The moral I should draw from the history of medical statistics is that the intellectual courage of an amateur often succeeds where erudition fails. While even the purest of mathematicians would not claim that statistics is only a branch of mathematics, the hardiest contemner of algebra would admit that a training in mathematical method is an advantage to the practical statistician. The mathematician would surely agree that a knowledge of the material subjected to analysis was valuable, even if not so essential as a 'practical' man would claim.

Judged by contemporary intellectual standards, neither Graunt nor Farr was a mathematician; Graunt had no medical training, Farr's clinical experience was meagre. In respect neither of method nor subject-matter was either man an expert. But they both had intellectual curiosity and courage: one may say, if one pleases, the spurious courage of the man who is brave because he does not know what the dangers are. But, as Gilbert Chesterton once said, 'There is no real hope that has not once been a forlorn hope.' In graver matters than medical statistics and more than once in our national history salvation has been wrought by courageous amateurs who acted while professionals doubted.

Those who cannot disclaim a professional status in statistics, whether officials or professors, may learn a lesson from history. It is conveyed in the four words: *maxima debetur puero reverentia*, construing *puer* by amateur or beginner or enthusiast. It is weary work to read statistical 'proofs' of this or that aetiological theory of cancer, or proposals for this or that impossible statistical investigation. But it is treachery to science to rebuff any genuinely inquisitive person; the discovery of another Graunt in a shop or another Farr in the surgery of a general practitioner would repay the life-long boredom of all extant civil servants and professors of statistics.

THE PRINCIPLE OF THE ARITHMETIC MEAN

By R. L. PLACKETT

The history of the problem of combining a set of independent observations on the same quantity is traced from antiquity to the appearance in the eighteenth century of the arithmetic mean as a statistical concept.

The problem of estimating parameters from observational data appears first to have presented itself to the Babylonian astronomers of the last three centuries B.C. Their achievements are recorded in cuneiform script on clay tablets and have been analysed by Neugebauer (1951) who has also (1955) published a collection of the texts. The following summary is abstracted from his researches. Between about 500 and 300 B.C., the Babylonians developed a systematic mathematical theory to account for the motions of the sun, moon and planets; and they evolved simple arithmetical schemes by which the positions of these bodies could be calculated at regular intervals of time. Beyond the fact that the basic parameters in the schemes represent a compromise between observation and the needs of computation, nothing has survived to indicate how they were estimated from the original data, which are themselves almost wholly absent.

Rather more information is available concerning the methods by which the Greek astronomers analysed their observational data, for their discoveries were made possible, partly by developments of mathematical technique, and partly by the steady accumulation, since about 300 B.C., of a series of observations on the positions of stars and planets, made with graduated instruments. The *Syntaxis* of Claud Ptolemy not only presents a complete account of what was known to them, but also contains nearly everything that survives of the work of their greatest representative, Hipparchus. In what follows, we refer to the edition in two volumes translated and annotated by Karl Manitius (1913).

According to I, p. 133, Hipparchus noticed inequalities in the intervals of time between successive passages of the sun through the same solstitial point, and this suggested to him the question whether or not the length of the tropical year is constant. He considered, however, that the error in his observations and in the calculations based on them might amount to as much as $\frac{1}{4}$ day, and he concluded that any variation in the length of the year was quite insignificant. Subsequently, Hipparchus estimated the maximum variation in length as $\frac{3}{4}$ day, apparently by taking half the range of his observations (I, pp. 136–7).

> In fact, Hipparchus calculates with the help of certain eclipses of the moon, observed in the immediate neighbourhood of fixed stars, how far the star called Spica was west of the autumnal point at each eclipse, and finds some indication in this way that it shows in his time a maximum distance of $6\frac{1}{2}°$ and a minimum of $5\frac{1}{4}°$. Whence he draws the conclusion, since it is not well possible that Spica should have undergone such a considerable change of position in so short a time, that probably the sun, from whose position Hipparchus determines the positions of the fixed stars, does not accomplish its return at equal intervals.

The technique of taking the arithmetic mean of a group of comparable observations had not yet, however, made its appearance as a general principle. This is shown by Ptolemy's estimation of the amount by which the length of a year exceeds 365 days. Hipparchus had made the observations given below (I, pp. 134–5):

Autumn equinox

(1)	162 B.C.	Sept. 27	18^h
(2)	159 B.C.	Sept. 27	6^h
(3)	158 B.C.	Sept. 27	12^h
(4)	147 B.C.	Sept. 26/27	midnight
(5)	146 B.C.	Sept. 27	6^h
(6)	143 B.C.	Sept. 26	18^h

Spring equinox

(1)	146 B.C.	March 24	6^h (11^h at Alexandria)
(2)	135 B.C.	March 23/24	midnight
(3)	128 B.C.	March 23	18^h

Ptolemy gives (I, p. 142) a single observation of his own on the Autumn equinox, namely, A.D. 139 Sept. $26^d\, 7^h$, and compares it with the fourth observation of Hipparchus, whence he finds that in 285 Egyptian years of 365 days, the Autumn equinox advances by $70^d\, 7^h$, which he writes as $70+\frac{1}{4}+\frac{1}{20}$ days. He then gives (I, p. 143) a single observation of his own on the Spring equinox, namely, A.D. 140 March $22^d\, 13^h$, and by comparing it with the first observation of Hipparchus, again arrives at an advance of $70+\frac{1}{4}+\frac{1}{20}$ days in 285 Egyptian years. A year of $365\frac{1}{4}$ days would imply an advance of $71\frac{1}{4}$ days in 285 years, and the decrement of $71\frac{1}{4}-70\frac{6}{20} = \frac{19}{20}$ day in 285 years is equivalent to 1 day in 300 years. Thus Ptolemy reaches the value of $365\frac{1}{4}-\frac{1}{300}$ days for the length of the year, and this is precisely the value which Hipparchus is quoted (I, p. 145) as having found.

A similar example of Ptolemy's veneration for Hipparchus is provided by his discussion of the precession of the equinoxes, a phenomenon discovered by Hipparchus, and caused by the motion of the pole of the equator round the pole of the ecliptic, the annual movement being about 50″. According to a quotation in II, p. 15, Hipparchus estimated the change in the position of the solstices and equinoxes to be at least $\frac{1}{100}°$ per annum. Ptolemy then gives (II, pp. 18–20) a catalogue of the declinations of 18 stars as observed by (i) Timocharis and Aristyllus, about 290 B.C., (ii) Hipparchus, and (iii) himself. He selects 6 stars from the catalogue and shows that they all lead to a precessional constant of approximately $\frac{1}{100}°$ per annum, which is thus his estimate, whereas for Hipparchus it was a lower limit. These unique data have been analysed by several commentators, beginning with Delambre (1817, pp. 254–5) who showed that the average precessional constant from all 18 stars is near the correct value, whether the changes of declination from (i) to (ii), or from (ii) to (iii), are taken. Recently Pannekoek (1955) has confirmed the accuracy of Ptolemy's arithmetic; and he suggests that Ptolemy selected the 6 stars which agreed best with the value of $\frac{1}{100}°$ per annum, but which actually each exhibit too small a change of declination.

The technique of repeating and combining observations made on the same quantity appears to have been introduced into scientific method by Tycho Brahe towards the end of the sixteenth century. According to his biographer, Dreyer (1890, p. 350):

> Each observation thus gave a value for the right ascension of α Arietis. During the following six years Tycho repeated these observations as often as an opportunity offered, and, in order to eliminate the effect of parallax and refraction, he combined the results in groups of two, so that one was founded on an observation of Venus while east of the sun, the other on an observation of Venus west of the sun; while the observations were selected so that Venus and the sun as far as possible had the same altitude, declination and distance from the earth in the two cases. From the observations of 1582 Tycho selects three single determinations, and from the years 1582–88 twelve results, each being the mean of two results found in the manner just described. The fifteen values of the right ascension of α Arietis agree wonderfully well *inter se*, the probable error of the mean being only $\pm 6''$, but the twenty-four single results in the twelve groups show rather considerable discordances, the greatest and smallest differing by 16′ 30″. But anyhow the final mean adopted by Tycho is an exceedingly good one, agreeing well with the best modern determinations. He adopts for the end of the year 1585 26° 0′ 30″, the modern value for the same date being 26° 0′ 45″.

The observations to which Dreyer refers are reproduced below from Tycho's collected works (**2**, 170–97):

1582 February 26		26° 0′ 44″
1582 March 20		26 0 32
1582 April 3		26 0 30
1582 February 27	26° 4′ 16″	26 0 20
1585 September 21	25 56 23	
1582 March 5	25 56 33	26 0 38
1585 September 14	26 4 43	
1582 March 5	25 59 15	26 0 18
1585 September 15	26 1 21	
1582 March 9	25 59 49	26 0 32
1585 September 15	26 1 16	
1586 December 26	25 54 51	26 0 42
1588 December 15	26 6 32	
1586 December 27	25 52 22	26 0 37
1588 November 29	26 8 52	
1587 January 9	26 2 5	26 0 27
1588 December 6	25 58 49	
1587 January 24	26 6 44	26 0 29
1588 October 26	25 54 13	
1587 August 17	26 5 40	26 0 14
1588 April 16	25 54 48	
1587 August 17	26 1 1	26 0 4
1588 April 16	25 59 6	
1587 August 18	25 54 35	26 0 28
1588 March 28	26 6 20	
1587 August 18	25 54 49	26 0 39
1588 April 16	26 6 30	

The process of combining the first pair is thus described by Tycho (*ibid.* p. 171).

> Ab hac rursus Differentia Ascensionis vsque ad Lucidam ♈ subtracta, quae est part. 83. min. 57 . //. 20, prouenit Ascensio Clarae ♈, part. 25 . /. 56 . //. 10, cui pro Mensibus 3 residuis addantur //. 13, & obtinebimus Ascensionem Rectam Lucidae ♈ part. 25. min. 56 . //. 23, Anno 1585 completo correspondentem. Sed Anno 82 ex Die 27 Februarij, fuit eadem Ascensio Recta prius data part. 26. min. 4 . //. 16, vt sit differentia vtriusque min. 7 . //. 53: Dimidiata min. 3 . //. $56\frac{1}{2}$ addita minori vel subtracta a maiore, prodit vera & limitata Ascensio Recta Lucidae ♈ part. 26 . /. 0 . //. 20. Quam hac Methodo nulla habita ratione Parallaxium atque Refractionum, sed illis sese mutuo sic corrigentibus, inquirere propositum erat.

The average of the twelve determinations by means of two is 26° 0′ 27″, and the average of all fifteen is 26° 0′ 29″. How Tycho arrives at 26° 0′ 30″ is not described, but we note that the co-ordinates of the nine standard stars in his catalogue are all given at 5″ intervals, more than adequate for observational purposes. In fifteen cases out of eighteen, the co-ordinates differ from their exact values by less than 1′, and Kepler has described in a famous passage (*Astronomia Nova*..., Chap. 19; *Werke*, **3**, 178) how he was able to calculate the elements of a circular orbit for Mars, differing from Tycho's observations by 8′ or less, but rejected it because he knew that errors of 8′ could not be neglected with so diligent an observer.

We see that Tycho used the arithmetic mean to eliminate systematic errors. The calculation of the mean as a more precise value than a single measurement is not far removed and had certainly appeared about the end of the seventeenth century, as is shown by the following extract from Flamsteed's discussion of the errors produced by his mural arc on the right ascensions of stars (1725, vol. 3, p. 137):

Rectarum SOLIS Adscensionum *Differentia* inter 14um *Martii* ac 15um *Septembris* [of 1690] ex Observationibus circa *Solem* pro istis Diebus reperitur, viz.

per *Calcem* CASTORIS	μ	178° 36′ 0″	
per PROCYONEM		178 36 5	
per POLLUCEM		178 36 20	
Media inter has Differentia		178 36 8	
At hanc *Mediam* subtrahendo a SOLIS *Recta* Adscensione 15to *Septembris*, viz.			182 31 53
			178 36 8
remanet eius *vera Recta Adscensio* 14to *Martii* Meridie			3 55 45
quae *verum* dat eius *Locum*	♈		4 17 7

A third example illustrates the combination of data from different observers. During 1736–7, a French expedition under Maupertuis was sent to Lapland in order to measure the length of a degree of latitude and, by comparing it with the corresponding length in France, to decide whether the earth was flattened at the poles, as maintained, e.g. by Newton, or at the equator, as held by the Cassini family. Their method of observation, as described by Outhier, has been summarized by Clarke (1880, p. 5) as follows:

Each observer made his own observation of the angles and wrote them down apart, they then took the means of these observations for each angle: the actual readings are not given, but the mean is.

In the event, the degree proved to be longer in Lapland, and Voltaire congratulated Maupertuis on having flattened both the poles and the Cassinis.

At about this time, the calculus of discrete probability assumed an organized form, and the appearance of the differential calculus made extensions to continuous probability possible. The distribution of the arithmetic mean now began to receive the attention of mathematicians who were conversant with the new techniques, and a pioneer study by Simpson was followed by a long memoir from Lagrange.

In his paper of 1755, Simpson gives the probability that the mean of t observations is at most m/t for the following two error distributions:

(i) possible errors are $-v, \ldots, -2, -1, 0, 1, \ldots, v$ and equal probabilities are attached to them;

(ii) the same set of errors with probabilities proportional to $1, 2, \ldots, v+1, \ldots, 2, 1$, respectively.

The solution for (i), when expressed as a gaming problem, was known by 1710 and Simpson's treatment by generating functions is the same as de Moivre's (Todhunter, p. 85); since the generating function for (ii) is the square of what it is for (i), Simpson's initial contribution amounted mainly to realizing the physical interpretation of a mathematical result.

What is novel in Simpson's work appears in the four pages of additional material published in 1757. Here he extends the solution of the second problem to the limiting case where the error distribution is continuous, in the form of an isosceles triangle, and, by integration, finds the probability that the mean is nearer to zero than a single independent observation.

Simpson's debt to de Moivre is clear and the widespread respect which *The Doctrine of Chances* inspired during this period is notably attested by the following quotation from a letter written by Lagrange to Laplace on 30 December 1776.

Il est vrai que j'ai eu autrefois l'idée de donner une traduction de l'Ouvrage de Moivre, accompagnée de notes et d'additions de ma façon, et j'avais même déjà traduit une partie de cet Ouvrage; mais j'ai depuis longtemps renoncé à ce projet, et je suis enchanté d'apprendre que vous en avez entrepris l'exécution, persuadé qu'elle répondra à la haute idée qu'on a de tout qui sort de votre plume.

In the first fifty pages of his memoir, Lagrange presents a detailed discussion of discrete error distributions, on lines essentially the same as those followed by Simpson; he again makes free use of generating functions, and again extends results from discrete to continuous distributions by appropriate limiting processes. This section also includes (problem 6) a derivation of what we would now describe as the maximum likelihood estimates of the parameters in a multinomial distribution; and purports to show (problems 4 and 5) that the mode of the distribution of sample means is the same as the population mean. The chief contribution of the memoir to the probability theory of the arithmetic mean occurs in its last twelve pages, where Lagrange gives a method of obtaining the results for continuous distributions directly. He begins by evaluating

$$\int_0^\infty \frac{x^{m-1}dx}{a^x} = \frac{(m-1)!}{(\log a)^m},$$

where a is larger than unity. He now says that the coefficient of a^{p-x} in

$$(Pa^p + Qa^{p-1} + Ra^{p-2} + \ldots)/(\log a)^m, \tag{1}$$

is obtained on replacing

$$1/(\log a)^m \quad \text{by} \quad \int_0^\infty x^{m-1}a^{-x}dx/(m-1)!$$

and is thus given by

$$\{Px^{m-1} + Q(x-1)^{m-1} + R(x-2)^{m-1} + \ldots\}\,dx/(m-1)!.$$

He next asserts that the probability element for the sum of n independent variables, each with density function $y(x)$, is the coefficient of a^z in $\left\{\int y\,.\,a^x dx\right\}^n$, where the term 'coefficient' is used in the sense just defined. Several examples follow, in all of which the error distribution has a finite range, so that $\int y\,.\,a^x dx$ is a sum of terms like (1), and is therefore amenable to the processes he has described. The last error distribution is given by

$$y = K\cos x \quad (-\tfrac{1}{2}\pi \leqslant x \leqslant \tfrac{1}{2}\pi),$$

and the memoir concludes with a set of ingenious manipulations involving imaginary quantities.

At this interval of time, we can recognize the last part of Lagrange's memoir as a starting point for the theory of integral transforms, although its merits were scarcely visible to Todhunter, writing in 1865. However, they were at once appreciated by Laplace, who refers to 'la belle méthode que vous donnez' in a letter written to Lagrange on

11 August 1780, and who subsequently made the technique a basic part of his attack on the problem of combining observations.

I am very grateful to Dr A. Fletcher for his invaluable suggestions and guidance on astronomical matters, and for greatly improving my translations.

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A NOTE ON THE EARLY SOLUTIONS OF THE PROBLEM OF THE DURATION OF PLAY

By A. R. THATCHER

It is now just 300 years since the publication by Huygens of the first result on the famous problem which became known as the Duration of Play. The aim of this note is to summarize the early development of this problem and to show how easily some of the solutions found at the beginning of the eighteenth century can be linked with modern work on sequential tests, random walks and certain storage problems.

We use throughout the following notation. Call the two players A and B, and let their chances of winning a game be p and $q = 1-p$, respectively. A starts with a counters and B starts with b counters, and after each game the loser hands one counter to the winner. It is desired to find first the probability P_a that A will eventually lose all his counters without having previously won all B's, and more generally the probability $P_{a,n}$ that this will happen within n games. P_b and $P_{b,n}$ are defined similarly. $P_{a,n}+P_{b,n}$ is the probability that the play will terminate (with the 'ruin' of one of the players) within n games. It can be shown that the play must end sooner or later, so that $P_a+P_b = 1$.

In 1657 Huygens gave without proof, in the fifth and last problem of his treatise *De ratiociniis in ludo aleae*, the numerical value for P_a in a case where $a = b = 12$ and where p and q had particular values. The general result for P_a was found by James Bernoulli, who died in 1705, but it remained in manuscript until it was published 8 years later in his *Ars Conjectandi*; Bernoulli says that the proof is laborious and leaves it to the reader. Before the *Ars Conjectandi* appeared, however, de Moivre had found a simple derivation independently and published it in his treatise *De Mensura Sortis* (1711).

De Moivre's original proof, which was later reproduced in his *Doctrine of Chances* (see 1711, pp. 227–8; 1718, pp. 23–4; 1738, pp. 45–6; 1756, pp. 52–3), is very ingenious and so much shorter than the demonstrations usually given in modern textbooks that it is worth quoting. Its essence is as follows. Imagine that each player starts with his counters before him in a pile, and that nominal values are assigned to the counters in the following manner. A's bottom counter is given the nominal value q/p; the next is given the nominal value $(q/p)^2$, and so on until his top counter which has the nominal value $(q/p)^a$. B's top counter is valued $(q/p)^{a+1}$, and so on downwards until his bottom counter which is valued $(q/p)^{a+b}$. After each game the loser's top counter is transferred to the top of the winner's pile, and it is always the top counter which is staked for the next game. Then *in terms of the nominal values* B's stake is always q/p times A's, so that at every game each player's nominal expectation is nil. This remains true throughout the play; therefore A's chance of winning all B's counters, multiplied by his nominal gain if he does so, must equal B's chance multiplied by B's nominal gain. Thus

$$P_b\left\{\left(\frac{q}{p}\right)^{a+1}+\left(\frac{q}{p}\right)^{a+2}+\ldots+\left(\frac{q}{p}\right)^{a+b}\right\} = P_a\left\{\left(\frac{q}{p}\right)+\left(\frac{q}{p}\right)^{2}+\ldots+\left(\frac{q}{p}\right)^{a}\right\}.$$

The use of $P_a+P_b = 1$ now gives immediately

$$P_b = \frac{(q/p)^a-1}{(q/p)^{a+b}-1}, \tag{1}$$

and this is the probability of the 'gambler's ruin'.

In terms of the counters, A's total expected gain is $bP_b - aP_a$, while his expectation per game is $p-q$. These obvious facts are indeed only special cases of a more general result given by de Moivre (1718, pp. 135–6; 1738, pp. 48–9; 1756, pp. 55–6). De Moivre does not actually divide one expression by the other, but, since the total expectation equals the expectation per game times the expected number of games, this division is all that is required in order to get the expected number of games

$$E(N) = \frac{bP_b - aP_a}{p-q}. \tag{2}$$

De Moivre was also the first to discover and publish a general method for calculating $P_{a,n}+P_{b,n}$, thus finding the chance that the play would terminate within n games. For the case where a is infinite (so that $P_{a,n} = 0$) and $n-b$ is odd, he found

$$P_{b,n} = \text{first } \tfrac{1}{2}(n-b+1) \text{ terms of } (p+q)^n + \text{first } \tfrac{1}{2}(n-b+1) \text{ terms of } (p/q)^b(q+p)^n. \tag{3}$$