Biometrika (1975), 62, 2, p. 503
Printed in Great Britain

Studies in the History of Probability and Statistics. XXXIV

Napoleonic statistics: The work of Laplace

By STEPHEN M. STIGLER
Department of Statistics, University of Wisconsin, Madison

SUMMARY

The work of Pierre Simon, Marquis de Laplace, was more important to the early development of mathematical statistics than that of any other individual. This paper reviews both his major contributions to statistical theory, and his application of this theory to scientific problems. These applications paradoxically reveal both surprising depth and unexpected limitations to his understanding of statistical concepts. Two of Laplace's investigations are examined in detail. The first of these is a solution of a multiple regression problem arising in an astronomical problem he solved in 1787. Secondly, a meteorological investigation Laplace made in 1823 and 1827 is discussed, and his methods of dealing with a multiple regression problem with correlated observations and with a multiple comparison problem are explained.

Some key words: Bayes; Correlation; History of statistics; Laplace; Power; Regression; Significance test.

1. INTRODUCTION

In December 1797, Napoleon Bonaparte, then a general, was elected to the First Class of The Institute of France. This was the section of the Academy of Sciences devoted to the study of Mathematics and Physics, and Napoleon's election was a recognition of both his political position, and of his interest and ability in science. Shortly after he was accorded this great honour, he attended the Academy's annual meeting; following Napoleon's name on the signed record of attendance was that of Pierre Simon Laplace. Their proximity was not accidental; they are said to have met in 1785 when Laplace, then at age 36 the examiner at the École Militaire, examined the 16-year-old Corsican and, fortunately for both their careers, passed him (Crosland, 1967, pp. 9 ff., 63 ff.).

Laplace lived through the Napoleonic era, and his life's work was Napoleonic in its scope. Laplace's work influenced nearly every scientific discipline of that time, but it is the aim of this paper to concentrate upon his contributions to mathematical statistics. Laplace's work touched an impressive variety of areas in statistics: Bayesian and non-Bayesian decision theory, estimation and hypothesis testing, regression and experimental design, large-sample theory, order statistics, sample survey, nonparametrics, time series and applications to both physical and social science. Based on the breadth and depth of his contributions, and the wide notice his work received, I claim that Laplace was more responsible for the early development of mathematical statistics than was any other man.

Laplace was born in Normandy in March 1749, at a time when mathematics was flourishing, but mathematical statistics was only in its infancy. When Laplace was born, Abraham DeMoivre was 81, Thomas Bayes was 47, and Lagrange was 13. Laplace's birth preceded Gauss's by 28 years and Ronald Fisher's by 141 years. Laplace was both talented and ambitious, and within 2 years after he moved to Paris at the age of 18, he gained a post at the École Militaire. He was admitted to the Academy of Sciences in 1773, and he was launched on a career that became truly illustrious; over the following half century he produced a body of work concerned with celestial mechanics, mathematics, and probability theory which was of such importance, that well before his death he had earned a full measure of the fame that he coveted: he was known as the Newton of France.

Laplace taught at the École Normale in 1795, and served briefly as Napoleon's Minister of Interior in 1799, before being appointed to the Senate. He rose to the presidency of the Senate, and survived the deposition of Napoleon to be made a marquis by Louis XVIII in 1817. In his later years he remained a productive scholar, working part of the time with the Bureau des Longitudes. He died in 1827, just weeks before his 78th birthday.

During this most tumultuous period of French history, Laplace's scientific work prospered; indeed his two major treatises were largely composed during the French Revolution and the Napoleonic Wars. It is to this scientific work I shall now turn.

The emphasis of this paper will be upon the light Laplace's application of statistics sheds upon the development of his statistical understanding and upon the level of statistical thought in Napoleonic times. In the following section I will summarize the whole of Laplace's work as it relates to mathematical statistics, while §§ 3 and 4 will concentrate on applications over a 40-year span, and § 5 will comment generally on his role in the development of statistics.

2. LAPLACE'S WORK

The main body of Laplace's scientific work has been described as falling into two different areas: celestial mechanics, and probability and mathematical statistics. However, if one looks at his work from the viewpoint of a statistician, such a dichotomy seems more apparent than real. Laplace was an applied mathematician who worked as hard developing his mathematical tools as he did applying them to model and explain the phenomena he was studying, not merely in celestial mechanics, but also in meteorology, physics, chemistry, demography, and geography.

His work on areas of application inspired and exploited his mathematical advances, while his work in mathematics contains almost nothing which was not useful for applications, although Laplace apparently showed some early interest in number theory. On 30 December 1776 Lagrange wrote to him, complimenting him on new proof of a result of Fermat's, adding that 'It is a great satisfaction for me to see that you have acquired a taste for this sort of research...'. But in Laplace's case a taste seems to have been enough (Lagrange, 1892, pp. 67, 75).

Laplace's life-work could be roughly described as falling into three overlapping phases. In the first phase, running from his earliest memoirs in 1770 until the early 1780s, a primary concern was with mathematical tools, with differential equations, with series and difference equations, and with probability. To statisticians, perhaps his most notable works of this period were his memoirs of 1774 and 1781 in which he discovered and elaborated on Bayes's Theorem.

Laplace's early work on Bayes's Theorem was apparently done entirely independently of, or more correctly, in ignorance of Bayes's posthumously published 1764 essay. At the time Bayes's essay appeared, Laplace was only 15, likely without access to or interest in the *Philosophical Transactions* of the Royal Society, and I have been unable to trace a single reference to the paper by anyone, save Richard Price, in the intervening years (1764–74). It is true that scientists at that time were far from conscientious in their citation practices, but in this case there is corroborative evidence that neither Lagrange, nor Condorcet, nor any other continental mathematician with an interest in probability was aware of Bayes's essay before about 1780. There are frequent instances in the literature of this time (Todhunter, 1865) where mathematicians attempt arguments related to 'inverse probability' and commit major errors, such as appealing to the law of maturity of chances, errors which might have been avoided had they known of Bayes's essay. Even Lagrange, writing about 1769 (Lagrange, 1770–3, p. 199ff.), gave such a cloudy discussion of a problem in inverse probability that it is doubtful whether he had read Bayes. And Condorcet, a man more likely to have known of Bayes's work than any other French mathematician, neglected any mention of Bayes in his discussion of Laplace's memoir in the preface to the volume containing the 1774 memoir; rather, by his emphasis on the great importance of the problem and its relationship to past work of Daniel Bernoulli and d'Alembert, he gave the distinct impression that he thought Laplace's memoir entirely original. In 1781, on the other hand, introducing another of Laplace's memoirs on probability, Condorcet made pointed reference to Bayes and Price, a circumstance I take to mean that he had come upon Bayes's essay in the intervening years, and was correcting for his earlier oversight.

In addition to applications of Bayes's Theorem to problems in probability and to estimating the ratio of male to female births, these early papers also contain Laplace's introduction and advocacy of a particular loss function. He proposed in 1774 that an unknown quantity be estimated by that number which minimizes the expected absolute error, the expectation being taken with respect to the posterior distribution relative to a uniform prior, and proved that this would be accomplished by using the median of the posterior distribution. In his 1781 memoir, he extended a calculation performed for a particular case in 1774 to show that for any reasonable, symmetric error density, the posterior median approached the sample mean as the scale parameter increased indefinitely.

A second phase of Laplace's work spanned the period from the middle 1770's until about 1805. This was the period in which he applied mathematical tools to the physics of the solar system, and culminated with the publication in 1798–1805 of the first four volumes of his *Traité de Mécanique Céleste*. The *Mécanique Céleste* was Laplace's major single work, a colossal attempt to complete the work Newton had begun on the theory of gravity by incorporating all the major advances of the previous century and Laplace's own prodigious efforts. John Playfair (1808, p. 277–8) in a review of this work called it 'the highest point to which man has yet ascended in the scale of intellectual attainment'; Mary Somerville (1831, p. 2) quoted Laplace as having called it 'a monument to the genius of the age in which it appeared'.

The *Mécanique Céleste*, and the memoirs leading up to it, contains much that is of interest to historians of statistics. A discussion of theory was often followed by a comparison with observation, and in these comparisons Laplace rarely used exactly the same method twice. The comparisons frequently took the form of regression problems, and his methods ranged from those involving complicated algorithms to the 'quick and dirty'.

The former included the minimization of the sum of the absolute deviations in regression problems involving one unknown, after a constant term had been eliminated by requiring that the residuals sum to zero, and the minimization of the largest absolute deviation in multiple regression problems. The first of these methods was introduced by Boscovich in

1757; see Eisenhart (1961), Stigler (1973) and Sheynin (1973). Both are discussed and applied in the *Mécanique Céleste* **2**, pp. 417–86; see also his *Oeuvres Complètes* **11**, pp. 477–558, 3–32, for earlier treatments. Whichever method was used, the fit would usually be judged by seeing if the size of the residuals was consistent with the likely size of observational errors.

Laplace's 'quick and dirty' methods are less well known and probably more typical of the time. For example, in *Mécanique Céleste* **3**, pp. 676 ff., he fits a linear equation involving two independent variables and three unknowns to six sets of observations by first fitting the equation to only three of the observations, then adjusting the constant term to reduce the residuals for the remaining three observations. He also employed simple variations of the method to be discussed in § 3 to fit a straight line to four points (**2**, 758 ff.), and a quadratic to six points (**2**, 691 ff.) in order to determine the times of maximum and minimum tides. For some of Laplace's comments on the design of regression experiments, see Stigler (1974).

The final phase of Laplace's scientific work, 1805–27, was largely but not exclusively concerned with the theory and application of mathematical statistics. In 1805 Legendre had published the method of least squares, making no attempt to tie it to the theory of probability. In 1809 Gauss had derived the normal distribution from the principle that the arithmetic mean of observations gives the most probable value for the quantity measured; then, turning this argument back upon itself, he showed that, if the errors of observation are normally distributed, the least squares estimates give the most probable values for the coefficients in regression situations. These two works seem to have spurred Laplace to complete work toward a treatise on probability he had contemplated as early as 1783.

In two important papers in 1810 and 1811, Laplace first developed the characteristic function as a tool for large-sample theory and proved the first general Central Limit Theorem. Then in a supplement to his 1810 paper written after he had seen Gauss's work, he showed that this result provided a Bayesian justification for least squares: if one were combining observations each one of which was itself the mean of a large number of independent observations, then the least squares estimates would not only maximize the likelihood function, considered as a posterior distribution, but also minimize the expected posterior error, all this without any assumption as to the error distribution or a circular appeal to the principle of the arithmetic mean.

In 1811 Laplace took a different, non-Bayesian tack. Considering a linear regression problem, he restricted his attention to linear unbiased estimators of the coefficients and, after showing that members of this class were approximately normally distributed if the number of observations was large, argued that least squares provided the best linear estimators in the sense that they minimized the asymptotic variance and thus both minimized the expected absolute value of the error, and maximized the probability that the estimate would be in any symmetric interval about the unknown coefficient, no matter what the error distribution. His derivation included the joint limiting distribution of the least squares estimators of two parameters; see Todhunter (1865) and Plackett (1949).

While the use of the modern word 'unbiased' to describe the restriction Laplace placed upon his estimators may be slightly misleading, it is not totally inaccurate. In fact, the restriction was accomplished by insisting that the estimator give the correct value when the given linear function of the errors equals zero. The similarity of Laplace's notation here to that he used when discussing mechanics (Laplace, 1798–1805, **1**, 74 ff.) suggests that he considered this condition as analogous to a condition for the equilibrium of a mechanical system, which would be very much in line with the 'centre of gravity' interpretation often given to mathematical expectation and the modern concept of unbiasedness.

These achievements were crowned by the publication of the *Théorie Analytique des Probabilités* which appeared in three editions from 1812 to 1820. The first half of this treatise was concerned with probability methods and problems, the second half with statistical methods and applications. In addition to topics already mentioned, the *Théorie Analytique* and its Supplements contain material on the asymptotic expansion of posterior distributions, the limiting behaviour of minimum deviation estimators, the asymptotic equivalence of the sum of squared errors and the residual sum of squares, and a variety of other subjects. Laplace's proofs are not always rigorous according to the standards of a later day, and his perspective slides back and forth between the Bayesian and non-Bayesian views with an ease that makes some of his investigations difficult to follow, but his conclusions remain basically sound even in those few situations where his analysis goes astray. In a review, Augustus De Morgan (1837) wrote,

> The *Théorie des Probabilités* is the Mont Blanc of mathematical analysis; but the mountain has this advantage over the book, that there are guides always ready near the former, whereas the student has been left to his own method of encountering the latter.

A brief description of Laplace's work in mathematical statistics can be deceptive. By its resemblance to a syllabus for a modern course in statistics, it could leave some with the very mistaken impression that the field has developed but little in the past century and a half, and in any case it does not convey an accurate picture of the level at which difficult and complex concepts were understood. With the aims of both illuminating the rapid development of mathematical statistics during Laplace's life and of demonstrating the depth and limitations of the actual level of understanding he achieved, I shall now turn to a detailed discussion of two of Laplace's applications of statistical methods. The first, published in 1787, provides a good view of the methods employed at that time; the second illustrates the dramatic change that had taken place by 1827, forty years later.

3. The theory of Jupiter and Saturn

In the latter part of the seventeenth century, Edmund Halley had noticed defects in the then existing tables of the motions of Jupiter and Saturn. Comparing the actual positions of Jupiter and Saturn with those tabled on the basis of centuries of past observation, it appeared that the mean motion of Jupiter was accelerating, while that of Saturn was retarding. Halley was able to improve the accuracy of the tables by an empirical adjustment, and speculated that the irregularity was somehow due to the mutual attraction of the planets, but he was unable to provide a mathematical theory that would account for this inequality. To account for this apparent trend remained one of the major unsolved problems of astronomy for a full century after Halley's observation. Unless it could be shown that the apparent acceleration and retardation could be explained as periodic, it was feared that the phenomenon was evidence of a basic instability in the solar system.

From 1748 until 1780, a number of determined attempts were made to solve this problem: memoirs by Euler, Lagrange, Lambert and Laplace himself made major advances in planetary theory, with the works of Euler and Lagrange being awarded prizes by the Academy of Sciences of Paris. But for all this effort, the main problem remained unresolved. Finally, in a series of memoirs presented to the Academy during the years 1784–8, Laplace gave the solution, showing how the apparent behaviour could be accounted for theoretically by a very long, 917-year, periodic inequality in the planets' mean motion, due to their mutual attraction and the coincidence that their times of revolution about the sun are approximately in the ratio 5:2.

Laplace's solution to this long-standing problem was one of the major scientific triumphs of that century, yet it would have been incomplete without the verification by comparison with observation which he presented at the end of the 1787 memoir. Laplace accomplished this verification by linearizing his equations for the motion of Saturn and reducing observations spanning 200 years to a system of 24 equations. For example, the sixth equation, based on observations for the year 1672, was given as

$$0 = -3'\ 32{\cdot}8'' + \delta\epsilon^I - 77{\cdot}28\delta n^I - 2\delta e^I 0{\cdot}98890 - 2e^I(\delta\tilde{\omega}^I - \delta\epsilon^I)\, 0{\cdot}14858.$$

He then sought to solve these equations for the four unknowns $\delta\epsilon^I$, δn^I, $2\delta e^I$ and $2e^I(\delta\tilde{\omega}^I - \delta\epsilon^I)$, where $\delta\epsilon^I$, δn^I, δe^I and $\delta\tilde{\omega}^I$ were the rates of change of the mean longitude of Saturn in 1750, its mean annual motion, its eccentricity, and the position of its aphelion; the eccentricity e^I was given elsewhere. The complete set of data is given in Table 1, where the X's represent coefficients of the second to fourth unknowns, the first, $\delta\epsilon^I$, being a constant term in the regression, and Y being an adjusted measure of the observed longitude of Saturn.

This manner of formulating a problem in astronomy as a multiple linear regression problem was by no means novel. Among others, Euler, Boscovich and Tobias Mayer had been able to find reasonable solutions to systems of inconsistent linear equations in one or two unknowns. In his 1748 paper on the inequalities of Jupiter and Saturn, Euler had wrestled without success with a system of 21 equations in 6 unknowns, apparently unable to convince himself that increasing the number of such equations potentially increased, rather than decreased, the accuracy of the results. In the same year, Mayer, in an investigation of the librations of the moon, had solved three systems of 27, 9 and 12 inconsistent linear equations involving 3 unknowns each. Mayer had, in each of the three cases, divided the equations into three equal disjoint groups according to the values of the coefficient of the first independent variable, the equations with the largest coefficient in one group, etc. He then added the equations in each group, and solved the resulting three linear equations (Euler, 1749; Mayer, 1750; Eisenhart, 1961; Sheynin, 1972).

Laplace arrived at a solution to his regression problem using a complex variant of the method Mayer had used. He reduced the 24 equations to 4 by (*a*) summing equations (1)–(24); (*b*) subtracting the sum of equations (1)–(12) from the sum of (13)–(24); (*c*) taking the linear combination $-(1)+(3)+(4)-(7)+(10)+(11)-(14)+(17)+(18)-(20)+(23)+(24)$; and (*d*) taking the linear combination $+(2)-(5)-(6)+(8)+(9)-(12)-(13)+(15)+(16)-(19)+(21)+(22)$. He then solved (*a*)–(*d*), and checked the degree to which the resulting equation fits the observations by computing each residual, defined as the 'excess' of the fitted value over the observed value, the negative of the modern definition of residual.

Laplace's selection of four linear combinations was not explained by him, but seems to have been based upon their effect upon the coefficients of the unknowns in (*a*)–(*d*). Thus (*a*) and (*b*) are natural linear combinations to consider: (*a*) maximizes the coefficient of the constant term, while (*b*) eliminates it, and, nearly, the reverse is true for the coefficient of δn^I. Evidently, the choice of which equations were included in (*c*) and which in (*d*) was made according to whether $|X_2| < |X_3|$ or $|X_2| > |X_3|$. The one exception to this rule is the reversal of it with respect to (3) and (5), a minor exception which may have been made to reduce the coefficient of the constant term in (d) by two. Once the 24 equations were divided between (c) and (d), the signs + and − were chosen according to the signs of X_3 and X_2, thus nearly maximizing the contrasts between the coefficients of the last two unknowns.

Table 1. *Laplace's Saturn data, from Laplace* (1787)*; residuals are fitted values minus observed values*

Year, i	Y_i	X_{1i}	X_{2i}	X_{3i}	Laplace residual	Halley residual	L.S. residual
1591	1′ 11·9″	−158·0	0·22041	−0·97541	+1′33″	−0′54″	+1′36″
1598	3′ 32·7″	−151·78	0·99974	−0·02278	−0·07	+0·37	+0·05
1660	5′ 12·0″	−89·67	0·79735	0·60352	−1·36	+2·58	−1·21
1664	3′ 56·7″	−85·54	0·04241	0·99910	−0·35	+3·20	−0·29
1667	3′ 31·7″	−82·45	−0·57924	0·81516	−0·21	+3·50	−0·33
1672	3′ 32·8″	−77·28	−0·98890	−0·14858	−0·58	+3·25	−1·06
1679	3′ 9·9″	−70·01	0·12591	−0·99204	−0·14	−1·57	−0·08
1687	4′ 49·2″	−62·79	0·99476	0·10222	−1·09	−4·54	−0·52
1690	3′ 26·8″	−59·66	0·72246	0·69141	+0·25	−7·59	+0·29
1694	2′ 4·9″	−55·52	−0·07303	0·99733	+1·29	−9·00	+1·23
1697	2′ 37·4″	−52·43	−0·66945	0·74285	+0·25	−9·35	+0·22
1701	2′ 41·2″	−48·29	−0·99902	−0·04435	+0·01	−8·00	−0·07
1731	3′ 31·4″	−18·27	−0·98712	−0·15998	−0·47	−4·50	−0·53
1738	4′ 9·5″	−11·01	0·13759	−0·99049	−1·02	−7·49	−0·56
1746	4′ 58·3″	−3·75	0·99348	0·11401	−1·07	−4·21	−0·50
1749	4′ 3·8″	−0·65	0·71410	0·70004	−0·12	−8·38	+0·03
1753	1′ 58·2″	3·48	−0·08518	0·99637	+1·54	−13·39	+1·41
1756	1′ 35·2″	6·58	−0·67859	0·73452	+1·37	−17·27	+1·35
1760	3′ 14·0″	10·72	−0·99838	−0·05691	−0·23	−22·17	−0·29
1767	1′ 40·2″	17·98	0·03403	−0·99942	+1·29	−13·12	+1·34
1775	3′ 46·0″	25·23	0·99994	0·01065	+0·19	+2·12	+0·26
1778	4′ 32·9″	28·33	0·78255	0·62559	−0·34	+1·21	−0·19
1782	4′ 4·4″	32·46	0·01794	0·99984	−0·23	−5·18	−0·15
1785	4′ 17·6″	35·56	−0·59930	0·80053	−0·56	−12·07	−0·57

It is a tribute to Laplace's insight into either the geometry or the mechanics of the problem that he arrived at a solution very close to that which least squares would give. Laplace's solution to this problem has a residual sum of squares only 11 % larger than that given by the least squares solution, while the residual sum of squares from Halley's 1719 empirical adjustment, extrapolated to 1786, is 80 times that of Laplace! It may be noted that the pattern of either Laplace's or the least squares residuals hints correctly that not all of the periodic inequalities in Saturn's motion had been accounted for, even if the most controversial one had been. Nevertheless, Laplace's success in solving his system of equations, twenty years before the publication of the method of least squares, is evidence that the development of statistical methods has been more continuous than is generally realized.

4. The tides of the atmosphere

Nearly forty years after his solution of the long inequality in the motions of Jupiter and Saturn, and nearly twenty years after Legendre's first publication of the method of least squares, Laplace wrote two papers on the tides of the atmosphere in which his application of statistical methods contrasts dramatically with his work of 1787. In 1787, Laplace's analysis of a complex set of data had been made without reference to probability, notwithstanding his earlier theoretical work on the application of probability to observational data. In this respect his work was similar to that of Boscovich and Legendre: systematic methods were given for combining inconsistent observational equations, thereby acknowledging the existence of observation error, but no explicit attempt was made to incorporate a probabilistic model for this error into the analysis, either to permit an assessment of the uncertainty of the resulting calculations, or to allow for the derivation of methods of combination which minimize this uncertainty. By 1823, major changes had occurred: Laplace was able to apply the mathematical tools he had developed over the previous 15 years to a complicated and delicate problem in the physics of the atmosphere, an application exemplifying the manner in which the early ideas of Laplace and others had been transformed into powerful tools for scientific research. While it is true that Laplace's work prior to 1823 contains many applications of his theory, it is only in his later applications that one can fully appreciate the level of his understanding of statistical methods, both his achievements and his limitations. For these reasons, it is worthwhile examining these papers in some detail.

In the *Mécanique Céleste*, one of the problems Laplace had treated successfully was to model the effect of the moon upon the tides of the sea. Later Laplace (1823) attempted to apply the same theory to model the effect of the moon upon the tides of the atmosphere; specifically, he wished to measure the moon's effect on the barometric pressure at Paris by comparing the daily variations in pressure at each of the phases of the moon.

Because the moon exerts such a strong influence upon the sea, scientists going back to Newton had believed that some similar effect must exist upon the atmosphere. Laplace sought to prove the existence of such an effect, which he thought could be due either directly to gravitational attraction or indirectly to the rising and falling of the sea. His tidal theory indicated that the magnitude of the effect could be gauged by comparing the changes in barometric pressures between 9.00 a.m. and 3.00 p.m. on the four days surrounding the syzygies, those two days a month when the moon, earth and sun are aligned, with the daily change in pressure on the four days surrounding the quadratures, those two days a month when the moon, earth and sun form a right angle.

In order to take advantage of the available data, which was an eight-year series of barometric measurements taken three times a day at the Paris Observatory, at 9.00 a.m., noon and 3.00 p.m., Laplace expressed his model as a multiple linear regression problem:

$$x \cos(2iq) + y \sin(2iq) = E_i, \quad y \cos(2iq) - x \sin(2iq) = F_i,$$

for $i = -1, 0, +1, +2$. Here x and y are unknown quantities, to be estimated from the data; q is a known quantity, the synodic movement of the moon; and E_i and F_i are calculated from the data, the index i representing the day of the phase of the moon. The manner in which E_i and F_i are calculated from the data is important to the analysis:

$$E_i = A_i'' - A_i + B_i - B_i'', \quad F_i = \{2A_i' - (A_i + A_i'') - 2B_i' + (B_i + B_i'')\}(1 + \tfrac{1}{19}), \tag{1}$$

where A_i is the mean, taken over 8 years, of the 9.00 a.m. measurements for the ith day after a syzygy, A_i' the same for the noon measurements, A_i'' the same for the 3.00 p.m. measurements. The B_i, B_i' and B_i'' represent the corresponding mean measurements for the ith day after quadrature.

That is, E_i is the mean daily, 9.00 a.m. to 3.00 p.m, barometric change for the ith day after syzygy, minus the mean change for the ith day after quadrature, while F_i is proportional to the difference between the mean rates of change, or second differences, for the two days.

Laplace then assumed that the deviations, due to 'irregular causes', of the daily measurements from their mean heights all followed the same law, not necessarily normal, and, treating the different measurements as having been made independently, he applied a method of analysis we might now call weighted least squares.

He had presented this method in the Third Supplement (1820) of the *Théorie Analytique*, where he called it 'the most advantageous method'. As Laplace had explained it in the Third Supplement, the method differed from the least squares of Legendre, which Laplace emphasized was only a special case of his method, in that the observations, in the present case the E_i's and F_i's, might be calculated as linear combinations of other independent observations, each possibly having a different distribution. This structure would then be exploited to find the best method of combining the observations, through an appeal to the Central Limit Theorem. While we might consider Laplace's method as only a generalization of Legendre's, and not very different from Gauss's (1809) generalization at that, Laplace, by his insistence upon the distinction between them, showed a keen awareness of the great conceptual difference between least squares as a numerical algorithm (Legendre, 1805) and least squares as a method of mathematical statistics, demonstrably best for arbitrary distributions, with large samples (Laplace, 1811, 1820).

Having applied his method to calculate x and y, Laplace then addressed the question of assessing 'the probability with which these observations indicate a lunar tide'. Earlier in the paper, Laplace had noted that it was not enough to combine a large number of observations in the most advantageous manner, that one must have

> a method for determining the probability that the error in the obtained results is contained in narrow limits, a method without which one risks presenting the effects of irregular causes as laws of nature; this has happened often in meteorology.

To test the hypothesis that barometric changes were not influenced by the phase of the moon, he compared the mean change on 792 days near syzygies with the mean change on 792 days near quadratures and found that, based upon the approximation given by the Central Limit Theorem, if there were no actual regular difference in barometric changes, chance alone would produce a difference in means no larger than that actually observed with probability 0·843. Laplace considered this number not sufficiently large to confirm the existence of an actual difference, and calculated that, if the lunar effect were actually of the estimated size, it would require 40000 observations to confirm its existence. In modern terminology, the observed difference between means would be significant at the 0·01 level only if it were based on 72 years of data!

Now interesting as this paper is as an example of the application of statistics to a delicate scientific problem a century and a half ago, one of the more illuminating aspects of it is that in his investigation Laplace had made three subtle but important errors.

First, Laplace's implicit assumption that different barometric readings were independent was severely violated, for daily readings are quite highly correlated. One facet of this correlation did not escape Laplace's attention: it was precisely his observation that day-to-day variations were much larger than daily variations which had led him to formulate his model in terms of daily changes instead of the readings alone. In fact, his basing the analysis upon differences thus avoided a pitfall which was to trouble meteorologists for the better part of the next century, for day-to-day variations are so great that many later workers thought the moon's effect far less significant than had Laplace. But while he was aware of one implication of this correlation, he ignored it in his application of the 'most advantageous method', *leading to an incorrect weighting of the observations.*

Had Laplace taken this correlation into account, he might have treated the variances of E_i and F_i as being approximately in the ratio 3:1, instead of the ratio 1:3 he actually used. Laplace's use of 1:3 for the ratio of the variance of E_i to that of F_i neglected the factor $1+\frac{1}{16}$, and was based upon the definitions (1) and the assumptions that all A's and B's were independent with the same variance. On the other hand, if one lets $U_i = A_i'' - A_i'$ and $V_i = A_i' - A_i$, then $U_i + V_i = A_i'' - A_i$ and $V_i - U_i = 2A_i' - (A_i + A_i'')$. An analysis of limited meteorological data from Paris for 1823 suggests that the correlation of U_i and V_i was not far from $\frac{1}{2}$, a value which would give the ratio 3:1, again neglecting the factor $1+\frac{1}{16}$.

Laplace's second error was in his test of significance, comparing the two mean changes. For while his use of the Central Limit Theorem was correct, at least if one assumes that the readings on different days are independent with equal variances, he had estimated the variance using the two samples pooled as one, rather than by pooling two estimates of the variance, one based on changes near syzygies, the other on changes near quadratures. What makes this error particularly interesting is that it is clear from his other work, in particular from the Third Supplement, that Laplace knew how to estimate a variance by using a residual sum of squares rather than a total sum of squares; it was only in this situation of testing a null hypothesis that he felt compelled to estimate the variance under the assumption that the null hypothesis was true. This would tend to inflate greatly his estimate of the variance if the null hypothesis were false, and shows that, while Laplace understood significance level very well, he had little sense of the concept of the power of a test.

Laplace's third error is just as interesting. It lies in his implicit assumption that the eight regression equations are independent. What makes this assumption questionable is that both E_i and F_i involve the same measurements. Now the actual effect of this error is small; because E_i involves differences of changes and F_i, second differences, E_i and F_i would be uncorrelated if the variance of the change from 9.00 a.m. to noon equalled that of the change from noon to 3.00 p.m., and this seems to be not far from the truth. But I think this was a lucky accident, and one that Laplace did not fully understand. What is remarkable, however, is that he did sense the possible difficulty a bit later. For in his very last paper, which only appeared after his death, he returned to this problem and repeated his 1823 analysis, but with one major difference.

In the Third Supplement to the *Théorie Analytique*, Laplace had presented the equation

$$l^{(s)}x + p^{(s)}y + q^{(s)}z + \ldots = a^{(s)} + m^{(s)}\gamma^{(s)} + n^{(s)}\lambda^{(s)} + r^{(s)}\delta^{(s)} + \ldots$$

as a stereotype for one of the 'equations of condition' in situations in which his most advantageous method would be applicable. Here the l, p, q, a, m, n and r represented given coefficients; x, y and z quantities to be estimated; and γ, λ and δ were independent observations with possibly different distributions. Laplace viewed the right-hand side as a derived observation, one calculated from others according to known rules. Except for the previously mentioned lack of independence, the equations of condition (1) for the lunar tides are of this form, and one can easily imagine how pleased Laplace must have been to apply his theory in its most general form to a problem which had not been formulated when the theory was developed! But in 1827, in his 'Mémoire sur le flux et reflux lunaire atmosphérique', Laplace returned to this problem with an additional three years of data and handled it differently, announcing that

I have determined, ***with special care***, the factors by which one must multiply the different equations of condition in order to obtain the most advantageous results... (my italics).

In 1827 Laplace first solved the equations of condition (1) in pairs for x and y, to obtain eight new equations, four involving each of x and y, then he collected together those terms involving the same measurements, applied his most advantageous method, using only the special case relative to a single unknown. I believe that he would only have changed his analysis in this way if he had been sensitive to the fact that, because E_i and F_i were both calculated from the same measurements, they may not be independent. Thus, this change in his analysis shows that Laplace, in 1827, was able to correctly deal with multiple linear regression problems with correlated errors and a known covariance structure!

Laplace did repeat his other, earlier errors, assuming that different measurements were independent, and assessing whether or not his estimate of x differed significantly from zero using a pooled sample to estimate the variance of daily pressure changes. Here he estimated x to be 0·031758, and calculated the probability that chance alone would produce an estimate within the limits $\pm$ 0·031758 to be 0·3617, stating that

If this probability had closely approached unity, it would indicate with great likelihood that the value of x was not due solely to irregularities of chance, and that it is in part the effect of a constant cause which can only be the action of the moon on the atmosphere. But the considerable difference between this probability and the certainty represented by unity shows that, despite the very large number of observations employed, this action is only indicated with a weak likelihood; so that one can regard its perceptible existence at Paris as uncertain.

Ironically, while Laplace was able to recognize and take account of the possible dependence between observations in this one instance where his notation made the form of this dependence clear, e.g. with A_i appearing in both E_i and F_i, the final section of this 1827 paper indicates that this understanding was very limited and perhaps tied to the situation where the dependence was notationally explicit. In the last section of this, his last paper, Laplace undertook to investigate the homogeneity of the data over the course of the year. Specifically, he sought to determine whether an apparent difference in the mean changes in barometric pressure over four quarters of the year was in fact significant; the variation is shown in Table 2.

Table 2. *Mean diurnal variation in barometric pressure at Paris*, 1815–26; *from Laplace* (1827) *as given in Oeuvres Complètes*

Months	Mean change (mm) 9.00 a.m. to 3.00 p.m.
Nov. to Jan.	0·557
Feb. to Apr.	0·940
May to July	0·752
Aug. to Oct.	0·802
Nov. to Oct.	0·763

Laplace chose to handle this analysis of variance problem by performing a sequence of four separate tests. First he considered the difference between the February to April mean change and the overall mean change, $0{\cdot}940 - 0{\cdot}763 = 0{\cdot}177$, and evaluated the probability that a difference of this size or larger would be due solely to chance. He found the value 0·000 001 5815 for this probability by means of a continued fraction expansion for the normal integral, and judged that it was extremely likely that the discrepancy was indicative of some 'constant' cause. He repeated his analysis for the other three-quarters, finding that the November to January mean change also differed significantly from the overall mean, but that the discrepancies between the other quarterly means and the overall mean could 'without improbability be attributed solely to the irregularities of chance'.

But while Laplace's analysis had been correct in many respects, including his recognition of the effect the differing sample sizes would have upon the relative variances of a quarterly mean and the overall mean, his derivation of the distribution of the difference between these means was based implicitly upon the assumption that they were independent! Earlier, where his notation had made it explicitly clear that the same measurements were entering into the calculation of two different quantities, for example E_i and F_i, Laplace seems to have noticed this difficulty and allowed for it in his analysis. Here, where he has only presented the means numerically and used no notation for them which would make explicit the fact that the quarterly means determine the overall mean, Laplace had missed noticing the correlation between these means, much less allowing for the dependence between his four significance tests.

Also, as he had before, Laplace here based his estimate of the variance of daily barometric changes upon the pooled 11-year sample, grouped into 132 months; that is, upon the total sum of squares.

Table 3. *Mean change in barometric pressure* 9.00 *a.m. to* 3.00 *p.m.*, 1816–26, *by month; from Bouvard* (1827)

	1816	1817	1818	1819	1820	1821	1822	1823	1824	1825	1826	Means
Jan.	0·513	1·234	0·840	0·751	0·310	0·288	0·521	0·762	0·882	0·756	0·599	0·677
Feb.	0·846	0·685	1·306	0·802	0·912	1·077	1·081	1·104	0·662	0·886	0·863	0·929
Mar.	0·836	0·568	1·085	0·861	0·750	0·576	0·425	0·514	1·042	1·141	0·882	0·797
Apr.	0·894	1·118	1·040	1·071	1·256	0·956	0·880	0·914	0·873	1·222	0·887	1·010
May	0·613	0·840	1·045	1·163	0·975	0·642	0·692	0·646	0·440	0·887	1·002	0·813
June	0·596	0·820	0·805	0·821	0·396	0·579	0·797	0·660	0·704	0·849	0·744	0·707
July	0·537	0·686	1·083	0·720	0·376	0·584	0·918	0·521	0·714	1·084	0·888	0·737
Aug.	0·951	0·702	0·989	0·961	0·763	0·929	0·950	0·812	0·632	0·687	1·024	0·854
Sept.	0·534	0·719	0·828	0·808	0·712	0·579	0·958	1·047	0·863	0·889	0·871	0·801
Oct.	1·043	0·903	0·750	0·374	1·271	0·793	0·448	0·564	0·679	0·535	0·908	0·751
Nov.	0·014	0·624	0·438	0·331	0·249	0·964	0·598	0·665	0·474	0·734	0·906	0·545
Dec.	0·730	0·246	0·696	0·476	0·600	0·173	0·331	0·243	0·614	0·459	0·371	0·449
Means	0·676	0·762	0·909	0·762	0·714	0·678	0·717	0·704	0·715	0·844	0·828	0·756

Interestingly, Laplace's general conclusions in all of these investigations seem to have been correct, despite these errors in his analyses. According to Chapman (1951), Laplace could scarcely have chosen a worse location than Paris to attempt to measure the lunar atmospheric tide. While the tide in fact exists, its effect is extremely small, and it was not until 1945 that its magnitude at Paris was successfully determined! Also, using contemporary (1823) data to estimate within-quarter variability of changes in barometric pressure, Laplace's statements relative to the simultaneous comparison of the four quarterly means can be confirmed at the 0·05 level using Newman's (1939) multiple range test. This would remain true if the data upon which both analyses are based are corrected; for the figure 0·940 should apparently be 0·910, in order to be consistent with the data in Table 3. It seems reasonable to surmise that Laplace, or Bouvard, misread a handwritten 1 for 4.

I should emphasize that I do not dwell upon Laplace's errors or near errors in order to criticize his abilities. At the time of his final paper he was 78 years old, and indeed who was there at that time who could have properly refereed the paper? Probably only Laplace himself. Rather, I feel that the subtle nature of these errors helps to show both the progress and the limitations in the development of mathematical statistics in Laplace's time, as well as give us a proper appreciation of the major developments of our own century.

5. Conclusions

In surveying, even briefly, the contributions of Laplace to the development of mathematical statistics, one cannot help but be impressed by the breadth of his researches. If one reads further in Laplace's work, examines his proofs and his applications of statistical theory, one is also struck by the depth of his understanding of the concepts and tools he employed. And if one reads yet further, in the scientific literature prior to and contemporary with Laplace, one can see how profoundly original many of his works were.

Early in the 18th century the works of Jacques Bernoulli and Abraham De Moivre had excited an interest in the possible applications of probability in various scientific fields. The greatest mathematicians of that century had taken up the challenge; Euler, Lagrange, Daniel Bernoulli, Simpson, d'Alembert, all had in different ways worked towards the construction of a usable mathematical theory of inference, the application of the mathematical theory of probability to areas of scientific research well beyond games of chance. Each succeeded, but while these successes were important, and the foundation of all later work, they were severely limited. Lagrange and Simpson developed De Moivre's generating functions into a tool for the study of the distribution of means. They also, together with Daniel Bernoulli, explored some of the mathematical consequences of modelling observational errors by probability distributions. But the completion of this program remained for Laplace.

It was Laplace who developed the technical tools of asymptotic theory, proved the Central Limit Theorem, and showed how this could eliminate arbitrary hypotheses about error distributions. And it was Laplace who, by his introduction of a loss function and his attention to the expected size of errors of estimation, permitted the improvement and evaluation of techniques of estimation, and allowed and encouraged the quantification of uncertainty in observational data. Whether or not Laplace's discovery of Bayes's Theorem was made, as we have argued, without knowledge of Bayes's work, it was Laplace who seized on this theorem and developed it as a tool for scientific research far beyond anything Bayes had envisaged. And it was Laplace who merged the linearized models Euler and others had used to represent astronomical phenomena, with probability theory to give birth to the fertile field of linear statistical inference, directly inspiring the so-called Gauss–Markov theorem, which Gauss proved in 1823. It is true that Laplace's applications of statistical methods reveal that his understanding of the concepts of independence and of the power of tests of significance was limited, but the very subtlety of these limitations also serves to emphasize the remarkable level of understanding he did achieve.

In an 1850 review of the works of the Marquis de Condorcet (1743–94), a contemporary of Laplace whose early interests overlapped Laplace's, Lockhart (1850, p. 5) wrote that

> He [Condorcet] had the advantage of appearing at a season very favourable for the exercise of ingenuity, when the Calculus was in rapid development, and there was something for any sharp eye to discover. These eras are the Californias of science: a new source of wealth is opened which the first comers gather – and then follows a period of severer toil and slender gains until a fresh and unwrought region is again disclosed. Condorcet was an eager adventurer, but he found grains rather than lumps, and above all he did not persevere.

Laplace did persevere and struck and mined lodes so rich and numerous that the scientific landscape was forever changed.

6. Bibliographical note

No full biography of Laplace presently exists, although Crosland's excellent book (Crosland, 1967) nearly suffices. The vast majority of the short treatments of Laplace's life which do exist give uncritical, anecdotal accounts which often show evidence of a lack of familiarity with his works, and can be grossly misleading. One exception is Pearson (1929), although this too contains minor inaccuracies. Laplace's work in statistics has been commented upon too frequently to permit a listing, although mention of Todhunter (1865) and Molina (1930) must be made. For an exposition of the discovery of the periodic inequality in the mean motions of Jupiter and Saturn, see Berry (1898). For a discussion of the lunar tide of the atmosphere, see Chapman (1951).

This research was supported in part by the Wisconsin Alumni Research Foundation. I am grateful to George Barnard, William Kruskal, John Kutzbach, E. S. Pearson, and George Stigler for comments in the course of this research, and to David Stigler for bibliothecarial assistance.

References

BAYES, T. (1764). An essay towards solving a problem in the doctrine of chances. *Phil. Trans. R. Soc. Lond. for* 1763 **53**, 370–418. Reprinted (1958), *Biometrika* **45**, 293–315, with biographical note by G. A. Barnard; and (1970) in *Studies in the History of Statistics and Probability*, Eds. E. S. Pearson and M. G. Kendall. London: Griffin.

BERRY, A. (1898). *A Short History of Astronomy*. London: John Murray. Reprinted (1961). New York: Dover.

BOUVARD, A. (1827). Mémoire sur les observations météorologiques, faites a l'Observatoire Royal de Paris. *Mém. Acad. R. Sci. Inst. France* **7**, 267–341.

CHAPMAN, S. (1951). Atmospheric tides and oscillations. In *Compendium of Meteorology*, Ed. T. F. Malone, pp. 510–30. Boston: American Meteorological Society.

CROSLAND, M. (1967). *The Society of Arcueil*. Harvard University Press.

DE MORGAN, A. (1837). Review of *Théorie Analytique des Probabilités*. *Dublin Review* **2**, 338–54; **3**, 237–48.

EISENHART, C. (1961). Boscovich and the combination of observations. In *R. J. Boscovich, Studies of his Life and Work*, Ed. L. L. Whyte, Chapter 7. London: Allen and Unwin. Reissued (1963). New York: Fordham University Press.

EULER, L. (1749). Recherches sur la question des inégalités du mouvement de Saturne et de Jupiter.... In *Leonhardi Euleri, Opera Omnia*, Ser. 2, Vol. 25, pp. 130–41. Issued (1960). Turici: Soc. Sci. Nat. Helveticae.

GAUSS, C. F. (1809). *Theoria motus corporum celestium*. Translated as *Theory of Motion of the Heavenly Bodies Moving About the Sun in Conic Sections*. Reprinted (1963). New York: Dover.

LAGRANGE, J. L. (1770–3). Mémoire sur l'utilité de la méthode de prendre le milieu entre les résultats de plusieurs observations; dans lequel on examine les avantages de cette méthode par le calcul des probabilités; & ou l'on résoud différens problêmes relatifs à cette matière. *Miscellanea Taurinensia* **5**, 167–232.

Lagrange, J. L. (1892). Correspondance de Lagrange avec Laplace. *Oeuvres de Lagrange* **14**. Paris: Gauthier-Villars.

Laplace, P. S. (1774). Mémoire sur la probabilité des causes par évènements. *Mém. Acad. Sci. Sav. Étranger* **6**, 621–56; *Oeuvres Complètes* **8**, 27–65.

Laplace, P. S. (1781). Mémoire sur les probabilités. *Mém. Acad. Sci. Paris* 1778, 227–332; *Oeuvres Complètes* **9**, 383–485.

Laplace, P. S. (1787). Théorie de Jupiter et de Saturne. (Offprint.) (1788). *Mém. Acad. Sci. Paris année* 1785, 33–160. Reprinted in *Oeuvres Complètes* **11**, 95–239.

Laplace, P. S. (1798–1805), (1825). *Traité de Mécanique Céleste*, Vols. I–IV, V. In *Oeuvres Complètes* **1–5**. (1829–1839), Vols I–IV transl. N. Bowditch; reprinted (1966) as *Celestial Mechanics*. New York: Chelsea; page references are to this translation.

Laplace, P. S. (1810). Mémoire sur les approximations des formules qui sont fonctions de très grands nombres et sur leur application aux probabilités. *Mém. Acad. Sci. année* 1809, 353–415, 559–65. Reprinted in *Oeuvres Complètes* **12**, 301–53.

Laplace, P. S. (1811). Mémoire sur les intégrales définies et leur application aux probabilités, et spécialement à la recherche du milieu qu'il faut choisir entre les résultats des observations. *Mém. Acad. Sci. année* 1810, 279–347. Reprinted in *Oeuvres Complètes* **12**, 357–412.

Laplace, P. S. (1812, 1820). *Théorie Analytique des Probabilités*. Paris: Courcier. Third edition (1820) reprinted with suppls. *Oeuvres Complètes* **7**.

Laplace, P. S. (1823). De l'action de la lune sur l'atmosphère. *Annales de Chimie et de Physique* **24**, 280–94. Reprinted with minor changes in *Mécanique Céleste* **5**, 184–8, 262–8; *Oeuvres Complètes* **5**.

Laplace, P. S. (1827). Mémoire sur le flux et reflux lunaire atmosphérique. In *Connaissance des Tems pour l'an* 1830, pp. 3–18. Reprinted *Mécanique Céleste* (*suppl.*); *Oeuvres Complètes* **5**, 489–505; *Oeuvres Complètes* **13**, 342–58.

Laplace, P. S. (1878–1912). *Oeuvres Complètes de Laplace*, 14 volumes. Paris: Gauthier-Villars.

Legendre, A. M. (1805). *Nouvelles méthodes pour la détermination des orbites des comètes.* Paris. Appendix, 'Sur la méthode des moindres quarrés'. Transl. D. E. Smith (1959). In *A Source Book in Mathematics*, Vol. 2, pp. 576–9. New York: Dover.

Lockhart, J. G. (1850). Review of *Oeuvres de Condorcet.... Quarterly Review* **87**, 1–44.

Mayer, T. (1750). Abhandlung über die Umwälzung des Mondes um seine Axe. *Kosmographische Nachrichten u. Sammlungen* 1748, 52–183.

Molina, E. C. (1930). The theory of probability: some comments on Laplace's Théorie analytique. *Bull. Am. Math. Soc.* **36**, 369–92.

Newman, D. (1939). The distribution of range in samples from a normal population, expressed in terms of an independent estimate of standard deviation. *Biometrika* **31**, 20–30.

Pearson, K. (1929). Laplace, being extracts from lectures delivered by Karl Pearson. *Biometrika* **21**, 202–16.

Plackett, R. L. (1949). A historical note on the method of least squares. *Biometrika* **36**, 458–60.

Playfair, J. (1808). Review of *Traité de Méchanique Céleste* [*sic*] par P. S. La Place. *Edinburgh Review* Jan. 1808, Vol. 11, No. 22, pp. 249–84.

Sheynin, O. B. (1972). On the mathematical treatment of observations by L. Euler. *Arch. Hist. Ex. Sci.* **9**, 45–56.

Sheynin, O. B. (1973). R. J. Boscovich's work on probability. *Arch. Hist. Ex. Sci.* **9**, 306–24.

Somerville, M. (1831). *Mechanism of the Heavens*. London: John Murray.

Stigler, S. (1973). Laplace, Fisher, and the discovery of the concept of sufficiency. *Biometrika* **60**, 439–45.

Stigler, S. (1974). Gergonne's 1815 paper on the design and analysis of polynomial regression experiments. *Historia Mathematica* **1**, 431–47.

Todhunter, I. (1865). *A History of the Mathematical Theory of Probability*. Reprinted (1949, 1965). New York: Chelsea.

[*Received June* 1974. *Revised December* 1974]