

## Studies in the History of Probability and Statistics. XXIX

### The discovery of the method of least squares

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#### SUMMARY

The circumstances in which the discovery of the method of least squares took place and the course of the ensuing controversy are examined in detail with the aid of correspondence. Some conclusions are drawn about the attitudes of the main participants and the nature of historical research in statistics.

*Some key words:* Calculus of probability; Gauss; History of statistics; Legendre; Method of least squares; Priority in scientific discovery.

#### 1. INTRODUCTION

The technique of combining independent observations on a single quantity by forming their arithmetic mean had appeared by the end of the seventeenth century (Plackett, 1958). Early in the eighteenth century, a generalized version in which weights are assigned to the observations was introduced by Cotes. A clear account of his work is included in an unpublished National Bureau of Standards report by C. Eisenhart. Subsequent developments in the analysis of data were concerned with methods of estimation from linear models when the number of unknown parameters was two or more. A method devised independently by Euler and Mayer consisted of subdividing the observations into as many groups as there are known parameters, and then equating the observed total for each group to its theoretical value (Eisenhart, 1964). The subdivision is arbitrary and the subjective element was removed by Boscovich. Suppose that we have observations  $y_i$  on  $\alpha + \beta x_i$  ( $i = 1, \dots, n$ ). Call  $y_i - \alpha - \beta x_i$  the  $i$ th deviation and  $y_i - \hat{\alpha} - \hat{\beta} x_i$  the  $i$ th residual, where  $\hat{\alpha}$  and  $\hat{\beta}$  are estimates of  $\alpha$  and  $\beta$ . Boscovich proposed to estimate  $\alpha$  and  $\beta$  by quantities which satisfy the following conditions: (a) the sum of the deviations is zero; and (b) the sum of the absolute values of the deviations is a minimum. He gave a geometrical method of solution, and applied it to the lengths of five meridian arcs. A study of the work of Boscovich on the combination of observations, and its influence on Laplace, is made by Eisenhart (1961). Another method was proposed by Euler and Lambert, according to which the estimates of  $\alpha$  and  $\beta$  should be the quantities which minimize the absolute value of the largest deviation (Sheynin, 1966). An algorithm for finding the minimax residual was given in 1783 by Laplace, and in 1789 he simplified his earlier procedure. The second memoir also contains a simplified version of what Laplace describes as the ingenious method which Boscovich has given for the purpose of determining the most probable values. Both methods are applied to geodetic data, and in each case a careful analysis is made by comparing the largest residuals with the errors to which the observations are susceptible.

The method of least squares was discovered independently by Gauss and Legendre during the period which followed. Although Gauss had been using the method since about

1795, the first explicit account was published in 1805 by Legendre. Four years later Gauss gave his version, in the course of which he referred to his earlier work. This remark led to much controversy concerning the priority for the discovery. The technical innovations of the period have already been fully reviewed (Seal, 1967). We shall examine the germination of Gauss's ideas on the subject in the intervals before his publications, his reactions to the work of Laplace and Legendre and the impact on all concerned of the questions of priority then raised. The material on which the analysis is based is presented in the next section. Readers who are mainly interested in the discussion can turn there at once and refer back for further information as required.

## 2. MATERIAL

Galle (1924) states that the basic ideas of the method of least squares occurred to Gauss in the autumn of 1794 when he read about the treatment of a surplus of observations in the first volume of Lambert (1765). Gauss was 17 years old at the time, attending the Collegium Carolinum in Brunswick and preparing for his university studies. No evidence is cited in support of Galle's statement, but in the correspondence and other material which is reproduced below Gauss mentions 1794 on several occasions as the year of the discovery. He arrived at Göttingen in October 1795. A list of the books which he borrowed from the university library during his student years has been compiled by Dunnington (1955, pp. 398–404). The first item is volumes I and II of Richardson's novel *Clarissa*, then popular throughout Europe, and the second (24 October 1795) is the three volumes of Lambert's work.

From 1796 to 1814, Gauss recorded brief and often enigmatic summaries of his work in a mathematical diary. An entry for 1798 reads as follows:

# Calculus probabilitatis contra La Place defensus. Gott. Jun. 17.

Here # signifies that Gauss attached some importance to this entry, although much less than for others where there is multiple vertical and horizontal scoring. The entry is mentioned in letters from Gauss to Olbers on 24 January 1812, and from Gauss to Laplace on 30 January 1812. On the basis of these references, Klein and Schlesinger argue convincingly (*Werke*, X/1, 533) that the entry is concerned with the method of Boscovich and Laplace. They also suggest that Gauss had just become acquainted with Laplace's memoir of 1789. Such may indeed be the case, but although the list of books which Gauss borrowed from the library of the University of Göttingen in 1798 before 17 June contains several volumes of *Mémoires de l'Académie de Paris*, those for 1789 receive no specific mention.

An exchange of letters between Gauss and Schumacher in 1832 identifies the following two items as applications of the method of least squares. The German word 'Modulen' which occurs in the original of the first item has been translated as 'units of length'. Gauss is using 2 toises (=  $\frac{1}{1000}$  league  $\simeq$  3.898 metres) as his unit of length (Delambre, 1814, vol. 3, p. 566). The first item (Gauss, 1799) takes the form of a letter to the editor, von Zach, who has added two footnotes, and the second (Gauss, 1800) is a set of corrections.

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,  $2^{\circ} .66868 - v.Z.$

† 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 ellipticity 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*'.

The method of least squares was first named and published by Legendre in *Nouvelles méthodes pour la détermination des orbites des comètes*, which appeared in 1805 (Merriman, 1877). Gauss became aware of Legendre's work not long after publication and took an early opportunity to study the details. We give first an extract from Gauss (1806, p. 184), which again takes the form of a letter to von Zach, and then the relevant portion of a letter to Gauss's friend, the physician and astronomer Olbers (Schilling & Kramer, 1900).

*Brunswick, 8 July 1806*

... I have not yet seen Legendre's work, which you take this occasion to mention. I intentionally did not go out of my way to do so, in order not to disrupt the sequence of my own ideas while working on my method. It was through a couple of words which De la Lande let fall in the recent *Histoire de l'Astronomie* 1805, *méthode des moindres quarrés*, that I tumbled to the idea that a principle which I had made use of for twelve years in many a calculation, and which, too, I intended to employ in my book, although it admittedly does not form an essential part of my method – that this principle had been employed by Legendre as well....

*Gauss to Olbers. Brunswick, 30 July 1806.*

... Hr. v. Zach writes to me further that you have offered to review Legendre's work on the orbits of comets. I will, therefore, with pleasure send to you at Bremen the copy which Hr. v. Zach sent me; but will you please allow me first to keep it for a few weeks yet. From a preliminary inspection it appears to me to contain much that is very beautiful. Much of what was original in my method, particularly in its first form, I find again also in this book. It seems to be my fate to compete with Legendre in almost all my theoretical works. So it is in the higher arithmetic, in the researches on transcendental functions connected with the rectification of the ellipse, in the fundamentals of geometry, and now again here. Thus, for example, the principle which I have used since 1794, that the sum of squares must be minimized for the best representation of several quantities which cannot all be represented exactly, is also used in Legendre's work and is most thoroughly developed. He does not seem to know your work....

Eight months later Gauss made the same point concerning prior use and he also referred somewhat more explicitly to the matter concealed behind the entry in his diary for 17 June 1798.

*Gauss to Olbers. Brunswick, 24 March 1807.*

... I am occupied at present with treating the problem: 'To determine the most probable values of a number of unknown quantities from a *larger* number of observations depending on them', on the basis of the calculus of probability. The principle that the sum of squares of the differences between the calculated and observed quantities must be minimized, I have employed for many years; I mentioned it to you long ago, and it is now also advanced by Legendre. In this connection also, the principle is preferable to that of Laplace, according to which the sum of all differences must be zero, and the sum of the same differences, each taken with positive sign, should be a minimum. It can be shown that this is not admissible on the basis of the calculus of probability, but leads to contradictions....

Gauss completed in 1806 a German version of the book he had been preparing, but conditions were such that he had difficulty in finding a publisher. Eventually, Perthes agreed to

publish provided that the work was translated into Latin. Thus it was that *Theoria Motus Corporum Coelestium* appeared in 1809. Section 186 contains a simple but penetrating comparison of the method of least squares with its competitors in which the idea of admissibility briefly appears. The following is the translation published by Davis (1857), with a few corrections.

On the other hand, the principle that the sum of the squares of the differences between the observed and computed quantities must be as small as possible may, in the following manner, be considered independently of the calculus of probabilities.

When the number of unknown quantities is equal to the number of the observed quantities depending on them, the former may be so determined as exactly to satisfy the latter. But when the number of the former is less than that of the latter, an absolutely exact agreement cannot be determined, in so far as the observations do not enjoy absolute accuracy. In this case care must be taken to establish the best possible agreement, or to diminish as far as practicable the differences. This idea, however, from its nature, involves something vague. For, although a system of values for the unknown quantities which makes *all* the differences respectively less than another system, is without doubt to be preferred to the latter, still the choice between two systems, one of which presents a better agreement in some observations, the other in others, is left in a measure to our judgement, and clearly innumerable different principles can be proposed by which the former condition is satisfied. Denoting the differences between observation and calculation by  $\Delta, \Delta', \Delta''$  etc., the first condition will be satisfied not only if  $\Delta\Delta + \Delta'\Delta' + \Delta''\Delta'' +$  etc., is as small as possible (which is our principle), but also if  $\Delta^4 + \Delta'^4 + \Delta''^4 +$  etc., or  $\Delta^6 + \Delta'^6 + \Delta''^6 +$  etc., or in general, if the sum of any of the powers with an even exponent becomes as small as possible. But of all these principles ours is the most simple; by the others we shall be led into the most complicated calculations.

On the other hand our principle, which we have made use of since the year 1795, has lately been published by LEGENDRE in the work *Nouvelles méthodes pour la détermination des orbites des comètes*, Paris 1806, where several other properties of this principle have been explained, which, for the sake of brevity, we here omit.

If we were to adopt a power with an infinitely great, even exponent, we should be led to that system in which the greatest differences become as small as possible.

LAPLACE made use of another principle for the solution of linear equations, the number of which is greater than the number of unknown quantities, which had been previously proposed by BOSCOVICH, namely that the differences themselves, but all of them taken positively, should make up as small a sum as possible. It can be easily shown, that a system of values of unknown quantities, derived from this principle alone, must necessarily\* exactly satisfy as many equations out of the number proposed, as there are unknown quantities, so that the remaining equations come under consideration only so far as they help to determine the choice: if, therefore, the equation  $V = M$ , for example, is of the number of those which are not satisfied, the system of values found according to this principle would in no respect be changed, even if any other value  $N$  had been observed instead of  $M$ , provided that, denoting the computed value by  $n$ , the differences  $M - n, N - n$ , were affected by the same signs. On the other hand, LAPLACE qualifies in some measure this principle by adding a new condition: he requires, namely, that the sum of the differences, the signs remaining unchanged, be equal to zero. Hence it follows, that the number of equations exactly represented may be less by unity than the number of unknown quantities; but what we have said before will still hold good if there are at least two unknown quantities.

\* Except the special cases in which the problem remains, to some extent, indeterminate.

In fact, both of the conditions on the differences are due to Boscovich. There appears to be a slip of the pen in the last sentence, with 'less by unity' in place of 'more by unity'.

The *Theoria Motus* became known to Legendre through Sophie Germain, whose career is summarized by Dunnington (1955, p. 68). A letter from Legendre to Gauss soon followed. Only a short extract is given in *Werke*, X/1, 380, but the original is in the Gauss archives at Göttingen. The interest of this letter, and the part it plays in the relationship between Legendre and Gauss, are such as to justify the complete translation which follows.

*Legendre to Gauss. Paris, 31 May 1809.*

I imagine Sir, that Mademoiselle Germain will have delivered the message which she kindly agreed to take, which was to thank you very much for the paper which you were kind enough to send me on the

summation of a number of series. Your writings on a subject of which I have always been fond could not fail to interest me deeply, and I noted the fecundity of your genius which led you to discover a fourth or fifth demonstration of the proposition to which I have given the name of the law of reciprocity between two prime numbers.

A few days ago Mlle. Germain received from Germany your *Theoria Motus Corporum Coelestium*; she has passed on extracts, and from the little I have managed to read, I can see that this work is worthy of your reputation, and that you have vindicated analysis from the reproach which might have been levelled at it, of not providing astronomers with practical methods of resolving with the necessary exactitude the problem of determining the orbit of a planet by three observations.

In my researches into determining the orbits of comets, I too had the aim of demonstrating that analysis, properly handled, was capable of providing solutions which were as quick as, and more reliable than, those arrived at by synthetic methods, such as those of Olbers. Your object, however, is more extensive, and you have endowed science with a method which will become very useful in the art of astronomy, especially if new planets continue to be discovered.

It was with pleasure that I saw that in the course of your meditations you had hit on the same method which I had called *Méthode des moindres quarrés* in my memoir on comets. The idea for this method did not call for an effort of genius; however, when I observe how imperfect and full of difficulties were the methods which had been employed previously with the same end in view, especially that of M. La Place, which you are justified in attacking, I confess to you that I do attach some value to this little find. I will therefore not conceal from you, Sir, that I felt some regret to see that in citing my memoir p. 221 you say *principium nostrum quo jam inde ab anno 1795 usi sumus* etc. There is no discovery that one cannot claim for oneself by saying that one had found the same thing some years previously; but if one does not supply the evidence by citing the place where one has published it, this assertion becomes pointless and serves only to do a disservice to the true author of the discovery. In Mathematics it often happens that one discovers the same things that have been discovered by others and which are well known; this has happened to me a number of times, but I have never mentioned it and I have never called *principium nostrum* a principle which someone else had published before me. You have treasures enough of your own, Sir, to have no need to envy anyone; and I am perfectly satisfied, besides, that I have reason to complain of the expression only and by no means of the intention.

I will make the point as well, Sir, taking quite the opposite standpoint, that you have inadvertently attributed to M. de La Place something which belongs to Euler. You say on page 212, 'per theorema elegans primo ab ill. La Place inventum integrale  $\int e^{-t^2} dt$  à  $t = -\infty$  ad  $t = +\infty$ ,  $= \sqrt{\pi}$ '. This theorem was discovered a long while beforehand by Euler who gave in general the integral  $\int dx \left( l \frac{1}{x} \right)^{\frac{1}{2}(2n+1)}$  from  $x = 0$  to  $x = 1$ . Therefore one has only to set  $e^{-t^2} = x$  to change  $\int e^{-t^2} dt$  into  $\frac{1}{2} \int dx \left( l \frac{1}{x} \right)^{-\frac{1}{2}}$ . See Com. petr. vol. v, Nov. Comm. vol. xvi, Nova acta petr. vol. v, etc.

I have the honour to be, Sir, your obedient servant.

Le Gendre

With regard to the final paragraph, De Moivre derived the normal function in November 1733, using a constant which Stirling almost at once showed to be  $\sqrt{\pi}$  (K. Pearson, 1924). However, the force of this letter for Gauss resided in the paragraph immediately preceding. When writing to Olbers later in the year, Gauss took the opportunity to seek his support.

*Gauss to Olbers. Göttingen, 4 October 1809.*

...Do you still remember, dearest friend, that on my first visit to Bremen in 1803 I talked with you about the principle which I used to represent observations most exactly, namely that the sum of squares of the differences must be minimized when the observations have equal weights? That we discussed the matter in Rehburg in 1804 of that I still clearly recollect all the circumstances. It is important to me to know this. The reason for the question can wait...

A formal letter from Laplace to Gauss two years later indicates that the question of priority was still under active discussion. The letter may have accompanied the memoirs mentioned by Gauss in his reply (30 January 1812).

*Laplace to Gauss. 15 November 1811.*

M. Gauss says in his work on elliptical movement that he was conversant with it before M. Le Gendre had published it; I would greatly like to know whether before this publication anything was printed in Germany concerning this method and I request M. Gauss to have the kindness to inform me about it.

Soon afterwards, Gauss put his case at length to Olbers and Laplace in letters separated by less than a week. In the first letter Gauss again asks Olbers for support but still withholds the reason. The second letter opens with a problem in metric number theory, first solved in 1928 by Kuzmin (Uspensky, 1937, Appendix III; Gnedenko, 1957). No reply from Laplace is recorded, and all his papers were destroyed by a fire at the Château de Mailloc in 1925 (Smith, 1927; K. Pearson, 1929, p. 215).

*Gauss to Olbers. Göttingen, 24 January 1812.*

... I hear that Hr. Delambre has given *ad modum suum*, a very prolix theory about *moindres carrés* in the French *Moniteur*; I do not read the *Moniteur*, which also only comes here in yearly deliveries. Perhaps you will find an opportunity sometime, to attest *publicly* that I already stated the essential ideas to you at our first personal meeting in 1803. I find among my papers that in June 1798, when the method was one which I had *long* applied, I first saw Laplace's method and indicated its incompatibility with the principles of the calculus of probability in a short diary-notebook about my mathematical occupations. In Autumn 1802 I entered in my astronomical notebook the eighth set of elements of Ceres, found by the method of least squares. The papers have now been lost, in which I applied that method in earlier years, e.g. in Spring 1799 on Meyberg's table of the equation of time. The only thing which is surprising is that this principle, which suggests itself so readily that no particular value at all can be placed on the idea alone, was not already applied 50 or 100 years earlier by others e.g. Euler or Lambert or Halley or Tobias Mayer, although it may very easily be that the last, for example, has applied that sort of thing without announcing it, just as every calculator necessarily invents a collection of devices and methods which he propagates by word of mouth only as occasion offers....

*Gauss to Laplace. Göttingen, 30 January 1812.*

I express my deep thanks for the two memoirs which you did me the honour of sending me and which I received a few days ago. The functions which you deal with therein, as well as the questions of probability, on which you are preparing a large work, have a great attraction for me, although I have worked little myself on the latter. I am reminded however of a curious problem which I worked at 12 years ago, but which I did not succeed in solving to my satisfaction. Perhaps you would care to study it for a few moments: in this case I am sure that you will find a complete solution. Here it is. Let  $M$  be an unknown quantity between the limits 0 and 1, for which all values are either equally probable or varying according to a given law: and that we suppose it expressed as a continued fraction

$$M = \cfrac{1}{a' + \cfrac{1}{a'' + \text{etc.}}}$$

What is the probability, that in stopping the expansion at a finite term,  $a^{(n)}$ , the following fraction

$$\cfrac{1}{a^{(n+1)} + \cfrac{1}{a^{(n+2)} + \text{etc.}}}$$

lies between the limits 0 and  $x$ ? I denote it by  $P(n, x)$  and I obtain for it supposing that all values of  $M$  are equally probable

$$P(0, x) = x;$$

$P(1, x)$  is a transcendental function depending on the function

$$1 + \frac{1}{2} + \frac{1}{3} + \dots + 1/x$$

which Euler calls inexplicable and on which I have given several results in a memoir presented to our scientific society which will soon be printed. But in the cases when  $n$  is larger, the exact value of  $P(n, x)$  seems intractable. However I have found by very simple arguments that for infinite  $n$  we have

$$P(n, x) = \frac{\log(1+x)}{\log 2}.$$

But the efforts which I made at the time of my researches to determine

$$P(n, x) - \frac{\log(1+x)}{\log 2}$$

for a very large, but not infinite, value of  $n$  were unfruitful.

I have used the method of least squares since the year 1795 and I find in my papers, that the month of June 1798 is the time when I reconciled it with the principles of the calculus of probabilities: a note about this is contained in a diary which I kept about my mathematical work since the year 1796, and which I showed at that time to Mr. De Lindenau.

However my frequent applications of this method only date from the year 1802, since then I use it as you might say every day in my astronomical calculations on the new planets. As I had intended since then to assemble all the methods which I have used in one extensive work (which I began in 1805 and of which the manuscript originally in German, was completed in 1806, but which at the request of Mr Perthes I afterwards translated into Latin: printing began in 1807 and was finished only in 1809), I am in no hurry to publish an isolated fragment, therefore Mr. Legendre has preceded me. Nevertheless I had already communicated this same method, well before the publication of Mr Legendre's work, to several people among other to Mr. Olbers in 1803 who must certainly remember it. Therefore, in my *theory of the motions of planets*, I was able to discuss the method of least squares, which I have applied thousands of times during the last 7 years, and for which I had developed the theory, in section 3 of book II of this work, in German at least, well before having seen Mr Legendre's work – I say, could I have discussed this principle, which I had made known to several of my friends already in 1803 as being likely to form part of a work which I was preparing, – as a method derived from Mr. Legendre? I had no idea that Mr. Legendre would have been capable of attaching so much value to an idea so simple that, rather than being astonished that it had not been thought of a hundred years ago, he should feel annoyed at my saying that I had used it before he did. In fact, it would be very easy to prove it to everyone by evidence which could not be refuted, if it were worth the trouble. But I thought that all those who know me would believe it on my word alone, just as I would have believed it with all my heart if Mr Legendre had stated that he had already been conversant with the method before 1795. I have many things in my papers, which I may perhaps lose the chance of being first to publish; but so be it, I prefer to let things ripen.

...

Continue, Sir, to honour me with your good will, which I rank among the things most essential to my happiness.

Ch. Fr. Gauss.

Olbers readily agreed to support Gauss and some four years later his promise was fulfilled by the addition of a footnote to a paper on the period of a star (Olbers, 1816, p. 192).

*Olbers to Gauss. Bremen, 10 March 1812.*

... I have now received the November issue of *M.C.*, and with it your beautiful elimination method for *moindres carrés*. I can attest publicly at the first opportunity, and will do so with pleasure, that you had already told me the basic principle in 1803. I remember this quite well as if it had happened today. There must also be something concerning it written down among my papers. For I noted it then, together with your interpolation formula, which you communicated to me at that time....

A letter to Olbers in 1819 describes the progress of the work which freed the method of least squares from assumptions of normality and culminated in the monographs presented to the Royal Society of Göttingen in 1821, 1823 and 1826.

*Gauss to Olbers. Göttingen, 22 February 1819.*

... I am also occupied at present with a new basis for the so-called method of least squares. In my first basis I supposed that the probability of an observational error  $x$  was represented by  $e^{-hx^2}$ , in which event that method gives the most probable result with complete rigour in all cases. When the law of error is unknown, it is *impossible* to state the most probable results from observations *already made*. Laplace has considered the matter from a different angle and chosen a principle, which leads to the method of least squares, and which is quite independent of the law of error, when the number of observations is indefinitely large.

With a moderate number of observations, however, one remains quite in the dark if the law of error is unknown, and Laplace himself has also nothing better to say in this case, than that the method of least squares may also be applied here because it affords convenient calculation. I have now found that, by the choice of a principle somewhat different from that of Laplace (and indeed, as cannot be denied, one such that its assumption can be justified at least as well as that of Laplace, and which, in my opinion, must strike *anyone without a previous predilection* as more natural than Laplace's) – all those advantages are retained, namely that in all cases for every error-law the method of least squares will

be the most advantageous, and the comparison of the precision of the results with that of the observations, which I had based in my *Theoria* on the error-law  $e^{-\frac{1}{2}n^2}$ , remains generally valid. At the same time there is the advantage, that everything can be demonstrated and worked out by very clear, simple, analytical developments, which is by no means the case with Laplace's principle and treatment, thus, for instance, the generalization of his conclusions from two unknown parameters to any number does not yet appear to have the justification necessary....

In 1827 Legendre received a letter from Jacobi on the subject of elliptic functions. Legendre was then 75 whereas Jacobi was 23. Notwithstanding the disparity in age, a correspondence was established and lasted about five years (Legendre, 1875). Here is a brief extract from the inaugural letter, followed by the closing section of Legendre's reply.

*Jacobi to Legendre. Königsberg in Prussia, 5 August 1827.*

...It is only very recently that these researches have taken shape. However they are not the only investigations in Germany with the same object. Mr. Gauss, being informed of them, told me that he had already developed in 1808 the cases of 3 sections, 5 sections, and 7 sections, and discovered at the same time the new scales of modules which are related to them. It seems to me that this news is quite interesting....

*Legendre to Jacobi. Paris, 30 November 1827.*

...How can Mr. Gauss have dared to tell you that the greater part of your theorems were known to him and that he discovered them as early as 1808?...This extreme impertinence is incredible on the part of a man who has sufficient personal merit to have no need of appropriating the discoveries of others.... But this is the same man who, in 1801, wished to attribute to himself the discovery of the law of reciprocity published in 1785 and who wanted to appropriate in 1809 the method of least squares published in 1805. - Other examples will be found in other places, but a man of honour should refrain from imitating them.

Meanwhile the controversy originated by the publication of the *Theoria Motus* continued to reverberate in Germany also (Peters, 1860-5).

*Schumacher to Gauss. Altona, 30 November 1831.*

...I believe I have already said to you that Zach printed a letter from you in the G.E. (1799 October, p. 378), in which you evidently mentioned the method of least squares, which you had thus already communicated to Zach at the time. You write of the French survey 'I discovered this error when I applied my method, a specimen of which I have given you' etc. Zach noted at that place 'Here at another time' but the other time has never come. Because you gave the results of your calculations, it seems to me that it is easy to show that these were derived by the method of least squares. Besides, Zach is still alive, and has surely preserved your letter. Do you not find it worth the trouble to establish the matter beyond doubt once and for all, even in the face of the polite doubts of the French, which I find particularly offensive?

*Gauss to Schumacher. Göttingen, 3 December 1831.*

...The place you mention in Zach's A.G.E. is well-known to me; the application of the method of least squares mentioned there concerns an extract from Ulugh Beigh's table of the equation of time, printed earlier in the same journal, which had led to a number of quite curious results. I had communicated these results to Zach with the remark that in connection with them I had employed a method of my own, which I had used for years, of combining quantities involving random errors in a consistent way free from arbitrariness. However, I did not inform him of the nature of the method. I believe I have written to you once before, that in no event will I discuss this passage, where the method was publicly indicated for the first time, also that I do not wish one of my friends to do it with my assent. This would amount to recognizing that my announcement (Th.M.C.C.) that I had used this method many times since 1794 is in need of justification, and with that I shall never agree. When Olbers attested, that I communicated the whole method to him in 1802 [sic], this was certainly well meant; but if he had asked me beforehand, I would have disapproved it strongly....

After 1827 Gauss published nothing further about the theoretical basis, but he lectured on the method of least squares from 1835 (Dunnington, 1955, p. 409), and anticipated ideas

of decision theory in a letter to Bessel. A section of the following extract has been previously translated by Edgeworth (1908, pp. 386–7).

*Gauss to Bessel. Göttingen, 28 February 1839.*

... I have read with great interest your paper in *Astronomische Nachrichten*, and on how the law for the probability of errors of observation arising simultaneously from several sources, approaches the formula  $e^{-\pi x^2/h^2}$ ; yet, speaking sincerely, this interest was less concerned with the thing itself than with your exposition. For the former has been familiar to me for many years, though I myself have never arrived at carrying out the development completely. Moreover, the fact that I later abandoned the metaphysics used in the T.M.C.C. for the method of least squares also happened rather for a reason which I myself have not mentioned publicly. Namely, I must consider it less important in every way to determine that value of an unknown parameter for which the probability is largest, although still infinitely small, rather than that value, by relying on which one is playing the least disadvantageous game; or if  $f(a)$  denotes the probability that the unknown  $x$  has the value  $a$ , then it is less important that  $f(a)$  should be a maximum than that  $\int f(x)F(x-a) dx$ , taken over all possible values of  $x$ , should be a minimum, where for  $F$  is chosen a function which is always positive and which always increases with increasing arguments in a suitable way. Choosing the square for this function is purely arbitrary and this arbitrariness lies in the nature of the subject. Without the known extraordinarily large advantages afforded by choosing the square, any other function satisfying those conditions could be chosen, and moreover is chosen in quite exceptional cases. But I do not know whether I have expressed myself sufficiently clearly....

The last letter which refers to the discovery of the method of least squares has essentially the same theme in the relevant portion as the letter from Gauss to Laplace in 1812, but the expression of ideas is now calm and even light-hearted. Gauss had been looking through the papers of Tobias Mayer and was led by easy stages to a reconsideration of the whole affair.

*Gauss to Schumacher. Göttingen, 6 July 1840.*

... Among other papers by Mayer (where incidentally, I have found nothing suitable for dissemination), which are still extant and now in my possession, I can find a few quarto sheets of rough notes or draft calculations which contain a small number of passages which are plainly numerical calculations comparing his theory with observations. As far as I can recall at the moment there are about 4 to 6 such passages.

...

These sketch notes, incidentally, have been somewhat of a disappointment to me in another respect. You know that I have never laid great store myself upon the procedures which I have been using since 1794, and which have subsequently been given the name *Méthode des moindres quarrés*. Please understand me correctly, I do not mean in relation to the great benefits which they yield, that is clear enough; but that is not how I measure the value of things. Rather it was for this reason, or to this extent, that I did not rate it very highly, that from the very beginning the idea seemed to me to be so natural, so accessible, that I did not doubt in the slightest that many people, who had to deal with numerical calculations, must of their own accord have arrived at such a device, and used it, without thinking it worthwhile to make much fuss about a thing so natural. To be precise, I had Tobias Mayer particularly in mind, and I remember very clearly that, when I used to discuss my method with other people (as, for example, happened really frequently during my student days, 1795–1798), I often expressed the view, that I would lay a hundred to one that Tobias Mayer had already used the same method in his calculations. I now know from these papers that I should have lost my bet. In the event they contain eliminations, e.g., of three unknown quantities from four or five equations, but in such a way that the most commonplace calculator would have done it, without any trace of a more refined style.

At all events, I pass this on in confidence. It has really pained me to have my opinion of Mayer somewhat lowered; but what good would it do to publicize it. I loathe minxit in patrios cineres....

Gauss died in 1855 and in 1856 his friend Sartorius published the biography which has remained an important source of information for other biographers. The section which refers to the method of least squares (pp. 42–3) confirms many of the points already made, and ends with a couple of significant questions.

...One time, mentioning the dispute, he expressed himself to us in the words 'The method of least squares is not the greatest of my discoveries'. Another time, to several listeners, he stressed only the words 'They might have believed me.'...

### 3. DISCUSSION

The principle that publication establishes priority is supported by reason as much as by precedent. A mathematician or scientist attaches great importance to his reputation as a scholar, both in respect of personal esteem and because it determines his livelihood. His reputation is based on the work which he has achieved, and consequently he needs to establish that such work is indeed his own and not derived from others. The publication of his work by any method which is generally accepted, such as a doctoral dissertation, a book, or in a recognized journal, clearly defines the point in time when his discoveries become known to others. Thus the originator of a piece of research can be assessed in such a way that justice is seen to be done. These arguments are given because some commentators on Gauss take another view. For example, Dunnington (1955, p. 19) writes 'according to the custom then in vogue Legendre gained the right of priority'. In fact, the custom has changed little since the time of the dispute between Newton and Leibniz about the discovery of the differential calculus.

Sartorius had no hesitation in assuming that practical need, the observation of nature itself, led Gauss to the method of least squares. The year in which he first applied the method is given variously as 1794 and 1795. Gauss then turned to the problem of establishing a relationship between the principle of minimizing a sum of squares and the calculus of probability. According to a contemporary account of his lecture to the Royal Society of Göttingen on 15 February 1826 (*Werke*, 4, 98), he formulated in 1797 the problem of selecting from all possible combinations of the observations that one which minimized the uncertainty of the results. However, he found that no progress was possible in terms of the most probable values of the unknown quantities when the distribution of errors was unknown. He therefore decided to follow the opposite path first, and look for the distribution which gave the arithmetic mean as the most probable value. Both at this stage and subsequently, the work of Laplace was a stimulus which led Gauss to improve upon the results of his great contemporary. Indeed, Laplace is cast in the role of an adversary from whom the calculus of probability must be protected, and with whom Gauss wrestles in his extraordinary letter to Olbers in 1819. The fact that Gauss completed his theoretical researches on the method of least squares in the same year that Laplace died is doubtless a coincidence, but his letter to Bessel in 1839 tempts us to speculate on what further advances he might otherwise have made.

From first to last, Gauss regarded the method of least squares as his own discovery: he refers to 'meine Methode' both in 1799 and in 1840. When he first saw Legendre's work in July 1806, Gauss commented very favourably and expressed regret about the competition between them. But after Legendre made known his disapproval of the references in the *Theoria Motus* to 'principium nostrum', there was a change of emphasis. On a number of subsequent occasions, Gauss remarked that the idea of minimizing the sum of squared differences was essentially simple and likely to have been used previously. According to his 1840 letter to Schumacher, this had always been his opinion, but he does not mention in it his 1806 letter to Olbers. Possibly he considered that the controversy was disproportionate in size compared with what was involved, and that another aspect should be stressed. The name which Legendre gave, 'méthode des moindres quarrés', must have been generally

accepted almost at once and, is, of course, still in use today, suitably translated. According to Dunnington (1955, p. 113), 'by adopting this nomenclature Gauss showed that he did not feel hurt because he was anticipated', but the fact that we find 'der sogennanten Methode der Kleinsten Quadrate', and 'dem später der Name Méthode des moindres quarrés beigelegt ist' suggests a certain amount of antipathy towards this description of 'meine Methode'.

We now turn to Legendre. A full account of his career would be out of place but some aspects of his character are important in a study of his relationship with Gauss. His attitude towards the question of priority is well expressed by the following translated extract from *Mémoires de l'Académie des Sciences* for 1786 (Élie de Beaumont, trans. Alexander, 1867):

I shall not conclude this article without giving notice that the greater part of the propositions contained therein have been discovered by M. Euler and published in the 7th volume of the *Nouveau Mémoires de Petersbourg* and in some other works, a fact of which I was ignorant when I was engaged in these researches.

According to his biographer in *Nouvelle Biographie Générale* (1859, vol. 30, cols. 385–388), Legendre had studied the works of Euler so assiduously that it could be said he knew them by heart. He always spoke his mind, even when his interests were adversely affected. At the time of the French Revolution he was forced to hide himself, and he was unable to resume public instruction before December 1795 (Élie de Beaumont, 1867). In 1824 he lost his pension because he refused to vote in favour of a candidate for the National Institute, proposed by the government. Immediately after the publication of the first two volumes of his *Traité des fonctions elliptiques et des intégrales eulériennes* in 1825 and 1826, the theory was transformed by Abel and Jacobi. He generously recognized the merits of this work and incorporated their discoveries in a third volume, which appeared shortly before his death (Nielsen, 1929).

Legendre was thus a man of integrity. The full text of his letter to Gauss, which has not been previously published, contains a judicious mixture of praise and criticism, whereas the portion reproduced in Gauss's collected works is critical only. An account of the least squares controversy is given by Bell (1939, pp. 294–5) in his well-known set of biographical sketches of mathematicians. Unfortunately, he makes mistakes about names and dates, and there are reasons for rejecting his conclusions about Legendre. According to Bell, Legendre (i) practically accused Gauss of dishonesty, (ii) regarded the method of least squares as his own ewe lamb, and (iii) passed on his unjustified suspicions to Jacobi and so prevented Jacobi from coming to cordial terms with Gauss. Now Bell was almost certainly unaware of all that Legendre's letter contained. But even if we confine attention to the section which refers to *principium nostrum*, the emphasis is on two points only: the reason why a claim of priority has to be established by publication; and the convention that a rediscovery is passed over in silence. The suggestion about a ewe lamb is quite out of character, as is shown by Legendre's treatment of the discoveries of Abel and Jacobi. As regards the alleged subversion of Jacobi, who was quite capable of forming his own opinions, the comments made by Gauss on his discoveries must already have been very disheartening. The caustic tone of Legendre's letter of 1827 can reasonably be interpreted as encouragement for a young man of great promise whom he considered to be treated unfairly. But the entire course of Bell's account is so tendentious that it scarcely merits a detailed refutation.

For his part, Gauss was less than wholehearted in accepting the principle that publication establishes priority. This is illustrated by his use of the term 'principium nostrum', the

passage with Jacobi, the letter to Bessel, and numerous other instances (Dunnington, 1955). His procedure was to wait until all the aspects of a problem had been rigorously explored before publishing, and treat casually what others did meanwhile. As he wrote to Laplace, 'j'aime mieux faire mûrir les choses'. He published nothing on non-Euclidean geometry, and his work on elliptic functions became fully known only after his death.

Another consequence of Legendre's critical remarks was that Gauss felt concerned to ensure that his announcement of earlier work was accepted as truthful, and his concern was increased after the note from Laplace. He asked Olbers to attest publicly that he had previously communicated the matter to him, but after Olbers had done so, Gauss expressed disapproval because he considered that his announcement did not need justification. Presumably the opportunity for reflection which came with the passage of time, and a dislike of controversy, had led Gauss to have second thoughts about his request. But his disappointment with the response to his announcement is evident in the remark 'Man hätte mir wohl glauben können.'

The last word may appropriately be left with Laplace (1820, p. 353), although he was writing long before the controversy died away.

M. Legendre eut l'idée simple de considérer la somme des carrés des erreurs des observations, et de la rendre un minimum, ce qui fournit directement autant d'équations finales, qu'il y a d'éléments à corriger. Ce savant géomètre est le premier qui ait publié cette méthode; mais on doit à M. Gauss la justice d'observer qu'il avait eu, plusieurs années avant cette publication, la même idée dont il faisait un usage habituel, et qu'il avait communiquée à plusieurs astronomes.

We must distinguish the history of statistics and probability from the search for priorities in published work. The two are not identical, as Seal (1967) appears to suggest, and some important differences are as follows. First, a historical account should include if possible the process of challenge and discovery which precedes each publication, and which is illustrated by the episode described above. Secondly, we must attempt to decide whether a publication had any real influence at the time, or whether it sank at once into oblivion and was only rediscovered many years later after the same results had been found independently by other investigators. For example, the work of Adrain on the method of least squares was contemporary with that of Gauss, Laplace and Legendre, but it had no effect whatsoever on the development of the subject. Finally, published work must be interpreted so as to include correspondence, and unpublished work may also be important. Many scientists whose influence has been decisive left behind unpublished manuscripts, and wrote hundreds of letters to their contemporaries. Fortunately, all such material relating to Gauss has been collected and published as the result of immense labours of scholarship. Such are the grounds for believing that the detailed description of published scientific papers in chronological order of appearance gives only a partial and possibly a biased interpretation of the history of statistics and probability.

My debt to the editors of Gauss's collected works and correspondence has already been mentioned. I am grateful to the State and University Library of Lower Saxony for permission to publish a translation of the letter from Legendre to Gauss. They retain the copyright of the manuscript. I express my deep thanks to Mr R. M. White, who made the translation, and to Dr A. Fletcher who gave advice on units of measurement. Other translations have greatly benefited from the help kindly given by Mr White, Dr Fletcher and Professor D. A. West. The comments of Professor G. A. Barnard, Professor G. Waldo Dunnington, Dr C.

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