L. ÎA. NEÏSHULER, "Tabulation of functions," p. 1157–1176. There are here two references (p. 1157, 1175) to *MTAC* and to U. S. A. as "the country with the greatest development in the industry of calculating machines."

CHRONICLE: M. L. BYKHOVSKIĬ, "The new differential analyzer of Bush," p. 1177–1198. An illustrated description based on the long article of V. BUSH & S. H. CALDWELL, "A new type of differential analyzer," Franklin Inst., *Jn.*, v. 240, 1945, p. 255–326; see *MTAC*, v. 2, p. 89–91.

R. C. A.

NOTES

68. DOCTOR COMRIE'S ADDRESS.—We regret that we omitted to state in connection with the L. J. C. article, published v. 2, p. 149–159, which has been much in demand, that it was the address which he delivered 31 October 1945 at the Conference on Advanced Computation Techniques (*MTAC*, v. 2, p. 65–68), as chairman of subcommittee *Z* of the Committee on Mathematical Tables and Other Aids to Computation.

69. GIBBS' PHENOMENON.—I feel that the Note on the sine integral in *MTAC*, v. 2, p. 195, will give the impression that any description of the Gibbs' phenomenon in which the number 1.08949 (approx.) occurs is wrong, and that this number should be replaced by 1.17898 (approx.) as the result of a new and careful evaluation of $K = (2/\pi)\text{Si}\,\pi$. This is not so, as the appropriate number depends upon the way the phenomenon is described.

The series $F(t)$ represented by $\frac{1}{2}(\pi - t)$ for $o < t < \pi$, and by $-\frac{1}{2}(\pi + t)$ for $-\pi < t < o$ [not by $\frac{1}{2}(t - \pi)$ as stated in the Note if we interpret t algebraically] is an odd function with a discontinuity or jump of π from $-\frac{1}{2}\pi$ to $\frac{1}{2}\pi$ at $t = 0$. The Fourier series representing this function, i.e.,

$$\sum_{n=1}^{\infty} \sin nt/n,$$

exhibits the Gibbs phenomenon as an overshoot at each end of amount, say, δ, and, measured from the origin, the function jumps each way by an amount $\frac{1}{2}\pi + \delta$ which is given by

$$\text{Si}\,\pi = \int_0^{\pi} \sin t\,dt/t = \tfrac{1}{2}\pi + \text{si}\,\pi,$$

where $$\text{si}\,\pi = -\int_{\pi}^{\infty} \sin t\,dt/t \sim .28114, \quad \text{so that} \quad \delta = \text{si}\,\pi.$$

The phenomenon may be defined as in *MTAC* by the ratio K of $\frac{1}{2}\pi + \delta$ to $\frac{1}{2}\pi$, i.e., $K = (2/\pi)[\frac{1}{2}\pi + \text{si}\,\pi] = 1 + (2/\pi)\,\text{si}\,\pi \sim 1.17898$. This ratio is also that of the jump including both the overshoots to the jump itself, i.e., $\pi + 2\delta$ to π.

We can, however, define the phenomenon by the ratio K' of the jump + either overshoot to the jump itself, i.e., $\pi + \delta$ to π, so that

$$K' = (1/\pi)(\pi + \delta) = 1 + (1/\pi)\,\text{si}\,\pi \sim 1.08949.$$

When the jump is not necessarily at the origin nor of amount π symmetrically disposed about the t axis, it is usual to describe the phenomenon as an overshoot at each end by an amount which is about 9% of the jump itself,[1] a definition which follows naturally from the ratio K' above. (See, for instance, T. v. KÁRMÁN & M. A. BIOT, *Mathematical Methods in Engineering*, New York and London, 1940, p. 335). The ratio K is equivalent to saying that the sum of the two overshoots is about 18% of the jump.

K. KNOPP in his *Theory and Application of Infinite Series* (London, 1928) stated on p. 380 [p. 392 of the 1931 German ed.] that the first maximum has a value $\frac{1}{2}\pi(1.08949)$ i.e. our K', whereas his diagram on p. 379 clearly suggests that the value is really K. Obviously somewhere in the literature there has been a confusion between the definition of the overshoots as a percentage of the jump, and Knopp has used the correct diagram with a wrong description.

It is useful therefore to draw attention to the matter, but the Note in *MTAC* has obscured the issue by relating it to the accurate computation of the sine integral. Actually, to explain the difference between K and K' there was no need to use the Harvard Automatic Sequence Controlled Calculator and prove $K = 1.17897975. \ldots$ If we eliminate si π between K and K', we have $K' = \frac{1}{2}(K + 1)$, so that we can obtain the interesting relation referred to in the footnote to the article in *MTAC*, without having to know the value of si π at all.

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[1] This result was stated by M. Bôcher, *Annals Math.*, s. 2, v. 7, 1906, p. 131.—EDITOR.

NOTE BY R. C. A.: We are glad to have Mr. Millington's communication which will doubtless interest many readers. It seems desirable, however, to make clear that justification for his sweeping first paragraph (to which he returns in the last) is doubtful. I now refer to the following five places (which are the only ones) of my Note where the 1.08949 is involved: (i) Zygmund states that $K = 1.089490$, which is, of course, an error; (ii–iii) the two Szász papers before 1944 (Amer. Math. Soc., *Trans.*, v. 53, 1943, p. 440, and v. 54, 1943, p. 497) where Zygmund's incorrect value is copied. (iv) HARDY & ROGOSINSKI state that $k = \int_0^\pi \sin t\, dt/t = 1.71 \ldots$ (when it should have been $1.85 \ldots$) which is really equivalent to Zygmund's erroneous statement, as I pointed out. There remains, then, one and only one statement involving 1.08949, namely: (v) that of ZALCWASSER, who obtains the limit $\frac{1}{2}\pi(1.089)$ which, as we remarked in the Note, is exactly the error of HARDY & ROGOSINSKI. Thus in every one of the "five places", indubitable errors were listed. In not one of these errors enters the question of misinterpretation of "the way the phenomenon is described".

70. THE GRAEFFE PROCESS.—In view of our previous articles on this subject by D. H. L. "The Graeffe process as applied to power series," *MTAC*, v. 1, p. 377f, and by Mr. MITCHELL, "The Graeffe process," *MTAC*, v. 2, p. 57f, the following references to three articles published elsewhere may be noted: E. BODEWIG, "On Graeffe's method for solving algebraic equations," *Quart. Appl. Math.*, v. 4, 1946, p. 177–190; JOSE L. MASSERA, "El método de Gräffe para resolver ecuaciones algebraicas," Montevideo, Universidad, Facultad de Ingenieria, *Boletin*, año 10, v. 3, Dec. 1945, p. 1–20; R. SAN JUAN, "Complementos al método de Gräffe para la resolución de ecuaciones algébricas," *Revista Matem. Hispano-Americana*, s. 3, v. 1, 1939, p. 1–14.

71. WAS THERE AN ITALIAN REPRINT OF VEGA'S *Thesaurus* AFTER 1896?—We may begin by quoting the following five authorities which state that there was: (**a**) In *Jahrb. ü.d. Fortschritte d. Math.*, *1910*, p. 1054, is the entry, "G. Vega, Thesaurus, logarithmorum completus. Vollständige Sammlung grösser logarithmisch-trigonometrischer Tafeln. Neudruck. Mailand. 684 S. 4°." (b) H. ANDOYER, *Nouvelles Tables Trigonométriques Fondamentales*, Paris, 1911, p. vi; in listing the photozincographic reproductions of the *Thesaurus*, Istituto Geografico Militare of Florence, is the statement "un troisième tirage vient d'être effectué (1910)." (c) *Modern Instruments and Methods of Calculation*, ed. by E. M. HORSBURGH, London, Bell, and Royal Soc. of Edinburgh, [1914], p. 50, 52, "reprinted Milan, 1909." (d) F. J. DUARTE, *Nouvelles Tables Logarithmiques . . .*, Paris, 1933, p. xxiv, apparently quotes (b) as his authority for a third reprint by the Istituto in 1910. (e) FMR, *Index*, 1946, p. 440, lists, as in (b) and (d) a third Istituto reprint of 1910. A possible explanation of the 1909 date in (c) is that there was confusion with the sixteenth edition of Cremona's Italian translation in that year, of Vega's *Manuel logarithmique et trigonométrique* (see *Intern. Cat. Sci. Lit.*, v. 9A, p. 125.

In spite of such an array of authorities I was puzzled that I could find (i) no mathematical bibliography except (a), and no Italian bibliography, which listed an Italian edition after 1896; (ii) not a single library which had a copy, and (iii) no review in any periodical. Hence I wrote to the Director of the Istituto Geografico Militare offering to purchase a copy of the 1909 or 1910 edition. In his reply dated 2 Oct. 1946 occurs the following paragraph, in translation: "As a matter of fact besides not possessing any copy of the 1910 edition of Vega's Thesaurus we cannot assure you that such a copy was an exact reprint of the 1896 edition, since the documents relative to it in the Library were destroyed by the Germans. From oral testimony gathered from clerks who were more or less directly concerned with the reprinting of the Thesaurus it would seem that the 1910 edition would have been in complete conformity with the 1896 edition." As a result of this somewhat inconclusive paragraph, I published the rather vague statement about edition 4, *MTAC*, v. 2, p. 163.

Shortly after reading this L. J. C. sent me a copy of a translation of a statement which he had received from the Istituto during 1922–24 (when he was on the staff of Swarthmore College), in reply to his varied queries including one about a 1909 or 1910 edition. The following sentences there occur: "No other reprint of these tables has been issued by this Institute since the issue of the 1896 edition, and there is no record of any edition published in Milan in 1908 or subsequent to that date." "The zincographic plates prepared for the reprint are still preserved in the Institute." L. J. C. also drew my attention to his published statement in the *Observatory*, v. 52, 1929, p. 325 about these plates not being used for any edition subsequent to 1896. Hence I am now inclined to subscribe to the following statement by L. J. C. in a personal letter: "I will not believe in the 1909 or 1910 edition until I have an affidavit from somebody who has seen one."

R. C. A.

72. WHAT IS AN ERROR?—Those who delight in pointing out trivial end-figure errors may like to be reminded of the following words of wisdom from Glaisher's pen (R. A. S., *Mo. Not.*, v. 32, 1872, p. 261): "The increase of the last figure in tables, when the succeeding figures are greater than 500. . .,

seems to deserve more attention than it has received. Errata, such as some noticed in this communication, where the succeeding figures are 499..., are by no means uncommon; and it appears that the discoverers of them imagine they are doing some service by noting them. Take, for example, one of the cases in this note: the figures starting from the tenth are 5 49998...; if we take 5 as the tenth figure, the error is 49998..., if 6, the error is 50002, differing by 00004. Now, as our table only professes to give 10 places correctly (regard being paid to the magnitude of the figure in the eleventh place) a difference in the fifteenth place does not come in question at all: 5 and 6 are both equally correct; they only differ by quantities, which throughout all the rest of the table we agree to neglect. It is a matter of regret that all such valueless refinements are not avoided by the author always explaining the exact convention on which the last figure is increased. A very convenient arrangement would be to understand that when x figures of a number were tabulated, the error was less than 6 in the next figure; or, if the calculator wished to be more accurate, 56 in the next two figures. To obtain a table of x figures, it is usual to calculate $x + 1$, or $x + 2$ figures, and the inconvenience of extending the calculation further in the particular case when the next figure is 5, or the next two 50, is, in many cases, excessive, and as the result is of no additional value when obtained, a figure "wrong" under these circumstances ought not to be styled an error. Probably a good many of Lefort's errata are of this class. Babbage, in the introduction to his well-known table of seven-figure logarithms, states, that in ninety-three instances the next three figures in Vega were 500, and that in all these cases the logarithms were carried to more than ten places to determine whether the figures were really 500... or 499..., and decide whether the least figure was to be increased or not. This appears to me to have been quite needless. It sets up an unnecessary and artificial standard of accuracy for the numbers whose seventh, eighth, and ninth figures happen to be 4, 9, 9 or 5, 0, 0. To the user of a table of seven-figure logarithms it is a matter of really no importance whether his error is 499 or 501; he is content to make an error of 5, and an additional error of $\pm$ 0.01 is of no consequence."

With this I thoroughly agree—so much so that on more than one occasion I have written to our beloved editor saying "I have found . . . errors of less than one unit in . . . tables, but am not sending them to you, lest you should be tempted to publish them." In the Introduction to the BAASMTC, *Mathematical Tables*, v. 6, *Bessel Functions*, *Part I*, which I edited, I wrote: "In general all values have been computed to two decimals more than are given in these tables; the error of any tabulated value should not exceed $\pm$ 0.52 units of the last decimal." Incidentally Professor H. H. AIKEN informs me that he has not found any error exceeding my assigned limits in the functions J_0 and J_1. It would be futile to go to the trouble of altering any end figures where the error lies between $\pm$ 0.50 and $\pm$ 0.52, since an interpolate is liable, in any case, to be in error by at least a whole unit.

Errors, including those in the last decimal, often enable the table detective to ascertain how a table has been computed. One has only to instance the case of Buckingham, who refused to give any information about the compilation of his eight-place tables. But he was hopelessly betrayed by his errors (see *MTAC*, v. 1, 1943, p. 88f). The 2000 errors (exclusive of those of a unit in the last decimal) in HAYASHI, *Siebenstellige . . . Tafeln* (1926) give ample proof (a) that he did not check by differencing and (b) that he used a building-up process for intermediate values. Curiously enough, his end figures are fairly reliable. A run of errors in an early volume of DAVIS[1] showed (a) that he had made independent subtabulations in each interval, (b) that he had relied on repetition—the poorest possible check, and (c) that he had not checked by differencing. Other errors showed (d) that he had neglected second differences when interpolating 10-figure logarithms, and (e) that he had taken 10-figure logarithms of rounded-off quantities containing only five or six significant figures. But to his credit be it said that he was an apt "pupil" and can be trusted not to fall into any of these traps again!

Mrs. GIFFORD's end figures,[2] especially in the tangents, show the neglect of higher order differences; at one point there is a perfect wave in each 10″ interval, with an amplitude of 3 units. The observation that her sines near 90° were often in error by 99, 100 or 101 units led to a confession (in the true Sherlock Holmes style) that she "pre-fabricated" the first six decimals, and later added the seventh and eighth, with the not unnatural result that the sixth is often one out!

DUFFIELD's claim to have computed his logarithms to 12 decimals, increasing the tenth when the last two were 50 or more, is immediately shown to be false by the fact that his end-figure errors are (with a few exceptions, which can be accounted for) the same as those of Vega![3]

The fact that BENSON had copied from BRANDENBURG was revealed by his end-figure errors. He, too, was forced into a confession (*MTAC*, v. 1, 1943, p. 9) that shows he had not been honest either in his compilation, or in his preface. IVES, who also wrote a deceitful preface, provided at least a part of the clue to his plagiarisms by his errors.[4]

I have seen a 5-figure navigational table which contained just five per cent of end-figure errors, because it had been prepared from a six-figure table, but rounding off all 5's in the *same* direction.

L. J. C.

EDITORIAL NOTES: In *MTAC*, v. 1, p. 144 and 58, accuracy of half a unit in the last decimal place by PETERS, and PETERS & STEIN has been noted; and also on p. 145 accuracy less than .502 in the last decimal place. See further "Cayley and tabulation," p. 98. In the quotation of a passage from Glaisher's pen "Lefort's errata" are those referred to in *MTAC*, v. 2, p. 164–165. The well-known 7D table of CHARLES BABBAGE is *Table of the Logarithms of the Natural Numbers from 1 to 108 000*, London 1827, and various later editions.

[1] H. T. DAVIS, *Tables of the Higher Mathematical Functions*, v. 1. Bloomington, Ind., 1933.

[2] E. GIFFORD, see *MTAC*, v. 1, p. 11, 24f, 64f.

[3] See *MTAC*, v. 2, p. 164.

[4] H. C. IVES, see *MTAC*, v. 1, p. 9f.

QUERIES

21. PORTRAITS AND BIOGRAPHIES OF BRITISH MATHEMATICAL TABLE MAKERS.—Where may portraits be seen, or copies possibly be procured, of any of the following individuals: PETER BARLOW (1776–1862), HENRY BRIGGS (1561–1630), OLIVER BYRNE (publs. of 1838–77), ALLAN JOSEPH CHAMPNEYS CUNNINGHAM (1842–1928), JAMES DODSON (d. 1757), RICHARD