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Author(s): Stephen M. Stigler
Source: *The Annals of Statistics*, Vol. 9, No. 3 (May, 1981), pp. 465-474
Published by: Institute of Mathematical Statistics
Stable URL: https://www.jstor.org/stable/2240811

The Annals of Statistics
1981, Vol. 9, No. 3, 465–474

GAUSS AND THE INVENTION OF LEAST SQUARES[1]

BY STEPHEN M. STIGLER

University of Chicago

The most famous priority dispute in the history of statistics is that between Gauss and Legendre, over the discovery of the method of least squares. New evidence, both documentary and statistical, is discussed, and an attempt is made to evaluate Gauss's claim. It is argued (though not conclusively) that Gauss probably possessed the method well before Legendre, but that he was unsuccessful in communicating it to his contemporaries. Data on the French meridian arc are presented that could, conceivably, permit a definitive verification of the claim.

1. Introduction. The method of least squares is the automobile of modern statistical analysis: despite its limitations, occasional accidents, and incidental pollution, it and its numerous variations, extensions, and related conveyances carry the bulk of statistical analyses, and are known and valued by nearly all. But there has been some dispute, historically, as to who was the Henry Ford of statistics. Adrien Marie Legendre published the method in 1805, an American, Robert Adrain, published the method in late 1808 or early 1809, and Carl Friedrich Gauss published the method in 1809. Legendre appears to have discovered the method in early 1805, and Robert Adrain may have "discovered" it in Legendre's 1805 book (Stigler, 1977, 1978), but in 1809 Gauss had the temerity to claim that he had been using the method since 1795, and one of the most famous priority disputes in the history of science was off and running. It is unnecessary to repeat the details of this dispute—R. L. Plackett (1972) has done a masterful job of presenting and summarizing the evidence in the case. My aim instead is to adduce two new pieces of evidence, and to attempt an evaluation of Gauss's claim in their light.

2. The extant evidence. Legendre's claim to priority for the discovery of the method of least squares has always been quite straightforward: he published a clear explanation of the method, with a worked example, in 1805. Because Gauss could not (and did not) claim prior publication, his case had to rest on more indirect evidence. Four principal pieces of evidence that Gauss was the method's earliest discoverer have been presented, either by Gauss or in his behalf by his often reverential followers: (1) Gauss's word in 1809 and later that he had used the method since 1795 (or 1794; the date varied in later retellings of the tale). (2) A cryptic entry in Gauss's mathematical diary, dated June 17, 1798: "Calculus probabilitatis contra La Place defensus." ("The Calculus of probability defended against Laplace.") (3) Gauss's claim that he had told other astronomers, notably Olbers, Lindenau, and von Zach, about the method prior to 1805. (4) A letter of Gauss's that was published in 1799 in *Allgemeine Geographische Ephemeriden* which alludes to "meine Methode": the following translations of the complete letter (with footnotes by von Zach, the editor) and a correction note published in 1800, are quoted from Plackett (1972):

Received March 1980; revised May 1980.

[1] This research was supported by the National Science Foundation while the author was at the Center for Advanced Study in the Behavioral Sciences, under grants BNS 76-22943 A02 and SOC 78-01668, and completed at the University of Chicago under grant SES 80-07482.

AMS 1970 *subject classifications.* Primary 62A03; secondary 01A55, 86A30.

Key words and phrases. Legendre; history of statistics; geodesy; metric system; priority disputes; nonlinear least squares.

Allow me to point out a printer's error in the July issue of the *A.G.E.* Page xxxv of the introduction, in the account of the arc between the Panthéon and Évaux, must read 76145.74 instead of 76545.74. The sum is correct and the error cannot be in any other place.* I discovered this error when I applied my method, a specimen of which I have given you,† to determine the ellipse simply from these four measured arcs, and found the ellipticity to be 1/150; after correction of that error I found 1/187, and 2565006 units of length in the whole quadrant (namely without consideration of the degree in Peru). The difference between 1/150 and 1/187 is certainly not important in this case, because the end-points lie too close together.

Brunswick, 24 Aug. 1799. C. F. Gauss

* This printers error is confirmed, and may also be recognized by the decimal degree figure set beside it, $2^{D}.66868$—v.Z.

† Here at another time—v.Z.

Corrections to Volume 4 of the Allg. Geogr. Ephemer.

At the very same place p. 378 nr. 3 line 9, instead of ellipticity 1/150 must be meant 1/50. In line 12, instead of the words *'The difference between 1/150 and 1/187 is certainly not important in this case....'* greater intelligibility can be achieved as follows. *'The difference between 1/150, the ellipicity which was found by the French surveyors* (A.G.E. volume 4, p. xxxvii of the Introduction and p. 42) and 1/187, *which I have found, is certainly not important in this case'.*

These four[2] evidential exhibits are not equally compelling. The first—Gauss's word that he had used the method since 1795—is supported by Gauss's extraordinary ability as a mathematician and the fact that he had no need to make false claims. But his claim is still open to doubt on the following grounds: it was a later recollection and he may have been subject to possible self-deceptive exaggeration concerning what he had done years earlier. The *mathematics* of least squares would have been so trivial for Gauss that even had he come upon the method he might have passed it over as but one of many, not noticing its true significance until Legendre's book prodded his memory and produced a *post facto* priority claim. Perspectives change in time, and it is no disservice to Gauss to seek corroborative evidence, both that he was in possession of the method at an early date, and that he attached importance to the method.

The second exhibit, the 1798 diary entry, has the virtue of a pre-1805 date, but it also has the defect of such near total ambiguity that again we find ourselves falling back on Gauss's later recollections for an interpretation. The entry itself is conclusive only in showing he had considered questions relating to probability in June 1798.

The third claim—that Gauss explained the method to other astronomers before 1805—also lacks convincing contemporary corroboration. The astronomer Olbers did support Gauss's claim in an 1816 footnote, but only after seven years of repeated prodding by Gauss (Plackett, 1972). If Olbers's reluctant testimony is counted as weak support, arriving only after the army of Legendre's country had suffered a devastating defeat, then what of the other witnesses, Lindenau and von Zach? During the period 1800–1813, von Zach edited an astronomical periodical that consisted primarily of reviews and letters, *Monatliche Correspondenz*. Lindenau assisted him in conducting the *Monatliche Correspondenz* from 1807 on and it is in this journal that I have found new evidence, in some pertinent references to least squares. First, in a review article on geodesy signed by Lindenau and

[2] A fifth bit of evidence has been cited (e.g., by Mansion (1906), and Eisenhart (1968)); it is based on an undated fragmentary calculation found in Gauss's papers. It was not published until 1929 (see Gauss, 1929).

dated August 1806, there is a detailed reference to the method of least squares and even a short explanation of how the normal equations can be derived by differentiating the sum of squared errors. There is, however, no reference to Gauss—the method is described as Legendre's, his work *Nouvelles Méthodes pour la détermination des orbites des Comètes* is cited (together with the page number where the method is introduced), and the method's name is even given in French: "... nach von Legendre vorgeschlagenen Methode, die er *Méthode des moindres quarrés* nennt..." (Lindenau, 1806, page 138). A little over a year later, in the November 1807 issue of *Monatliche Correspondenz*, there are two more references to least squares in unsigned articles (presumably by the editors). Both refer to the method as Legendre's; both give the method's name in French. (von Zach or Lindenau, 1807, pages 428 and 455). In neither case is Gauss referred to. The relevance of these references is that they appeared in the period after the publication of Legendre's work and before that of Gauss, they specifically credited the method to Legendre, and they were written by men supposed to have known of Gauss's prior work, writing under no editorial restraint. Rather than taking the opportunity to note Gauss's personal communication of the idea to them prior to 1805, they make no reference to Gauss at all. This is not conclusive evidence that Gauss did *not* communicate the method to them before 1805—they may have felt constrained by the scientific norm that first publication determines priority, or they may have been too obtuse to understand what Gauss was telling them—but it cannot be construed as evidence in *support* of Gauss's claim.

3. The meridian arc. We come then to the final piece of evidence, the results of Gauss's calculations using "meine Methode" which were published in 1799. These published results suggest an intriguing question—can the same answers be derived from the original data using the method of least squares? Fortunately, though the journal in question is rare, it is nonetheless available, and thus we can recover exactly the data that were available to Gauss. They are presented in Table 1. As Gauss pointed out, the value 76545.74 should read 76145.74.

This is a famous data set in the history of metrology, for it was these data which determined the first meter. In 1793 the French had decided to base the new metric system upon a unit, the meter, equal to one 10,000,000th part of the meridian quadrant, the distance from the north pole to the equator along a parallel of latitude passing through Paris. These data were obtained in a 1795 geodetic survey, and consist of four measured subsections of an arch from Dunkirk to Barcelona. For each section the length of the arc is given in modules (S; one module = two toises ≅ 12.78 feet) and in degrees of latitude (d), and the latitude of the midpoint of the arc (L) is given.

TABLE 1.

French arc measurements, from Allgemeine Geographische Ephemeriden, **4,** 1799, *page* xxxv. *The number* 76545.74 *is a misprint; the correct number is* 76145.74. *The table gives the length of four consecutive segments of the meridian arc through Paris, both in modules* S *(one module ≅* 12.78 *feet) and degrees* d *of latitude (determined by astronomical observation). The latitude of the midpoint* L *of each arc segment is also given.*

	Modules S	Degrees d	Midpoint L
Dunkirk to Pantheon	62472.59	2.18910	49° 56′ 30″
Pantheon to Evaux	76545.74	2.66868	47° 30′ 46″
Evaux to Carcassone	84424.55	2.96336	44° 41′ 48″
Carcassone to Barcelona	52749.48	1.85266	42° 17′ 20″
Totals	275792.36	9.67380	

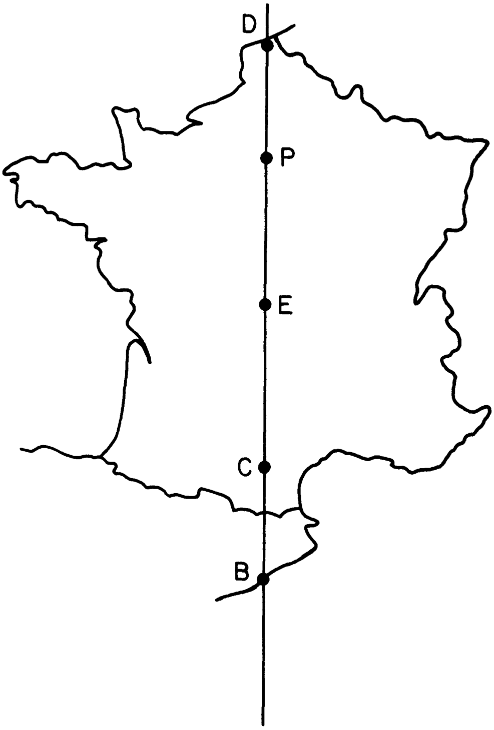

FIG. 1. *The French meridian arc, through Dunkirk* (D), *the Pantheon* (P) *in Paris, Evaux* (E), *Carcassone* (C), *and Barcelona* (B).

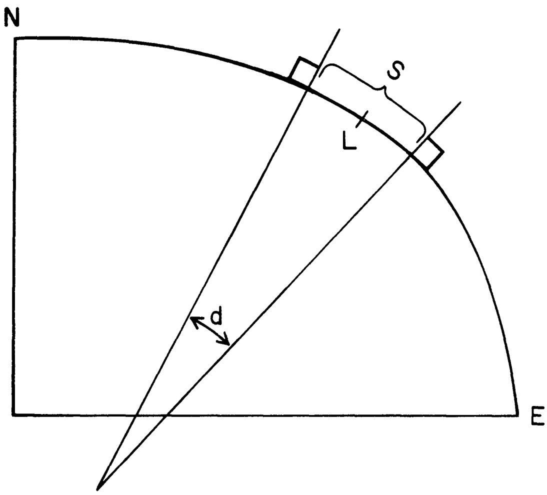

FIG. 2. *A meridian quadrant, from the equator* (E) *to the north pole* (N), *showing an arc segment of* d *degrees latitude and length* S *modules, centered at latitude* L.

The extract from *Allgemeine Geographische Ephemeriden* quoted earlier indicates that, based on these data alone, Gauss had first determined the ellipticity of the earth to be 1/50, then discovered the typographical error and recalculated to find an ellipticity of 1/187 and a meridian quadrant of 2565006 modules. A naive view would hold that by analysing these data using the method of least squares we should easily arrive at an unequivocal answer to the question "Did Gauss use least squares?" This naive view is wrong, for the matter is not so simple, as I propose to show. For having spent many, many hours with this data set I find myself in the paradoxical position of being unable to reproduce Gauss's answers, yet of being reasonably confident that he *did* use some variant of least squares.

The problems are that the relationships between arc length, latitude, ellipticity, and the meridian quadrant are nonlinear, that there are many ways of converting the problem into a linear least squares problem, and that all are nearly singular with respect to the ellipticity (and thus very sensitive to round off error and the precise mode of calculation employed). The usual linear formation of the problem, the one employed by Boscovich in 1755, by Laplace in the 1780's, by Legendre in 1805, and by most other early scientists is quite simple: for short arcs of the type we are concerned with, if the earth is ellipsoidal, then to a good approximation we should have

$$a = z + y \sin^2 L, \tag{3.1}$$

where $a = S/d$ (the arc length in modules per degree latitude), $z =$ the length of a degree at the equator, and y is the excess of a degree at the pole over one at the equator. Other versions of essentially the same relationship are

$$S = zd + y\, d \sin^2 L, \tag{3.2}$$

$$S = zd + y\, \rho \sin d \sin^2 L, \tag{3.3}$$

(where $\rho = 180/\pi$, since $d \cong \rho \sin d$ for small d)

$$a = D + c \cos 2L, \tag{3.4}$$

(where $D = z + y/2$, $c = -y/2$, since $2 \sin^2 L = 1\text{-}\cos 2L$) and

$$S = Dd + c\, \rho \sin d \cos 2\, L. \tag{3.5}$$

If one of these equations could be solved for z and y (or D and c), say by applying the method of least squares to the data of Table 1 treating S or a as the dependent variable, then the ellipticity would be found as

$$1/(\text{ellipticity}) = 3z/y + 3/2 = -\frac{3}{2}\cdot\frac{D}{c}, \tag{3.6}$$

and the meridian quadrant would be simply 90 times the length of a degree at latitude 45°,

$$\text{meridian quadrant} = 90\ (z + y/2) = 90D. \tag{3.7}$$

Gauss's values 1/ellipticity = 187 and meridian quadrant = 2565006 would correspond to $z = 28271.4565$ and $y = 457.2203$.

No matter which of these formulations is chosen, the answer differs little from that found by applying simple (unweighted) least squares to the relationship (3.1),

$$a = z + y \sin^2 L.$$

In this case we find, using (3.6) and (3.7)

$$z = 28227.16205$$

$$y = 541.2639353$$

$$1/\text{ellipticity} = 157.95$$

$$\text{meridian quadrant} = 2564801.46.$$

With the misprinted data, the results are

$z = 28074.82697$

$y = 906.7901314$

$1/\text{ellipticity} = 94.38$

$\text{meridian quadrant} = 2567539.98$

These results differ considerably from those of Gauss, and repeated attempts using weighted least squares (weighting by d^γ, $\gamma = \pm .5, \pm 1, \pm 2$) were also unsuccessful: in no case did the computed length of the meridian quadrant come closer to Gauss's value than 200 modules. Attempts using d as the dependent variable were also unsuccessful. Ironically, Boscovich's method[3] gives almost the same answer as least squares, 1/ellipticity = 158!

TABLE 2

The results of several first-order fits, compared with Gauss's results. (Note that (3.1) *and* (3.4) *give the same results, and* (3.1) *fit by weighted least squares with* $\gamma = 1$ *gives the results for fitting* (3.2), (3.3), *or* (3.5) *by unweighted least squares. The approximation* $d \cong \rho \sin d$ *has almost no effect upon the results for these arcs.)*

	Meridian Quadrant	1/(ellipticity)
Gauss		
Misprinted Data	—	50
Corrected Data	2565006	187
Least Squares, (3.1)		
Misprinted Data	2567540	94
Corrected Data	2564801	158
Weighted (by d^γ) Least Squares, with (3.1) and Corrected Data		
$\gamma = -2$	2564749	165
$\gamma = -1$	2564786	162
$\gamma = -.5$	2564798	160
$\gamma = 0$	2564801	158
$\gamma = .5$	2564793	155
$\gamma = 1$	2564771	151
$\gamma = 2$	2564698	141
Boscovich's Method, (3.1)		
Misprinted Data	—	77
Corrected Data	2564803	158

We are left with two possibilities. Either Gauss applied least squares to a version of the relationship $a = z + y \sin^2 L$ and made an error, either one due to round-off or an arithmetical error, or Gauss did not apply least squares to $a = z + y \sin^2 L$. The first possibility should be dismissed. Gauss was a phenomenal calculator, and would not have made an error of the necessary size in this short sequence of calculations. Also, I have verified that the needed accuracy was easily available using Vlacq's 1794 table of logarithms, a table that Gauss was likely to have used, and thus innocent round-off error is not a plausible explanation. We are left with the remaining possibility: Gauss did not apply least squares to $a = z + y \sin^2 L$, nor, for that matter, did he apply Boscovich's method.

What *did* Gauss do? I cannot answer this question with certainty, but I have a conjecture and some evidence to support it. Gauss was primarily a mathematician, not a statistician; his cast of mind was toward exact rather than approximate calculation. Where

[3] A 1757 adumbration of robust regression, in which the sum of the absolute values of the residuals is minimized, subject to the condition that the residuals sum to zero. See Eisenhart (1961), Sheynin (1973), Stigler (1973).

Boscovich and Laplace would have been content to have relied on the first order approximation $a = z + y \sin^2 L$, Gauss may not have been so easily satisfied. Boscovich and Laplace would have known that errors in the determination of S, d, and L were quite large compared with those due to the use of a first order approximation, and they would have seen no statistical reason for going further. No important improvement in accuracy could be expected; the assumption that the earth was ellipsoidal in shape was itself an approximation. Gauss, with his incredible facility for analysis and calculation, would not have answered to the same call; at 22 he lacked Laplace's practical experience, and he was responding to a different drive—the drive to push mathematical analysis further than others had. It thus seems plausible that Gauss, if he indeed applied least squares to this problem, would have applied it to, say, a second order approximation.

Unfortunately, the larger the class of relationships we consider, the more difficult our problem becomes. A likely second order expansion is that used later by Bowditch (1832) and by Bessel (1837),

$$S = xd + y \sin d \cos 2L + z \sin 2d \cos 4L. \tag{3.8}$$

Here x, y, and z may be viewed as nonlinear functions of the ellipticity and the length of a degree at the equator. My evidence for supposing that Gauss did apply least squares to a second order expansion is twofold. First, the wide discrepancy between the ellipticities he found before and after correcting the typographical error suggests a formula with greater flexibility than that available with the first order expansion, using either Boscovich's method or least squares. Second, I have made a variety of attempts to determine the meridian arch from a second order expansion using least squares, and the results are encouraging for Gauss's case. For example, fitting (3.8) by unweighted linear least squares (and thus ignoring the nonlinear constraints on x, y, and z) gave a meridian quadrant of 2565012, and another attempt using a version of Bessel's approach (see appendix) via nonlinear least squares gave (after experimentation with several starting values) Gauss's 2565006 right on the nose.[4] Unfortunately, none of my attempts was successful in simultaneously reproducing Gauss's values for the meridian quadrant and for the ellipticity, although the variety of possible expansions and the sensitivity of the results to starting values and computational precision is such that I expect they are attainable; the failure is likely mine, not Gauss's.

Examination of the French arc data must, I feel, be taken as supportive of the view that Gauss treated these data with least squares in 1799, more than five years before Legendre published on the subject. The case is not unchallengable, but considering the complexity of the problem, it is as strong as we have a right to expect. Gauss did not derive his results from the first order expansion in vogue at the time, either using least squares or using any other available method (such as Boscovich's method or Mayer's method, a scheme that added or subtracted the equations in groups). He must then have used another formulation, probably a second order expansion, and the use of least squares with such a formulation can produce at least a value of the meridian quadrant that is near Gauss's. Some alternatives to least squares that were available before 1800, such as Boscovich's method, were not developed to the point that they were available for more than two unknowns, and given that Gauss worked with the second order expansion, he would have needed some method "like" least squares. That he actually did use least squares then becomes the most plausible of the alternatives.

Let us grant, then, that Gauss's later accounts were substantially accurate, and that he did devise the method of least squares between 1794 and 1799, independently of Legendre

[4] Other attempts were further from the mark, but there was a tendency for most to be closer to Gauss's 2565006 than 200 modules, and therefore closer than the values attainable from the first order expansion.

or any other discoverer. There still remains the question, what importance did he attach to the discovery? Here the answer must be that while Gauss himself may have felt the method useful, he was unsuccessful in communicating its importance to others before 1809. He may indeed have mentioned the method to Olbers, Lindenau, or von Zach before 1805, but the total lack of applications by others, despite ample opportunity, suggests the message was not understood. The fault may have been more in the listener than in the teller, but in any case this failure serves only to enhance our admiration for Legendre's 1805 success. For Legendre's description of the method had an immediate and widespread effect—as we have seen, it even caught the eye and understanding of at least one of those astronomers (Lindenau) who had been deaf to Gauss's message, and perhaps it also had an influence upon the form and emphasis of Gauss's 1809 exposition of the method.

When Gauss did publish on least squares, he went far beyond Legendre in both conceptual and technical development, linking the method to probability and providing algorithms for the computation of estimates. His work has been discussed often, most recently by Seal (1967), Eisenhart (1968), Goldstine (1977, § 4.9, 4.10), Sprott (1978), and Sheynin (1979). But much of this development had to wait a long time before finding an appreciative audience, and much was intertwined with others' work, notably Laplace's. Gauss was the first mathematician of the age, but it was Legendre who crystalized the idea in a form that caught the mathematical public's eye. Just as the automobile was not the product of one man of genius, so too the method of least squares is due to many, including at least two independent discoverers. Gauss may well have been the first of these, but he was no Henry Ford of statistics. If there was any single scientist who first put the method within the reach of the common man, it was Legendre.

APPENDIX

Should, as I hope is the case, an adventuresome reader wish to attempt to reproduce Gauss's answers with a second order analysis, the following development (taken from Bessel (1837)) may be of use. The length of an elliptical meridian arc with eccentricity e between latitudes L_1 and L_2 is given by the integral (with a = the length of the semimajor axis)

$$S = a(1 - e^2) \int_{L_1}^{L_2} (1 - e^2 \sin^2 \theta)^{-3/2} \, d\theta. \tag{A.1}$$

In our earlier notation, this can be developed as

$$S = D \{d - 2 \rho \alpha \sin d \cos 2L + \rho \alpha' \sin 2d \cos 4L - (2/3) \rho \alpha'' \sin 3d \cos 6L + \cdots\}, \tag{A.2}$$

where α, α', α'' are defined by

$$N\alpha = \frac{3}{2} n + \frac{3 \cdot 5}{2 \cdot 4} \cdot \frac{3}{2} n^3 + \frac{3 \cdot 5 \cdot 7}{2 \cdot 4 \cdot 6} \cdot \frac{3 \cdot 5}{2 \cdot 4} n^5 + \cdots \tag{A.3}$$

$$N\alpha' = \frac{3 \cdot 5}{2 \cdot 4} n^2 + \frac{3 \cdot 5 \cdot 7}{2 \cdot 4 \cdot 6} \cdot \frac{3}{2} n^4 + \cdots \tag{A.4}$$

$$N\alpha'' = \frac{3 \cdot 5 \cdot 7}{2 \cdot 4 \cdot 6} n^3 + \frac{3 \cdot 5 \cdot 7 \cdot 9}{2 \cdot 4 \cdot 6 \cdot 8} \cdot \frac{3}{2} n^5 + \cdots \tag{A.5}$$

with

$$N = 1 + \left(\frac{3}{2}\right)^2 n^2 + \left(\frac{3 \cdot 5}{2 \cdot 4}\right)^2 n^4 + \cdots. \tag{A.6}$$

Here $n = (a - b)/(a + \mathrm{b})$

where a and b are the earth's two semiaxes. Since the ellipticity is defined to be

$$f = (a - b)/a \; (= 1 - (1 - e^2)^{1/2});$$

we have, to a good approximation, $2n = f$. Bessel treated S as determined without error, and let x and x' be the errors made in the astronomically determined latitudes of the endpoints of the arc, so instead of d, $d + x' - x$ was the true arc length. Neglecting the effect of error on $\cos 2L$ and $\cos 4L$, Bessel then had the second order approximation

$$S = D \{d - 2\,\rho\,\alpha \sin d \cos 2L + \rho\,\alpha' \sin 2d \;\cos 4L + \eta\,(x' - x)\}, \tag{A.7}$$

where

$$\eta = 1 - 2\alpha \cos d \cos 2L + 2\alpha' \cos 2d \cos 4L. \tag{A.8}$$

He parametrized this in terms of θ_1 and θ_2, with $D = D_1/(1 + \theta_1)$ and $\alpha = \alpha_1\,(1 + \theta_2)$ where D_1 and α_1 are suitably selected starting values, and he expanded to get, as a first order approximation, $x' - x$ as linear function of θ_1 and θ_2. How one proceeds from there depends on how one handles the correlated x's, since the same error x affects each of two adjacent arc segments. One approach, used by Laplace as early as 1799 and repeated by Bessel, was to treat one of the extreme endpoint's error as a location parameter, to be estimated along with θ_1 and θ_2. In one attempt along these lines I found 2565006 for the meridian quadrant and 1/151 for the ellipticity, when $D_1 = 28500$ and $\alpha_1 = 1/265$, although the value for the meridian varied from 2565000 to 2565020 as α_1 varied from 1/250 to 1/400.

Acknowledgment. I am grateful to Earl Rolph and Harriet Zuckerman for bibliothecarial assistance.

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DEPARTMENT OF STATISTICS
THE UNIVERSITY OF CHICAGO
5734 UNIVERSITY AVENUE
CHICAGO, ILLINOIS 60637