) \sim q^p \qquad \textbf{for some } q$$

Then we have

- **Theorem**

$$|<\Psi_k,\Phi_l^\lambda>_\lambda \Phi_l^\lambda| \;\leq\; (\frac{\alpha N}{k})^\lambda$$

  The number α is less then unity provided that

$$\lambda \leq \beta N\,, \qquad \beta \leq (b-a)\frac{e}{27q}$$

- **We can summarize:**

  The Gegenbauer polynomials Φ_k^λ is the Gibbs complementary to Ψ_k

We thus have a new meaning to Spectral Accuracy:

- **If a function $f(x)$ is analytic then its expansion in terms of Ψ_k converges exponentially.**

- **If $f(x)$ is piecewise analytic then the expansion coefficients in terms of Ψ_k contain enough information such that an exponentially convergent approximation can be constructed.**

Robust Gibbs Complementary

- **The use of Gegenbauer polynomials is not robust.**
- **It suffers from numerical round off errors (Boyd, Gelb and Tanner)**
- **A detailed analysis of parameters was done by Jackiewicz, Gelb and J.**
- **A more robust method is needed.**
- **A robust Gibbs complementary basis (Gelb - Tanner)**
  - **For an analytic function the expansion of the function in the new basis converges exponentially.**
  - **The projection of high modes in the original basis on the low modes in the new basis is exponentially small.**
  - **As the order . of the original expansion increases the weight function of the new basis converges to a weight whose associate basis satisfies the first requirement.**

Examples:

- $$\omega = \exp\left(\frac{\xi^2}{\xi^2 - 1}\right)$$

- **Freud Polynomials (Gelb Tanner)**

  - $$\omega(\xi) = e^{-c\xi^{2\lambda}}$$

  - **For optimality**

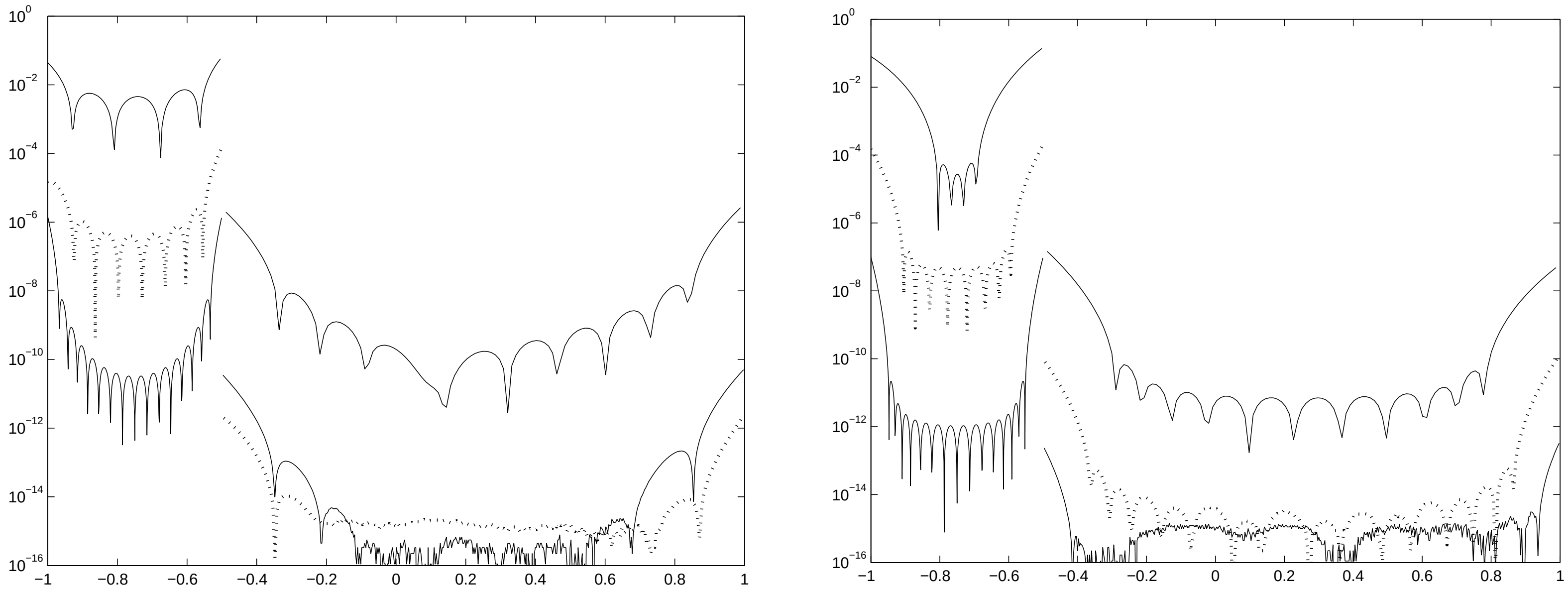
    $$\lambda = round\left(\sqrt{\frac{N(b-a)}{2}} - 2\sqrt{2}\right)$$

  - $$c = ln\epsilon$$

    and ϵ is the machine error.

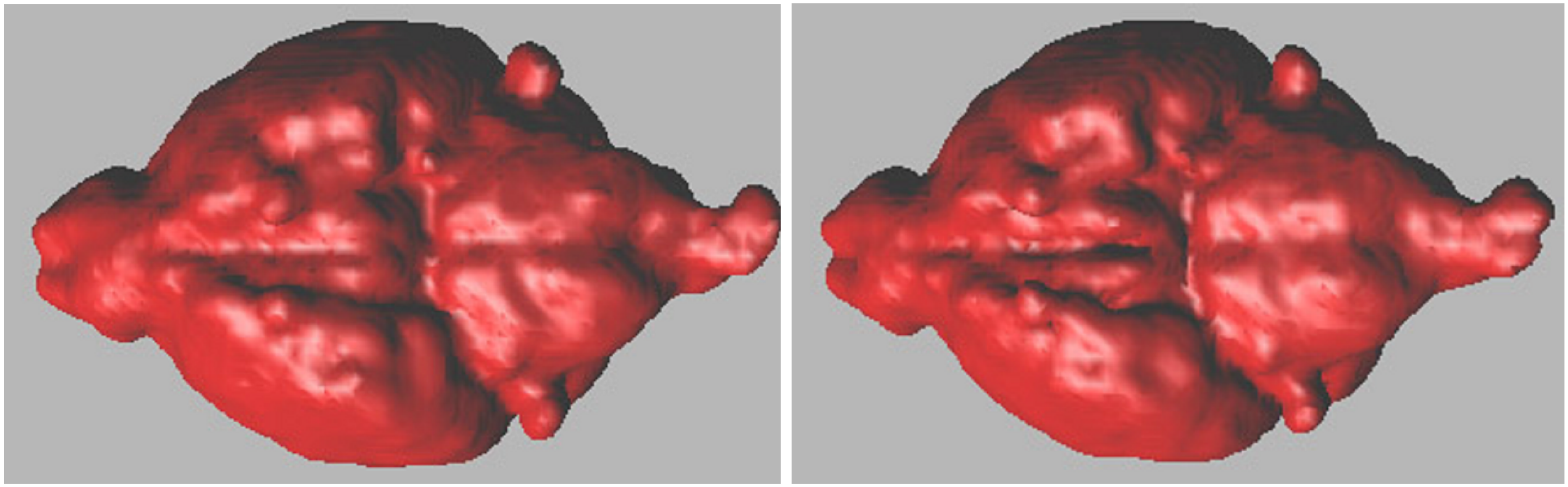
Comparsion of Freud and Gegenbauer Reprojection

$$f(x) = \begin{cases} (2\exp(2\pi(x+1)) - 1 - \exp(\pi))/(\exp(\pi) - 1) & -1 \leq x < \frac{1}{2} \\ -\sin(2\pi x/3 + \pi/3) & -\frac{1}{2} \leq x \leq 1 \end{cases}$$



- **Error plot of the Gegenbauer (left) and Freud (right) reprojection of the function with $N = 64, 128, 256$.**

Gegenbauer Reconstruction of 3D Image



- **Segmentation of Mouse Brain MRI with $256 \times 256 \times 181$ pixel points. (Source : Southwest Small Animal Imaging Resource, University of Arizona)**

- **Definition:**

$$S_N^\sigma f(x) = \sum_{|k| \leq N} \sigma(\frac{|k|}{N}) \hat{f}(k) e^{ikx}$$

The function σ has to satisfy the *Accuracy Condition* (Vandeven)

- $$\begin{aligned} \sigma^{(n)}(0) &= \delta_{n0} \qquad n \leq p \\ \sigma^{(n)}(1) &= 0 \qquad n \leq p \end{aligned}$$

- **Applied efficiently.**
- **Recovers p_th order accuracy away from discontinuities.**
- $$|f(x) - S_N^\sigma f(x)| \leq \frac{K}{d(x)^{p-1} N^{p-1}}$$

  Here $d(x)$ is the distance from the discontinuity.

- **Examples: (in all examples $\frac{|k|}{N} = y$)**
  - **Fejer**

$$\sigma(y) = 1 - y$$

  - **Raised cosine**

$$\sigma(y) = \frac{\cos(.5\pi y)^p}{(.5\pi y)^p}$$

  - **Exponential (The most popular)**

$$\sigma(y) = e^{-\alpha y^\beta}$$

  - **Erfc-Log (Boyd)**

$$\sigma(y) = \frac{1}{2} erfc\left(2\sqrt{p}(|y| - \frac{1}{2})\sqrt{\frac{-ln(1 - 4(y - \frac{1}{2})^2}{4(y - \frac{1}{2})^2}}\right)$$

  - **(Vandeven)**

$$\sigma(y) = 1 - \frac{(2p-1)!}{(p-1)!} \int_0^y t^{p-1}(1-t)^{p-1} dt$$

  - **See Scot Sarra's web page for descriptions and Matlab programs.**

Exponentially Accurate Filter (Tanner, Tadmor and Tanner)

- **Consider the filter function**

$$\sigma(y) = e^{\frac{(\delta y)^2}{2}} \sum_{j=0}^{p} \frac{1}{2^j j!} (\delta y)^{2j}$$

Where

–
$$\delta = \sqrt{\theta d_x N}$$

– d_x **defines neighborhood of analyticity around a point** x

–
$$p = \theta^2 d_x N$$

- **Then**

$$|f(x) - \sum_{|k| \leq N} \sigma(\frac{k}{N}) \hat{f}(k) e^{ikx}| \leq \frac{N}{d_x} e^{-\eta d_x N}$$

Edge Detections - Tadmor, Gelb and Tadmor

- **Most reconstruction methods need the location of jump discontinuities**
- **We assume that we are given $\hat{f}(k)$ the Fourier coefficients of a discontinuous function $f(x)$.**
- **We will review some of the results, and only for the Fourier case. Many other methods exist and also for other expansions, as Chebyshev and Legendre.**

- **Idea: Look at**

$$ue(x) = \frac{d}{dx} S_N f(x)$$

- **Let c_j be the location of the jump discontinuity with magnitude $[f](c_j) = f(c_j^+) - f(c_j^-)$**
- **Then**

$$\begin{aligned} ue(x) &\rightarrow O(\frac{1}{N}) \qquad x \neq c_j \\ ue(c_j) &\rightarrow [f](c_j) \end{aligned}$$

Edge Detections - Tadmor, Gelb and Tadmor (Cont.)

- **Nonlinear Enhancement:**

$$un(x) = N^{\frac{Q}{2}}(ue(x))^Q$$

- **Then we have**

$$\begin{aligned} un(x) &\rightarrow O(N^{-\frac{Q}{2}}) \qquad x \neq c_j \\ un(c_j) &\rightarrow [f](c_j)^Q N^{\frac{Q}{2}} \end{aligned}$$

For more refinements consult Sarra.

- **Define *Concentration Kernels***

$$
\begin{aligned}
K_N^\sigma(y) &= -\frac{1}{c_\sigma}\sum_{k=1}^{N}\sigma(\frac{k}{N})\sin(ky) \\
\frac{\sigma(\xi)}{\xi} &\le C \\
c_\sigma &= \int_0^1 \frac{\sigma(\xi)}{\xi}d\xi
\end{aligned}
$$

(i) K_N **is odd.**

(ii)

$$\int_{y\geq 0} K_N(y)dy = -1+\epsilon_N$$

(iii) Small first moment

$$\int_y K_N(y)\omega(y)dy \sim \epsilon_N||\omega|\_BV$$

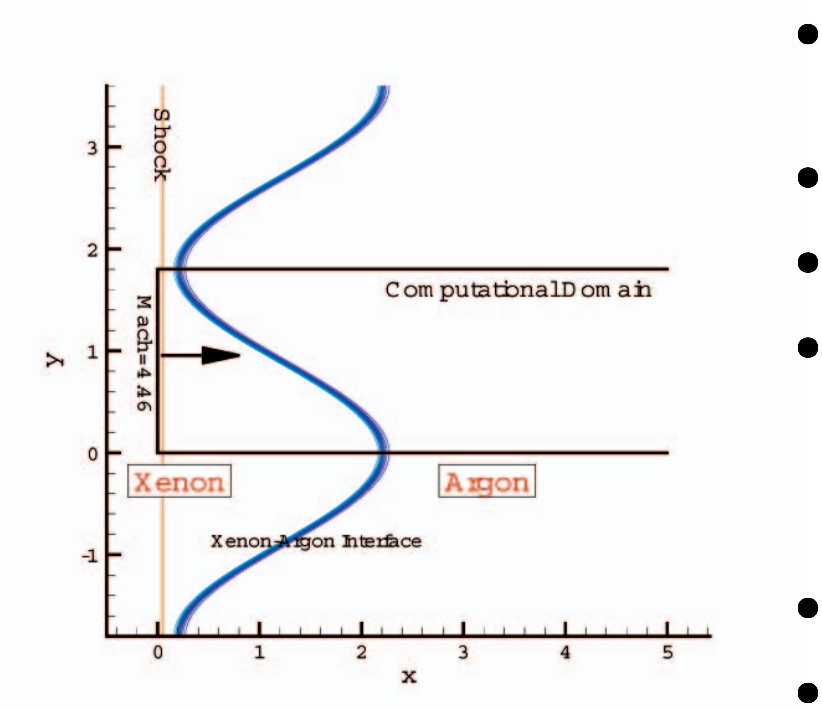
Then

$$|K_N * f(x) - [f](x)| \leq \epsilon_N.$$

Topic not covered

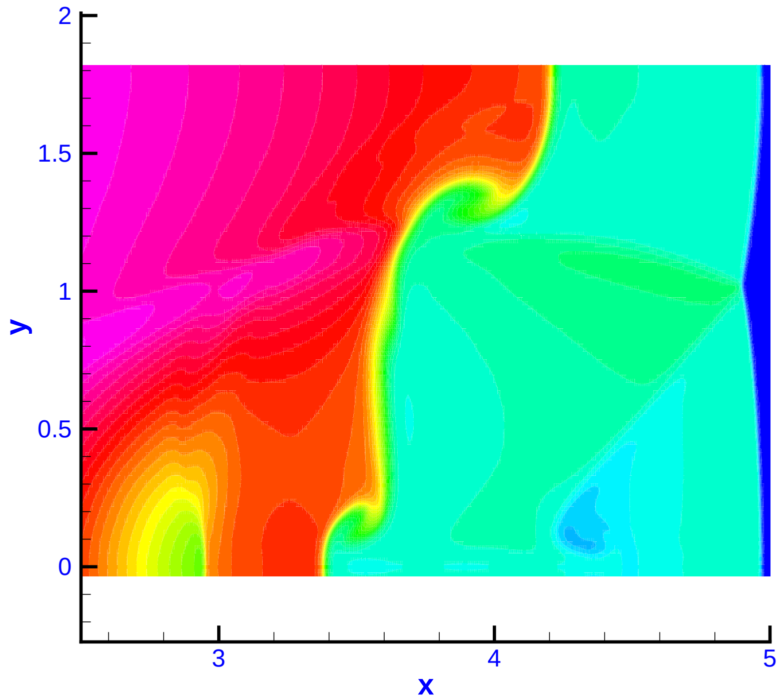
- Most of the results can be carried out for interpolation in Gauss points.
- Inverse Gegenbauer methods (Jung and Shizgal, Pasquetti and more).
- Pade reprojection, (Fornberg, Don Kaber and Min, Boyd)
- Gibbs phenomenon in many other expansions.
- Data coming from a solution of linear hyperbolic equations with discontinuous initial conditions (Mock and Lax, Osher and Majda, Gottlieb and Tadmor).
- Solutions of nonlinear hyperbolic equations (Lax (2005) showed that there must be a Gibbs phenomenon for accuracy greater than first order, He also argued that there is enough information to postprocess high order accuracy).

Richtmyer-Meshkov Instability

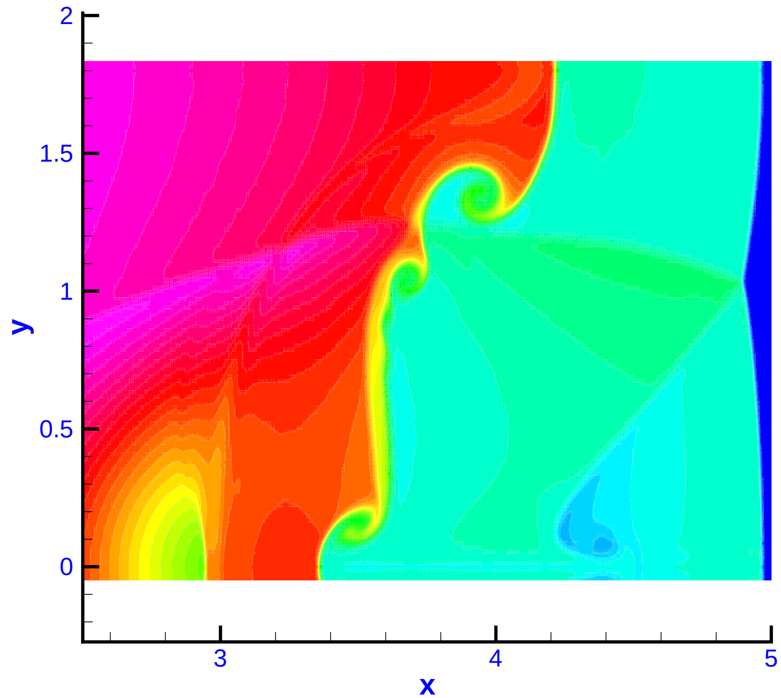


- **Hugoniot-Rankine condition for the shock**
- **Pre-Shock Temperature** $T = 296\ K$
- **Pre-Shock Pressure** $P = 0.5\ atm$
- **Xenon and Argon density are** $\rho_{Xe} = 2.90 \times 10^{-3}\ \frac{g}{cm^3}$ **and** $\rho_{Ar} = 0.89 \times 10^{-3}\ \frac{g}{cm^3}$ **respectively, at half of the normal atmospheric pressure**
- **Specific heat ratio** $\gamma = \frac{5}{3}$
- **Atwood number** $At = 0.54$
- **Mach number** $M = 4.46$
- **Wave Length** $\lambda = 3.6\ cm$
- **Amplitude** $a = 1.0\ cm$

Richtmyer-Meshkov Instability (Cont.)

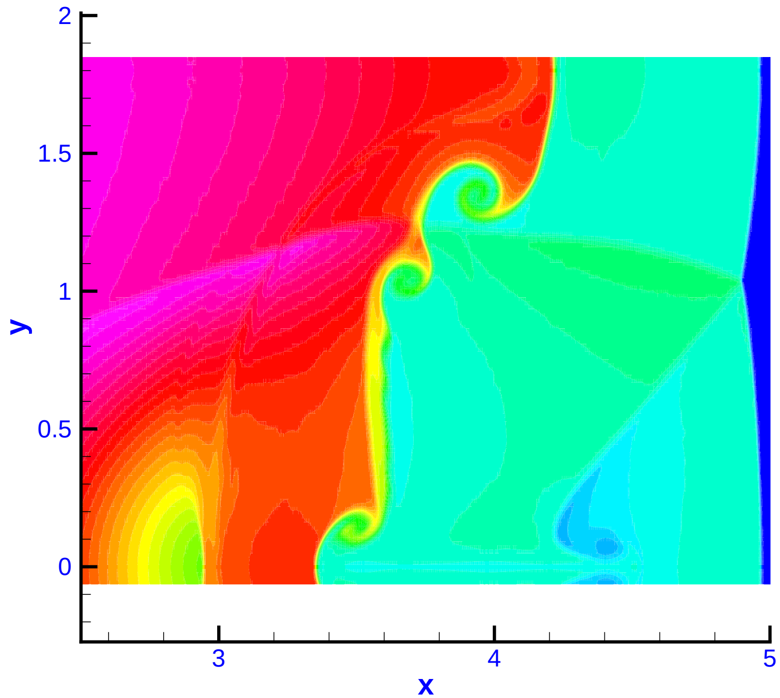


WENO Third Order

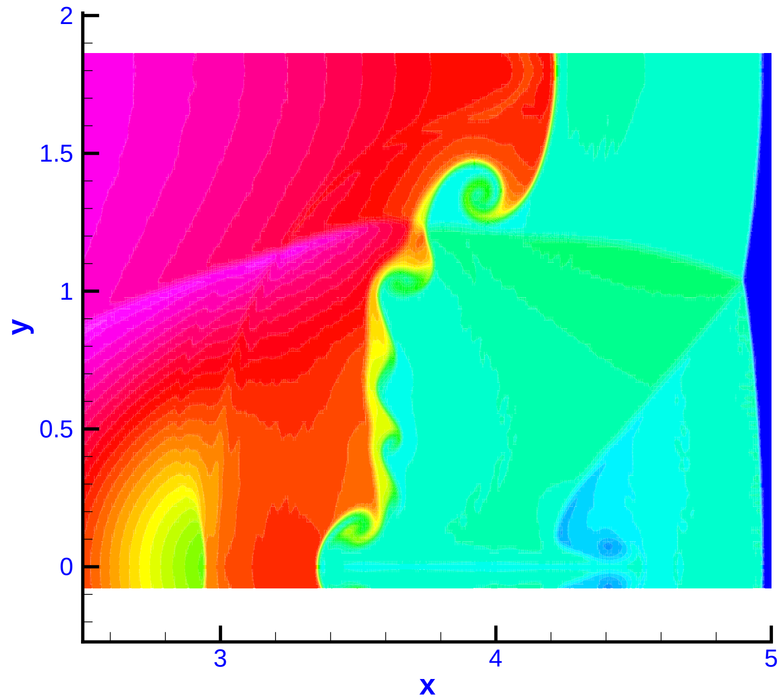


WENO Fifth Order

- **Density contour plot for the WENO third order (left) and fifth order (right) finite difference scheme with** 1024×256 **grid points.**



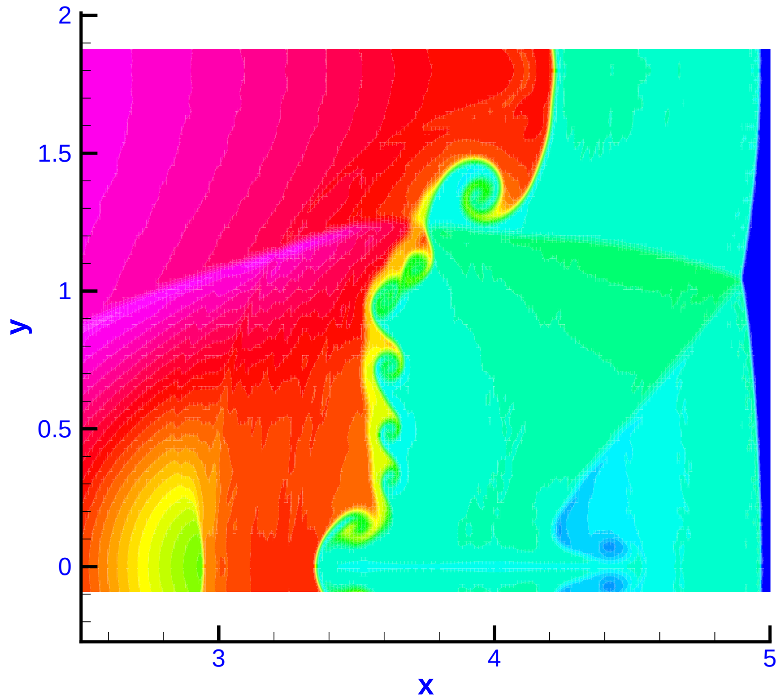
WENO Seventh Order



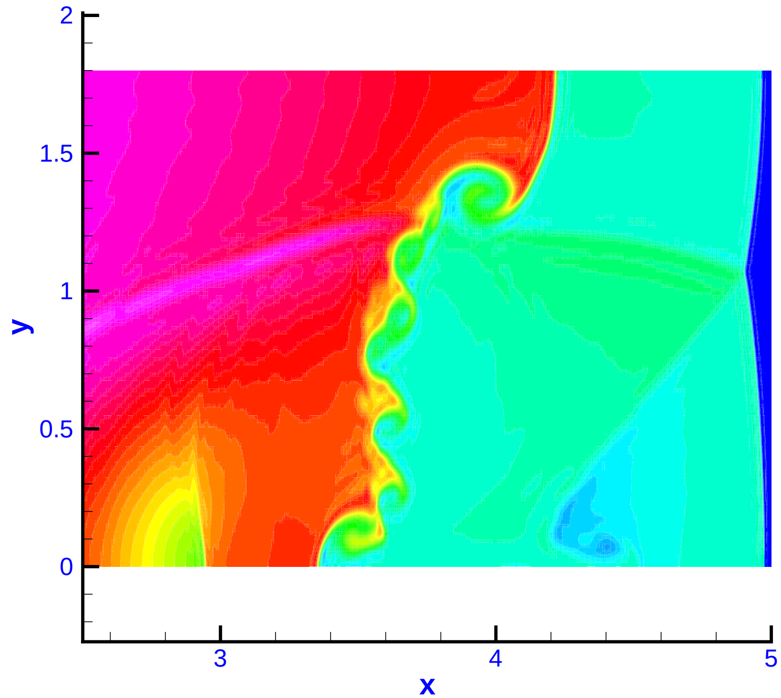
WENO Ninth Order

- **Density contour plot for the WENO seventh order (left) and fifth order (right) finite difference scheme with** 1024×256 **grid points.**

Richtmyer-Meshkov Instability (Cont.)



WENO Eleventh Order



Spectral method

- **Density contour plot for the WENO eleventh order (left) and spectral Chebyshev collocation method with 1024×256 grid points.**

The Euler system is

$$\begin{pmatrix} \rho \\ \rho u \\ E \end{pmatrix} + \begin{pmatrix} \rho u \\ P + \rho u^2 \\ u(P+E) \end{pmatrix}_x = -\frac{A_x}{A} \begin{pmatrix} \rho u \\ \rho u^2 \\ u(P+E) \end{pmatrix}$$

- $A = A(x)$ **is the cross area function of the nozzle and** $A_x = \frac{dA}{dx}$**.**
- **The shape of the nozzle is calculated by the requirement of piecewise linear distribution of Mach number from** $M_{in} = 0.8$ **at the inlet to** $M_{out} = 0.46665578743659$ **at the exit.**
- **The steady-state solution has a shock at** $x_s = \frac{1}{2}$**, halfway across the domain (the domain is** $(0, 1)$**).**
- **The Mach number just before the shock is** $M = 1.3$ **and just after the shock is** $M = 0.78595708016148$**.**
- **The other relevant quantities are**

$$\begin{aligned} \rho_{in} &= 0.74 \quad & u_{in} &= 0.8912 \quad & P_{in} &= 0.6560 \\ \rho_{out} &= 0.8804 \quad & u_{out} &= 0.5405 \quad & P_{out} &= 0.8436 \end{aligned}$$

Nozzle (Cont.)

- **The spatial discretization is achieved using a fifth order WENO method (third order near the shock)with Roe building blocks.**
- **Steady state is achieved by timestepping (using a third order SSP Runge-Kutta method) until the residuals go down to machine zero.**
- Results
  - **The pre-shock region maintains high order accuracy away from the shock**
  - **In the post shock region the accuracy is first order.**

Nozzle : Results (Cont.)

The raw data errors (compared to the exact solution) are:

N	l_1 error	l_1 order	l_2 error	l_2 order	l_∞ error
600	1.85E-8		0.86E-5		1.37E-3
800	1.24E-8	1.39	0.80E-5	1.20	1.09E-3
1000	9.82E-6	1.04	7.00E-5	0.29	1.27E-3
1200	8.26E-6	0.95	6.55E-5	0.63	1.19E-3
1400	7.16E-6	0.93	5.84E-5	0.34	1.27E-3
1600	6.20E-6	1.08	5.08E-5	0.65	1.19E-3
1800	5.55E-6	0.94	4.79E-5	0.50	1.19E-3

Nozzle : Results (Cont.)

The postprocessed data errors (compared to the exact solution) are:

N	λ	m	l_1 error	l_1 order	l_2 error	l_2 order	l_∞ error	l_∞ order
600	3	3	7.00E-5		1.60E-4		8.16E-4	
800	3	4	2.64E-5	3.8	5.80E-5	3.5	3.33E-4	3.11
1000	4	5	1.20E-5	3.5	2.60E-5	3.6	1.66E-4	3.13
1200	5	6	7.19E-6	2.8	1.31E-5	3.7	8.19E-5	3.8
1400	6	7	4.08E-6	3.7	6.17E-6	4.9	4.09E-5	4.5
1600	6	8	3.20E-6	1.8	3.97E-6	3.3	1.73E-5	6.46
1800	7	9	2.34E-6	2.6	2.53E-6	3.8	8.95E-6	5.57

Ap.plications to Nanophotonics Problems

Nanophotonics Problems:

•What is nanosystems:

Arrays of nanoparticles.

•What to study:

Photonic behaviors of nanosystems.

•What matters:

Incident light wavelength is much bigger than the nanoparticle size.

•What phenomenon do we observe:

Incident fields excite nanoscale fields and dramatically enhance the fields close to the surface of nanoparticles.

Numerical Challenges:

•Efficiently resolve surface-enhanced field interactions as few grid points as possible.

•Accurately represent the fields around the sharp jumps at the surface of nanomaterials.

We solve the Maxwell's equations by:

(1) Use the Fourier method,

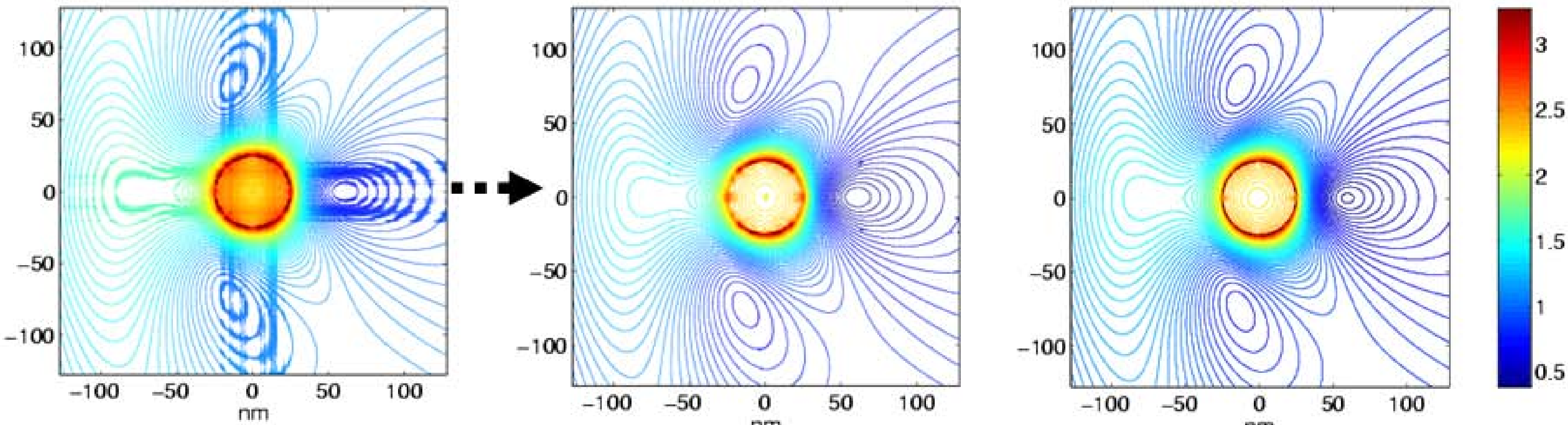
(2) Apply a Gegenbauer Reconstruction method to the Fourier data,

(3) Obtain accurate results with less cost compared to the FDTD results.

Computational Results

Electric Field Energy Calculations at Timesteps = 8,500 with DT = 1.35E-18
Nanocylinder Radius = 50 nm, Wavelength of Incident Planewave = 340 nm

Fourier Method | **Gegenbauer Reconstruction** | **Finite-Difference Method**



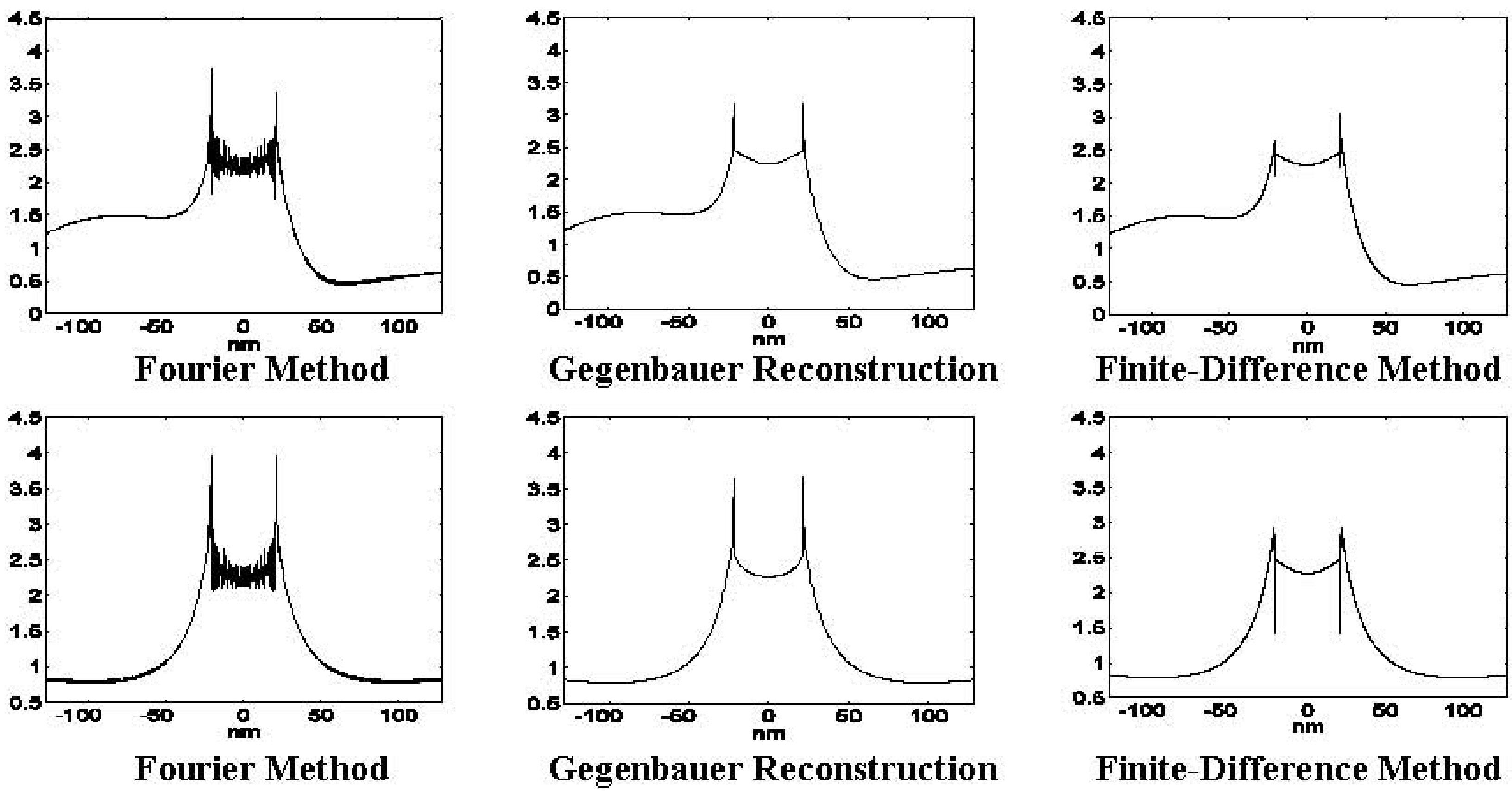
Grid Size = 0.5 nm
512 x 512

Grid Size = 0.5 nm
512 x 512

Grid Size = 0.1 nm
2,560 x 2,560

Reconstructed result shows a good agreement to the FDTD result on 5 times finer grids.

Comparisons on 1-D Slices



Gibbs oscillations are successfully reduced "up to" the surface of the nanocylinder whereas the finite-difference results shows unreasonable profiles for the fields.

JCP 2006, Min, Lee, Fischer, Gray