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Contents

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Introduction by the compiler

Notation

Notation **S, G,** *n* refers to downloadable file *n* placed on my website www.sheynin.de which is being diligently copied by Google (Google, Oscar Sheynin, Home). I apply this notation in case of sources either rare or those in my translation into English.

L, M, R = Leningrad, Moscow, in Russian

I

Around the theory of errors

Silesian Stat. Rev., No. 16/22, 2018, pp. 69 – 75

Summary: the aims of the theory of errors and its relations with statistics are described. The term true value of a constant and the subjective approach in the error theory and statistics are explained and illustrated and a hint at the emergence of a new theory of errors is provided.
Keywords: theory of errors and statistics, true value of constant, subjective approach in error theory and statistics

1. The theory of errors and its goals

Here, I do not describe the new possibility of proving the main conclusion of Gauss' memoir of 1823 [vi, *Later note* at end of References].

The term *Theory of errors* (or rather *Theorie der Fehler*) is due to Lambert (1765, Vorberichte and § 321). He defined its goals as discovery of the relations between errors, their consequences (Folgen), the circumstances of measurement, and the trustworthiness of the instruments. Those goals are now different; moreover, Lambert himself had also defined what was later understood as the *determinate branch* of the error theory which I briefly mentioned earlier (Sheynin 2014b). For example, the investigation of the trustworthiness of the instruments is one of its aims.

As understood nowadays, the stochastic branch of the theory of errors has the aim of adjusting *direct* and *indirect* observations.

1. Suppose that the observations (measurements) of an unknown constant are

$$x_1, x_2, \ldots, x_n, x_1 \le x_2 \le \ldots \le x_n.$$

It is required to assign its value optimal in some sense, and to estimate the precision of the result. The observations are supposed to be physically independent and possessing equal weight, observations of unequal weight can be appropriately weighed.

2. Suppose now that we have equations

$$a_ix + b_iy + \ldots + l_i = 0,\ i = 1, 2, \ldots, n. \qquad (1)$$

Here, the free terms are the results of physically independent observations, and the coefficients are provided by the underlying theory. The number of the observations, n, is larger than the number (k) of the unknown constants $x, y, \ldots$ Indeed, the solution is otherwise either impossible ($n < k$) or becomes purely algebraic ($n = k$), although complicated in case of large values of n and k.

However, if $n > k$ no solution is possible either, and any set of numbers $\hat{x}, \hat{y},\ldots$ leading to reasonable values of residual free terms (call them v_i) has to be called a solution. Such sets are obtained by imposing some restriction on those residuals, for example, the restriction (condition, principle) of least squares

$$v_1^2 + v_2^2 + ... + v_n^2 = \min.$$

The method of least squares (MLSq) squarely belongs to the theory of errors.

2. Adjustment of direct measurements

During Kepler's lifetime or somewhat earlier the arithmetic mean became the standard estimator of the unknown in the case of direct observations (Sheynin 2019, p. 32). But Al-Biruni (973 – 1048), for example, when measuring the densities of metals, applied the mode, the midrange and some unspecified values between the extreme observations (Al-Khazini 1983, pp. 60 – 62).

Ancient astronomers had been choosing the sought estimator almost arbitrarily, as indirectly, but definitely follows from the writings of several historians of astronomy. This attitude was justified by the large errors of ancient observations. To a large extent the same can be stated about statisticians, who, up to the beginning of the 20th century, did not turn to mathematics. And rather late Kaufman (1922, p. 152) argued that curves of distribution, adjustment of series of observations (?), interpolation and correlation are harmful. Earlier, Bortkiewicz (1894 – 1896, Bd. 10, pp. 353 – 354) stated that the study of precision was an accessory aim, a luxury, and that the statistical flair was much more important. Perhaps he also had in mind large errors corrupting the data, but his statement was too general.

Bernstein (1928/1964, p. 231), a most eminent scholar and foreign member of the Paris Academy of Sciences, had an unusual opinion about correlation, or, rather, about its unreasonable applications, regrettably left without explanation:

Excluding biological applications, most of its [of the correlation theory] *practical usage is based on misunderstanding*.

3. The true value of a constant

True value of the constant sought is a common expression of the theory of errors, and some statisticians (Chatterjee 2003, p. 264) wrongly consider it outdated, so I (2007) am refuting this opinion.

Fisher (1922), introduced the first version of mathematical statistics. In particular, without mentioning the theory of errors or real values, he (p. 309) defined the notions of consistent, effective and sufficient estimators of the parameters of distribution functions. Since then, the aim of mathematical statistics has been the estimation of those parameters rather than the determination of their real values.

Now, philosophers long ago indicated that mathematics has been becoming ever more abstract but, at the same time, ever more useful. Fisher, therefore, made a significant step in the right direction. But on the very next page he mentioned the true value of an unknown! Many other authors can also be cited here, for example Gauss (1816, §§ 3 and 4), who cannot be separated from statistics, and Hald (2004, p. 105). Gauss even considered measures of precision which do not exist (at least, in the usual sense) in the real world. Moreover, the theory of errors is applied in experimental science, not only in astronomy and geodesy as in the time of the first meridian arc measurements. And, scientists, to name only metrologists and physicists, cannot, and do not abandon true values.

It was Fourier (1826/1890, pp. 533 – 534) who offered the first formal definition of true value, and who thus provided this term with a mathematical (instead of its previous philosophical) meaning. The true value, as he stated, is the limit of the arithmetical mean of the measurements of the studied constant as the number of those measurements unboundedly increases.

Obviously, the mean of a sufficiently large number of measurements as well. Several authors (Lambert, Laplace) stated the same or almost the same even earlier, and many authors later repeated the Fourier definition without referring to anyone. Note also that it, the definition, heuristically resembles the definition of probability introduced by Mises.

Fourier also remarked that the observations ought to be carried out under unchanged conditions (circumstances) which is indeed obligatory in metrology, but simply wrong for geodesy: lateral refraction which corrupts geodetic measurements changes during the day, and a representative sample of measurements ought to reflect those changes. But even in metrology it is natural to compare the results of measurements made in different laboratories.

Unavoidably, the Fourier definition means that the residual systematic error of measurements is included in the true value. Here, indeed, is the testimony of a metrologist (Eisenhart 1963/1969, p. 31):

The mass of a mass standard is […] *specified* […] *to be the mass of the metallic substance of the standard plus the mass of the average value of air adsorbed upon its surface under standard conditions*.

4. The subjective approach

It is necessary. In the theory of errors the weighing of observations and the rejection of outliers (Sheynin 2014a, p. 24) are to a large extent carried out subjectively. In statistics, many decisions have to be made in the same way (a simplest example: the grouping of observations). Then, the same is true concerning the planning of sample surveys, the work of experts etc.

5. The theory of errors is not known sufficiently well

Donahue, the meritorious translator and commentator of Kepler's (1609) fundamental contribution, did not say a word about Kepler's adjustment of observations. Modern astronomers obviously lost interest in that subject. The same is, and even was true about mathematicians. Chebyshev (1879 – 1880/1936, pp. 250 – 252) described the MLSq according to Laplace, criticized Gauss (1809) and did not mention Gauss (1823). Fisher (1925/1990, p. 260) thought that the MLSq is a particular application of the principle of maximum likelihood. This is only true in the case of the normal distribution but does not concern Gauss (1823). And Poincaré (1896/1912, § 127) did not recognize that fundamental contribution but he obviously had not studied Gauss. Statisticians (Karl Pearson and Yule) discovered (likely, rather too late) that the results of Gauss could apply for developing the theory of correlation; in more detail, see Sheynin (2014a, p. 26). And here is a distinguished Russian mathematician Tsinger (1862, p. 1):

In Laplace's work we find a rigorous [?] *and impartial study of this problem. His analysis shows that the results of the method* […] *only enjoy a more or less substantial probability when the number of observations is large whereas Gauss attempted to attach absolute meaning to this method* [a damned fabrication], *using extraneous considerations*

and applying it to a small number of observations when

we cannot at all reckon upon the mutual cancellation of errors […] *and* […] *any combination of observations can* […] *lead as much to the increase of errors as to their decrease.*

The author was ignorant of the second Gaussian justification of theMLSq; of Gauss' qualification remark (1823, § 6) about the arbitrariness of his method; or of Gauss' correct decision to restrict his attention to the case of a small number of observations. Finally, both the history of the sciences of observation and of mathematical statistics proved that Tsinger's last lines contradicted reality and theory, respectively.

A very special point is provided by the non-existing Gauss – Markov theorem which, nevertheless, is still mentioned (Dodge 2003, p. 161). Actually, it only concerns Gauss (1823). Here is the story of that mysterious theorem.

Neyman (1934, p. 595) mistakenly attributed to Markov the second Gaussian justification of least squares of 1823. David & Neyman (1938) repeated that mistake, but then Neyman (1938/1952, p. 228) admitted it. H. David (after 2001) noted, in an unpublished manuscript, that it was Lehmann (1951) who invented that unfortunate name. Neyman's wrong initiative seems strange since he (1934, p. 593) contradicted himself:

The importance of the work of Markov concerning the best linear estimates consists, I think, chiefly in a clear statement of the problem.

6. A new theory of errors

A new theory seems to be emerging. I can only refer to June (2015) who mentions immense numbers of observation in several branches of natural sciences and the ensuing necessity of a new theory. He was not really versed in the history of the theory of errors and, as it seems, too easily all but rejects it, but in any case new thoughts are probably needed. Regrettably, he had not concisely described the essence of this new theory. Anyway, his theory is faulty since it cannot say anything definite about the observations: the locations of their extremal points remain unknown.

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II

Gauss and the method of least squares

Jahrbücher f. Nationalökonomie u. Statistik, Bd. 219, 1999, pp. 458 – 467

Summary

I describe the discovery of the method of least squares (MLSq). Gauss developed it and applied it, at first in astronomy and geodesy. In recent times the MLSq became important to the analysis of statistical data in economics and social sciences and to the application of statistical methods in econometrics.

I follow both justifications of the MLSq and list several fields where Gauss applied the *principle* of the yet non-existing *method* before Legendre's relevant publication of 1805. Contrary to a recently formulated opinion, Gauss had indeed communicated his discovery, again before 1805, to several colleagues.

1. The appearance of the principle of least squares

The main problem of the classical theory of errors was, and still is, the deduction of some final values for the unknown constants *x, y, z,* … from a redundant system of equations

$$a_i x + b_i y + c_i z + \ldots s_i = 0,\ i = 1, 2, \ldots, n \qquad (1)$$

with coefficients indicated by the appropriate theory and measured free terms, and the estimation of the plausibility of these values and of their functions. The linearity of (1) was not restrictive: the approximate values were either known, or calculated from any subsystem of (1), which was not always easy if (1) was not linear. The free terms were reasonably considered independent (linear independence was not yet introduced) so that systems (1) were inconsistent. Any set of *x, y, z,* … leading to reasonably small residual free terms (call them v_i) was admitted as a solution. Each such set was determined by imposing some restriction(s) on the v_i's so that the unknowns were uniquely determined. The MLSq which requires that

$$v_1^2 + v_2^2 + \ldots + v_n^2 = \min \qquad (2)$$

(and transforms system (1) into normal equations) is no exception.

Another special condition was

$$|v_i|_{\max} = \min \qquad (3)$$

Unlike the MLSq, it has no stochastic justification. However, Euler and possibly Kepler applied its rudiments and Laplace devised an algorithm for its application in general. Condition (3) allowed to determine whether the underlying theory was suitable and the observations were good enough. Indeed, even if the minimal $|v_i|_{\max}$ was too large, either the theory or the observations did not answer the necessary requirements.

A classical problem leading to equations (1) was the determination of the two parameters of the Earth's ellipsoid of rotation (of its semi-axes a and b, $a > b$, or rather of a and the flattening $\alpha = (a - b) \div a$ by means of numerous meridian arc measurements, by measuring the lengths of one degree of the meridian at various latitudes φ_i. These lengths constituted the magnitudes s_i in (1) and the coefficients were $a_i = -1$ and $b_i = \cos\varphi_i$ (Boscovich, end of the 18th century). The errors of the coefficients were either insignificant or even non-existing, and the errors of s_i were supposed random, and to obey therefore some law of distribution.

Nowadays the situation is more complicated since the observations and the MLSq are also the basis for studying populations and investigating economic data, which requires the notion of correlation and regression and analysis of time series as well as estimation of the parameters of empirical functions in econometrics. The approximate values of the unknowns are often unknown and the hypothesis of linearity of (1) had to be rejected. There also exists a conceptual difference between old and new (Pearson 1920/1970, p. 187):

There is no trace in Gauss' work of observed physical variables [?] *apart from equations of condition* [introduced in another pattern of the adjustment of observations] *associated organically which is the fundamental conception of correlation*.

In 1805, Legendre was the first to recommend publicly condition (2), but he only substantiated it qualitatively. Worse: his explanation all but implied condition (3)!

Gauss discovered condition (2) in 1794 or 1795. He stated this in 1806 and repeated this claim in many letters, for example to Olbers on 30.7.1806 (W/Erg-4, Tl. 1, p. 305) and to Laplace on 30.1.1812 (W-10, Tl. 1, p. 273)[1]. Gauss' personal traits injured the picture. Not only he was slow in publishing his discoveries; he never intended to make known *isolated fragments* (his description of Legendre's proposal made in the letter to Laplace). And for him, priority meant being first to discover rather than to publish. Biermann (1966, pp. 17 – 18) correctly remarked:

Was einem normalen Autor verboten ist, einem Gauß wohl gestattet werden muss, zumindest müssen wir seine Gründe respektieren.

An additional circumstance was that Gauss was initially mistaken: he thought that condition (2) was introduced by someone else long ago (letter to Schumacher of 24.6.1850, W-6, p. 89).

In itself, condition (2) is only the principle of least squares. Additional work was required for it to become the essence of the *method* of least squares. It was necessary to justify (2) stochastically, and to find out how to estimate the plausibility of the obtained results. Furthermore, since Gauss later rejected his own first substantiation of (2) and offered its new justification (and at the same time concluded his study of the plausibility of the obtained result); and since modern statisticians uphold his reasoning, it is advisable to say that the MLSq came into existence with this new justification.

2. The first justification of least squares

Gauss published his first substantiation of condition (2) in 1809. He proved that among unimodal, symmetric and differentiable densities

$\varphi(x - x_0)$ there existed only one law (the *normal* distribution) for which the maximum likelihood estimator[2] of the location parameter x_0 coincided with the arithmetic mean of the appropriate observations. Once more applying maximum likelihood, Gauss arrived at condition (2). In addition, he calculated the measure of precision of the unknowns[3] as compared with that of a direct observation of unit weight.

Astronomers and geodesists were quick to apply the MLSq. They had indeed found a tool which did not lead them to any unpleasant situation and delivered them from subjective or at least not really justified previous methods. True, some arbitrary decisions are still hardly eliminated, for example, when dealing with outlying observations, but this circumstance was unavoidable. Practitioners did not object either to the arithmetic mean or to maximum likelihood, and observational errors at least approximately obeyed the newly discovered normal law, which certainly appeared in case of a large number of observations owing to the (not really proven by Laplace) central limit theorem[4]. For this and another (see § 3) reason Gauss' second justification (§ 3) had not been sufficiently known. Even Fisher (1925, p. 260) lamely declared that the MLSq was *a special application of the method of maximum likelihood*.

3. The second justification

From the very beginning Gauss (1809, § 179) was hardly satisfied with his first substantiation: the principle of least squares *muss überall* […] *als Axiom gelten*. Indeed, he had to introduce the postulate of the arithmetic mean (as it was later called by Bertrand) and the normal law became the only possible distribution of observational errors. In addition, as Gauss (1823, § 6) himself publicly recognized, was inferior to minimizing an integral measure of precision. In a letter to Bessel of 1839 he (W-8, pp. 146 – 147) later repeated this statement which, as he also said, he did not make known earlier.

Denote the density of the observational errors by $\varphi(x)$ and introduce, as Gauss did in the same § 6, such a measure (the variance, as Fisher later called it,

$$m^2 = \int_{-\infty}^{+\infty} x^2 \varphi(x) dx. \qquad (4)$$

The minimization of (4) was naturally tantamount to the principle of least squares which Gauss then indeed derived once again in an unimaginably complicated method instead of the obvious elementary way. It is likely that many readers of this memoir understood it, and in 1862 V. Ya. Tsinger, when commenting on the formula below, see below, remarked that *the rule of least squares is here already implied* (Sheynin 1977, p. 53). In 2012, I was likely the first to describe in detail this most essential methodological point (Sheynin 2017, p. 148). Indeed, the mentioned difficulty of the Gauss derivation of least squares mentioned just above was the second reason why only the 1809 justification of the MLSq became universally recognized.

Gauss admitted that another measure could have been chosen instead of (4) and named Laplace (§ 4), but he also stated that the treatment of observations was inseparably linked with some arbitrariness. His viewpoint proved quite reasonable and the MLSq as derived in 1823 became classical (I repeat: but barely known).

Gauss also derived a formula for estimating the variance given observations. For k magnitudes x, y, …, see § 1, he obtained

$$m^2 = \frac{\mathrm{E}\sum v_i^2}{n-k}$$

where E is the present-day symbol for expectation. It remains, however, unknown and, as Gauss remarked, we have to admit that formula without it. He proved the unbiasedness of this estimator and derived the bounds for its variance. Regrettably, he made a mistake in his calculations which was corrected by Helmert (whose formula was prone to error) and then by Kolmogorov et al (Sheynin 2017, pp. 154 – 155).

4. Laplace

He began studying the treatment of observations in 1774 and turned to the principle of least squares in 1810. From then onward Laplace mostly considered a large number of observations and applied he central limit theorem which he proved non-rigorously, to say it politely. Previous mathematicians developed the theory of probability as a branch of pure mathematics, but Laplace resolutely (and quite reasonably for his time) formed it as an applied mathematical discipline.

And so, Laplace managed to arrive at the principle (2) without assuming the postulate of the arithmetic mean, but he had to introduce an artificial restriction. Then, in 1811, Laplace applied the condition of least absolute expectation rather than variance for a normal density. This led him to the principle of least squares, but for non-normal densities his approach would have required extremely involved computations. Laplace's MLSq remained unpractical and mostly served to corroborate (non-rigorously) Gauss findings in the case of a large number of observations.

Consider the case of one unknown (the general case is similar). Equations (1) with the residual free term s written out will be

$$a_ix + s_i = v_i.$$

Multiply them by m_i respectively, add up the products:

$$x\sum a_im_i + \sum s_im_i = \sum v_im_i$$

and solve this summary equation assuming that

$$\xi = \sum v_im_i = 0.$$

First, however, the multipliers m_i should be chosen. Laplace non-rigorously proved that the density of this sum, $\varphi(x)$, which of course included the m_i's as parameters, was normal. He required that

$$\mathrm{E}|\xi| = \int_{-\infty}^{+\infty} |x|\,\varphi(x)\,dx = \min.$$

5. Practical considerations

A special feature of Gauss' work is its practicality. Witness the opinion of an eminent astronomer on a related topic (Subbotin 1956, p. 297): Lagrange and Laplace had

Restricted their attention to the purely mathematical aspect [of determining the orbits of celestial objects] *whereas Gauss had thoroughly worked out his solution from the point of view of computations and took into account all the conditions of the work of astronomers and* [even] *their habits.*

Gauss had introduced apt notation[5] for describing the structure of the normal equations and the method of their solution. This method itself (successive elimination of the unknowns) as devised by him became canonical. In general, the practitioner had only to worry about his observations[6] after which everything was taken care of by one or another of Gauss' formulas. Even so, Gauss never obediently followed them. Thus (Schreiber 1879, p. 141),

Aus seinen […] *Protokollen geht vielmehr hervor, dass er auf jeder Station so lange gemessen hat, bis er meinte dass jeder Winkel sein Recht bekommen habe.*

Indeed, no formulas allow for systematic errors. Hence, only after closing a triangle the geodesist will know something about the precision of its three angles. And only after measuring baselines and astronomical bearings at both ends of his chain of triangles, calculating the two relevant discrepancies and adjusting his observations, he will know the answer more or less surely by applying the Gauss formula for m^2.

This is an example showing how difficult it is to apply modern statistical ideas and methods (for example, sequential analysis) to geodetic (or even metrological) observations.

6. Legendre or Gauss?

Legendre was obviously the first to publish his finding, the principle of least squares. Gauss had begun to use it about ten years earlier ad later developed it into a really stochastic MLSq. Nevertheless, some recent authors question Gauss' merits and even doubt his integrity. The most outspoken critic is Stigler (1986). In spite of my refutation (1993, § 7) his astonishing and repugnant accusations are still in vogue[7]. No one joined me in condemning Stigler; not a single German statistician or historian of science defended Gauss (or Euler, whom Stigler had also attempted to blemish[8]). **The scientific community is seriously ill**.

1. Stigler (p. 143). Only Laplace saved Gauss' first justification of condition (2) from joining *an accumulated pile of essentially ad hoc constructions*. Apparently, Laplace saved the normal distribution as well. And how about Legendre? His was *One of the clearest and most elegant introduction of as new statistical method in the history of statistics* (p. 13). His work revealed his *depth of understanding of his method* (*notwithstanding the lack of a formal probabilistic framework*), p. 57, etc. etc. Stigler goes for an isolated and faulty fragment (§ 1) rather than for a mathematical proof. This is an antiscientific approach.

2. Stigler (p. 146): *There is no indication* that Gauss had noticed the *great general potential* of least squares. But where did Stigler look for such indications? Gauss had applied condition from 1794 or 1795 onward, and communicated it to several colleagues (§ 8), but no, he did not *indicate* anything in a newspaper and no, once more, he did not hire a public crier to proclaim it.

3. Stigler (p. 145): *Gauss solicited reluctant testimony from friends that he* [had] *told them of the method* [of least squares] *before 1805*. A ridiculous statement (§ 8), a humiliation of the memory of one of the greatest scientist in human history.

4. Stigler (p. 145): *Although Gauss may well have been telling the truth* [another humiliating expression appropriate for a suspected rapist; since 1855, the date of Gauss' death, hardly anyone doubted his integrity] *about his prior use of the method, he was unsuccessful in whatever attempts he made to communicate his discovery before 1805*.

Once more, see § 8.

7. Gauss: the first applications of least squares

In his letters and in a few of his early publications Gauss claimed that he had applied condition (2) for treating meridian arc measurements and determining the orbits of the first four just discovered minor (now called dwarf) planets. Several authors attempted to prove his statements but at best they were only partly successful. True, it would have been extremely difficult either to prove, or disprove them[9] because of possible mistakes in the data or calculations[10]; of weighing the observations in one or another way; of short cuts or application in trial calculations, etc. Such circumstances can prevent a proper decision. However, Gauss' contemporaries did not doubt Gauss, see Brandel (1924) or Galle (1924, p. 9). Marsden (1995, p. 185) did not agree but, surprisingly, he did not mention either of these authors[11].

Finally, drawing on archival sources, Gerardy (1977) stated that in 1802 – 1807 Gauss had participated in land surveying (in part, for his own pleasure). He (p. 19, Note 6) concluded that Gauss had started to apply condition (2) not later than in 1803. Regrettably, Gerardy concentrated on the description of Gauss' simple calculations and his statement was not quite definite.

8. Gauss: notification of colleagues

1. Olbers. In 1809 Gauss asked him whether he remembered having heard about least squares from him (from Gauss) in 1803 and then again in 1804. No answer is known but on 103.3.1812 (W/Erg-4, Tl. 1, p. 495) he answered Gauss' second request: Yes, *gern und willig* (gladly and willingly). And indeed (Olbers 1816, p. 192n):

Gauss bereits im Junius 1803 die Güte hatte, mir diese Methode als längst von ihm gebraucht, mitzuteilen.

But why such a delay? Because in 1812 – 1815 Olbers had not published anything suitable (*Catalogue of Scientific Papers* of the Royal Society).

By that time, or at least later, Gauss became sick and tired of defending his scientific priority. In a letter of 3.12.1831 to Schumacher (W/Erg-5, Tl. 1, p. 292) he remarked:

War dies [statement made by Olbers] *zwar gut gemeint, hätte er mich aber vorher gefragt, so würde ich es hautement* [apparently: (French) strongly] *gemissbilligt haben*.

He, Gauss, explained everything in 1809 and did not need any confirmations.

2. Von Zach. Gauss (1799) published a letter in Zach's periodical about a misprint in the data on meridian arc measurements. It contained the phrase:

Ich entdeckte diesen Fehler indem ich meine Methode, von der ich Ihnen eine Probe gegeben habe, anwandte.

Zach inserted a comment: *Hievon ein andermal*.

Later, in the same letter to Schumacher, Gauss explained that he had mentioned his method to von Zach *Ohne ihm jedoch das Wesen der Methode selbst mitzuteilen.* This phrase is apparently less known than Schumacher's previous remark (to which Gauss answered) of 30.11.1831 (W/Erg-5, Tl. 1, p. 290): *Das andere Mal is aber nie gekommen*. Moreover, either Schumacher or Gauss hardly knew that von Zach (1813, p. 98n) had by that time indirectly supported Gauss:

The celebrated Doctor Gauss already from 1795 was in possession of this method, and he advantageously applied it for determining the elements of the elliptical orbits of the four new planets, as it is possible to see from his remarkable work (1809)[13].

3. Wolfgang Bolyai, the father of the co-founder of non-Euclidean geometry. On 28.8.1856 (W/Erg-2, pp. 158 – 159) answered Sartorius von Waltershausen (the author of a biography of Gauss published the same year). Gauss, as it occurred, informed Bolyai about his discovery of condition (2) in 1802 or 1803.

4. Bessel (1832, p. 27) learned about condition (2) before 1805 *durch eine mündliche Mitteilung von Gauss*.

This will suffice. So what about the abominable Stigler's statement *solicited reluctant testimonies*? Prompted by him, I myself have discovered the three new witnesses, but at least he should have known all about Olbers as well as Gauss' qualifying remark about von Zach. And it is evident that a disproval of a non-mathematical statement is tricky. Stigler had slandered the memory of a colossal giant. Shame, great shame on the scientific community for upholding that monster even until now.

Notes

1. Notation: W-i = Gauss, *Werke*, Bd. i; W/Erg-i = Gauss, *Ergänzungsreihe*, Bd. i.

2. I discovered that Lambert (*Photometria*, 1760, in Latin) had introduced the principle of maximum likelihood. A German translation of the appropriate passage is in Schneider (1988). Lambert considered an unspecified unimodal density function and remarked that its location parameter can be estimated by that principle. He only mentioned photometric measurements. He also introduced the term *Theorie der Fehler* (Sheynin 1971).

3. Or rather their least-squares estimators.

4. In 1818 Bessel was the first to claim that the errors of classical astronomical observations were normally distributed. He missed the opportunity to note that the distribution was not quite normal, and he repeated the same in 1838. I am sure that his mistake was intentional (Sheynin 2017, p. 156). The normal law became entrenched in natural science, especially after Maxwell, in 1860, introduced it into the kinetic theory of gases.

5. Regrettably disregarded by statisticians.

6. Observation in astronomy and geodesy and adjustment of geodetic measurements was another field in which Gauss left his mighty imprint.

7. Hald (1998, p. xvi) called his book *epochal*. Indeed, *epochal* in the worst sense. His opinion was most certainly biased by some personal reasons. And it prompts me to say that Stigler paid scanty attention to foreign (non-English) sources, for example, to Lambert and Daniel Bernoulli.

8. Stigler, especially on pp. 27 – 34, attacked Euler and accused that great scholar of ignorance. Thus (*just one example*), instead of adjusting some observations, Euler dared to apply condition (3)! I discussed that condition in § 1 and Stigler could have learned from my previous publication (1977, p. 48).

9. Disprove, if Gauss' integrity or memory is questioned.

10. Maennchen (1918, p. 65 et seq.) stated that Gauss had indeed made mistakes in his calculations: he calculated rapidly and did not always check his results.

11. That such critics do not know or simply disregard earlier authors is their usual practice. Stigler is most certainly guilty in this respect as well.

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III

The Discovery of the principle of least squares

Historia Scientiarum, vol. 8, 1999, pp. 249 – 264

Mein Wahlspruch ist aut Caesar, aut nihil].
(Gauss to Olbers 30. 7.1806)

1. Introduction

1.1. The Aim of This Paper. Some Notation. My subject seems to be known, and I myself had published a few relevant contributions, notably [Sheynin 1979; 1993; 1994; 1996]. Nevertheless, new details continue to emerge and the interpretation of some facts established long ago is questioned, and I aim at tidying up the general picture. Neither am I satisfied with the bibliographic description offered even by worthy commentators and I am providing all the necessary passages in their original languages (except those in Latin), and, as far as Gauss is concerned, I refer exclusively to his *Werke* and to a highly reputed collection of some of his contributions.

After a brief discussion of the achievements of Gauss's predecessors in § 2, I go on to describe his own work: § 3 is given over to his early applications of least squares, and § 4 dwells on the dissemination of his discovery; finally, in § 5 I study the computational aspect of least squares. The mathematical essence of Gauss [1809a] was discussed time and time again; the latest reliable source is Hald [1998].

When citing Gauss's correspondence with Olbers and Schumacher I use notation such as G – O or G – S; W-i means Gauss's *Werke*, Bd. i and, finally, W/Erg-i is a reference to Bd. i of the Ergänzungsreihe of the same *Werke*. One of the main pertinent commentaries is due to Brosche und Odenkirchen [1996 – 1997] which I abbreviate to B & O.

1.2. The Condition, the Principle, and the Method of Least Squares. The condition of least squares

$$\Delta w_1^2 + \Delta w_2^2 + \ldots + \Delta w_n^2 = \min \qquad (1)$$

is the restriction imposed on the residuals Δw_i of an inconsistent redundant system of n equations in m unknowns ($n > m$),

$$a_i x + b_i y + c_i z + m + w_i = 0,\ i = 1, 2, ..., n, \qquad (2)$$

to obtain a reasonable set of values, or (estimators of the) unknowns. Contrary to tradition, I retain the term method of least squares (MLSq) only for the theory as developed by Gauss in 1823 – 1828, and use *principle of least squares* for his results of 1809. In the sequel, *unknown* usually denotes its least-squares estimator.

1.3. Previous Methods of Solving Systems of Equations. Before 1805, the advent of least squares, several methods of solving systems (2) have been applied [Sheynin 1993, § 4]. Here is one of them: Solve all the subsystems of m equations each (this was possible for m = 2 and perhaps $m = 3$) and assume that the unknowns are equal to the respective mean values of the *partial* unknowns over all the subsystems. Later on, Jacobi and Binet, independently, proved that, by assigning appropriate weights to the subsystems, the solution thus obtained was identical with the one derived by least squares.

2. The Predecessors

2.1. Legendre. Legendre [1805, pp. 72 – 73] was the first to formulate publicly condition (1):

De tous les principes qu'on peut proposer pour cet objet [the solution of systems (2)], *je pense qu'il n'en pas de plus général, de plus exact, ni d'une application plus facile que celui … Par ce moyen il s'établit entre les erreurs une sorte d'équilibre qui empéchant les extrémes de prévaloir, est trés-propre à faire connoitre l'état du système le plus proche de la vérite*.

When applying this condition, he continued, the

Erreurs [residuals] *extrémes, sans avoir égard a leurs signes, soient renfermées dans les limites les plus étroites qu'il est possible*.

This is not so [Sheynin 1973, p. 124] and it follows that Legendre advocated least squares for a wrong reason. Again, De la Vallée Poussin [1911, p. 2], as noted by Harter [1977, p. 17], wrongly stated that Legendre had introduced condition (l) because he was unable to minimize the maximal absolute error.

2.2. Robert Adrain. In 1808, or, rather, 1809 [Hogan 1977], Adrain [1808] justified the normal law for the errors of observation, derived the condition of least squares and applied it to solve several important problems [Sheynin 1965; Dutka 1990]. The theoretical part of Adrain's work was deficient, but one of his substantiations of the normal law was repeated later by John Herschel and Maxwell (who hardly knew of their predecessor).

2.3. Daniel Huber. To this very day, the Swiss astronomer and mathematician Daniel Huber was thought to have discovered condition (l) before 1802 [Sheynin 1993, p. 49]. Dutka [1990], however, apparently refuted this opinion (never really justified) by discovering a forgotten paper [Spiess 1939, p. 12] whose author had referred to Huber himself. Huber named Legendre as the author of the *Masstab der kl. Quadrate*.

3. Gauss

3.1. The Discovery. Gauss discovered condition (1) in 1794 or 1795. He stated this fact publicly [1806, 1809a; 1809b, § 186], and he repeated his claim ln several letters, e. g., G – O 30.7.1806, W/Erg-4, p. 305; Gauss – Laplace 30.1.1812, W-10, Tl. 1, p. 373; G – S, 3.12.1831 and 6.7.1840, W/Erg-5, Tl. 1, p. 292 and Tl. 2, p. 387.

In his letter to Laplace, Gauss made known an additional fact:

Cependant mes applications frequentes de cette méthode ne datent que des l'année 1802, depuis ce tems j'en fait usage pour ainsi dire tous les jours dans mes calculs astronomique[s] sur les nouvelles planètes.

I quote now the passage from Gauss [1809b]:

Übrigens ist unser Princip, dessen wir uns schon seit Jahre 1795 bedient haben, kürzlich auch von Legendre … aufgestellt worden.

It was the expression *unser Princip* that angered Legendre. In a letter to Gauss dated 31 5.1809, W-9, p. 380, he indicated in strong wording that priority in scientific discoveries could be established only by publication. Having received no answer, Legendre [1820, pp. 79 – 80] accused Gauss of appropriating condition (l), see the relevant passage in [Sheynin 1973, p. 124, note 83].

Gauss did answer Legendre's criticism, although only in his letter to Laplace (above). After describing the chronology of his discovery and application of least squares, he asked rhetorically:

Pouvais je parler de ce principe, que j'avais annoncé a plusieurs de mes amis déjà en 1803 comme devant faire partie de l'ouvrage que je preparois [the *Theoria motus*], *comme d'une méthode empruntée de Mr. Legendre*?

Perhaps not, but *aufgestellt und schon veröffentlicht* in the passage above would have been much better. Other unpleasant episodes from Gauss's life are also known, see for example Biermann [1966, pp. 17 – 18], who nevertheless concludes that

Was einem normalen Autor verboten ist, einem Gauss wohl gestattet werden muss, zumindest müssen wir seine Gründe respektieren.

Yes, but there is yet one more point. Gauss [1823a, § 17] had another opportunity to correct himself, but, on the contrary, he avoided any mention of the French scholar[1]:

Das Verfahren … welches von uns schon lange … gebraucht wurde, und jetzt unter dem Namen der Methode der kleinsten Quadrate von den meisten Rechnern angewandt wird …

Apparently Gauss regarded Legendre's contribution as insignificant. Here is another phrase from his letter to Laplace:

Je ne me suis pas haté d'en publier un morceau detaché, ainsi Mr. Legendre m'est prevenu.

In a letter to Olbers of 24.1.1812 (W/Erg-4, p. 494), i.e., only a week previously, Gauss had already expressed his low opinion about unsubstantiated rules of adjusting observations and added that, surprisingly, the principle of least squares was not discovered a hundred years ago.

Moreover (G – S 24.6.1850, W-6, p. 89), Gauss for a long time thought that he had only rediscovered it. Finally, Gauss is known to have been reluctant to refer to others. Biermann [1983, pp. 422 – 423] quoted his letters to Schumacher of 1840 and 1842 as well as other sources to this effect.

3.2. The Normal Distribution. It might well be that Gauss derived the normal distribution in 1798: it was then that he wrote his celebrated phrase *Calculo probabilitatis contra La Place defensus* (W-10, Tl. 1, p. 533) in his diary. In his letters (G – O 24.3.1807 and 24.1.1812, W/Erg-4, pp. 329 and 493 – 494) he explained that he had shown in 1798 that the [Boscovich] method of adjusting observations [also] applied by Laplace did not conform to the principles of probability theory. And in a letter to Laplace (§ 3.l) he mildly remarked that the MLSq *rapprochée aux principes du calcul des probabilités*. Indeed, for normally distributed errors the most reasonable method of adjusting the observations is that of least squares.

Elsewhere Gauss [l821, p. 193] stated that in 1797 [!] he

Nach der Grundsätzen der Wahrscheinlichkeitsrechnung the combination of observations *zuerst untersuchte*, and *fand bald, dass die Ausmittelung der wahrscheinlichsten Werthe der unbekannten Größe unmöglich sei, wenn nicht die Funktion, die die Wahrscheinlichkeit der Fehler darstellt, bekannt ist.*

He went on to describe these attempts (as published later in the *Theoria motus*), but did not specify any more dates.

3.3. The First Form of the Principle. Gauss (G – O 30.7.1806, W/Erg-4, p. 305) indicated that, by that time, his *Methode* was

So durchaus verändert, dass sie ihrer ersten Gestalt, worüber Sie [Olbers] *den Aufsatz hatten, fast gar nicht mehr ähnlich sieht*.

Elsewhere Gauss [1806] explained that since 1802 he had

Noch immer an der Vervollkommnung der Methode selbst gearbeitet, besonders in dem vorigen Winter, und ihre jetzige Gestalt sieht ihrer ersten fast gar nicht mehr ähnlich

Here and in many other places Gauss used the term *Methode* without specifying it. At least sometimes he might have thought, in the first instance, of the entire process of determining orbits from redundant observations. Here, however, Gauss definitely meant the MLSq: in his previous lines, he mentioned his own use of it *seit zwölf Jahren*. Recalling Gauss's stochastic studies of 1797 – 1798 (§ 3.2), how should we interpret the two passages, above? My own understanding is that, beginning with 1805 or 1806, Gauss resumed these studies both in essence and in a methodological way, cf. § 3.4.

I cannot agree with B & 0 (p. 19) who hardly believe that Gauss derived normal equations until perhaps 1805 and 1806. They also state (p. 12) that Gerardy [1977] did not find any normal equations in Gauss's manuscripts of 1802 – 1807. Actually, however, this statement is wrong, see Gerardy's pp. 10 – 11. In accord with their belief, B & 0 (p. 12) concluded that Gauss had to apply iterative procedures. Their implication is hardly correct: Gauss paid due attention to iterations, see § 5.2.

There is also a strong case for stating [Stewart 1995b, p. 209] that Gauss had developed his celebrated method of solving normal equations from the very beginning; an entry in his diary dated June 1798 (W-10, Tl. 1, p. 533) reads

Problema eliminationis ita solutum ut nihil desiderari possit.

Moreover, this sentence resembles one of his later phrases [Gauss 1823a, § 31], where he referred to the estimation of the relative precision of the unknowns along with eliminating them consecutively: *Nihil amplius desiderandum relinquere videtur* [evidently nothing more to be desired]. Stewart believes that Gauss was thus able, already in 1798, to estimate precision, but this is less evident.

Here, finally, is my last source [Gauss, ca. 1805, p. 161]:

Ich habe in dieser Zwischenzeit [from October 1801] *nach und nach so vieles an meinen zuerst gebrauchten Methoden abgeändert, so manches hinzugesetzt, und für manche Theile ganz neue Wege eingeschlagen, dass sich zwischen der Art, wie ich Anfangs die*

Planetenbahnen wirklich berechnete und der im gegenwärtigen Werke vorgetragenen. nur noch geringe Ähnlichkeit finden würde.[2]

3.4. The Stochastic Approach. The change from the first form to the new approach occurred *besonders in dem vorigen Winter* [Gauss 1806]. that is, in the beginning of 1806 or at the very end of 1805. Gauss started working on the *Theoria motus* in the autumn of 1806 and completed its (original German) manuscript in April [I would say, May] 1807. In May, he began to translate it into Latin, and [apparently] in November *begann der Druck*.[3]

The section on the stochastic treatment of observations was obviously the last one to be compiled. Here are Gauss's own words (G – O 24.3.1807, W/Erg-4, p. 329):

Jetzt bin ich damit beschäftigt, das Problem [of determining the most probable values of the unknowns from a redundant number of observations] *nach Gründe der Probabilitätsrechnung abzuhandeln.* I think that *bin beschäftigt* meant *am adding finishing touches to*, or *am developing the practical side of*, or perhaps *am studying the method of estimating the precision of the unknowns.* Indeed, it is difficult to imagine that Gauss, at that date, needed to do anything else. Recall also (above), that he completed the German manuscript in April or May 1807.

3.5. The Published Text of the Theoria Motus. We may assume that Gauss had given much thought to perfecting the exposition of the principle of least squares (and perhaps to changing it in some essential way) when translating its German version into Latin. Here is Olbers's relevant testimony (O – G 27.6.1809, W/Erg-4, p. 436):

Sie hatten wohl Recht, wenn Sie mir sagten, dass durch die successive Ausbildung Ihre Methode, wie sie jetzt ist, der anfänglichen Form derselben kaum mehr ähnlich ist. Auch die lateinische Umarbeitung scheint mir, so viel ich mich noch von der damals nur flüchtigen Durchsicht des deutschen Textes erinnere, noch vieles mehr vervollkommnet zu haben.

Much can be said about the methodological shortcomings of the *Theoria motus* (for example, of its § l77), but at least Gauss had indeed striven for all-round perfection, see the appropriate passage from Gauss [1806] in § 3.3. In any case, the principle of least squares as published in 1809 included

a) The derivation of the normal law as the distribution of observational errors.[4]

b) A corollary: the appearance of the condition (1) of least squares.

c) The determination of the relative precision of the unknowns.

Gauss (§ 184) also provided an example of adjusting observations. Taking four observations with three unknowns, he wrote out the corresponding system of normal equations and indicated the three numbers comprising its solution. He hardly said anything about the computational aspect of this problem, see § 5.3.

4. Gauss: Application and Dissemination of the Discovery

4.1. Application of Least Squares. In 1986, an able, impudent American statistician misguidedly called in question Gauss's achievements (in particular, his use of least squares before 1805) and extolled to the skies Legendre's merits. I think that this was the first (and, hopefully, the last) time that the memory of the great German scholar was profaned. I [Sheynin 1993, § 7] refuted the astonishing accusations (as well as an unfounded attack against Euler), but modern statisticians apparently continue to trust him.[5]

So, for what purposes did Gauss apply condition (l) before 1805?

1) He had *apparently* formulated this condition *while adjusting unequal approximations* [when calculating square roots] *and searching for regularity in the distribution of prime numbers* [May 1972, p. 299]. May did not elaborate, but here is a partly relevant passage [Maennchen 1918/1930, pp. l9 – 20]:

Die Frage liegt nahe, wie man mit Hilfe dieser beiden Näherungswerte dem wahren Werte möglichst nahe kommen kann. Diese Frage in allgemeinerer Fassung hat ihn bekanntlich intensiv beschäftigt und in der berühmten Methode der kleinsten Quadrate ihren Abschluss gefunden.

Feci quodpotui, faciant meliora potentes! [I accomplished everything possible for me. Let those who are able, do better.]

2) Treatment of meridian arc measurements [Gauss 1799b]. B & O, who studied Gauss's attempt, concluded, on p 19, that its extant text does not prove that he had indeed applied least squares in this case. However, several factors (mistakes or misprints in the data or calculations, weighing of observations, introduction of short cuts, cf. § 5.1) could have made any reconstruction hardly possible. And Gauss certainly made mistakes, apparently because he did not always check his work and calculated too rapidly (see below). See for example Gerardy [1977] or his own methodological contribution [1823b] where the solution of a system of normal equations is wrong (the signs of *dx* and *dy* should be reversed). Then, Maennchen [1918/1930, p. 65 et seq] had much to say on this point. In his opinion, one of the reasons for the mistakes to occur was that Gauss had *ungewönlich rasch rechnete*.

B & 0 additionally indicate that Gauss did not state, on an appropriate occasion (G – S 3.12.1831, W/Erg-5, Tl. 1, p. 292),that he had applied least squares in this adjustment.[6] I disagree. Gauss actually wrote:

Die von Ihnen erwähnte Stelle in Zach's [periodical] *ist mir wohl bekannt; die Anwendung der M. der kl. Q., deren dort Erwähnung geschieht, betrifft einen früher in derselben Zeitschrift abgedruckten Auszug aus Ulugh Beighs Zeit-Gleichungs-Tafel*
(see Item 3). And on another occasion Gauss (G – O 24.1. 1812, W/Erg-4, p. 493) mentioned both cases on a par.

Less thorough studies of this topic are due to Stigler [1981] and Dutka [1996] who believe, although do not actually prove, that Gauss had indeed applied least squares for adjusting meridian arc measurements. Neither were Gilstein & Leaner [1983, p. 946] able to show this. No definite confirmation is perhaps possible here.

3) The reduction of Ulugh Beg's table of the equation of time [Gauss 1799a]. Dutka [1996, p. 362] agrees that Gauss had indeed applied condition (l) in this case. Gauss himself said so (G – O 24.1.1812, W/Erg-4, p. 493, and G – S 3.12.1831, W/Erg-5, Tl. 1, p. 292). Stigler [1981, p. 466n] was hardly justified in dismissing Gauss's study as an

undated fragmentary calculation. For their part, B & 0 (p. 43) state that Dutka's conclusion is not convincing. Indeed (cf. Item 2), it would be extremely difficult to say yes or no.

4) The determination of the orbits of the first four minor planets. Gauss stated that he had applied his *Methode* for this purpose, see his letters to Zach [1806], Olbers (24.1.1812, W/Erg-4, p. 493) and Laplace (30.1.1812, W-10, Tl. 1, p. 373). In the last two instances he mentioned the MLSq. I also quote a previously unknown letter from Gauss to Maskelyne dated 19.5.1802 (Roy. Greenwich Obs., Code 4/122:2):

When I had received Dr. Olbers's observations till April 17, for curiosity's sake I attempted to apply to them the same method, which I had made use of in my calculations about Ceres Ferdinanden, and which without any hypothetical supposition yields the true conic section as exactly as the nature of the problem & the precision of the observations will permit.

Nevertheless, Gauss's statements are not generally accepted. Marsden [1995, p. 185], for example, expressed doubts and indicated that [in any case] Gauss *was quite reluctant* to use condition (l). B & 0 (p. 11) approvingly mention him and additionally cite Gerardy [1977] who stated in his Abstract that he was describing Gauss's first application (from 1803 onward) of least squares in geodetic calculations.

I choose to differ. First, we are not allowed to dismiss the direct indication of the Master. Second, Gauss applied condition (1) only when using at least a few redundant observations, see Brendel [1924] or Galle [1924, p. 9] to whom Marsden surprisingly did not refer. Third, Gerardy's statement is hardly correct (and should have been cited, if at all, in connection with meridian are measurements). I think that Gerardy really meant the first application of least squares in adjusting geodetic networks (Item 5 below).

5) Adjustment of geodetic networks. Drawing on archival sources, Gerardy [1977] stated that Gauss, in 1802 – 1807, had participated in land surveying (in part, for his own satisfaction) and concluded, on p. 19 (note 16), that he had started using condition (l) not later than m 1803. Regrettably, Gerardy concentrated on simple calculations and his statement was not definite enough.

4.2. Notification of Friends and Colleagues before 1805. One of Stigler's infamous accusations formulated against Gauss is that he solicited reluctant testimony from friends that he had told them of condition (l) before 1805. I [1993, § 7.2] had refuted it, notably by referring to Bessel. Now, I name several more witnesses, but even without them the accuser had enough material at hand for refraining from slandering the memory of Gauss. I am listing those who undoubtedly may be named here.

1) Olbers. On 4.10.1809 Gauss (W/Erg-4, p. 441) asked him whether he remembered having heard about least squares from him (from Gauss) in 1803 and then again in 1804. On 24.1.1812 Gauss (Ibidem, p. 493) asked Olbers whether he was prepared to confirm publicly that s*chon* in 1803 he came to know about least squares from him, from Gauss. This time Olbers's answer (10.3.1812, Ibidem, p.

495) is known: yes, *Gern und willig*, and at the first opportunity. He was indeed as good as his word [Olbers 1816, p. 192n]:

Gauss bereits im Junius 1803 die Güte hatte, mir diese Methode als längst von ihm gebraucht, mitzuteilen und mich über die Anwendung derselben zu belehren.

But why such a delay? Because in 1812 – 1815 Olbers had not published anything suitable (*Catalogue of Scientific Papers*, Royal Society).

Much later Gauss (G – S 3.12.1831,W/Erg – 5, Tl. 1, p. 292) remarked however that

War dies zwar gut gemeint, hätte er [Olbers] *mich aber vorher gefragt, so würde ich es hautement* [French] *gemissbilligt haben*.

He, Gauss, explained everything in 1809 and did not need any confirmation. This, of course, was a reversal of his previous attitude. Or, was there a previous stable attitude at all? In a letter to Laplace Gauss (30.1.1812, W-10, Tl. 1, p. 374) stated:

J'ai cru que tous qui me connaisent le croiroient, méme sur ma parole, ainsi que je l'aurait cru de tout mon coeur si Mr. Legendre avait avancé, qu'il avait possedé la méthode déjä avant 1795.

Then, Sartorius von Waltershausen [1856, p. 43], without providing the relevant date, quoted Gauss as saying *Man hätte mir wohl glauben können*.

2) and 3) The same author also stated (l. c.) that Gauss had explained his method to Olbers and two other scientists, [Wolfgang] Bolyai and *einen süddeutschen Freund*. At about the same time (12 and 28.8.1856, W/Erg-2, pp. 157 and 158 – 159) Sartorius von Waltershausen exchanged letters with Bolyai and wrote:

Gauss hat mir gelegentlich erzählt, er habe Ihnen die Methode der kleinsten Quadrate mitgetheilt, Sie wissen welcher Streit mit den Französen darum gewesen. Besitzen Sie über diesen Gegenstand etwa nähere Aufzeichnungen oder Bemerkungen?

Bolyai answered:

Vom französischen Lärm habe ich nie das Mindeste gehört … mein Rath wäre, die Wuth nicht zu reizen, damit des schöne Licht der sanft und groß untergegangegen Sonne [Gauss died 23.2.1855] *nicht von rauhen Stürmen verdunkelt werde. Conteme et vinces*. [Despise and win.] … *Mein Sohn … hat den Brief* [from Gauss] *abgeschrieben, und die Copie schicke ich hiemit. Der Brief ohngefähr von 1802 oder 1803 könnte in obiger Hinsicht Bescheid geben, aber er ist … verbrannt. Ohne Zweifel hat er mir gelegentlich der in der Anwendung nützlichen Regel erwähnt, aber ich finde nichts aufgeschriebenes*.

That the *Streit* continued until the mid-century is doubtful. True, a new debate about the two different approaches to the MLSq, those of Laplace and Gauss, was not yet settled, but this is another story.

4) As I noted [Sheynin 1993, p. 51], Bessel [1832, p. 27] had come to know condition (l) before 1805 *durch eine mündliche Mittheilung von Gauss*, and B & 0 (pp. 14 – 15) quoted Bessel's appropriate letter to Humboldt of 19.4.1844 [Fe1ber 1994, p. 174].

5) Lindenau. In a letter to Laplace, Gauss (30.1.1812, W-10, Tl. 1, p. 373) mentioned Lindenau. In June 1798, he wrote, he had found but that the MLSq was *rapprochée aux principes du calcul des*

probabilités: une note la dessus se trouve dans un journal que j'ai tenu sur mes occupations mathématiques et que j'ai montré dans ces jours à Mr. De Lindenau.

The *note* (§ 3.2) did not cite the MLSq, but Gauss hardly omitted to say something about this subject (not necessarily explaining the principle of least squares) when speaking with Lindenau.

6) Von Zach. Zach's case is complicated. Gauss [1799b] published a letter in Zach's periodical concerning a misprint in the date on certain meridian arc measurements. The letter contained the phrase

Ich entdeckte diesen Fehler, indem ich meine Methode, von der ich Ihnen eine Probe gegeben habe, anwandte.

Zach inserted a comment: *Hievon ein andermal*.

Then, Gauss (G – S 3.12.1831, W/Erg-5, Tl. 1, p. 292) explained that he had indeed mentioned his method to Zach *ohne ihm jedoch das Wesen der Methode selbst mitzutheilen*. Contrary to an expressed opinion [Sheynin 1979, p. 26, note 11; B & 0, p. 15] I do not think that Gaus's's explanation completely exonerates Zach, whom Schumacher, in a previous letter to Gauss (30.11.1831, W/Erg-5, Tl. 1, p. 290), had accused of an apparent unwillingness to clear up the picture: *das andere Mal ist aber nie gekommen*[7].

There are two additional points in Zach's favour; but, considering the bitterness of the situation, he should have acted more resolutely.

a) His editorial acceptance of Gauss 's claim [1806]:

Es ist mir übrigens lieb, dass ich nicht schon 1802 meine Methode[8] *wie ich die Ceres- und Pallas-Bahn berechnet hatte, bekannt gemacht habe so viele Aufforderungen auch deshalb an mich gelangten. Denn seitdem habe ich noch immer an der Vervollkommnung der Methode selbst gearbeitet*.

b) Zach [1813, p. 98n] once more indirectly accepted Gauss's claim by repeating it without expressing any misgivings:

Le célébre Docteur Gauss était déjà depuis 1795 en possession de cette méthode, et il s'en est servi avec avantage dans la détermination des élémens des orbites elliptiques des quatre nouvelles planètes comme on peut voir dans son bel ouvrage [1809b].

Regrettably, it is not seen there.

Several unfavourable remarks about Zach are contained in the correspondence of Gauss and his colleagues including Bessel [Sheynin 1995, p. 171, note 14]. However, at least in 1801 – 1802 Zach wrote quite friendly letters to Gauss [Brendel 1924, pp. 17 and 12].

There is one more point. An anonymous review of Gauss [1809b], published the same year in the *Monatliche Correspondenz*, contained, on p. 191, a few lines about the relevant section of the *Theoria motus*, and, in particular, we find there the following phrase:

Diese Untersuchung führt ihn [Gauss] *auf die neuerlich von Le Gendre zu diesem Behuf gegebene Méthode des moindres quarrées*[9], *die der Verfasser aber schon seit 1795 zu seinen Rechnungen brauchte und schon damals einiger mathematischen Freunde mittheilte*.

Item a) above is an editorial acceptance of Gauss's claim by Zach. Indeed, the title-page of the appropriate volume of that periodical clearly stated: *Herausgegeben vom Freyherrn F. von Zach*. Moreover, Dutka [1996, p. 357], who discovered this passage (and quoted it in English translation), naturally attributed the review to von Zach. B & 0 (p. 43) state, however, that the reviewer was Lindenau, but do not put forward any proof. Nevertheless, in describing Lindenau's visit to him in 1809, Gauss (G – O 12.9.1809, W/Erg-4, p. 439) reported that

Er beklagte sich sehr, dass die Astronomen ihn bei der Herausgabe der M. C. so wenig unterstützen.

Gresky [1968, note 1 and p. 22] provided other facts to the same effect although without proving that Lindenau was editor in 1809. Be that as it may, Lindenau remains in my list as a person in whom Gauss had at least partly confided.

Gauss had certainly initiated several other persons as well (although perhaps of lesser stature):

Ich erinnere mich sehr bestimmt, dass ich oft, wo ich mit andern von meiner Methode sprach (wie z. B. während meiner Studirzeit 1795 – 1798 wirklich vielfach geschehen ist), geäußert habe, ich wolle die allergrößte Wette eingehen, dass Tobias Mayer bei seinen Rechnungen dieselbe Methode schon gebraucht habe
(G – S 6.7.1840, W/Erg-5, Tl. 2, p. 387).

To end this subsection, I note, after Plackett [1972, p. 250] did, that Laplace [1812/1886, p. 353] had accepted Gauss's priority both in *un usage habituel* of least squares, and in communicating his discovery to *plusieurs astronomes*.

5. Gauss: the Computational Aspect

Here, I supplement or even refute some previous statements. Thus, it was alleged that at least up to 1807 Gauss did not use normal equations (§ 3.2), and that in 1809 he did not think about the computational aspect of least squares (§ 5.3). Then, when mentioning iterations, some authors (B & 0, p. 12) overlook Gauss's role in originating this procedure (§ 5.2).

5.1. Ad Hoc Methods. Astronomers continued to apply more or less arbitrary methods for solving linear systems (cf. § 1.3) even after least squares had become generally accepted. Thus, when calibrating thermometers, Bessel [1826, p. 229] was confronted with a system of 26 initial (not normal) equations with the same number of unknowns and had to abandon condition (l).

Gauss accused Mayer (in whose lifetime least squares were of course yet unknown) of adjusting his observations

Nicht nach eines systematischen Princip, sondern nur nach hausbackenen Combinationen (G – S 24.6.1850; W/Erg-5, Tl. 3, p. 90). He cited Mayer's manuscripts, but it is possible that the latter's published trick was almost the same. And in any case Gauss himself, in an earlier letter to Schumacher of the same year (Ibidem, pp. 66 – 67), described his own similar procedure which he recommended for calibrating aneroids.

5.2. Iterations. Gauss was the originator of the so-called method of geodetic relaxation, or at least of its non-cyclic one-step version. He (G – Gerling 26.12.1823, W/Erg-3, pp. 298 – 302) indicated that he had applied iterations for solving normal equations which appear in station adjustment (in determining the final values of angles or directions at a given station of triangulation). A special point is that Gauss added up all his equations and included the summary equation obtained into his system thus providing himself with a means for checking his calculations at any step.[10]

Helmert [1872, pp. 133 – 136] described iterations and mentioned Gauss, but did not furnish the exact reference. This was done by Forsythe [1951], also see Sheynin [1963].

Elsewhere Gauss [1828, §§ 18 – 20] described what would now be called block Gauss – Seidel relaxation [Stewart 1995b, p. 230] and what he himself termed *successive Annäherungen* (§ 20): He thus originated the *gruppenweise Ausgleichung*, a procedure widely used in adjusting geodetic networks. Dedekind [1901/1931] testified that Gauss, in his lectures on the MLSq, had paid much attention to iterations and described Gauss's explanation of the same procedure as the one recommended in Gauss's letter to Gerling.

Iterations are also applied to adjust observations by least squares without working out the normals, see for example Black [1938], but there is no evidence that Gauss had originated this trick as well.

5.3. The Gaussian Algorithm. Still, successive elimination of the unknowns from systems of normal equations was Gauss's main method of solving them.[11] Indeed, iterations do not provide any means for estimating the precision of the unknowns. The Gauss method is extremely simple; Berezkina [1970] even claimed that ancient Chinese scholars had knew and applied it.

She dwelt on the Chinese *Mathematics in Nine Books* definitely completed before 150 BC (and translated into Russian by herself in 1957), but apparently written by several authors over some period of time.

Book 8 of this source, as she reports on her p. 165, contains a regular algorithm for solving linear systems in n equations and the same number of unknowns, essentially coinciding with the Gauss method and differing from it in that all the operations are there made by means of a calculating board.

Berezkina also describes the solution of the system

$$x+1/2y=48,\ 2/3x+y=48$$

as given by the Chinese author(s).

However, the practical merit of the Gaussian algorithm greatly increases with the number of the equations involved, and it is much simpler when (as in the case of normal equations) the matrix of the system is symmetric and positive-definite. Add to this that

a) Gauss [1809b, §§ 183 – 184; 1823a, § 21] was able to estimate the relative precision of the unknowns along with the solution itself (although of course only when making some additional calculations), also see Sheynin [1994, p. 260].

b) Gauss [1811, § 13; 1828, § 5 et seq.] introduced exceptionally convenient notation which made his algorithm theoretically elegant and simple. True, beginning with Karl Pearson, statisticians had ignored it; already for this reason the achievements of the classical error theory remained for many decades hardly known.

c) Either Gauss, or his followers devised a simple means for checking each step of the computations beginning with the transition from equations (2) to the normal equations, see Helmert [1872, § 14]. Clearly, then, ancient scholars can hardly be called the predecessors of Gauss. Some authors, however [Farebrother 1996, p. 208], believe that

It is arguable that Gauss [1809b, § 184] *applied a variant of the Chinese method*. Farebrother draws on other descriptions of the Chinese mathematics, but I do not see how my reasoning can be called in question.

5.4. Gauss Always Thought About Computation. Stewart [1995a, p. 5] stated that *computational considerations are absent from the Theoria motus itself*. He did not say anything new, and the implication of his phrase is misleading, but, anyway, he [Stewart 1995b, p. 227] effectively abandoned his statement.

Maennchen [1918/1930, p. 3] noted that Gauss was often led to his discoveries *durch peinlich genaues Rechnen*. He continued: in Gauss's writings

Wir finden ganzen Tafeln, deren Herstellung allein die Lebensarbeit manches Rechners vom gewöhnlichen Schlage ausfüllen würde.

As to geodetic and astronomical work (which Maennchen did not study), I take up Gauss's *Theoria motus*. There, after justifying the principle of least squares, but before going on to assess the relative precision of the unknowns, Gauss (§ 180) stated that his principle merited attention also because it can lead to a convenient method of computing them. In § 185 he added, however, that, so as not to digress from his main goal, i.e., from the general theoretical discussion of least squares, he postpones until another time the description of the computational aspect. Very soon Gauss [1810, p. 205] noted that, especially with a somewhat large number of the unknowns, their elimination was *eine äusserst beschwerliche Arbeit*. I may therefore assume that the detailed explanation of his algorithm contained in [Gauss 1811] was to a large extent an aim in itself, necessarily delayed for about two years.[12]

Acknowledgment. I myself had found out the whereabouts of the letter from Gauss to Maskelyne (§ 4.1.4), but it was Prof. Curtis Wilson who managed to order its photostat copy. He also kindly sent me a copy and permitted me to quote the letter.

Notes

1. True, he mentioned Legendre in his much lesser known Selbstanzeige [1821, p. 194].

2. Olbers returned this *Entwurf* to Gauss in 1805 (O – G, 2.11.1805, W/Erg-4, p. 276), hence its dating. A previous version compiled in 1802 (G – O, 6.8.1802, Ibidem, p. 65) is lost, see Schilling's note on the same page.

3. M. Brendel (W-12, pp. 162 – 163) ascertained these dates by Gauss's correspondence with Olbers (letters of 29.9.1806 and 28.4, 26.5 and 29.10.1807, W/Erg-4, pp. 308, 350 – 351, 365 and 388 respectively). I reject any possibility of deliberate deception in another source (Gauss – Laplace 30.1.1812, W-10, Tl. 1, p. 373). There, Gauss stated that he had desired to unite all his methods *dans un ouvrage étendu* (*que j'ai commence* [which he began writing in] *1805 et dont le Manuscript d'abord en allemand étoit achevé en 1806*). And in any case deception would have been useless since Legendre published his contribution in 1805.

Von Zach vainly advised Gauss to publish his work in French (G – O 24.3.1807, W/Erg-4, p. 330). On the relations between Gauss and the French scholars see an interesting article by Reich [1996].
4. Strangely enough, this derivation is not sufficiently known. Thus, commentators assume, as Gauss (§ 176) did, a uniform prior probability of any set of observational errors. This, however, follows from the Gauss postulate of the arithmetic mean, see Whittaker and Robinson [1924, p. 219n], whose remark was forgotten.
5. One of them [Healy 1995, p. 284] even indirectly stated that the American author was the best historian of statistics of this century. Sancta simplicitas!
6. Dutka [1996, p. 368] attributes this omission [?] to Gauss's general dissatisfaction with the adjustment caused by incomplete data or errors of some kind.
7. Hardly known is Gerling's similar statement [1861, p. 274]: *ich habe … trotz vielfältigen Suchens keine Andeutung finden können, ob und wann dieses andermal verwirklicht sei*.
8. Gauss definitely meant the MLSq, see § 3.3.
9. As stated a few lines above, the method was applied to the determination of the orbits.
10. Gauss [1823b, p. 141] had twice used the term *normal equations* (in German); here, in his letter to Gerling, he called them however *Bedingungsgleichungen* and retained the same expression later (pp. 301 – 302) for equations (2)!
11. Needless to say, he also had to accomplish formidable preliminary calculations (and to work without even a simplest calculating machine).
12. Later Gauss [1823a, § 31] stated that in § 182 of the *Theor. motus* he had [already] *auf einen eigenthümlichen Algorithmus hingewiesen*. However, the explanation provided there was too abstract.

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IV

Quetelet as a statistician

Arch. Hist. Ex. Sci., vol. 36, 1986, pp. 281 – 325

1. Introduction

1.1. Statistics before Quetelet. As early as 1662, GRAUNT [109, p. 222] maintained that statistical data on population make *Trade, and Government … more certain, and Regular* while SÜSSMILCH, in the mid-18th century, drawing on these data, attempted to ascertain conditions favourable for society and for the increase in population. As LAZARSFELD [88, p. 218] put it, SÜSSMILCH's *Göttliche Ordnung* [120] *is filled with social analysis*.

It would seem that population statistics as a social discipline owes its origin to SÜSSMILCH, but this is not so since he mostly studied statistical data in general rather than using information about a given country.

In the 19th century public needs called for national statistical services and led to regular publication of data, mainly on the population of the countries concerned. For the first time ever, statistical studies of isolated regions, or states, became possible and demography emerged anew.

MEITZEN [92, p. 26] pointed out that in France, at the very beginning of the 19th century, *die Statistik … allgemeines Interesse erregte und Modesache wurde*. This is indeed true. At the end of the 18th century (and later) LAPLACE [107, §§ 2.4 – 2.7] busied himself with statistics. He used sampling to determine the population of France and he did not shirk from estimating the corresponding error.

In 1817, the Paris Academy of sciences initiated *un prix annuel destiné aux récherches statistiques* [61, pp. LX – LXII] and, during the next few years, awarded this prize to several scholars; see the memoirs of the Academy published in 1824 and 1827. From 1821 to 1829, FOURIER edited a statistical study of Paris and the *Département de la Seine* [67] filling four volumes. His research pertained to demography as well as to economic, medical, and meteorological statistics.[1]

While delivering his report on the activities of the Paris Academy of sciences in 1817, DELAMBRE [61, p. LXX] argued that

Les mesures géodésiques, les observations relatives aux températures et à l'état de l'atmosphère, aux maladies communes, à la salubrité de l'air, des alimens et des eaux, l'exposition des procédés des arts, les descriptions minéralogiques appartiennent sans doute à la statistique … [cf. § 1.2][2] *mais cette science n'a point pour but de perfectionner les théories*.

DELAMBRE (p. LXVII) believed that statistics *a pour objet de rassembler et de présenter avec ordre, les faits qui concernent directement l'économie civile* [facts that concern climate, territory, soil, waters, population, and economy]. He also maintained (p. LXVIII) that statistics

Diffère beaucoup de la science de l'économie politique, qui examine et compare les effects des institutions, et recherche les causes principales de la richesse et de la prospérité des peuples. Ces considerations … ne sont point le premier objet de la statistique qui exclut presque toujours les discussions et les conjectures.

L'arithmétique politique … doit aussi être distinguée de la statistique.

The Academy taken as a body apparently adhered to the same opinion with regard to both the field of application and to the methodology of statistics.**3**

During the greater part of the 19th century, public opinion in Europe continued to be fascinated by findings in the field of moral statistics, i.e., in statistics on acts depending on man's free will (crimes, suicides, marriages, etc.).**4**

Many works of the 18th century contained elements of moral statistics. CONDORCET and LAPLACE [107, p. 173] noticed that receipts from lotteries had been stable. In 1763, KANT [105, p. 320, note] put on record the stability of the relative number of marriages and he repeated his statement in 1784 [91, p. 368]. Even before KANT, SÜSSMILCH [120, p. 106] pointed out that the ratio of the number of marriages to that of births was almost constant. In the third edition of his book, this time two years later than KANT, SÜSSMILCH (p. 126) expressly maintained that the relative number of marriages was stable.**5**

Official criminal statistics had been published regularly since the 1820's in France [69]. This immediately caught the attention of several scholars, among them QUETELET (§ 4).

CHUPROV [49, p. 403] devoted a few lines to the early history of moral statistics:

Above all else, moral statistics owes its revival, in the 1820's, to the general enlivening of theoretical thought, the first impulse to which was given by the brilliant school of French mathematicians. The appearance of abundant materials pertaining to some fields of moral statistics, chiefly to criminal statistics, had a no lesser influence.

Above all else and *no less an influence* are contradictory expressions, but CHUPROV'S general description is correct. What he did not say, though, was that French mathematicians, possibly FOURIER in the first place, had directed QUETELET'S attention towards statistics (§§ 1.2 and 5.1).

FREUDENTHAL [71, p. 8] divided the writings on social sciences published before QUETELET into two groups:

In de ene die werken, waarin te veel formüles en te weinig (*of in't geheel geen*) [if any] *empirische cijfers voorkomen; de andere* [works] *met veel cijfers en weinig of geen* [if any] *wiskunde***6**.

Only the writings of LAPLACE, FREUDENTHAL contended, belonged to both groups at once. LAPLACE'S predecessors from DE MOIVRE to EULER and DANIEL BERNOULLI should also have been mentioned, but at any rate before QUETELET statistics hardly existed as a scientific discipline, cf. § 6.1. FREUDENTHAL evidently attributes the *Tabellenstatistik* to the second group. This branch of statistics, which can be traced in the applications of the statistical method to natural science even in the 19th century, deserves a higher appraisal.

1.2. Quetelet as Natural Scientist. I have described the meteorological work of QUETELET (1796 – 1874) [116, § 5.3]. He collected and systematized observations and introduced elements of probability theory into this branch of science. In his letters to QUETELET, written in 1850 and 1851, FARADAY (Ibidem, p. 79, note 50) highly praised QUETELET'S observations of atmospheric electricity;[7] in 1875, KÖPPEN (p. 79) called Belgian meteorological observations the *most lasting* [in Europe] and *extremely valuable*. QUETELET (Ibidem, § 4.4) reasonably contended that meteorology (and natural science in general) was alien to statistics. Indeed stellar statistics, say, is primarily a branch of astronomy, etc. Still, QUETELET did not add that the statistical method will gradually penetrate into ever new branches of science. Moreover, he never spoke about a universal statistical method; thus HANK1NS [80, pp. 58 – 59] was probably wrong when he mentioned QUETELET'S [general] conception of statistics; see, however, § 2.1.

In 1853, QUETELET was chairman of the *Conférence maritime pour l'adoption d'un système uniforme d'observations météorologiques à la mer* [98, pp. 56 – 57], and HANKINS [80, pp. 25 – 27] described his other efforts to organize observations on national and international levels. QUETELET was a pioneer in anthropometry, a term that he himself introduced on HUMBOLDT'S recommendation [112, p. 333], and his writings contain dozens and dozens of pages devoted to various measurements of the human body. In this field he was apparently influenced by BABBAGE (Ibidem, p. 328) and in turn impressed GALTON (§ 6.2).

Even before GALT0N, QUETELET became interested in the development of talent with age [41, p. 74; 4, t. 2, p. 126; 7, pp. 132 – 134]. He also maintained [4, t. 2, p. 111, note] that it was possible to study

Les effets produits par la mémoire de l'homme, soit pour sa facilité à saisir, soit pour son énergie à retenir.

For his part, GALTON is credited with actually introducing statistical methods into psychology in 1869 [93, pp. 58 and 62][8]. QUETELET' s works also contain elements of medical statistics (data on deaths in various age-groups tabulated in connection with meteorological factors [113, p. 280], cf. § 2.4), and I repeat [112, p. 344] that DARW1N commented favourably on one of QUETELET'S ideas pertaining to the application of statistics in medicine.

LOMBARD, the founder of medical climatology, dedicated his monograph published in 1877 – 1880 *à la mémoire vénérée* of several scholars including his *amis*, Sir JAMES CLARK and QUETELET [113, p. 281]. Unlike some physicians, mathematicians and statisticians, QUETELET did not study statistically epidemic diseases, the then scourges of mankind (Ibidem, § 7). He never mentioned DARWIN and on one occasion [6, p. 259] he came out against the theory of evolution:

Les plantes et les animaux sont restés tels qu'ils sont sortis de la main du créatéur. Quelques espèces, à la vérité, ont disparu, et d'autres se sont montrées successivement.

According to DARWIN [112, p. 353], male animals (and men in particular) show larger variations in body measurements than do females. QUETELET (Ibidem, p. 333) made only one comparison of variations in men and women, but he did not comment on his results although they contradicted the views held by DARWIN.

Another of QUETELET's statements [7, p. 37; 10, t. 2, p. 36] was not in accord with DARWIN' s opinion either [57, p. 382] when he claimed, or rather repeated, the accepted idea that

Les parties [of the human body] *les moins sujettés à varier* [é. g., the head], *sont précisément les plus essentielles*
whereas DARWIN believed that such an assertion was nothing but a vicious circle.

QUETELET'S main interest in life was population statistics and moral statistics. As a young man, he met with the most eminent French mathematicians including LAPLACE [34, p. 669] and returned home a staunch partisan of statistical research.

Le goût de la statistique, he explained (Ibidem), *s'était particulièrement développé, en 1822, pendant mon séjour à Paris*.

1.2.1. Digression: Alph. De Candolle. I mention ALPH. DE CANDOLLE in § 4.4. My purpose here is to draw attention to his broad view on the applications of the statistical method and, incidentally, to show once more [112, p. 332] that in a sense he remained a statistician throughout his life. As early as 1833, DE CANDOLLE [60, pp. 333] contended that

Chaque branché des connaissances humaines a besoin de la méthode statistique, qui, dans le fait, n'est que la méthode numérique.

In this connection he mentioned geography, medicine, and (p. 334) *le nombre et la distribution géographique des êtres*. He also defined statistics as a science (Ibidem): This science

Consiste à savoir réunir les chiffres, les combiner et les calculer, de la manière la plus propre à conduire à des résultats certains. Mais ceci n'est, à proprement parler, qu'une branche des mathématiques.

This definition seems to be close to the modern concept of treating observations. True, DE CANDOLLE did not mention stochastic considerations and, besides (above), he referred to the numerical method that had nothing in common with probability theory [113, p. 250]. Nonetheless, I am not sure that he really was a proponent of this method; cf. his later (1855) statement [112, p. 332] on the statistical method where he additionally declared how much he loved to discuss the results of calculation.

From 1830 to 1833 DE CANDOLLE published a few more articles and a number of reviews on criminal statistics in the same periodical (*Bibliothèque universelle*), and, in 1834, again in the same journal, appeared his study of a cholera epidemic in Paris. Later on, DE CANDOLLE practically abandoned population statistics and medical statistics when he became the cofounder of the geography of plants, a discipline directly linked with statistical studies. I have largely mentioned him in a biological context [112, pp. 327, 329 – 332, 355] and remarked (Ibidem, p. 345, note 41) that in 1873, in one of his later books, he had devoted a short chapter to the description of the stability of the number of accidents and crimes.

DE CANDOLLE [96] *followed a classical curriculum, and, in 1825, received a bachelor's degree in science.* He then turned to jurisprudence and *earned his doctorate in law in 1829* but, in 1835, he succeeded his father in the chair of botany at Geneva University.

1.3. Quetelet's writings. QUETELET'S statistical writings cover the period from 1826 through 1873. KNAPP [85, p. 342] divided this interval into three stages whose internal boundaries, as he believed, were QUETELET'S books *Sur l'homme* [4] (1836) and *Du système social* [7] (1848). KNAPP (pp. 352 and 358) also maintained that QUETELET achieved nothing after 1836 and merely expanded his work on moral statistics and continued his study and compilation of mortality tables.

Other authors offer somewhat different opinions; all students of QUETELET agree, however, that he exhausted himself rather early and that his later work is of comparatively little interest[9]. The most favourable commentary [121, p. 492] credits QUETELET with creative work up to 1859:

A partir de 1859, les publications de Quetelet consistèrent surtout en réimpressions, compléments ou coordinations d'observations ou de travaux antérieurs.

In turn, I note that

(1) QUETELET worked on mortality tables even after 1859; this aspect of his activities has still not been studied.

(2) As indirectly follows from § 6.1, QUETELET accomplished everything he really could.

(3) In 1859 he was about sixty-three.

1.4. The Purpose of This Paper. My goal is to emphasize the mathematical aspect of QUETELET'S work[10]. Consequently, in describing his efforts in population statistics, I restrict my exposition (a short one at that) to problems in mortality; just the same, in the field of moral statistics I take up little else than crimes. However, I think it desirable to present in full QUETELET'S views on the aims and methodology of statistics and on the preliminary analysis of observations, the more so, since the boundaries of statistics are fuzzy and some of its elements have features of natural science or even of mathematics.

The literature on QUETELET is vast, but I hope to present the opinions held by his more mathematically minded commentators. While referring to him, I only mention his main contributions and, as a rule, I never refer to the *Physique sociale* [10] if the appropriate passage is to be found in its first edition, in the *Sur l'homme* [4]. Again, when quoting QUETELET, I often mention several of his publications.

Within the limits of my subject, I describe QUETELET'S work in more detail than has ever been done before.[11]

2. Statistics

2.1. The Aims of Statistics. In QUETELET'S time statistical data were not yet reliable (§ 2.3), so no wonder he did not reckon statistics among scientific disciplines [6, p. 266]. Elsewhere QUETELET [39, p. iv] stated that

La statistique … est arrivée la dernière dans l'ordre des sciences … toutes les sciences d'observation à leur début … c'étaient des arts … La statistique doit donc entrer … dans la même voie que les sciences d'observation.

QUETELET held a broad view on the aims and methodology of statistics (below) which he had to defend [14, p. 177]:

Cependant pour quelques écoles, la Statistique est encore une science stérile qui se réduit à apprendre ce que les Babiloniens ou les Carthaginois consommaient de boeufs ou de moutons, et quelle était la population que renfermait la fameuse Thébes aux cent portes.

This seems to be an exaggeration, but it is hardly possible to fail to recall, in this connection, the Paris Academy of Sciences (§ l.1) and the London [Royal] Statistical Society. According to MOUAT [94, p. 15], in 1831 – 1833, i.e. somewhat after QUETELET had published the passage just quoted above, the inquiries of the statistical section of the British Association for the Advancement of Sciences were restricted

To facts relating to communities of men which are capable of being expressed by numbers and which promise when sufficiently multiplied to indicate general laws.

(MOUAT quoted an official document.) QUETELET, who was a member of the permanent commission of this section,

Considered this to be too limited … and suggested … the formation of a Statistical Society in London.

The London society was indeed founded in 1834. It declared its refusal to study compiled statistical data, but this restriction was soon forgotten, at least de facto [106, p. 121, note 103; 112, p. 328, note 10].

But what exactly did QUETELET himself advocate? In one of his first declarations [6, p. 432] he denounced the two extreme approaches:

Les une voudraient tout réduire à des nombres et faire consister la science dans un vaste recueil de tableux; d'autres, au contraire, semblent craindre les nombres et ne les regardent que comme donnant des idées incomplètes et superficielles des choses. Ces deux excès seraient également nuisibles.

QUETELET did not offer a clear definition of the aims of statistics (or, what is almost the same, of statistics itself), but his statements on the subject are worth of description.

La statistique, he maintained [6, p. 261], *ne se borne pas à faire une énumération consciencieuse des éléments d'un état … elle peut avec succès porter ses investigations plus loin*.

He continued in the same vein (p. 269) comparing a statistician with an architect rather than with a porter. QUETELET [6, p. 268] argued that various statistical data should be made comparable and combined *de la manière la plus avantageuse*; he even held [2], p. 225] that *le but principal de la statistique* was attained by rendering different materials comparable; cf. § 2.3. Indirectly, QUETELET also included the study of causes (§ 2.4) in statistics and, finally, he believed in improving society by means of statistical studies (§ 3.2); cf. § 4.1.[13]

In particular, QUETELET reasoned on economics. He maintained that statistics should *réuni*[r] *les éléments* that pertain to a state [10, t. 1, p. 101] and to the economic conditions of its population (lbidem, p. 430). He noted that cuts in postal charges both in England (p. 422) and Belgium (Ibidem, t. 2, p. 173) had led to a rise in the number of letters exchanged and in the corresponding profit. While discussing rail fares, he also reasonably argued [6, p. 353] that

Il y a donc un maximum [gain] q*ue l'on peut atteindre et qu'on ne déterminera qu'à l'aide de bons documents statistiques*.

Last but not least, QUETELET [Ibidem, p. 351; 10, t. 1, p. 419] recommended that the changes brought about indirectly by the construction of telegraph lines and railways

Dans les populations des villes, dans les prix des terres, dans les principaux sièges des differentes industries et en général dans toutes les transactions sociales

should be studied.[14]

In 1873 QUETELET [11, Intro.] obliquely compared the *statistique générale* and the international [metric] system of weights and measures with such discoveries as photography, the telegraph, and the steam-engine. QUETELET did not define his term but he mentioned both FOURIER'S *rémarquable ouvrage* [67] (§ 1.1) and *statistique relative aux phénomènes terrestres*. Thus, probably he did after all come to understand the statistical method in a broader sense than before (§ 1.2).

2.2. The Preliminary Analysis of Observations. This analysis is one of the stages in statistical studies. Nowadays research often depends on the treatment of vast amounts of material, and its preliminary analysis which aims to reveal the structures and anomalies in the initial data is extremely important. It was no less essential in QUETELET'S time though mainly for another reason, viz., because of the incompleteness of the data and the large errors in statistical observations.

True, QUETELET [18, p. 330; 4, t. 2, p. 321] did not always think so:

J'ai toujours raisonné, he explained, *dans l'hypothèse que nos résultats étaient basés sur un nombre d'observations si grand qu'il n'entrait plus rien de contingent dans la valeur des moyennes: mais ce n'est point ici le cas*[15].

In § 5.1 I comment on QUETELET' s last words. Later, QUETELET abandoned his optimistic point of view, which did not make any allowance for systematic influences. Even in his *Lettres* [6, p. 322; 113, p. 257, note 28] he admitted that the errors of censuses were much larger than one or two units (men) in 10,000 though he still maintained that the movement of population *atteint à peu près le degré d'exactitude désirable*. Nevertheless, in this same book (p. 332) and even earlier [21, p. 210] he indicated that the registration of deaths was accompanied by systematic mistakes; also see § 2.4.

QUETELET [21, p. 206] repeatedly advocated a critical appraisal of the initial data:

Un saine critique doit présider au choix des matériaux, et les règles de la plus sévère logique doivent en diriger l'emploi: c'est la que commence la science.

He later made a similar pronouncement [10, t. 1, pp. 102 – 103]:

La statistique a la mission d'appreciér la valeur des documents qu'elle rassémble et d'en déduire des conclusions.

QUETELET [9, p. LXV] even contended that

Il convient de ne jamais oublier qu'un document statistique n'est pas un document certain, mais probable seulement; c'est dans l'estimation de cette probabilité que consiste l'importance du résultat que l'on considère, et que réside en général toute l'utilité des calculs statistiques.[16]

No wonder that QUETELET [10, t. 1, p. 112] also remarked on the inanity of applying mathematical corrections:

Feut-on appliquer des corrections mathématiques à des nombres, quand on est persuadé que ces corrections sont dépassées de beaucoup par les erreurs qu'on néglige ?[17]

Apparently, QUETELET was actually thinking about the possible deviations from the *nombres.* Nevertheless, in the same source (p. 267) he referred to an appropriate decision of the International Statistical Congress made in 1867.[18]

QUETELET'S *Lettres* [6] contain many recommendations and remarks on statistical research, such as, for example,

l) *Pour reconnaître les causes variables, le moyen le plus simple est de partager par groupes ou séries les nombres que l'on suppose influencés par elles* (p. 199).[19]

2) Conclusions might well be biased *si l'on est préoccupé d'une idée systématique* (p 285).

3) Questionnaires should only ask for *renseignements absolument nécessaires et qu'on est sûr de pouvoir obtenir* (p. 289).

4) Preliminary investigation should reveal *s'ils présentent des changements brusques* and ascertain their causes (p. 304).

5) The former goal can be achieved by a graphical procedure (p. 305).

6) The initial data should be checked by comparing statistical indicators pertaining to various provinces of a state with one another (pp. 308 – 311).

7) As a rule, sampling should be avoided:

Cette manière d'opérer est très-expéditive, mais elle suppose un rapport [or indicator] *invariable en passant d'un département* [or province] *à un autre* (p. 293).

QUETELET only referred to the simplest procedures of sampling.

8) Too many subdivisions in the statistical data is a *luxe de chiffres*, an *éspèce de charlatanisme scientifique* (p. 278).

QUETELET devoted at least two of his memoirs [20; 25] to the estimation of the reliability of statistical data. In the first of these, taking into account various materials, he calculated anew the population of each of the Belgian provinces and revealed gross errors and even deliberate distortions in official figures. He aimed at establishing proper data for drafting young men into the territorial army. In his second memoir QUETELET brought to light errors in the registration of the movement of population. He thus refuted his own earlier opinion (above).

2.3. Standardization. Standardization of statistical data was one of the problems that QUETELET persistently tackled. In 1846, he remarked with disappointment [116, p. 67, note 32] that different states apparently

Ai[*en*]*t pris plaisir à rendre toute éspèce de rapprochement* [of data] *impossible*.

QUETELET caused several important events at a national and even international level. Thus [121, p. 480], in 1828 he

Réclamait un recensement complet de la population [of Belgium] *qui fut, en effet, décreté en 1828 pour … 1830, et au sujet duquelle le gouvernement consulta à diverses reprises Quetelet*.

In 1841 a state *Commission centrale de statistique* was set up in Belgium with QUETELET as its [permanent] chairman (Ibidem, p. 488)[20] and, in 1853, the first session of the International Statistical Congress was held in Bruxelles (Ibidem, p. 491).

QUETELET actively participated in each session of the Congress. Thus, in a short speech at the London session [55 (1861), pp. 207 – 208] he argued that

La statistique qui n'a pas d'uniformité est une science qui ne saurait avoir les moyens de produire, pour la société, tout le bien qu'elle doit et peut lui faire.

QUETELET was chairman of one of the sections at this session and he reported (pp. 119 – 121) on the need to unify population statistics.

A few years later QUETELET & HEUSCHLING [9] reprinted the former's report at the London session of the Congress (pp. ii – v) and published the *Projet de statistique international* (pp. vii – x) that had been released to the delegates at the session. The work of these authors was the first statistical reference book on the population of Europe (including Russia) and the USA that contained a critical study of the initial data.

The sessions of the International Statistical Congress have paved the way for gradual recognition of the metric system[21]. Note that QUETELET made a direct contribution in this field [121 p. 488]:

En 1839, le gouvernement [belge] *l'envoyait en mission à Paris et en Italie pour constater la conformité des étalons des poids et mesures belges avec ceux de la France*.

Also see § 1.2 where I mentioned that QUETELET took part in standardizing meteorological observations.

2.4. Mortality. Possibly following LAPLACE [107, p. 170], QUETELET [6, pp. 43 – 47; 7, p. 209] set high store by the institution of life insurance. Understandably, he began to study mortality in connection with insurance. Here is the first phrase of one of his early memoirs [37 p. 495]:

L'introduction de sociétés d'assurances sur la vie, dans nos provinces, et le désir de voir se consolider parmi nous ces établissemens qui peuvent devenir si utiles quand ils sont dirigés dans de louables intentions,[22] *nous ont porté à faire des recherches sur les* *lois de la mortalité et à examiner en même temps ce qui concerne les lois des naissances*.

At that time, or at least *encore au commencement de ce siècle*, as QUETELET [36, p. 19] later testified, mortality tables contained *les erreurs les plus apparentes, faites même avec intention.* This fact probably explains why he thought, in 1826 [37, p. 505], that DE MOIVRE'S uniform law of mortality held *sans trop s'éloigner de la vérite*[23].

Throughout his entire scientific life QUETELET compiled [26, 29; 13] and studied [24; 30; 36] the existing mortality tables[24]. Even in 1826 he published [37, pp. 502 – 504] data on mortality in Bruxelles subdividing them into groups by sex and age.

A few years later, he compiled separate mortality tables for Belgians of each sex [2, pp. 36 – 40] noting (p. 33) that

Ce n'est même que dans ces derniers temps que l'on a commencé à introduire dans les tables de mortalité la distinction des sexes.

HANKINS [80, p. 54] quite reasonably argued that QUETELET'S tables were *of great practical value in his own country*.

In a number of writings as, for example, in his book [4] QUETELET attempted to study such factors of mortality as the price of bread, estimated infant mortality[25] and the mortality of institutionalized populations.[26] He also discussed meteorological factors, viz., the air temperature [37, p. 501] and seasonal or monthly influences [2, p. 70; 4, t. l, p. 188; 42] and he formulated his conclusions for separate age-groups. QUETELET [42] compiled materials sufficient to draw up a summary table of monthly deaths related to an extensive set of conditions, but he did not present such a table; see, however, the end of § 4.2, where I mention instances of QUETELET'S multivariate tabulations.

QUETELET partly devoted one of his memoirs [21] to a general discussion of the constant and variable (in particular, periodic) factors of mortality. Elsewhere, he considered its natural and perturbative causes (§ 5.5).

3. L'homme Moyen

3.1. Social Physics. Population statistics originated in the works of PETTY and GRAUNT [109, §§ 2.4.2 – 2.4.3].While introducing the term *political arithmetic*, PETTY (Ibidem, p. 218) apparently thought that this discipline should examine a given state, first of all, its population, from a social and economic viewpoint by means of statistical data.

In 1794 CONDORCET [113, p. 258, note 29] proposed a new term, *mathématique sociale*, which did not, however, take root. Then, about 1823, COMTE [52, p. 4] offered the term *physique sociale,* but he did not elaborate on its meaning. This critical comment is due to SARTON [102, p. 237] who also quoted a passage from COMTE'S letter dated 1824 [53, p. 127]. Judging by this source, COMTE'S social physics was not connected with statistics:

Tu trouveras [in my work] *l'explication des contradictions et des anomalies apparentes que cette marche* [*historique de l'esprit humain*] *présente à celui qui ce borne a un apercu superficiel. Je crois* *que je parviendrai à faire sentir, par le fait même, qu'il y a des lois aussi déterminées pour le développement de l'éspèce humaine que pour la chute d'une pierre*.

In turn, POISSON [110, pp. 296 – 297] denoted demography, medical statistics and actuarial science by a single term, *arithmétique sociale*. Finally, QUETELET [40, p. 4; 41, p. 2] called the science of studying *l'homme moyen*, whom he himself introduced (§ 3.2), *mécanique sociale*. Later, however, he introduced another expression, *physique sociale* (below).

COMTE [52, p. 4] alleged that QUETELET had plagiarized him:

Cette expression et celle, non moins indispensable, de philosophie positive, ont été construites, il y a dix-sept ans, dans mes premiers travaux de philosophie politique. … Quoique aussi récents, ces deux termes essentiels ont déjà été en quelque sorte gâtés par les vicieuses tentatives d'appropriation de divers écrivains, qui n'en avaient nullement compris la vraie destination, malgré que j'en eusse, dès l'origine, par un usage scrupuleusement invariable soigneusement caractérisé l'acception fondamentale. Je dois surtout signaler cet abus, à l'égard de la première dénomination, chez un savant belge qui l'a adoptée, dans ces dernières années, comme titre d'un ouvrage où il s'agit tout au plus de simple statistique.

LOTTIN [91, p. 360] and, later, LAZARSFELD [88, p. 235, note 52] contrasted COMTE with QUETELET. Thus, the latter argued that

Comte was trying to derive from history broad developmental trends which could be projected into the future, while Quetelet was bent on finding precise regularities which would help to explain the contemporary social scene.[27]

This opinion is not really correct; cf. §§ 2.1 and 3.2. LOTTIN (l. c.) also remarked that COMTE had denied the use of mathematical, and, especially, of stochastic methods in his social physics.

Now, what did QUETELET mean by social physics ? At first [6, p. 263] he argued that it was *une science à part* formed by an *ensemble de … lois* that governs the *corps social*. Later [7, p. 234], he added that

Le Corps social a son anatomie ..., qu'on a désignée improprement sous le nom de statistique.[28]

Finally, QUETELET [10, t. l, p. 152] maintained that *la science* (on p. 150 he mentioned social physics) should, if possible, study the laws of reproduction and the growth of man, of the development of his intellect and of his inclinations to good and bad; inquire into the influence of the natural and perturbative causes (§ 5.5) on man; and examine whether man can *compromettre la stabilité du système social*.

This immense programme contemplated the solutions of problems in medicine proper, public hygiene, human physiology, jurisprudence, ecology and history!

For the most part, nevertheless, QUETELET'S works pertain to demography and moral statistics and include elements of medical statistics and general, often naive and unsubstantiated sociological speculations.

3.2. L'homme Moyen. As MEITZEN [92, p. 54] remarked, QUETELET had held

Fast mystische Hoffnungen Gesetze der Weltordnung und der Weltgeschichte aus den statistischen Zahlen aufzufinden and had attempted *dies Ziel zum Prinzip der Statistik zu machen*.

More precisely, QUETELET hoped to establish these laws, and to solve the other problems of social physics (§ 3.1) by determining the Average man[29] peculiar to each epoch. He introduced that notion in 1832 [40, p. 4], though at first he did not use the term itself:

L'homme que je considère ici est dans la société l'analogue du centre de gravité dans les corps; il est la moyenne autour de laquelle oscillent les élémens sociaux. Ce sera … un être fictiv.

Elsewhere, again in 1832, QUETELET [41, p. 1] added:

Si l'homme moyen était déterminé pour une nation, il presenterait le type de cette nation.

Moreover, in principle the appropriate Average man is l*e type de l'éspèce humaine tout entière*.

QUETELET [7, p. 38] believed that the Average man possessed average features in everything, viz., that he had a mean weight and height, average moral and intellectual qualities and that, at the same time, he was the *type de notre éspèce, et aussi le type de la beauté*.

Such utterances, not to mention QUETELET'S idea of applying the Average man to the study of society (below), gave rise to reasonable objections. In this connection LANDAU & LAZARSFELD [87, p. 832] mention COURNOT [56, p. 143], L. A. BERTILL0N [45, p. 295] and FRÉCHET (§ 6.1). COURNOT contended that

Lorsqu'on applique la détermination des moyennes aux diverses parties d'un système compliqué, il faut bien prendre garde que ces valeurs moyennes peuvent ne pas ce convenir … l'état du système … serait un état impossible.

For his part, BERT1LLON insisted that the Average man was the *type de la vulgarité*. It was BERTRAND [46, p. XLIII], however, who förmulated the most scathing criticism:

Dans le corps de l'homme moyen, l'auteur belge place une âme moyenne. L'homme type sera donc sans passions et sans vices [this is wrong; see § 4.5], *ni fou ni sage, ni ignorant ni savant, souvent assoupi: c'est la moyenne entre la veille et le sommeil; ne répondant ni oui ni non; médiocre en tout. Aprés avoir mangé pendant trente-huit ans la ration moyenne d'un soldat bien portant, il mourrait, non de vieillesse, mais d'une maladie moyenne que la Statistique révélerait pour lui*.

QUETELET [7, pp. 35 and 37] attempted to refute COURNOT'S pernicious argument. He took the body measurements of 30 men whom he divided into three equal groups in such a way that the mean heights taken over the groups were the same. QUETELET stated that the means of every other body measurement also coincided and that he corroborated this fact by extending his experiment to other groups of men. Thus, he concluded (p, 37), the author of an *ouvrage remarquable, publié récemment sur la théorie des probabilités*, had been mistaken. QUETELET did not publish his measurements and, besides, his experiment did not *meet the issue* [80, p. 71].

I shall now try to ascertain what kind of a mean determined the Average man. Since he compared his *homme moyen* with a certain centre of gravity (above), QUETELET apparently thought about the arithmetic mean. But then, witness his reasoning [7, p. 45] on the height and weight of the Average man:

La courbe qui indique la manière dont la population [more precisely: people of the same sex pertaining to a certain age-group] *se trouverait groupée quant aux poids, n'aurait plus la symétrie de celle qui se rapporte aux tailles, mais elle serait encore régulière* [?] *et se calculerait* [in the stochastic sense] *d'après les mêmes principes. Les groupes se trouveraient encore distribués d'aprés la loi des variations accidentelles* [see § 5.4] *mais en admettant deux limites inégalement distantes de la moyenne … L'homme moyen pour le poids compterait probablement autant d'hommes plus pesants que lui, que d'autres qui le seraient moins*.

And, further (p. 46): *L'homme moyen serait donc à la fois un type pour la taille et pour le poids*. … Thus, with regard to weight, the *moyen* was, at the same time, the arithmetic mean and, *probablement*, the median, while, as far as the height was concerned, QUETELET believed without any reservations at all that these means coincided.**[30]**

At least in one instance while discussing the height of man, QUETELET [6, p. 216] referred to the POISSON generalization of the law of large numbers. He pointed out that the throws of several coins do not stochastically differ from the throws of a single coin.**[31]** This fact, he concluded, constituted

La première preuve mathématique qu'il existe véritablement un homme moyen, un homme type, du moins quant à la taille.

Regarding height, STIGLER [118, p. 338] maintains that *l'homme median would be more accurate*. Although QUETELET did not directly connect his appropriate example with the Average man, in essence STIGLER'S remark is correct.

But at least those mean indicators that QUETELET derived for use in moral statistics (§ 4.5) were arithmetic means rather than medians and he related them to the Average man!

Besides this reference to the *homme moyen* in moral statistics QUETELET [4, t. 2, p. 286] thought it possible, in any case *dans certaines circonstances*, to identify the laws of the development of the Average man with those pertaining to the entire human race. Obviously, he wanted to facilitate the aims of historical science, but his hopes proved futile.

GALTON [74, p. 350; 95, vol. 2, p. 297] later mentioned the Average man when introducing his composite portraits of criminals, men of certain professions or nationalities. Apparently, he did not know about GUERRY'S plans for isolating types of maniacal, idiotic, epileptic, etc. people by means of anthropometry.

QUETELET [41, pp. 83 – 87] published a letter from GUERRY which mentioned this idea and which asked rhetorically (p. 87): *Que sait ce que nous rencontrerons*?**[32]**

4. Moral Statistics

QUETELET'S deliberations in the field of moral statistics mainly pertained to the stability of the [relative] numbers of crimes, suicides, and marriages. I dwell almost exclusive on the first subject.

4.1. The Budget of Crimes. Even in 1829 QUETELET [39, pp. 28 and 35; 15, p. 178] became convinced that the [relative] number of crimes is almost constant. He then specified his idea [41, p. 81; 3, pp. 5 – 6; 4, t. l, p. 10; 6, p. 357]. Here is the famous passage from one of these sources [4, t. l]:

Il est un budget qu'on paie avec une régularité effrayante, c'est celui des prisons, des bagnes et des échafauds … et, chaque année, les nombres sont venus confirmer mes prévisions, à tel point, que j'aurais pu dire, peut-etre avec plus d'exactitude: Il est un tribut que l'homme acquitte avec plus de régularité que celui qu'il doit à la nature ou au trésor dé l'Etat, c'est celui qu'il paie au crime! … Nous pouvons énumérer d'avance combien [crimes of each kind will be committed] *à peu près comme on peut énumérer d'avance les naissances et les décès qui doivent avoir lieu*.

La société renferme en elle les germes de tous les crimes qui vont se comméttre, en même temps que les facilités nécessaires à leur développement. C'est elle, en quelque sorte, qui prépare ces crimes, et le coupable n'est que l'instrument qui les exécute. Tout état social suppose donc un certain nombre et un certain ordre de délits qui résultent comme conséquence nécessaire de son organisation.

Referring to VILLERME who, in turn, had mentioned NAPOLEON, QUETELET [41, p. 18; 4, t. 2, p. 173] maintained that man

Est autant le produit de son atmosphère physique et morale que son organisation.

He even thought [6, p. 358; 10, t. l, p 425] that there existed a certain minimal number of crimes which depended

De l'organisation intime de l'homme, et l'excédant est en quelque sorte le produit de l'organisation sociale.

A few years after QUETELET died, REHNISCH [99] 1evelled an annihilating criticism against his statements on the budget of crimes:[33]

Sehen wir also zu, he warned his readers significantly (p. 47), *wie es um diese wundersame Constanz und Regelmäßigkeit in einer Reihe viel celebrirter Parade-Beispiele derselben in Wahrheit bestellt ist*.

QUETELET, REHNISCH (p. 52) continued, had convinced himself in the constancy of crimes (and suicides) after studying official French figures pertaining only to three years:

Da war es doch wahrlich reichlich früh und Quetelet unmotivirt rasch bei der Hand mit der Behauptung von der grausen erregenden Exactitüde in der Wiederkehr der Zahlen.

And, further (p. 53), these data could only have served as a *Fingerzeig für weitere Forschung*[34].

REHNISCH analysed QUETELET'S figures. It turned out (p. 101) that QUETELET made mistakes when comparing French data for different years,[35] in particular (p. 61) since he did not account for the consequences of a law passed in 1832 (below)[36]. REHNISCH quoted a few passages from the French annual reports [69] and I feel myself obliged to do the same:

1) *L'augmentation … dans le nombre des accusés de crimes contre les personnes* [since 1825] *s'est manifestée notamment parmi les accusés de viol* [and others].

The number of accused of a certain type of crime *a augmenté progressivement de 135 pour 100* [69 (pour 1842), p. vi].

2) The decrease in the number of those condemned to capital punishment from 1831 to 1835 and from 1836 to 1840 compared with the period from 1826 to 1830, as stated on the same page of the report for 1842, was brought about by the law *du 28 avril 1832* in the absence of which the *nombre des faits que le Code pénal de 1810 qualifiait meurtres* would have undergone *une augmentation sensible*.

3) *Le nombre des suicides n'a pas cessé de s'accroitre, chaque année, depuis que la statistique criminelle le constate; mais en 1847 1'augmentation dépasse beaucoup celle que présentaient les années précédentes … en 1847 on eu compte 545 de plus* [suicides] *qu'en 1846, un sixième environ* [69 (pour 1847), p. xxxviii].

The above figures are absolute rather than relative; nonetheless, it is easy to understand why REHNISCH (p. 60) called QUETELET'S writings *Ein Literaten-Mach-werk oberflächlichter Art*.

None, at least in mathematical literature, ever referred to REHNISCH'S memoir. True, CHUPROV [50, pp. 213 – 215] was an exception, but even he only admitted (p. 215) that *it was in Quetelet's nature to err*[37]. CHUPROV pointed out that neither QUETELET nor REHNISCH had introduced any quantitative test for the stability of statistical series.

Granted. QUETELET, however, did have means of checking whether crime really was as regular as births and deaths (above). Denote relative yearly crime statistics by $x_1, x_2, \ldots, x_n$, calculate the mean of these numbers $\bar{x}$ and deviations $\Delta_i = (x_i - \bar{x})/\bar{x}$, $i = 1, 2, \ldots, n$ and take the mean square (say) value of Δ_i as a measure of stability of the series. Repeat this procedure for a series of births or deaths and compare the measures. Q. E. D.

The man who really initiated the study of the stability of statistical series was LEXIS [90]. He began by isolating various types of series according to their general behaviour. (This goes to show that his approach was indeed sound.) In connection with the stability of series LEXIS did not refer to QUETELET.

In 1924, at last, RIETZ [107, p. 181] remarked that QUETELET'S

Somewhat sensational language … caught the imagination; but … he often asserted the existence of stability on insufficient evidence. The activity of Quetelet cast upon statistics a suspicion of quackery.

QUETELET assumed that the ratio of the number of known crimes to that of all the crimes perpetrated is constant [41, p. 19; 3, p. 12; 4, t. 2, p. 174]. He understood, of course, that this ratio was constant only for a given type of crime, that felonies come to light relatively more often than misdemeanours, and that social and legal changes bring about serious variations in criminal statistics [6, p. 325].

4.2. The Causes and the Factors of Crime. Adopting an approach based upon common sense, QUETELET [3, p. 32; 41, pp. 44 and 47; 4, t. 2, p. 210] isolated the main causes of crime, for example [3],[38] *la misère, l'oisiveté et l'ignorance* and also warned against a special cause (imitation) that might arise when the Press describes a certain crime in an irresponsible way [7, p. 214].[39]

Naturally enough, QUETELET was compelled to study the influence of external factors on crime rather than its intrinsic causes; thus, he examined the effects of age, sex, the season of the year and education (not misery, idleness and ignorance; see above).

QUETELET even offered an empirical formula for the change in inclination to crime with age (§ 5.7) and portrayed a picture of a criminal passing from one type of crime to another to yet another as his age changed [41, p. 70; 4, t. 2, p 248].[40]

QUETELET [6, p. 317] also gave a good example of spurious correlation:

Le nombre des crimes est plus généralement en rapport direct avec le nombre des enfants envoyés aux écoles, qu'en rapport inverse.

He explained this fact by reasonably assuming (p. 320) that the number of school children was an indicator of crowding in the area concerned.

LANDAU & LAZARSFELD [87, p. 830] noted QUETELET'S attempts [10] to go far beyond simple two-variable correlations[41]. Thus, QUETELET subdivided crimes of separate types according to the sex (or, in another instance, the age) and education of the perpetrators; in his second example he also took into account the type of law-courts which considered the cases. The authors conclude:

These remarkable anticipations of modern techniques went largely unnoticed by Quetelet's contemporaries, and only in recent times have social scientists rediscovered and fully explored the possibilities of multivariate analysis.

It is possible to mention in this connection QUETELET'S earlier contribution [41, p. 66], but then FOURIER [67] had published such tables back m 1821. Witness, for example, his Table 20, this being a *Tableau des décès* [in 1817] *avec distinction d'âge, de sexe et d'état de mariage*. Even the work of some botanists of the 18th century on the classification of plants may be considered from the viewpoint of multivariate statistics [112, pp. 325 – 326].

4.3. The Jury. QUETELET [3, p. 18] noticed that the rate of conviction

En France a été beaucoup moins grande que chez nous: ce qui cessera sans doute d'avoir lieu, du moins d'une manière aussi prononcée, depuis que le jury a été rétabli chez nous.

He voiced similar considerations elsewhere [39, pp. 28 – 29; 41, p. 25; 3, pp. 21 and 27 – 28].

QUETELET [18, p. 331; 4, t. 2, p. 33] also stated that a law that had *appellé un plus grand nombre de citoyens à en faire le service* of jury members in France had resulted in less severe sentences. Finally, he remarked [6, p. 334] that the percentage of acquittals ln Belgium had doubled since the jury system was established ln that country.

4.4. The Probabilities of Conviction. Drawing on French materials, QUETELET studied how the defendant's personality determined the probability of his conviction. He presented his calculations in a table which I reproduce here (Table 1, excluded from translation). The table did not altogether correspond to its title; indeed, lines 3 and 12 were not (directly) connected with the personality of the accused. The type of crime imputed is another factor that influences the rate of conviction, as QUETELET himself admitted elsewhere [10, t. 1, p. 263], and as indirectly witnessed by his lines 3 and 12 in the table, there certainly was a correlation between this factor and the personality of the accused. QUETELET had no means to allow for the correlation,[42] but he could have pointed out that his subject of inquiry was much more complicated than it appeared to be.

QUETELET'S table had one more fault.

Especially surprising, LANDAU & LAZARSFELD [87, p. 831] remark, *is the fact that the idea of analysing* <u>*repeated offenders*</u> *completely eluded him. It seems like that these shortcomings*[43] … *were due* … *at least in part to the inadequacies of the data available at the time*.

Their conclusion is not really convincing. ALPH. DE CANDOLLE [59, pp. 182 – 184] dwelt on this subject and GUERRY [79, pp. 17 and 44] formulated some relevant inferences, which he based on statistical material. Finally, QUETELET did not estimate the plausibility of his conclusions; cf. § 5.6. But it is still necessary to add that he was able to determine that the personality of the accused (his education in particular) had a pronounced effect on the probability of his conviction. About 1870, physicians [113, p. 263] came to use regular changes of indicators to ascertain medical findings in the same way as QUETELET had done in the field of moral statistics.

4.5. The Inclination to Crime. QUETELET [41, p. 17; 4, t. 2, p. 171] introduced the concept of inclination to crime:

En supposant les hommés placés dans des circonstances semblables, je nomme penchant au crime, la probabilité ... de commettre un crime.

He went on to explain that *similar circumstances* should be understood as being

Égalemént favorables, soit par l'existence d'objets propres à exciter la tentation, soit par la facilité de commettre le crime.

Thus, QUETELET did not mention the real (or, at least, all the real) causes of crime (§ 4.2) and, furthermore, crimes against the person obviously did not come under his definition. Elsewhere [7, p. 82; 43, p. 20, 10, t. 2, p. 327] he discussed the *penchant apparent au crime* calculated statistically for the appropriate age-group. He did not fail to point out that the *penchant réel* of a given person might well differ considerably from the apparent tendency. Specifying this point, QUETELET [7, p. 93 10, t. 2, p. 333] contended that a certain number of people of a given sex and age corresponded to each possible value of the probability to commit a crime. He illustrated his idea by drawing an asymmetric curve with a single peak. The behaviour of the curve was obviously determined only by common sense.

QUETELET [7, p. 77; 43, p. 38] also introduced similar notions of apparent and real inclinations to marriage. He [41, pp. 17 – 20; 7, p. 92] related the [mean, or the apparent] inclination to crime (and to marriage [7, p. 91] to the Average man.

People, even belonging to the same sex and age-group, never find themselves in *circonstances semblables* (see QUETELET'S definition above). Nonetheless, the notion of the apparent inclination to crime seems as reasonable as the idea of the mean duration of life.[45]

Without paying attention to the difference between apparent and real inclinations to crime, QUETELET'S opponents gave a hostile reception to his ideas. RÜMELIN [100, p. 25] formulated his ill-founded resentment most effectively:

Wenn mir die Statistik sagt, dass ich im Laufe des nächsten Jahres mit einer Wahrscheinlichkeit von 1 zu 49 sterben, mit einer noch größeren Wahrscheinlichkeit schmerzliche Lücken in dem Kreis mir theurer Personen zu beklagen haben werde, so muss ich mich unter den Ernst dieser Wahrheit in Demuth beugen; wenn sie aber, auf ähnliche Durchschnittszahlen gestützt, mir sagen wollte, dass mit einer Wahrscheinlichkeit von 1 zu so und so viel [I shall commit a crime] *so dürfte ich ihr unbedenklich antworten: ne Sutor ultra crepidam*! [Cobbler! Stick to your last!]

Later RÜMELIN [101, p. 370] added;

Nur so unphilosophische Köpfe wie Ad. Quetelet und Thomas Buckle … konnten der nun viel verbreiteten Lehre das Wort reden, dass die Thatsachen der Moralstatistik zur Läugnung der menschlichen Willensfreiheit führen müssen.

Leaving BUCKLE aside, I note that QUETELET [23, p. 145; 43, pp. 5 and 38; 7, pp. ix and 65 – 72] did not deny man's free will, but he reasonably assumed that it acts like an insignificant random cause and is not therefore appreciable in mean values.[46]

In turn, KNAPP [86, p. 101] argued that, since each specific crime has its own cause, *darf man von keinem Hang zum Verbrechen reden*. In essence, he spoke out against statistics as such.

Finally, I quote CHUPROV [50, p. 23] whose opinion neatly summed up the arguments of QUETELET'S followers and opponents:

Their [he meant QUETELET'S worshippers *zealous beyond reasoning*] *naïve admiration for <u>statistical laws</u>; their idolizing of stable statistical figures; and their absurd teaching that regarded everyone as possessing the same <u>mean inclinations</u> to crime, suicide, and marriage, undoubtedly provoked protests. Regrettably, however, the protests were hardly made in a scientific manner*.

4.6. Marriages. QUETELET introduced the concepts of apparent and real inclinations to marriage (§ 4.5). He emphasized that since the data on marriages are stable, the action of the free will of man as manifested in these events is insignificant (Ibidem). In particular, he argued [7, pp. 68 – 69; 28, p. 455; 33, p. 232] that there were evident regularities in the ages of the bridegroom and the bride. Marriages contracted in Belgium between men younger than thirty and women older than sixty presented a striking example of such regularities [23, p. 143]: from 1841 to 1845 there were 7, 6, 8, 5, and 5 such marriages per year (total number, 31).[48]

When publishing his *law of small numbers*, BORTKIEWICZ [47] offered examples of rare events pertaining to the statistics for suicides and fatal accidents, but he did not mention marriages at all. The study of data on children born out of wedlock [21, p. 220] could have been one of the reasons why QUETELET became interested in the statistics of marriages. In a later contribution he [7, p. 169] remarked that

En Bavière, on a cherché à mettre obstacle à des mariages inconsidérés. As a result, he continued, *on trouve … que le nombre* *des enfants illégitimes* [in that country] *y est presque égal à celui des enfants légitimes*.

The birth of illegitimate children, QUETELET (p. 204) argued, testified to *une véritable plaie sociale*[49].

5. Elements of Probability Theory and Mathematical Statistics

QUETELET published a few popular books devoted in part or entirely to the theory of probability. He also expressed himself on the relations between statistics and the theory and, in his practical activities, he had to touch on the elements of mathematical statistics which did not then exist as a separate discipline. I describe these elements in §§ 5.2 – 5.7. In addition, the preliminary analysis of observations (§ 2.2) also pertains in part to theoretical (not to mathematical) statistics. Finally, in § 4.2 I mentioned multivariate statistics.

5.1. The Theory of Probability. The French scientists with whom QUETELET met in his younger years (§ 1.2) undoubtedly impressed on him the need to corroborate statistical inferences by stochastic reasoning. At any rate, in 1869 QUETELET [10, t. l, p. 103] quoted a letter from FOURIER written *il y a près d'un demi-siècle* ago.

Les sciences statistiques, FOURIER maintained, *ne feront de véritables progrès*, *que lorsqu'elles seront confiées à ceux qui ont approfondi les théories mathématiques*.[50]

QUETELET (Ibidem) also referred to POISSON who

Exprimait parfois dans sa correspondance [with the former] *avec une sévérité narquoise et peu rassurante pour les statisticiens qui prétendaient substituer leurs fantasies aux véritables principes de la science*.

QUETELET [116, p. 66, note 28] also contended that at the beginning of the 19th century scholars had felt it necessary to apply probability theory directly *aux affaires gouvernementales* [to population statistics], but that [10, t. l, p. 107] some difficulties

Les empêchaient de marcher avec l'activité nécessaire. Je pense, QUETELET continued, *pouvoir en attribuer la principale cause au manque de renseignements authentiques*[51]. Finally, QUETELET (Ibidem, p 134) correctly held that

La théorie des probabilités prit naissance presque en même temps que la statistique, sa soeur puînée; dont elle devait devenir la compagne la plus sûre et la plus indispensable. Cette concordance n'est point accidentelle, mais l'une de ces sciences interroge en quelque sorte par ses calculs et coordonne ce que l'autre obtient par ses observations et ses expériences.

In a more general sense, QUETELET argued that

Le calcul des probabilités n'est que l'instrument qui doit servir à régulariser les travaux d'exploitation … [6, p. 7, 8, p. 10][52] and that

La théorie des probabilités sert de base à l'étude des lois naturelles [10, t. l, p. 133].[53]

I do not know whom QUETELET thought of when he indicated [35, p. 633] that

Peu à peu les difficultés des méthodes [used in probability theory and its applications] *et l'on peut dire les abus des calculs, mal dirigés dans les affaires commerciales et politiques, ralentit le zèle des* *prétendus savants; le plus grand nombre de ces faux apôtres de la science finit par sentir son impuissance et sembla s'éteindre successivement*. And, further (p. 634):

La science des probabilités devrait se faire un devoir de reprendre de jour en jour sa place que l'on a usurpée, en méconnaissant tous ses mérites.

QUETELET made many a nice pronouncement on the theory of probability, but used it rather seldom, partially because of gross errors in statistical data (§ 2.2). It is possible, however, that while in Paris he had no time to become sufficiently acquainted with the theory. About a year before he first went there, FOURIER had published the initial volume of the *Recherches statistiques* [67] and I suspect that it was precisely this study rather than LAPLACE'S work that caught QUETELET'S attention.[54] Thus, in § 2.2 I have quoted QUETELET'S pronouncement where he admitted that in a certain case the number of observations was inadequate. His example pertained to the probability of convicting a white-collar defendant (§ 4.4), and he restricted himself to remarking that (I paraphrase) the plausibility of a statistical mean is proportional to the square root of the corresponding number of observations. QUETELET did not say that it was possible to estimate the absolute plausibility and that LAPLACE [107, pp. 157 – 158] had made such estimations. See note 17, however.

Nevertheless, QUETELET attempted to popularize the theory of probability. Drawing on the lectures[55] that he had given

Depuis plusieurs [five] *années au Musée de Bruxelles pour servir d'introduction à mes* [for his] *cours de physique et d'astronomie,* he wrote his first book [1] on the subject. In this work QUETELET described JAKOB BERNOULLI'S law of large numbers, explained the notion of moral expectation[56] and examined the credibility of testimonies. The exposition was indeed elementary; for example, QUETELET did not give BERNOULLI'S formulas on the law of large numbers and did not say a single word about the LAPLACEAN forms of the central limit theorem.

Two other popular books [6; 8] were also elementary.[57]

5.2. The Theory of Errors. A few decades ago the theory of errors found itself incorporated into mathematical statistics[58]. Before that time there was a gap between the two disciplines.

QUETELET was hardly acquainted with GAUSS'S *Theoria combinationis* [76] where (in § 5) the latter substantiated the method of least squares by the principle of least variance (or least mean square error)[59] and estimated the plausibility of observations by the inverse of the appropriate variance. For his part, QUETELET (true, just like astronomers and geodesists almost to our time [111, p. 39; 114, p. 177, No. 85]) preferred to attain the second goal by using the probable error. More precisely, QUETELET [8, p. 53] believed that the probable error was, practically speaking, more important than the *erreur moyenne*, as he called the mean square error.

QUETELET also gave some additional thought for estimating the plausibility of observations, but he did not put his recommendations into practice. Here are his ideas

1) Denote observations by x_i i= 1, 2, ..., n, and their mean by $\bar{x}$. Then, as QUETELET [l, p. 254] asserted without proof, the *degré d'approximation du résultat moyen* will be represented by formula

$$g = \frac{1}{\sqrt{n}}\sqrt{2(\frac{x_1^2 + x_2^2 + ... + x_n^2}{n} - \bar{x}^2)}. \qquad (1)$$

QUETELET, however, did not give any formula. He did no more than offer a numerical example, and a lame one at that. He assumed that a thousand observations give 2 ($x_1 = x_2 = \ldots = x_{1000} = 2$), two thousand are equal to 5 and one thousand more observations are equal to 12. QUETELET borrowed both the example and the formula from FOURIER [68, p. 532] and referred [l, p. iv] to the latter, but I am sure that even FOURIER would have failed to determine a reasonable density law for the errors of these invented observations.

QUETELET (p. 151) asserted that he calculated g in accordance with the *règle des moindres carrés*; this was, of course, an obvious mistake.[60]

FOURIER (p. 541) also gave an equivalent expression for g:

$$g = \frac{1}{n}\sqrt{2[(x_1 - \bar{x})^2 + (x_2 - \bar{x})^2 + ... + (x_n - \bar{x})^2]}.$$

The mean square error (i.e., the sample variance) of $\bar{x}$ is

$$m = \sqrt{\frac{(x_1 - \bar{x})^2 + (x_2 - \bar{x})^2 + ... + (x_n - \bar{x})^2}{n(n-1)}}$$

so that $g \sim m\sqrt{2}$[61].

2) QUETELET [6, pp. 398 – 399] maintained, again without proof, that the relation between the probable (r) and the *moyenne* (w) errors was

$$w = 0.59147r$$

(notation changed) and that in case of a finite number of observations (n) the probable error had values

$$r(1 \pm \frac{0.47694}{\sqrt{n}}). \qquad (2)$$

(QUETELET wrote this formula incorrectly.) He tacitly assumed that the errors of observations were normally distributed, so that

$$w \equiv \int_0^\infty x\varphi(x)dx = \frac{h}{\sqrt{\pi}}\int_0^\infty x\exp(-h^2x^2)dx,$$

and his formula (2) under the same assumption determined the probable limits of the most probable value of *r* [75, §§ 3 and 6]. We should remember, as has been stated above, that QUETELET later used the same term (*moyenne*) to denote the mean square error.

GAUSS preferred to use the mean square error (m) rather than the mean error. Assuming normal distribution, he got [76, §§ 11 and 15] the expression $m^2\sqrt{2/n}$ for the mean square error of m^2, so that the mean square error itself was contained within the limits $m(1 \pm 1/\sqrt{2n})$.

LAMBERT [104, p. 254] had invented the term *theory of errors* (in German) but neither Laplace nor Gauss (nor Quetelet) ever used it, but Bessel did. It was introduced into the *Fachliteratur* in the mid-19th century. Thus, FISCHER [66] called the first *Abschnitt* of his book *Die Theorie der Beobachtungsfehler*. In 1853 LIAGRE and in 1861 AIRY used this term in the titles of their books, the former in French and the latter in English.

5.3. The Theory of Means. QUETELET [6, p. 60] believed that a certain branch of probability theory, the *théorie des moyennes, sert la base à toutes les sciences d'observation*.**62**

Beginning with LAMBERT [104, p. 250], mathematicians and astronomers had endeavoured to determine the *best* mean for observations, but in the mid-19th century natural scientists who aimed at studying the mean states of nature [112, p. 330] had to go beyond the limits of the theory of errors. QUETELET did have grounds for paying particular attention to the theory of means. Note that while mentioning ARISTOTLE and ARCHIMEDES in connection with the theory of means, QUETELET [6, pp. 61 – 62; 8, pp. 48 – 49] did not refer to error theory at all and that CONDORCET [54, p. 183] had considered the former independently of probability theory (also see below).

In 1830, while discussing problems in the theory of errors, HAUBER [81, p. 33] used the term *Theorie der mittleren Werthe* and in 1850 HUMBOLDT [116, p. 68, note 36] mentioned *die einzig entsheidende Methode, die der Mittelzahlen*. Writing in 1876, L. A. BERTILLON called one of his contributions [45] *La théorie des' moyennes en statistique.* Even in 1857, however, DAVIDOV published a popular report, *The theory of mean quantities* [58]; also see note 63. This is what QUETELET [6, pp. 63 and 65] had to say with regard to the theory of means:

1) *La plupart des observateurs, les meilleurs même, ne connaissent que trés-vaguement, je ne dirai pas la théorie analytique des probabilités, mais la partie de cette théorie qui concerne l'appréciation des moyennes*.

2) *En prenant une moyenne, an peut avoir en vue deux choses bien différentes: an peut chercher à déterminer un nombre qui existe véritablement; ou bien à calculer un nombre qui donne l'idée le plus rapprochée possible de plusieurs quantités différentes, exprimant des choses homogénes, mais variables de grandeur*.

And further (p. 67): *Cette distinction est si importante, que … je réserverai le nom de moyenne pour le premier cas, et 1'adopterai celui de moyenne arithmétique pour le second*.

Nevertheless, QUETELET added, sometimes the arithmetic mean is also *une véritable moyenne*. He repeated this pronouncement elsewhere [8, p. 49; 35, pp. 627 – 628].

LAMONT (in 1867) and KÖPPEN (in 1874) noted [116, pp. 70 – 72] that some means were abstract quantities whereas even in 1857 DAVIDOV (Ibidem), on the contrary, stressed the universality of the methods of treating observations. The difference between the means, he contended, was significant only insofar as it led to differing properties of the deviations from them. None of those three authors mentioned QUETELET[63].

The theory of means had no new tools or methods as compared with the theory of errors. However, in considering averages which had no direct prototypes in the real world, it came closer to the aims and spirit of mathematical statistics. But why did the term *theory of errors* supersede the other one (cf. § 5.2)? This presumably happened because in those days statistical terms had no chance of competing with astronomical expressions.

BRU [56, p. 324] noted that even CONDORCET [54, p. 107] had distinguished between the *deux éspèces de valeurs moyennes*. Nevertheless, CONDORCET did not contrast the two means as distinctly as QUETELET was to do. Incidentally, QUETELET [6, p. 67; 8, p. 51] borrowed one of CONDORCET'S examples, viz., the calculation of the mean duration of life.

5.4. The Laws of Distributions. A transition from the study of mean values to the determination of distributions can be revealed in the history of the statistical method as applied to natural science [115, § 8; 116, § 1]. With regard to statistics proper, COURNOT [56, § 107], while listing the aims of this discipline as formulated from the viewpoint of probability theory, mentioned the determination of the

Loi de probabilité des valeurs, en nombre infini, qu'une quantité variable est susceptible de prendre, sous l'influence des causes fortuites[64].

However, the study of distributions did not begin in earnest before GALTON. According to H1LTS [83, p. 208],

Galton … was led to focus his attention upon statistical deviations and variations as something important in their own right; … with the works of Quetelet, Galton, Lexis, Edgeworth, and finally Karl Pearson, the focus of attention shifted from … the parameter to the distribution.

In principle, this is indeed true; however, I shall add a few words about QUETELET. First, during a certain period of time he was interested in various distributions and at least described the behaviour of some appropriate curves.

Second, while using the binomial distribution (which, along with its limit, the normal form, was his standard one), he calculated the theoretical frequencies of the possible values that the respective random variables may assume (I substantiate both point below). Thus, with regard to the laws of distribution, QUETELET as well as some of his contemporaries formed the link between LAPLACE and PEARSON [118, pp. 332 – 333; 116, pp. 74 – 75], or perhaps between

LAPLACE on the one hand and GALTON and PEARSON on the other.

QUETELET noticed that the curves describing the inclination to marriage for people of different ages (§ 4.5) were exceedingly asymmetric both for men and women [7, p. 80] and (another example) that the common distribution of the heights of people of *deux races*, or of the two sexes, had *deux sommets* [6, p. 143][66].

In 1846, QUETELET [116, pp. 74 – 75] came to understand that the curves of distribution are *assez fréquents asymmetric*[67] and published letters which BRAVAIS had written him in 1845. These letters contained appropriate examples from biology, astronomy and meteorology. But in 1853 QUETELET returned to traditional concepts and maintained, in essence (Ibidem), that *causes spéciales* and anomalies were responsible for the appearance of asymmetric distributions. This point of view did not, however, hinder him [10, t. 2, p. 304 and 347] from publishing graphs of the asymmetric distributions of inclination to crime (above) some sixteen years later[68].

While discussing the binomial curve, QUETELET [55 (1873), pp. 139 – 141] stated that

L'on nomme également la courbe de Newton, comme un mathématicien distingué among his friends (*de mes amis*) told him. He (Ibidem) also held that the [normal law] was *une des plus générales de la nature animée*.

Stigler [118, pp. 334 – 337] described how QUETELET had fitted binomial and geometric distributions to data and remarked (p. 334) that the latter had invented a method *fully equivalent to the modern use of probability paper*[69].

I especially bring to notice QUETELET'S

Loi des causes accidentelles [7, Intro, p. viii]; *une loi générale qui s'applique aux individus comme aux peuples, et qui domine nos qualités morales et intellectuelles tout aussi bien que nos qualités physiques* (Ibidem, p. ix, also see pp. 94 and 140); *une loi générale qui domine notre univers et qui semble destinée à y répandre la vie; elle donne à tout ce qui respire une variété infinite … Cette loi, que la science a longtemps méconnue et qui toujours est restée inféconde pour la pratique, je la nommerai la loi des causes accidentelles* (p. 16).

Nevertheless, for all the infinite variety,

Chez les êtres organisés, tous les éléments sont sujets à varier autour d'un état moyen, et … les variations qui naissent sous l'influence des causes accidentelles, sont réglées avec tant d'harmonie et de précision, qu'on peut les classer d'avance numériquement et par ordre de grandeurs, dans les limites entre lesquelles elles s'accomplissent. Tout est prévu, tout est réglé: notre ignorance seule nous porte à croire que tout est abandonné au caprice du hasard (p. 17).

Yes, numerous acts of randomness indeed lead to regularity, but QUETELET went as far as to deny randomness altogether and he repeated this idea many times over [1, pp. 8 and 230; 6, p. 14; 8, p. 101]. Since his reasoning had to do with life forms, it is small wonder that he did not recognize the theory of evolution (cf. § 1.2). Or, rather, it seems that he could not reconcile his religious belief with the idea of randomness.

In a later contribution QUETELET [8, pp. 54 – 55] argued that the deviations of individual observations from their mean follow the law of accidental causes. Did he equate this law with the normal distribution [116, p. 75]? No, not at all, since he pointed out [8, p. 57] that the corresponding curve can be asymmetric. Did he bear in mind the regularity that reveals itself in a large number of errors possessing a binomial distribution? No, since in one instance [7, p. 94], while considering the graph of the number of people having one probability or another of committing a crime (§ 4.5), he maintained that

Cette même ligne … affecte ... la forme de la courbe des causes accidentelles.

True, the curve represented a certain statistical regularity, but it did not describe any random magnitude. Neither this curve, nor the one formed by the appropriate inverse function was a density of any distribution.[70]

5.5. A Classification of Causes. QUETELET paid attention to the classification of causes of such statistical phenomena as mortality (§ 2.4) and included his study of causes in probability theory [8, pp. 58 – 62]. He separated causes according to their origin into *naturelles* and *perturbatrices* [4, t. l, p. 21; 6, p. 198; 7, p. 21].[71]

In a much more interesting sense, QUETELET [21, p. 207, 6, p. 159; 8, pp. 58 – 62] isolated constant, variable (in particular, periodic), and accidental causes.[72] His explanation (and understanding) of these terms was hardly successful. Thus, he [21, p. 207] discussed the *degré d'énergie* of constant and variable causes and contended [21; 6; 8 p. 58] that

Les causes accidentelles ne ce manifestent que fortuitement, et agissent indifféremment dans l'un ou l'autre sens.

Moreover, using an archaic expression, QUETELET [21, pp. 228 – 229; 6, pp. 159 – 160] argued that constant (variable) causes

A pour elle un certain nombre déterminé (*un nombre variable*) *de chances* whereas *la cause accidentelle n'a pas, à proprement parler, de chances pour elle, mais elle influe sur l'ordre de succession des* [random] *événements*.

The quotation is from the first source, the second one contains a similar passage. Elsewhere, while equating random and perturbative causes, QUETELET [7, p. 21] maintained that

Les dernières agissent comme le feraient des forces accidentelles; elles laissent une empreinte plus ou moins profonde; puis elles s'effacent et permettent à la nature … de rentrer dans tous ses droits.[73]

In discussing causes, QUETELET [21, p. 207] offered yet another (cf. § 2.1) indirect definition of the aims of statistics:

L'emploi des moyennes a surtout pour objet d'éliminer … les effets des causes accidentelles, et d'arriver a la connaissance des causes constantes et des causes variables. Thus, statistics *doivent offrir les moyens de constater ces dernières causes et d'en mesurer le degré d'énergie*.

QUETELET (p. 217) owned, however, that

Cette dernière determination est le plus souvent impossible; et l'on doit se borner à l'étudier les [constant and variable] *causes ainsi que leurs tendances.*[74]

I have quoted one of QUETELET'S recommendations on studying variable causes in § 2.2. Though his thoughts contained a hint at a correlation theory, QUETELET did not introduce any quantitative rules for the inquiry that he thought desirable.

He did not improve his classification by introducing a law of accidental causes (§ 5.4). Consider one of QUETELET'S examples of studying causes. If [6, p. 193] a small number of newly born are not registered, the ensuing mistake in the data regarding the sex ratio at birth is random and does not therefore corrupt this indicator. If, however, some parents do not register their newly born sons in order to save them from military service, corruption will exist, and it should be considered

Parmi les causes constantes, ou plutôt parmi les causes variables, puisqu'ellé changerait avec les chances de guerre et de danger.

KNAPP [86, p. 113] spoke out against the isolation of constant, variable, and accidental causes. I believe that he was mistaken, and especially so when he added (p. 114) that the only sensible division was into *wesentlichen and unwesentlichen Ursachen*.

5.6. The Significance of Causes. QUETELET'S study of the probabilities of convictions (§ 4.4) included some further considerations. Denote these probabilities by *x*, *i* is the appropriate category of defendants and assume that $i = 0$ stands for the accused *sans désignation auqune*. Then, as QUETELET [18, p. 327; 4, t. 2, pp. 314 – 315] asserted, the ratios

$$\Delta_i = \frac{x_i - x_0}{x_0}$$

expressed the significance of the defendant belonging to category *i*.

YULE [123, pp. 30 – 31] highly praised QUETELET'S reasoning. He denoted the coefficient of association that he introduced in this paper by *Q*; elsewhere [124, p. 114] he explained that he took that symbol from the initial letter of QUETE LET. In addition, YULE [123, p. 30] noted that

The method [of serial chances] *seems to have been first brought forward, as a definite statistical method, by Quetelet.*

YULE could also have mentioned KÖPPEN [78, p. 133].

5.7. Empirical Formulas. QUETELET [40, p. 22] offered an empirical formula for the height of man (*y*) at different ages (*x*):

$$y + \frac{y}{1000(T-y)} = ax + \frac{t+x}{1+4/3x}.$$

Here, *T* is the height of an adult. Later QUETELET [4, t. 2, p. 31; 10, t. 2, p. 30] omitted the second term in the left side.

QUETELET [41, p. 67] also introduced a formula for the inclination to crime (*z*), again in different ages (*x*):

$$z = \frac{1 - \sin x}{1 + 2^{18-x}}.$$

In a second instance [4, t. 2, p. 244] he replaced (18 – *x*) by (*x* – 18) without explaining the reason why. He either made or corrected a mistake. Anyway, no such formula is contained in the *Physique sociale* [10].

QUETELET compared statistical data with figures calculated according to his formulas. He did not estimate the degree of fit thus provided although he could have determined the mean square (say) discrepancy between observation and theory.

The origin of QUETELET'S formulas remains unknown. KNAPP [86 p. 105] characterized the first of them as a *willkürliche Menge von sinnlosen Constanten*. Generally speaking, this statement seems too strong since empirical regularities may have no intrinsic sense. Also note [115, p. 176] that by the mid-19th century empirical formulas were not yet generally used. Still, QUETELET should have explained his train of thought[75] and, in any case, he should have paid more attention to dimensionality.

6. Conclusions

6.1. Quetelet's Achievements and Shortcomings. QUETELET (§ 2.1) held that statistics should aim to collect, appreciate and apply quantitative data. Though this idea seems quite natural, he had to support it against opinions put forward by reputable scholars and institutions (§§ 1.1 and 2.l).

While successfully applying the statistical method in meteorology and anthropology, QUETELET did not say anything at all about using the method in natural science in general (§ 1.2). It is possible, however, that late in life he began to regard the statistical method in a somewhat broader sense (§ 2.1).

QUETELET (§ 2.2) advocated a preliminary examination of data, but he was far from being consistent in this respect. Thus (§ 4.1), without paying due attention to statistics of crime, he insisted that the yearly number of crimes was constant.

QUETELET'S attempts to standardize and unify population statistics on an international scale (§ 2.3) proved especially fruitful. PEARSON, who levelled harsh and unwarranted criticisms against JAKOB BERNOULLI and LAPLACE for their insufficiently accurate approximations [107, p. 161], highly praised QUETELET'S achievements in organizing official statistics in Belgium and in unifying international statistics [95, vol. 2, p. 420].

QUETELET' s study of inclinations to crime and to marriage and his recommendation of the corresponding mean values (§ 4.5) were innovations in method. Their practical importance was probably small, the more so since QUETELET had failed to consider some essential points, but they stimulated public interest in statistics.

My conclusion with regard to the Average man (§ 3.2) is similar: QUETELET did not adequately reason out this concept and he naively placed his greatest hopes in it, but the ensuing discussions immensely popularized statistics. Besides, the Average man was logically expedient: it was exactly to this fictitious being that QUETELET related the mean inclination to crime and it is the Average man to whom the per capita economic indicators are referred even now.[76]

Independently of these arguments, introducing an average into sociology was in line with studying mean values and conditions in natural science (§ 5.3). Note, however, that QUETELET did not refer to the POISSON form of the law of large numbers in moral statistics. [77]

QUETELET did not leave any mark on probability theory although his popularization of it merits mention (§ 5.1); also see end of § 6.2. He made hardly any use of the theory of errors (§§ 4.1, 5.2, 5.3, 5.5 and 5.7), and I believe that for this subjective reason the gap between statistics and this theory became wider than it would have been otherwise. Furthermore, since the stochastic branch of the theory of errors certainly was a mathematical discipline, QUETELET thus lost the opportunity to raise the scientific level of statistics.

When necessary, QUETELET was quick to introduce empirical distributions and he understood that the normal law was not universal, but he did not study distributions per se.

QUETELET'S explanation of the action of various causes (§§ 5.4 – 5.5) should be singled out for criticism. **1.** His understanding of randomness was self-contradictory. At best, it is reminiscent of POISSON'S unsuccessful explanation (note 64). **2.** He introduced a universal *loi des causes* (alternatively: *des variations*) *accidentelles* without explaining its essence but announcing it as a great discovery. Did he not, after all, imitate POISSON'S unfortunate definition of a law (the law of large numbers) in a much too generalized form [110, p. 273]? I think that QUETELET was simply unable to develop his ideas adequately. As KNAPP [86, p. 124] politely put it, QUETELET had

Einen gedankenreichen, aber unmethodischen und daher auch unphilosophischen Geist.

In turn, FREUDENTHAL [72] holds that QUETELET

Certainly gave science new aims and tools, although his philosophy was rather pedestrian and his thinking in somewhat sophisticated matters was rather confused.

He continues:

Quetelet's impact on nineteenth-century thinking can in a certain sense be compared with Descartes's in the seventeenth century.

I do not entire agree with this comparison since QUETELET regarded the statistical method in a restricted way (above). Finally, FREUDENTHAL [71, p; 36], while referring mostly to anthropometry and noticing (p. 37) that QUETELET had introduced the normal law into this branch of anthropology, maintains that the latter had *een goed mathematisch instinct.* I am inclined to differ and say that QUETELET'S introduction of stochastic ideas into anthropometry testifies rather to his keen intuition in natural science.

In a more direct sense, it is held that QUETELET created statistics as a scientific' discipline. Thus,

1) *Quetelet die Statistik erneuert hat* [86, p. 90].

2) *It was primarily his own* [QUETELET'S] activity that led to the conception of statistics as a method of observation based on enumeration and applicable to any field of social inquiry [80, p. 41].

3) QUETELET attempted

To transform statistics … to an exact method of observation, measurement, tabulation, and comparison of results [98, p. 833].

4) *Er was een statistisch bureau, er waren statistieken en stastici, maar er was nog geen* [but there was no] *statistiek* before QUETELET [71, p. 7].

5) *Es ist jedenfalls das Hauptverdienst Quetelets auf dem Gebiete der theoretischen Statistik, dass er die Bedeutung des typischen Mittels erkannt (rein zufälliger Störungen) und zugleich nachgewiesen hat, dass gewisse den Menschen betreffende Beobachtungsmassen sich annähernd der mathematischen Fehlertheorie entsprechend gruppiren* [89, p. 38].

The words in brackets seem out of place; anyway, I do not agree with the term *Hauptverdienst*.

Two of the authors quoted just above pronounced their high opinion on QUETELET'S moral statistics.

1) The *wahre Bedeutung* of QUETELET'S work consists in his moral statistics [86, p. 91].

2) *It was mainly by … addition* [of moral statistics] *and the results following thereupon that the term, used to designate a new discipline in the German universities, came to have that scientific character sought by the school of political arithmetic* [80, p. 60].

6.2. Quetelet and the Origin of Mathematical Statistics. QUETELET'S work contained elements of mathematical statistics (§§ 5.2 – 5.7 and 6.1) and YULE, for example, believed that QUETELET was his forerunner in studying the significance of causes (§ 5.6). It is much more important however, that QUETELET'S publications apparently led to the study of the stability of statistical series (§ 4.1) and, consequently, engendered what has been called *the Continental direction of statistics* (LEXIS and others)[78]. In this sense, it is really possible to say [71, p. 7] that QUETELET was the *grondlegger der mathematische statistiek*.

QUETELET also influenced GALTON to a certain extent. Apart from what I said in §§ 1.2 and 3.2, I note that the latter [73, p. 26] called QUETELET *the greatest authority on vital and social statistics.* While discussing this work [73] by GALT0N, PEARSON [95, vol. 2, p. 89] declared that

We have here Galton's first direct appeal to statistical method and the text itself shows that [the English translation of the *Lettres* [6]] *was Galton's first introduction to the normal curve*.

I ought to mention PEARSON (Ibidem, p. 12) once more:

Even while Galton's work seems to flow naturally from that of Quetelet, I am very doubtful how far he owed much to a close reading of the great [!] *Belgian statistician*.

But the point really is that GALTON hardly needed more than a general impression of the essence and usefulness of statistics and this he undoubtedly found in QUETELET'S writings. Anyhow, GALTON [73, p. 26] maintained that the *Lettres* [6] or, rather, their English translation published in 1849, deserve to be *far better known to statisticians than* [they appear] *to be*. Also note his pronouncement made in 1874 [95, vol. 2, p. 335]: this book, he argued, is perhaps the most suitable to the non-mathematical reader.

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Notes

1. FOURIER (like LAPLACE) was a member of the commission for the awarding of the statistical prize [62, pp. LXI – LXVI] and possibly for this very reason he himself did not get the prize.

2. The last two items can hardly be attributed to statistics, or even to the *Tabellenstatistik.*

3. Cf. FOUR1ER'S utterance [67, t. 1, pp. iv – v]: *L'esprit de dissertation et de conjectures est, en général, opposé aux véritables progress de la statistique, qui est surtout une science d'observation.*

4. GUERRY [79], who apparently coined this term, largely restricted his efforts to the study of crime. He was co-author of a book [44] that proved unavailable. CASPER [113, p. 258, note 30] initiated inquiries into the statistics of suicides.

5. I did not see the second edition of the *Göttliche Ordnung* [120].

6. In the sequel, I quote this source several times more, but I do not claim to read Flemish.

7. COLLARD [51, p. 701 testified that

Quetelet fut en relations scientifiques avec l'élite du monde intellectuel des deux hémisphères: mais il se fait malheureusement que tout ce commerce épistolaire est disséminé aux quatre coins du monde.

Nonetheless, WELLENS-DE DONDER [122] reported that a large part of QUETELET'S archive is extant and that among his 2.5 thousand (!) correspondents were such scholars as GAUSS, AMPÈRE, HUMBOLDT, and GOETHE.

8. STIGLER [119] described the forgotten statistical studies on memory (1876 and 1878) by an American physicist, NIPHER.

9. In a letter to FLORENCE NIGHTINGALE dated 1891, GALT0N [95, vol. 2, p. 420] asserted that

Quetelet' s promises and hopes and his achievements in 1835 – 36 remained in statu quo up to the last edition of [the *Physique sociale*] *in 1869. He achieved nothing hardly, of real value in all those 33 years*.

10. For the sake of comprehensiveness I should also mention that QUETELET made an original contribution to analytic geometry and was co-editor (1825 – 1827) and sole editor (l 827 – 1839) of a prestigious periodical, *Correspondance mathématiques et physiques*. These facts are generally known.

11. My statement mainly concerns sections 2.1 – 2.3, 3.1, 4.1 and 5.1 – 5.5 which contain previously unexplored or un-systematized material.

12. Cf. FREUDENTHAL'S opinion quoted in § 1.1.

13. QUETELET did not refer to CAUCHY [48, p. 242] who contended that

L'heureuse influence qu'exercent nécessairemént sur les individus et sur la société des doctrines vraies, de bonnes lois, de sages institutions, ne se trouve pas seulement démontrée par le raisonnement et par la logique, elle se démontre aussi par l'expérience. Par conséquent, la statistique offre un moyen en quelque sorte infaillible de juger si une doctrine est vraie ou fausse, saine au dépravée, si une institution est utile ou nuisible aux intérêts d'un peuple et a son bonheur. Il est peut-etre à regretter que ce moyen ne soit pas plus souvent mis en oeuvre avec toute la rigueur qu'exige la solution des problèmes; il suffirait à jeter une grande lumière sur des vérités obscurcies par les passions; il suffirait a détruire bien des erreurs.

14. Cf. QEUTELET'S general remarks [39, p. i; 19, p. 27]: statistics might formulate

Régles de conduite pour l'avenir; statistics should *estimer le degré de prospérité de … population, sa force, ses besoins, et jusqu'à un certain point se faire des idées justes sur son avenir*.

15. He know perfectly well [6, p. 302] that

Quelque grand que soit le nombre des observations, il devient insuffisant, quand on a des raisons de croire que des causes périodiques, ou une cause accidentelle très-prépondérante, a pu l'influencer.

16. This point of view was much more reasonable than his previous demand [6, p. 281] to abstain *en général de faire entrer dans les statistiques des données qui ne sont point parfaitement exactes.*

17. This was his longstanding opinion. Even in 1845, while discussing the sex ratio at birth, he maintained [21, p. 231] that

La théorie donne le moyen d'estimer la valeur de ce rapport, et la probabilité que la différence avec le rapport véritable [!] *ne dépasse pas une limite donné. Nous ne nous occuperons pas de cette estimation, du moins pour le moment*.

18. I quote [55 (1868), p. 6]:

Le Congrès considerant l'importance et l'étendue des questions statistiques, qui trouvent dans les mathématiques leur base scientifique; considerant que chez tous les peuples civilisés d'illustres géomètres ont appliqué le calcul des probabilités à ces questions, émet le voeu Que dans les futurs Congrès il y ait une section speciale, chargée de s'occuper des questions de statistique en rapport direct avec la théorie des probabilités.

This decision was not implemented. However, two years later, in 1869, the next session of the Congress carried the following resolution [55 (1870), p. 534]:

Le Congrés est d'avis: 1. *Que dans toutes recherches statistiques, il importe de connaître tant le nombre d'obseruations que la qualité ou la nature des faits observés*;

2. *Que dans une série de grands nombres, la valeur qualitative se mesure par le calcul des écarts de ces nombres, tant entre eux que du nombre moyen déduit de la série*;

3. *Qu'il est à désirer qu'on calcule non-seulement les moyennes, mais aussi le nombre d'oscillations afin de connaître la déviation moyenne des nombres d'une série de la moyenne de cette série même*.

The terminology used both here and at the previous discussions at this session of the Congress (p. 63 ff.) was really awkward. Also note that in those times statisticians did not yet estimate the scatter of observations by means of the appropriate variance; cf. § 5.2. The reference to *oscillations* seems extremely interesting.

19. QUETELET expressed this idea even somewhat earlier [21, p. 214]. Elsewhere [l, p. 245] he argued that

Il est presqu'inutile de présenter … des conséquences qui ne sont pas vérifiées par des comparaisons des valeurs moyennes.

QUETELET'S standpoint was hardly innovative. Anyhow, I believe that astronomers used a similar procedure even in the 18th century [105, p. 309].

20. *En 1853, le gouvernement belge, à la prière de sa commission centrale de statistique … fit un appel à tous les Etats civilisés* [10, t. l, p. iii].

The essence of this appeal consisted in recommending to *donner plus d'ensemble et plus d'unité aux statistiques des différents pays* (Ibidem, p. 110).

21. I give just one passage from the proceedings of the Congress [55 (1870), p. 542]:

Le Congrés, en vue des progrès de la science, comme pour hâter le développement des progrès économiques des peuples et favoriser l'accroissement de leurs relations commerciales, décide: Une adresse sera présentée aux Hauts Gouvernements par le bureau du Congrès, tendant à les inviter: 1. A introduire dans leurs Etats, s'ils ne le possédent pas déjà, un système uniforme de poids et mesures, conforme au système métrique déjà en usage en [five countries are named] *et dans quelques autres pays …*

22. In the 18th and 19th centuries, many businesses dealing with life insurance were no better than the lousy establishment of Mssrs. DODSON & FOGG (C. DICKENS, *Posthumous Papers of the Pickwick Club*, chapter 20) [109, p. 212].

23. DEMONFERRAND [63; 64] inquired into the reliability of official French demographic data. He revealed [63, p. 251]

Beaucoup de fautes. Quelquefois, he indicated (Ibidem), *on a simplement transcrit avec de légères variantes, le tableau d'une autre année*.

DEMONFERRAND (p. 261) used *un moyen simple d'apprécier le degré de probabilité des documents et des résultats auxquels ils ont servi de base. Ce moyen*, he continued, *est emprunté à l'Astronomie; il consiste à se servir de valeurs approximatives données par des observations imparfaites pour prédire des faits futurs et à comparer ensuite le calcul à de nouvelles observations*.

The same memoir [63] contained mortality tables

Fort étendues … et ayant égard aux dangers des principales classes de la société [10, t. l, p. 299].

24. At least in one instance [24, p. 16] the study was connected with a proposal to establish a national insurance system.

25. Later, GAUSS [111, pp. 60 – 62] used QUETELET'S data on infant mortality, a fact which QUETELET did not fail to put on record [32, p 12; 10, t. 1, p. 302].

26. By the mid-19th century, public hygiene began earnestly examining mortality in hospitals, barracks and prisons. SIMPSON, PIROGOV, FLORENCE NIGHTINGALE and others studied the general causes of high mortality in surgical hospitals [113, § 6.1.2].

27. Apparently, C0MTE'S contemptuous reference to *simple statistique* (above) could be understood in this very sense.

28. He himself held [39, p. ii] that *la statistique compare ... est a peu près pour la société ce que l'anatomie comparée est pour le règne animal*!

29. In translating QUETELET'S expression *l'homme moyen* (below) into English, I use the term *Average* with a capital a.

30. Nevertheless, in another passage QUETELET (Ibidem, p. 307) did not directly state as much:

.. *La taille de l'homme moyen en Belgique*, he reported, *est* 1^m,684; *et il est autant d'hommes qui mesurent* 1,^{m}784 *que d'autres qui ne mesurent que* 1,^{m}584.

31. This is POISSON'S own example [110, p. 272].

32. LOMBROSO took up a similar idea at least with respect to criminals, but most of his theories are now discredited (*Enc. Brit.*, vol. 14, 1965, p. 262). Beginning with BUFFON if not earlier, biologists have included pictures of animals in their writings. In some instances, the pictures most likely represented *average* animals of one species or another rather than specific individuals.

33. Only the second part of REHNISCH'S memoir is relevant. Regrettably, he did not complete his work: the third (obviously, the last) part of the contribution did not appear in print.

34. Cf. KNAPPS opinion [86, p. 96]:

Doch die Verwunderung über Regelmässigkeit ist noch kein Ergebnis wissenschaftlicher Behandlung, sondern nur der Anzeig zu einer solchen.

35. Thus (pp. 102 and 104) he wrongly identified the numbers of accusations and defendants.

36. Even in 1840 DUPAU [65, p. 261] put on record the essential influence of this law on criminal statistics.

37. In a lesser known article he [49, p. 406] testified that REHNISCH'S *sharp reproofs caused considerable discord … among statisticians*.

38. In different works he preferred different causes! He also noticed the existence of white collar perpetrators (§ 4.4) but did not say anything about the mental nature and habits of professorial criminals.

39. QUETELET [27, p. 542] made it known that one of his first contributions [3] *faillit etre étouffé à sa naissance* by the Belgian minister of the interior who *craignait le mauvais effet que pouvaient produire* the study of local causes of crime among the public.

LOTTIN [91, p. 145] remarked on this fact.

The publication (in 1823) of a report by the Russian economist and statistician K. F. GERMAN (HERMANN?) on the statistics of murders and suicides displeased Russia's minister for public education [97, p. 110; 84, pp. 420 – 423].

40. LANDAU & LAZARSFELD [87, p. 833] approvingly remarked that QUETELET always substituted a single observation of a population instead of repeated observations of the individual. Without implying far-reaching conclusions, I note that in statistical mechanics time averages and phase averages were sometimes assumed to be equal.

41. The term should not be understood in its modern quantitative sense.

42. PEARSON [95, vol. 3A, p. l] maintained that

Condorcet often and Laplace occasionally failed because [the] *idea of correlation was not in their minds. Much of Quetelet's works and of that of the earlier (and many of the modern) anthropologists is sterile for like reasons*.

Explaining his reference to LAPLACE, PEARSON mentions the correlation between the size of the population and the number of births [107, § 2.5.5]. The introduction of the quantitative theory of correlation into mathematical statistics was an extremely important step in the latter's development and PEARSON could have added that (with the single exception of SEIDEL [113, §§ 7.4.2 – 7.4.3]) no one before GALTON had studied correlation quantitatively.

43. The authors also discussed QUETELET'S concept of inclination to crime (§ 4.5).

44. In the second instance [43], while comparing, in a methodological sense, inclinations to marriage (and to crime), QUETELET suddenly contradicted himself giving a new explanation of why apparent and real tendencies to crime do not coincide:

La tendance déduite de l'observation des faits, he noted, *n'est qu'apparente, et, dans certaines circonstances, elle peut différer considérablement de la tendance réele. C'est ce qui a lieu pour les empoisonnements, par exemple; car … un grand nombre de ces crimes restent toujours inconnus*.

At best, he explained why the apparent tendency is an unreliable indicator.

45. *Les tables de criminalité, pour les différents âges, méritent au moins autant de confiance que les tables de mortalité* [5, p. 14].

LANDAU & LAZARSFELD [87, p. 831] believe that QUETELET

Failed to recognize that his concept of penchant could just as reasonably be applied in the study of physical attributes.

The passage above shows that their opinion is not altogether correct. HANK1NS [80, p. 104, note] remarked that since felonies come to light relatively more often than misdemeanours (§ 4.1), and since the type of crime perpetrated depended on the age of the criminal (§ 4.2), it is difficult to compare the statistical inclinations to crime for various age-groups.

46. The references above also pertain to free will as manifested in marriages (§ 4.6). In 1854, PIROGOV [113, p. 268] stated that the surgeon's skill [acts randomly and] is not as important as the over-all management of military surgery.

47. KNAPP (p. 117) also declared that statistics is not a poor relative of probability theory:

Man braucht mehr als nur die Urnen des Laplace mit bunten Kugeln zu füllen, um eine theoretische Statistik heraus zu schütteln.

His idea was correct, but its wording was certainly disrespectful with regard to LAPLACE who did not deserve references of this kind.

48. True, QUETELET (p. 144) indicated that the total was 26, but elsewhere [3], p. 93; 28, p. 455] he also corrected himself, giving the appropriate figures for 1841 – 1845, 1846 – 1850, and 1851 – 1856 (31, 29, and 26 respectively). He did not remark on the decrease in these figures.

49. It is not difficult to notice a similarity between this statement and QUETELET'S conviction (§ 4.1) that society is responsible for crimes.

50. QUETELET first published this passage in 1826 [14, p. 177]. He quoted FOURIER once more in 1860 [28, p. 436] stating that the letter was written *plus d'un quart de siècle* ago.

51. Cf. QUETELET'S late opinion [55 (1873), p. 139]:

. *Mais à cette époque* [apparently, in the mid-century] *les mathématiciens se rétirèrent ce qui eut pour suite de grandes erreurs dans les calculs.*

He could have said that mathematicians had moved away from probability theory altogether.

52. He added 13 lines more, expressing himself in the spirit of LAPLACE'S celebrated, even if not quite definite, pronouncement [112, p. 332, note 20]: *la théorie des probabilités n'est, au fond, que le bon sens réduit au calcul*. I have described LAPLACE'S attitude toward probability theory elsewhere [107, epigraph and § 3.3].

53. Alternatively [1, p. i],
Le calcul des probabilités … devrait … servir de base à l'étude de toutes les sciences et particulièrement des sciences d'observation.
54. On the contrary, his junior contemporary, COURNOT [56] is meritorious for his achievements in the theory. Thus, he offered an heuristic definition of geometric probability (§ 18); attempted to define a random event as an intersection of chains of determinate outcomes (§ 40); methodologically explained the notion of density (§§ 65 – 66); studied the significance of empirical discrepancies (§§ 107 – 117); and recommended the POISSON form of the DE MOIVRE – LAPLACE integral limit theorem for small values of probability (§ 182).
CHUPROV [50, p. 30] called COURNOT
One of the most original and profound thinkers of the 19th century, whom his contemporaries [including QUETELET] *had failed to appreciate und who rates higher and higher in the eyes of posterity*.
There can be no doubt that this opinion was based on COURNOT'S entire legacy, i.e., on his work in probability theory, statistics, and, presumably, economics.
MISES regarded COURNOT as *One of his predecessors* [103, p. 219, note 6; 82, p. 12].
55. QUETELET'S lectures marked the beginning of the teaching of probability theory in Belgium. In 1903 MANSION [110, p. 273, note 30] maintained that
Il est peu de pays … où le calcul des probabilités tienne une place aussi considérable dans l'enseignement qu'en Belgique.
He attributed the great respect for the theory in that country to the lasting influence of QUETELET.
56. In this connection, he mentioned BERNOULLI (p. 94) and readers doubtlessly assumed that QUETELET had referred to JAKOB. In actual fact the moral expectation was due to DANIEL BERNOULLI.
57. Though the first of these works included mathematical notes it still remained popular, or at least its mathematical level was not high enough. However, see end of § 6.2.
58. A later note. Pearson (1892, p. 15) maintained that *the unity of all science* [I would say, of any definite science] *consists alone in its method.* Therefore, medical statistics, for example, is the application of that method to medicine, and the theory of errors, its application to the treatment of observations. I also note a most important corollary of that statement: for beiong an independent discipline, statistics need not have its own subject.
59. Even LAPLACE [108, p. 36] indirectly used the mean square error.
60. Elsewhere, while discussing the plausibility of statistical observations on an elementary level, QUETELET [18, p. 330; 4, t, 2, p. 322] repeated his wrong reference to the method of least squares.
61. FOURIER (p. 543) maintained that *le triple de g est la limite des plus grandes erreurs*. QUETELET did not repeat this idea. FOUR1ER'S conviction did not agree with the *three sigma* rule that was later (since about the middle of the 19th century) accepted in the theory of errors.
62. Elsewhere (see § 5. 1) he expressed the same opinion with regard to probability theory in general!
63. DAVIDOV [58, p. 45] also argued that
The doctrine of mean quantities … should occupy the most distinguished position among the [various] *branches of human knowledge*.
I repeat a previous remark [77, p. 216] on the difference between the two kinds of means:
It is only to be regretted that, proceeding exactly from … the insignificant distinction [between the means], *astronomers and geodesists almost up to most recent times stubbornly refused to notice the achievements of mathematical statistics*.
63a. Still, D. HILBERT (Mathematische Probleme (1901). *Ges. Abh*., Bd. 3. Berlin, 1935, pp. 290 – 329. See § 6) used the obsolete term even in 1901:
Was die Axiome der Wahrscheinlichkeitsrechnung angeht, so scheint es mir wünschenswert, dass mit der logischen Untersuchung derselben zugleich eine strenge und befriedigende Entwicklung der Methode der mittleren Werte in der mathematischen Physik, speziell in der kinetischen Gastheorie Hand in Hand gehe.

64. It is obvious that COURNOT did not possess even a heuristic notion of a random magnitude introduced by POISSON [110, pp. 250 – 251]. He (Ibidem, p. 248), whom COURNOT still could have followed, unsuccessfully explained a [random] phenomenon as resulting from an

ensemble des causes qui concourent [in its] *production … sans influer sur la grandeur de sa chance ...*

65. Exactly for this reason QUETE LET [4, t. 2, p. 308; 6, p. 147; 7, p. 27] needed to know the extreme values of the [random] magnitudes that he studied.

66. L. A. BERTILLON [45, p. 289] discovered that the distribution of the heights of the conscripts in a certain *département* of France had two peaks. Without mentioning QUETE LET he concluded that

Le département … devait être habité par deux types à peu près aussi nombreux l'un que l'autre et notablement different par leur taille.

BERTILLON was mistaken: see CHUPROV'S letter dated 10 May 1916 in Ondar (1977/1981).

67. QUETE LET [6, p. 182] even argued that

Quelquefois les chances ne sont soumises à aucune loi appreciable, et la courbe de possibilité peut affecter les forms les plus capricieuses.

He possibly missed an opportunity to isolate *chaotic* randomness possessing no law of distribution.

68. QUETELET drew the graphs in the right-hand coordinate system, but sometimes [30, p. 3] in similar cases he used the left-handed system.

69. While determining the true typical direction of a mountain chain given the directions of its separate ridges, the mathematician and astronomer SPOTISWOODE [117, p. 149] compared the appropriate deviations with those that were studied in questions of gunnery. He mentioned error theory rather than the theory of means (§ 5.3) and he used the mean square and the probable errors for estimating the accuracy of his inferences.

SPOTTISWOODE (p. 154) contended that the solution of his problem will help

The geologist and the physical philosopher [to obtain] *good grounds for seeking some common agency which has caused their* [the ridges'] *upheaval. In this way*, he continued, *the Calculus of Probabilities … may … serve as a check and a guide to the physical philosopher by pointing out where he may and where he may not employ his study of causes with reasonable hope of a successful result*.

SPOTTISWOODE (p. 152) referred to QUETELET without mentioning any specific work but obviously bearing in mind the latter's use of binomial distributions. It is possible that SPOTTISWOODE was the first to apply stochastic considerations to physical geography.

70. To make matters worse, QUETELET [7, pp. 27 and 45] also introduced a *loi des variations accidentelles*, but I really think that he did not differentiate between the two terms. In the first instance QUETELET remarked (cf. note 65) that this new law enables one to calculate

À priori, quand on connait la moyenne et les deux terms limites, comment une population se fractionne sous le rapport des hommes qui ont tel poids ou telle force déterminée.

71. At first, QUETELET [4; 6] attributed the perturbative causes (e. g., those leading to excessive mortality in cities) to the action of man but then [7] he identified them with accidental causes (below). QUETELET also distinguished between natural and perturbative causes in his *Physique sociale* (§ 3.1).

72. He introduced variable and accidental causes even in 1836 [4, t. 2, p. 336].

73. Back in 1772, ADANSON [112, p. 334] made a similar statement concerning variations in plants.

74. Even before QUETELET [21], COURNOT [56, § 103] connected the aims of statistics as revealing *l'existence des causes régulières*.

75. This was just what he did when he introduced a new empirical law in botany [112, p. 327].

76. In 1949 FRECHET [70] attempted to rehabilitate the Average man or, rather, to replace him by the *homme typique,* a definite person who, on the whole, is closest to the Average man.

77. He only mentioned this concept in connection with the height of the Average man. CHUPROV'S utterance (1909/1959, p. 227) is highly relevant:

Poisson's generalized scheme irrevocably ends with the levelling tendencies of the simplified theory of statistical regularity advocated by Quetelet's disciples.

78. Note, however, that POISSON [110, § 5.2] and BIENAYMÉ [82, p. 49] are the predecessors of this direction.

79. DARWIN [112, p. 344] also referred to the English translation of' the *Lettres* and MAXWELL is known to have borrowed in 1860 his heuristic deduction of the [normal] law from J. HERSCHEL'S review of this book. Finally, in 1867 QUETELET [111, p. 66] made known SCHUMACHER'S intention of translating the *Lettres* [6] into German:

Il me parlait [in 1846] *de l'intention qu'il avait de donner une traduction* etc.

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V

Karl Pearson. A century and a half after his birth

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Note. When compiling the manuscript of this paper, I had to comply with its prescribed size. Now, I include additional material.

I describe Pearson's transition from history to mathematics, statistics and eugenics and his work as the leader of the Biometric school and creator of biometry. I also point out his studies in physics, describe how other scientists regarded him and notice that the Soviet statistical establishment did not recognize Pearson's merits.

1. Youth and broad interests

The *International Statistical Review* (vol. 77, No. 1, 2009) has recently devoted a special issue to Pearson. Its Preface and nine papers certainly add to our knowledge of the beginnings of mathematical statistics. Pearson's life and work and his influence on the subject are traced although the authors have prettified their hero. [In 2010 the Editor deleted this criticism.] My paper adds some extra facts.

Karl Pearson (1857 – 1936) was an applied mathematician and philosopher and the creator of biometry, of the main branch of what later became mathematical statistics. In 1875 he obtained a scholarship at King's College, Cambridge, and in 1879 he took there the Mathematics Tripos in which he graduated as Third Wrangler.

While a student, resenting coercion, he refused to attend compulsory religious lectures, but, when the regulations had to be softened, he continued to come voluntarily. Furthermore, in 1877 Pearson studied religious and philosophical issues, and, in 1880 – 1883, particularly read Spinoza. However, much later, in 1936, he (E. S. Pearson, hereafter ESP, vol. 28, p. 196), concluded that Spinoza was

The sole philosopher who provides a conception of the Deity in the least compatible with scientific knowledge.

Until 1884 Pearson had also been studying literature, history and politics and came independently, without being influenced by Ernst Mach, to comprehend science as the description of phenomena. In 1890 he began to consider himself a socialist and even offered to translate (the first volume of) *Das Kapital* into English, an enterprise to which Marx did not agree. Pearson spent about a year in the universities of Heidelberg where he read physics and Berlin, studying the social and economic role of religion, especially in medieval Germany.

In 1882 and 1883 Pearson gave lectures, in particular in Cambridge, on the history of Germany during the Middle Ages and Reformation, and on the role of science and religion in society. In 1884 he continued lecturing, this time in London, on Lassalle and Marx.

Pearson could possibly have become an outstanding historian, but his inherent mathematical ability was apparently stronger than his interest in history. In 1881 – 1882 he taught mathematics at King's College and in 1881 and 1883 unsuccessfully attempted to gain appointment to a professorship in mathematics.

In 1884 Pearson was finally appointed professor of applied mathematics at University College London. In the next year or two he gave a few lectures on the *Women's Question* and established the *Men and Women's Club*. It existed until 1889 for free and unlimited discussions of everything concerning the relations between the sexes as well as the tricky usual problem which faced women: How to fulfil family duties and work? Haldane (1957, p. 305/1970 p. 429) remarked that (apparently, in England)

If today association with prostitutes is generally regarded as degrading, while seventy years ago it was generally condoned and not rarely approved, we owe it largely to men like Karl Pearson.

We witness his refusal to accept without question the moral norms of the time.

2. Physics and philosophy of science

All through those early years and until about 1893, Pearson studied physics on which he expressed some extremely interesting ideas. Thus, *negative matter* exists in the universe (Pearson 1891, p. 313). *All atoms in the universe of whatever kind appear to have begun pulsating at the same instant* (1887a, p. 114). *Physical variations effects* were perhaps *due to the geometric construction of our space* (Clifford 1885/1886, p. 202). He did not however mention Riemann spaces whereas it is nowadays known that the curvature of space-time is caused by forces operating in it.

Remarkable also was the idea (Pearson 1892, p. 217) about the connection of time and space subjectively expressed as follows:

Space and time are so similar in character that if space be termed the breadth, time may be termed the length of the field of perception.

Mach (1897, Introduction) mentioned Pearson in the first edition of his book which appeared after 1892:

The publication [of the *Grammar of Science*] *acquainted me with a researcher whose erkenntnisskritischen* [Kantian] *ideas on every important issue coincide with my own notions and who knows how to oppose, candidly and courageously, extra-scientific tendencies in science*.

In that same contribution (p. 15) we find Pearson's celebrated maxim:

The unity of all science consists alone in its method, not in its material.

Entire science cannot have common material, so Pearson was apparently thinking of its definite branches For my part, I believe that statistics is defined by the statistical method (by counts of items in different categories). Medical and stellar statistics, for example, are the applications of that method to medicine and stellar astronomy, and the theory of errors, its application to the treatment of observations. See also an important remark [iv, Note 58].

Among those who praised the *Grammar of Science* were Bernstein, Neyman in 1916 (ESP 1936, p. 213). Lenin (1909/1961, pp. 190 and 174) called Pearson *a conscientious and honest enemy of materialism* and *one of the most consistent and lucid Machians*. In turn, Pearson (1978, p. 243) remarked:

Petersburg [Petrograd] *has now for some inscrutable reason been given the name of the man who has practically ruined it*.

Elsewhere Pearson (1978, p. 423) added:

Men of liberal ideas, in particular, liberal scientists, have not the foresight and the strength which are needed to control a revolution. As Kerensky was to Lenin, so was Condorcet to Robespierre.

Wilks (1941, p. 250), whose initials, S. S., were interpreted as *Statistician Supreme*, noted that in his *Grammar* Pearson had stressed the need to liberate science from theology and metaphysics.

Pearson's *Grammar* prompted Newcomb (Sheynin 2002, p. 163, Note 8) to invite him to the International Congress of Arts and Sciences (St. Louis, 1904) and deliver a talk on *Methodology of science*. A great honour: Among the reporters were Boltzmann and Kapteyn. Pearson refused to come but provided flimsy grounds. He apparently valued his time extremely high.

Pearson (1887b, pp. 347 – 348) rejected revolutions and thought (wrongly, as proved by Lenin) that eventually nothing will change.

In 1896 Pearson was elected Fellow of the Royal Society. In 1898 he declined the award of its Darwin medal (a proposal by Weldon, see ESP 1938, p. 194) since medals *must go to encourage young men*, as he explained on a similar occasion in a letter of 1912 (Ibidem). From 1912 to the end of his life Pearson continued to refuse prizes, medals and a knighthood (Magnello 2001, p. 255).

3. Statistics, eugenics and biology

When lecturing on statistics, Pearson had applied graphical methods and began to study the same methods in statistics, perceiving them as a general scientific tool for providing a broad mental outlook to students. Soon, however, discussions of the issues of evolution with Raphael Weldon as well as the writings of Francis Galton turned his attention to biology and eugenics and to their study by statistical means. Here are two of Pearson's statements on eugenics (Pearson 1887b, p. 375; Mackenzie 1981, p. 86):

Shall those who are deceased, shall those who are nighest to the brute, have the power to reproduce their like?

Do I […] *call for less human sympathy, for more limited charity, and for sterner treatment of the weak*? *Not for a moment*.

The second pronouncement made in 1909 concerns *negative eugenics* which involves *subjective and controversial matter* (*New Enc. Brit*., vol. 19, 15th edition, 2003, p. 725, Eugenics and heredity). I most strongly condemn the statement of Boiarsky et al (1947, p. 74) that Pearson's racist ideas *had forestalled the Goebbels department*. This is where the influence of the official Soviet environment and the opinion of the troglodyte Maria Smit (§ 6) had indeed been felt.

To reveal hereditary diseases in due time by the methods of genetics can also be considered as a problem of eugenics. In 1913 – 1914 and, with interruptions, until 1921 – 1929 Pearson and his collaborators read public lectures on eugenic subjects. He himself published a few papers on the influence of tuberculosis, alcoholism and weak-mindedness on the offspring. Some of his conclusions were extremely unusual and led to embittered debates.

In 1925 Pearson established the periodical *Annals of Eugenics* and edited it for five years. In the Editorial of its first issue he stated that that journal will be exclusively devoted to studies of race problems and favourably regard Galton's opinion about the stochastic essence of eugenics. It is perhaps significant that in 1954 this periodical changed its name to *Annals of Human Genetics*.

Weldon died in 1906; he and Galton had established the Biometric school for statistically justifying natural selection, and Pearson became the head of this School and the chief editor (for many years, the sole editor) of its celebrated periodical, *Biometrika*. Here are passages from the Editorial in its first issue of 1902, after a quote from Weldon's paper of 1893 (ESP 1936, p. 218).

It cannot be too strongly urged that the problem of animal evolution is essentially a statistical problem. […] *The problem of evolution is a problem in statistics*. […] *We must turn to the mathematics of large numbers, to the theory of mass phenomena, to interpret safely our observations.* [E]*very idea of Darwin – variation, natural selection* […] *seems at once to fit itself to mathematical definition and to demand statistical analysis*.

Much later Pearson (1923, p. 23) once more mentioned Darwin:

We looked upon Darwin as our deliverer, the man who had given new meaning to our life and the world we inhabited.

Pearson (1906) published a paper honouring Weldon's memory. He also compiled a contribution on Galton's life and achievements (1914 – 1930), a fundamental and most comprehensive tribute to any scholar ever published. It testified to its author's immense capacity for hard work.

The immediate cause for establishing *Biometrika* seems to have been the scientific friction between Pearson and Weldon on the one hand and biologists (especially Bateson), on the other hand, who exactly at that time had discovered the unnoticed Mendel. It was very difficult to correlate Mendelism and biometry: the former studied discrete magnitudes while the latter investigated continuous quantitative variations. T*hen, Mendelism is being applied wholly prematurely*, without taking into account *serious social* problems (ESP, vol. 29, p. 169). And here is ESP himself (vol. 28, p. 242):

A myth regarding some essential error in the biometricians' approach has persisted to this day. […] *But Pearson saw clearly, as most of his critics did not, that no theory of inheritance could discredit certain established facts following from a statistical analysis of observational data*.

Continental statisticians had not then thought about biology. Much earlier Quetelet (1846, p. 259) who lived until 1874 but never mentioned Lamarck, Wallace or Darwin, stated that

The plants and the animals have remained as they were when they left the hands of the Creator.

The speedy success of the Biometric School had been to a large extent prepared by the efforts of Edgeworth, see his collected writings (1996).

Pearson's results in statistics include the development of the elements of correlation theory and contingency (beginning with 1896); introduction of the *Pearsonian curves* for describing empirical distributions (1894); the derivation of a most important chi-squared test for checking the correspondence of experimental data with some law of distribution (1900) as well as the compilation of many important statistical tables. Another point is his study of asymmetric curves. One of its results (1898) was the treatment of asymmetric distributions of meteorological elements.

Pearson's posthumously published lectures (1978) examined the development of statistics in connection with religious and social conditions of life. On the very first page we find the statement about the importance of the history of science:

I do feel how wrongful it was to work so many years at statistics and neglect its history.

However, he (1925, p. 210) falsely appraised the Bernoulli law of large numbers. He did not notice that Bernoulli had solved his own philosophical problem; namely, he proved that in principle induction was not worse than deduction. Pearson evidently did not set high store on theorems of existence (in this case, of a certain limit), and he inadmissibly compared Bernoulli's result with the false Ptolemy system of the world. Neither did Pearson recall the Continental direction of statistics.

Pearson was apparently the first to stress the sociological and religious motives of statisticians:

Newton's idea of an omnipresent activating deity, who maintains mean statistical values, formed the foundation of statistical development through Derham [a religious philosopher], *Süssmilch, Niewentyt, Price to Quetelet and Florence Nightingale*.

De Moivre is forgotten Then, Newton had not stated such an idea (although *activating* is correct). In 1971, ESP answered my question:

From reading [the manuscript of Pearson (1978)] *I think I understand what K. P. meant.* […] *He has stepped ahead of where Newton had got to, by stating that the laws which give evidence of Deity, appear in the stability of the mean values of observations*.

Pearson was the head of the Biometric laboratory from 1895 and of the Eugenic laboratory (established in 1906 by Galton) from 1908. They were amalgamated in 1911, and in 1933 Pearson (ESP 1938, p. 230) submitted his final report to the University of London. In this he noted *the development in the last ten years* of laboratories in the rest of Europe *on the lines* of that amalgamated entity, i. e., the work on the *combination of anthropometry, medicine, and heredity, with a statistical basis*. It is opportune to add that physicians had recognized his merits by electing him, in 1919, Honorary Fellow of the Royal Society of Medicine (ESP 1938, p. 206).

During WWI Pearson assisted the Ministry of the Munitions. In a letter of 1918 to Pearson, Vice-Admiral R. H. Baker of the Munitions Inventions at that Ministry wrote that

The laboratories under your [Pearson's] *charge* rendered *very valuable assistance* […] *to the Ministry in general, and to this Department in particular* (ESP 1938, p. 244).

4. Other branches of science

Pearson attempted, often successfully, to apply the statistical method, and especially correlation theory, in many other branches of science. For instance, here is an interesting pronouncement (Pearson 1907, p. 613):

I have earnt from experience with biologists, craniologists, meteorologists, and medical men (who now occasionally visit biometricians by night) that the first introduction of modern statistical method into an old science by the layman is met with characteristic scorn. But I have lived to see many of them tacitly adopting the very processes they began by condemning.

And here is another important passage of 1907 (ESP, vol. 29, p. 164):

[The aims of the Biometric school:] *To make statistics a branch of applied mathematics … to extend, discard or justify the meagre processes of the older school of political and social statisticians, and, in general, to convert statistics in this country from being the playing field of dilettanti and controversialists into a serious branch of science. … Inadequate and even erroneous processes in medicine, in anthropology* [anthropometry]*, in craniometry, in psychology, in criminology, in biology, in sociology, had to be criticized … with the aim of providing those sciences with a new and stronger technique*.

Pearson (1907, pp. 517 – 518) attempted to apply correlation in astronomy, but Newcomb (Sheynin 2002, pp. 160 – 161) in a letter of the same year politely criticised Pearson. (I have not found his answer.)

Here is Pearson's statement:

Astronomers have been guilty of a considerable amount of circular reasoning. They start from the hypothesis that magnitude [brightness of a star] *is very closely related to parallax, and when the statistician shows that the* […] *parallaxes show no continuous relationship between parallax and magnitude, they turn around and say: <u>Yes, but our stars were selected because they had big proper motion</u>. They thereby screen entirely the fact that the fundamental hypothesis that the brighter stars are much the nearer as yet awaits statistical demonstration*.

Astronomers had started to doubt that fundamental hypothesis long before 1907.

Unlike statistics, the theory of errors has to do with constants, and Pearson (1920, p. 187) had apparently considered it rather one-sidedly. In any case, Eisenhart (1978, p. 382), supplementing Pearson's opinion cited above, stated:

When Karl Pearson and G. Udny Yule began to develop the mathematical theory of correlation in the 1890s, they found that much of the mathematical machinery that Gauss devised, […] *was immediately applicable in correlation analysis in spite of the fact that the aims of correlation analysis are the very antithesis of those of the theory of errors*.

Finally, Pearson (1928) studied Laplace's determination of the population of France by sampling. Laplace was the first to estimate the precision of such attempts and Pearson's paper was apparently his only incursion into population statistics. He indicated that Laplace's underlying model was faulty.

5. Pearson as seen by others

It is interesting to note the different views held of Pearson by other scientists. **Kolmogorov** (1947, p. 63):

The modern period in the development of mathematical statistics began with the fundamental work of English statisticians (Pearson, Student, Fisher) which appeared in the 1910s, 1920s and 1930s. Only in the contributions of the English School the application of probability theory to statistics ceased to be a collection of separate isolated problems and became a general theory of statistical testing of stochastic hypotheses [about the laws of distribution] *and of statistical estimation of parameters of these laws*.

Important work began to appear around 1902. And Slutsky (1912) contained a methodological description of the development of the correlation theory achieved by that school. At the time, Kolmogorov (1947, p. 64) had not duly appreciated Fisher:

The investigations made by Fisher, the founder of the modern British mathematical statistics, were not irreproachable from the standpoint of logic. The ensuing vagueness in his concepts was so considerable, that their just criticism led many scientists (*in the Soviet Union, Bernstein*) *to deny entirely the very direction of his research*.

A year later Kolmogorov (1948/2002, p. 68) criticized the Biometric school:

Notions held by the English statistical school about the logical structure of the theory of probability which underlies all the methods of mathematical statistics remained on the level of the eighteenth century.

Fisher (1922, p. 311) expressed similar criticisms as did Chuprov (Sheynin 1990/2011, p. 149). Chuprov also informed his correspondents that statisticians and mathematicians in mainland Europe (especially Markov) did not wish to recognize Pearson.

Here are some other opinions about Pearson.

Bernstein (1928, p. 228) discussed *a new cycle of problems in the theory of probability which comprises the theories of distribution and the general non-normal correlation* and turned to Pearson:

From the practical viewpoint the Pearsonian English school is occupying the most considerable place in this field. Pearson fulfilled an enormous work in managing statistics. He also has great theoretical merits, especially since he introduced a large number of new concepts and opened up practically important paths of scientific research. The justification and criticism of his ideas is one of the central problems of current mathematical statistics. Charlier and Chuprov, for example, achieved considerable success here whereas many other statisticians are continuing Pearson's practical work, definitely losing touch with probability theory.

Fisher, in a letter of 1946 (Edwards 1994, p. 100):

He was singularly unreceptive, and often antagonistic to contemporary advances made by others in [his] *field*. [Otherwise] *the work of Edgeworth and of Student, to name only two, would have borne fruit earlier*.

In about 1914 Fisher (Sheynin 1990/2011, p. 153) stated that Student, who by that time had published five papers in *Biometrika*. *Ist nicht ein Fachmann*. Fisher (1937, p. 306) also seriously objected to Pearson's view of maximum likelihood:

Plea of comparability [between the methods of moments and maximum likelihood] *is* […] *only an excuse for falsifying the comparison* […].

By that time, Pearson was dead, but ESP kept silent.

But there are also testimonies of a contrary nature. **Mahalanobis**, in a letter of 1936 (Ghosh 1994, p. 96):

I came in touch with [Pearson] *only for few months, but I have always looked upon him as my master, and myself, as one of his humble disciples*.

Newcomb, who had never been Pearson's student, wrote in a letter to him in 1903 (Sheynin 2002, p. 160):

You are the one living author whose production I nearly always read when I have time and get at them, and with whom I hold imaginary interviews while I am reading.

Hald (1998, p. 651) offered a reasonable general description of one aspect of the Biometric school:

Between 1892 and 1911 he [Pearson] *created his own kingdom of mathematical statistics and biometry in which he reigned supremely, defending its ever expanding frontiers against attacks*.

In 1956, **Fisher** (1990, p. 3) again criticized Pearson:

The terrible weakness of his mathematical and scientific work flowed from his incapacity in self-criticism, and his unwillingness to admit the possibility that he had anything to learn from others, even in biology, of which he knew very little. His mathematics, though always vigorous, were usually clumsy, and often misleading. In controversy, to which he was much addicted, he constantly showed himself without a sense of justice. In his dispute with Bateson on the validity of Mendelian inheritance he was the bull to a skilful matador. … His activities have a real place in the history of a greater movement.

I do not understand the *greater movement*. Anyway, Pearson paved the way for Fisher. And it is hardly possible to describe in a few lines the quality of mathematical work done during a few decades. Even if, for example, Pearson did not understand much in biology, he still achieved much in that science (ESP, vol. 28, p. 230), and in mathematics he apparently often managed by crude methods.

ESP (1936, p. 230) described **Bateson's** criticism of K. P:'s long article of the same year, 1901, and quoted his remark:

It is impossible to write of [it] *without expressing a sense of the extraordinary effort which has gone to its production and the ingenuity it displays*.

And in a letter to Pearson of 1902 Bateson (ESP, 1936, p. 204) stated:

I respect you as an honest man, and perhaps the ablest and hardest worker I have met, and I am determined not to take up a quarrel with you if I can help it. I have thought for a long time that you are probably the only Englishman I know at this moment whose first thought is to get at the truth in these problems. […]

6. Pearson and Soviet statisticians

In Russia, Chuprov and Slutsky defended Pearson's work against Markov's opposition (Sheynin 1990/2011, §§ 7.4 and 7.6). Tschuprov/Chuprov (1919, p. 133) wished to unite the direction of statistics in mainland Europe with biometry, but did not achieve real success.

Lenin's criticism of Pearson (§ 2) was in itself a sufficient cause of the negative Soviet attitude towards Pearson. Maria Smit's statement (1934, pp. 227 – 228) was its prime example (and a testament to her unthinkable ignorance): Pearson's curves are based

On a fetishism of numbers, their classification is only mathematical. Although he does not want to subdue the real world as ferociously as it was attempted by Gaus [!], *his system nevertheless only rests on a mathematical foundation and the real world cannot be studied on this basis at all*.

In 1939, Smit was elected corresponding member of the Soviet Academy of sciences. And she edited the statistical articles in the first few volumes of the first edition of the *Great Soviet Encyclopaedia* but we will never know the results of the subjectively honest work of that troglodyte … Recall also the blasphemous statement of Boiarsky et al in § 3.

The second edition of that *Encyclopaedia* (vol. 33, 1955) declared that Pearson advocated reactionary, pseudoscientific *<u>theories</u> of race and blood* and that Lenin *destructively* [how else?] criticized him.

However, the tone of the same item, *Pearson*, in the third edition (vol. 19, 1975/1978, p. 366 of its English edition) was quite different: he *considerably contributed to the development of mathematical statistics*, and Lenin had only criticized his *subjective-idealistic interpretation of scientific knowledge*. See Sheynin 1990/2011, § 16.2) for the Soviet attitude to *bourgeois statisticians* including Süssmilch!

7. Literature

For the bibliography of Pearson's writings see Morant et al (1939) and Merrington et al (1983). Many of his early papers are reprinted (Pearson 1948) and his manuscripts are kept at University College London. The best biography of Pearson is compiled by his son (ESP

1936 – 1938). Porter (2004 listed with my review of 2006), an ignoramus par excellence, is another biography, and many thoughtless authors will certainly quote him.

8. Egon Sharpe Pearson (1895 – 1980)

It is not amiss to add a few lines devoted to the son of Karl Pearson, especially since ESP likely became a statistician under the influence of his father.

He became Professor of statistics at University College London, in 1936 – 1966 edited *Biometrika* and is mostly remembered in connection with the Neyman – (E. S.) Pearson lemma of statistical hypothesis testing. Other fields of his work were the application of statistics in industry and history of statistics; he edited the book of his late father, Pearson (1978). His *Selected Papers* were published in 1966.

ESP was President of the Royal Statistical Society in 1955 – 1956 and Fellow of the Royal Society. I exchanged a few letters with him and can certainly say that he was a benevolent person.

Acknowledgement. Professor Joe Gani (whose death I most sincerely regret) suggested some useful adaptations and somewhat corrected my English.

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VI

Carl Friedrich Gauss

Statisticians of the Centuries. Editors, C. C. Heyde, E, Seneta.
New York, 2001, pp. 119 – 121

Gauss (1777 – 1855) shaped the treatment of observations into a practical tool. Various principles which he advocated became an integral part of statistics and his theory of errors remained a major focus of probability theory up to the 1930s.

Gauss was born on 30 April, 1777, in Brunswick, Germany, into a humble family, and attended a squalid school. At the age of ten, he became friendly with Martin Bartels, later a teacher of Lobachevsky. Bartels, an assistant schoolmaster in Gauss's school, studied mathematics together with Gauss and introduced him to influential friends.

From 1792 to 1806 Gauss was financially supported by the Duke of Brunswick and was thus able to graduate from college (1796) and Göttingen University (1798). He then returned to Brunswick and earned his doctorate from Helmstedt University (1799). Only in 1807 Gauss became director of the Göttingen Astronomical Observatory (completed in 1816) and his farther life was invariably connected with that observatory and the university of the same city. Gauss was twice married and had several children, but none became scientist. He died in Göttingen on 23 February 1855.

Gauss is regarded as one of the greatest mathematicians of all times. He deeply influenced the development of many branches of mathematics (e. g., algebra, differential geometry) and initiated the theory of numbers. He was an illustrious astronomer and geodesist, and together with Weber essentially contributed to the study of terrestrial magnetism. Gauss' importance for developing the mathematical foundation for the theory of relativity was overwhelming (Einstein, quoted by Dunnington p. 349 without an exact reference). His command of ancient languages was exceptional, and, until he discovered the possibility of constructing a regular 17-gon with a straightedge and compasses, he had remained undecided whether to pursue mathematics or philology. He also possessed an admirable style in his mother tongue.

Gauss was very slow in disseminating his findings many of which were published posthumously. He kept silent about his studies of the *anti-Euclidean* geometry, as he called it, but he (successfully) nominated Lobachevsky for Corresponding Membership of the Göttingen Royal Scientific Society. He was showered with honours from leading academies. In 1849, he became honorary citizen of Brunswick and Göttingen. He had no peers in science and remained isolated, partly because of his own disposition. He was reluctant to refer to other authors and did not befriend younger scholars [for example, Jacobi and Dirichlet). Gauss attached great importance to such problems as the relation of man to God, but thought that they were insoluble. He believed in enlightened monarchy, but in a letter wrote about the golden age to be expected in Hungary after the 1848 revolution.

As an astronomer, Gauss is best known for determining the orbits of the first minor planets (dwarf planets, as they are now called) from a scarce number of their observations and calculating their perturbations and for contributing to practical astronomy. He and Bessel independently originated a new stage in experimental science by introducing thorough examination of instruments. Gauss also detected the main systematic errors of angle measurements in geodesy and outlined means for eliminating their influence. For about eight years he directly participated in triangulating Hannover. After 1828, he continued to supervise that work which ended in 1844 and he alone performed all the calculations. His celebrated investigation of curved surfaces and study of conformal mapping were inspired by geodesy.

Gauss solved interesting problems in the theory of probability. In 1841, Weber described Gauss' opinion that, in applications, probability should be supplemented by *other knowledge* and that the theory of probability offers clues for life insurance and for determining the necessary number of jurors and witnesses. Gauss also studied the laws of infant mortality and for several years directed the widows' fund at Göttingen University.

The treatment of observations occupied Gauss at least from 1794 or 1795. He decided that redundant systems of physically independent linear equations should be solved according to the principle of least squares. He applied it in his astronomical and geodetic work and recommended it to his friends. In 1809 he published its justification. Issuing from ta postulate that the arithmetic mean of direct measurement of a constant should be assumed as its value and making use of the principle of maximum likelihood, he arrived at the normal distribution of observational errors as their only possible even and unimodal law. He also supposed a uniform prior distribution of errors which, however, was already implied by his postulate. He substantiated maximum likelihood by the principle of inverse probability. Gauss claimed to be the inventor of least squares although Legendre had introduced it publicly (without justification) in 1805. For him, priority invariably meant being first to discover.

In 1823 (supplemented in 1828) Gauss put forward a new substantiation of least squares. He pointed out that an integral measure of loss (more definitely, the principle of minimum variance) was preferable to maximum likelihood, and abandoned both his postulate and the uniqueness of the law of error. (The normal law still holds, more or less, on the strength of the central limit theorem.) In 1888, Bertrand nastily remarked that for small values of $|x|$ any even law

$$f(x) = a^2 - b^2x^2 \sim \alpha^2\exp(-\beta^2x^2).$$

Also in 1823, Gauss offered, for unimodal distributions, an inequality of the Bienaymé – Chebyshev type, and another one, for the fourth moment of errors, as well as the distribution-free formula for the empirical variance m^2 and for its own variance, varm^2. Owing to an elementary error, his varm^2 was wrong. First Helmert (1904), then

Kolmogorov et al (1947) corrected it. Gauss set high store by the formula for m^2 which provided an unbiased estimate of σ^2; however, in geodetic practice precision is characterized by its biased estimator m, whereas Helmert thought that only relative unbiasedness was important.

Gauss also estimated the precision of the estimators of the unknowns of the initial linear system and of linear functions of these. Partly owing to his apt notation, his method of solving normal equations by eliminating the unknowns one by one became standard. He also applied iterative processes, see Dedekind (1931), and introduced recursive least squares which mathematicians had not noticed until recently. In 1816, Gauss proved that, for normally distributed errors e_i the measure of precision $h = 1 \div \sqrt{2}\sigma$ was best estimated by $(e_1^2 + e_2^2 + \ldots + e_n^2)$ rather than by $(|e_1|^k + |e_2|^k + \ldots + |e_n|^k)$ for any other integer k.

Gauss's contribution to the treatment of observations somewhat extended by Helmert defined the state of the classical theory of errors. It seemed perfect, and geodesists hardly paid attention to statistics, whereas statisticians hardly studied Gauss and thus missed the opportunity to develop analysis of variance and regression with less effort. This situation did not begin to change until well into the 20th century.

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Later note. Concerning the second justification of least squares see my note in *Math. Scientist*, vol. 37, 2012, pp. 147 – 148.

VII

Johann Gregor Mendel

C. C. Heyde, E. Seneta, editors, *Statisticians of the Centuries.*
New York, 2001, pp. 190 – 193

Only in the 1930s Mendel (1822 – 1884) was definitively recognized as the originator of genetics. He was one of the first to apply statistical methods in biology.

Mendel was born in Heinzendorf, Austria (now, Hyncice, Czech Republic) into a peasant family of German – Czech origin. In 1834, as an able pupil of a village school, he moved to a local gymnasium. From 1838 he had to support himself by tutoring, and he graduated in 1840. In 1843, after a mental crises caused by straightened circumstances and an uncertain future, he completed a two-year course at the Olmuetz (Olomouc) Philosophical Institute whose curriculum included mathematics (with some combinatorial analysis) and physics.

Mendel was now entitled to study at a university, but instead, in 1843, he entered a monastery in Brunn (Brno). He thus freed himself from financial worries and found conditions for further study. He took the name Gregor adding it to his Christian name, Johann. In 1848 Mendel became curate, but his sensitivity hindered his duties and in 1849 the Abbot appointed him substitute gymnasium teacher of mathematics, Latin and Greek.

In 1850, nervous and lacking university education, he failed one of his examinations of teaching competence, and the monastery was advised to send him to Vienna. Mendel indeed studied mathematics and the natural sciences there (1851 – 1853). In 1854 he was appointed teacher of physics, zoology and botany at the Realschule in Brno and continued to carry out some ecclesiastic duties. In 1856 he failed his second teaching examination, this time only because of bad health but remained a highly respected substitute teacher. In 1868 Mendel was elected Abbot and gave up teaching. Ultimately he died in 1848 of a kidney disease and cardiac hypertrophy.

Mendel held liberal views and in 1848 he co-signed a petition for granting civil rights to members of religious orders. From 1875 he objected to the unjust (in his opinion) taxation of the monastery.

During his life, Mendel participated in local agricultural affairs. His free advice on growing plants and fruit trees and on beekeeping (an occupation in which he achieved practical success if not the desired confirmation of his theory in the animal kingdom) was greatly appreciated and his varieties of plants were grown locally for many decades. He was an active member of several provincial and national agricultural societies and a founding member of the Austrian Meteorological Society.

In 1857 he began recording meteorological data, promoted weather forecasting for farmers and correctly explained the origin of tornadoes in 1871, although at the time it remained unnoticed. Between 1856 and 1863, Mendel studied the hybridization of peas, first testing 34 of their varieties for the constancy of traits and selected 22 of them. He always examined a large number of plants to eliminate *chance effects* and thoroughly planned his experiments.

Suppose A and a are the dominant and recessive alleles (possible genes) at a single locus of a plant the phenotype (the appearance) of whose seeds (say, a round or angular pea) depends on the genetic composition at this locus. The possible genetic compositions (genotypes) are thus AA, Aa and aa, but Aa seeds have the same appearance as AA, and only the double-recessive seeds aa have a different phenotype. If we suppose that alleles A and a are equally represented in the genetic pool, a random union of these forming the next generation genotypes can be represented by

$$(A + a)^2 = AA + 2Aa + aa,$$

so that the genotypes AA, Aa and aa are as 1:2:1, but the two distinguishable phenotypes are as 3:1. This reasoning exemplifies *Mendel's first law* (of independent assortment of alleles).

Now suppose that another locus with alleles B, b determines another phenotypic feature (say, yellow or green colour in pees) with b recessive. Then the structure of the genetic material before the random union can be represented by

$$(A + a)(B + b) = AB + aB + Ab + ab,$$

so that the four kinds of gametes exist in equal proportions. This reasoning exemplifies *Mendel's second law* (of independent assortment). A random union of the gametes then gives a genotypic structure according to the combinations

$$(AB + aB + Ab + ab)^2 = AABB, AaBB, AABb, AaBb, AAbb, Aabb, aaBB, aaBb, aabb$$

which are as 1:2:2:4:1:2:1:2:1 but the distinct phenotypes (involving two physical features) are as 9:3:3:1. Mendel wrote out such segregation ratios even for seven pairs of different traits.

In the 1900s and finally in the 1930s the work of Mendel was recognized as marking the beginnings of genetics. And he was one of the first to apply the statistical method, to use algebraic notation and elements of the combinatorial analysis in biology. At the beginning of the 19th century, Alexander Humboldt initiated another branch of biology (of botany), the geography of plants, intrinsically connected with statistics. Other predecessors of Mendel were Alphonce De Candolle (also in geography of plants) and even Maupertuis, see Glass (1959).

Mendel could not have properly understood all the aspects of his findings, but he justified the existence of discrete hereditary factors (later terms: genes, alleles, gametes) and discovered the principles of their random segregation and recombination. He aimed at practical results, but a simultaneous study of heredity could have well been at the back of his mind. Now we know that individuals of the same species usually have different sets of genes so that their offspring are (intraspecific) hybrids. Therefore, Mendel's experiments with hybridization were crucial for the latter purpose.

After acquainting himself, not later then in 1863, with Darwin's *Origin of Species*, he stated in 1866 that research such as his own was important for *the history of the evolution of organic forms*. Indeed, Darwin himself felt that he had not adequately explained evolution, and Mendel stated that something was lacking in his system.

Darwin did not hear about Mendel and would hardly understand his contemporary. He never used mathematical language and even wrote *elongated* rather than *isosceles* triangles. Indeed, at least up to the turn of the century biologists did not grasp Mendel's work; no wonder that his posthumous papers at the monastery were burned. Biometricians had not recognized Mendel either. Even in 1930 Pearson considered it a barely proven theory. They were interested in measuring correlation between parent and offspring with regard to some trait rather than in studying the theory of heredity. However, according to a recent study (Magnello 1998) in 1909 Pearson suggested a synthesis of biometry and Mendelism. In 1926 Bernstein proved that under wide assumptions the Galton law of inheritance of quantitative traits was a corollary of the Mendelian laws (Kolmogorov 1938).

The trustworthiness of Mendel's experiments had been questioned. Fisher concluded that Mendel had correctly described their layout, but their data were biased. Van der Waerden inferred that Mendel had followed a sequential procedure, so that Fisher was only partly in the right and that Mendel was honest. Later authors noted that some biological facts (e. g., the failure of some seeds to germinate) even stronger exonerated Mendel. And indirect evidence, his meteorological work in the first instance, indicates that Mendel meticulously recorded his observations.

By 1935, the Soviet Union became a leading centre of Mendelian research. Then, however, genetics was called an idealistic science contrary to dialectical materialism and was mercilessly rooted out. Even Kolmogorov was criticised for his defence of its principles. The situation had not changed until the 1960s.

Later addendum

1. Heyde informed me that a biologist edited my description of Mendel's experiments. But at least nowadays three Mendelian laws are recognized: dominance and uniformity, segregation of genes; and independent assortment.

2. Bernstein (1922) should be studied as well.

3. See the main additions to Darwin's understanding of the causes of evolution (Sheynin 2017, § 10.8.2): a brief description of the ideas of De Vries and Johanssen).

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VIII

S. N. Bernstein

Chebyshev's influence on the development of mathematics

Math. Scientist, vol. 26, 2001, pp. 63 – 73
Translated by O. Sheynin
from *Uchenye Zapiski Mosk. Gos. Univ*., vol. 91, 1947, pp. 35 – 45

Translator's foreword

Pafnuty Lvovich Chebyshev (1821 – 1894) and Lobachevsky were the greatest Russian mathematicians of the 19th century. Chebyshev contributed to many branches of mathematics (not to mention mechanics): integral calculus, approximation of functions by polynomials, constructive theory of functions, theory of orthogonal polynomials (these items belong to mathematical analysis), number theory (distribution of prime numbers, Diophantine approximations), the theory of probability (the Bienaymé – Chebyshev inequality, limit theorems), and certainly the creation of the St. Petersburg mathematical school which included Markov and Liapunov.

Chebyshev maintained close ties with European scholars and was elected to the most prestigious foreign academies of sciences. He published his papers in Russian or French. Bernstein did not provide an account of the history of the St. Petersburg school, although he previously devoted a paper (Bernstein 1940) to its work in probability theory. I point out that in accordance with Chebyshev's interests the school had not joined in the development of mathematical analysis on a more abstract level, which was then successfully taking place in Europe.

Chebyshev, in spite of his splendid analytical talent, was a pathological conservative (Novikov 2002, p. 330).

But (Tikhomandritsky 1898, p. iv) in 1887 Chebyshev indicated that *the entire theory of probability should be reconstructed*. No details are known.

Sergei Natanovich Bernstein (1880 – 1968) was a leading Soviet mathematician (Aleksandrov et al, 1969) and a foreign member of the Paris Academy of Sciences. He achieved fundamental results in mathematical analysis and the theory of probability with applications to statistics, physics and biology. Four volumes of his works were published in Russian in 1952 – 1964.

I am grateful to Professor Gani for his help in rendering some of Bernstein's expressions more intelligible.

No matter how considerable are the increasingly important achievements of contemporary Russian mathematics, and how magnificent is its future, two important names in the history of our science will always occupy a place of special honour. Lobachevsky and Chebyshev are the two poles of mathematical thought. They were the first to reveal to the world, almost at the same time, the exceptional power, originality and versatility of the Russian mathematical genius. Chebyshev was younger, but for him the way to fame was happier and shorter than the arduous road of his older colleague who was understood and universally recognized only posthumously[1].

Unable as I am to offer any comprehensive review of Chebyshev's most important works, I only try to sketch his striking mathematical personality and the main features and aspirations of his scientific work. The exceptional diversity of the subject matter of Chebyshev's contributions becomes evident just by glancing at their titles. In addition to works on prime numbers and continued fractions, we find papers devoted to cogwheels, the compilation of geographical maps etc. He himself, in his own work [10], offers us a clue which reveals the unity in this diversity. I quote two passages from this contribution which was written for the grand meeting at the St. Petersburg University in 1896.

The accord of theory and practice provides a most favourable result. Not only the latter benefits from it, but the sciences themselves develop under the influence of practice. It reveals new subjects for study or new aspects of the subjects known for a long time. The great geometers of the last three centuries have brought mathematical sciences to a high degree of development. Practice, however, reveals that these sciences are in many respects imperfect. Practice offers essentially new problems, and thus challenges the search for quite new methods.

Chebyshev illustrated this idea by examples and concluded:

A large proportion of practical questions lead to problems in maximal and minimal magnitudes that are quite new for science. Only by solving these we can satisfy the demands of practice which always seeks the best, the most advantageous.

In this [last mentioned above] work, Chebyshev indicates how to solve an important cartographic problem, that of finding the best method of drafting a map of a given state to obtain the least possible differences between the scales of its various parts. Chebyshev also applied his method to the European part of Russia. It proved theoretically impossible to make variations in the scale over the entire territory less than 1 *mm* for 5 *cm* [this is not definite at all] and in his proposed map this minimal error was not exceeded.

In 1853, he laid the foundations of the theory of best approximation in his celebrated memoir [9]. And for him the unification of theory and practice, which he advocated in 1856, had a quite definite meaning. It was undoubtedly the most distinctive feature of the period of his complete scientific maturity. Beginning roughly when he was 30 or 35, the directions of his work were determined by the aspirations and peculiarities of his own mathematical genius. From then, the principle enunciated above directed Chebyshev's entire scientific work. He confidently progressed to his seemingly diverse aims, which were in fact closely connected.

Chebyshev's work of the previous period cannot be contained between the same boundaries. But it is all the more interesting to dwell on this period of research, when his mathematical individuality was being formed, was developing and maturing as Pafnuty Lvovich Chebyshev gradually became the immortal Chebyshev.

According to his biographer, Professor Vasiliev, Chebyshev, by his own account, had even in early childhood taken an interest in all sorts of mechanical toys which he enthusiastically built with his own hands. Later, during his very first lessons in geometry, he noted the connection between it and his favourite toys, and set to study this subject with special ardour. Such was his experimental and utilitarian approach to geometry, whose axioms and theorems without further experiments provided additional properties of various figures which interested him and some of which he had possibly foreseen by himself. It is easy to imagine that Chebyshev was therefore least inclined to doubt, as Lobachevsky did, the truth of the Euclidean geometry.

Mathematics conformed to his natural talent to such an extent that, when barely sixteen, he entered the physical-mathematical faculty of Moscow University. Already in 1838, while advancing to his second year of study, he compiled his first work, *On the calculation of the roots of equations*. There, he specified and modified the known classical methods of solving equations approximately. It is interesting to note, that, although only a freshman, he confidently and rather polemically criticised the mistakes made in the manuals recommended to the students. His teacher, Professor Brashman, foresaw his brilliant future, but, owing perhaps to his criticisms, Chebyshev was awarded a silver rather than a gold medal.

Chebyshev’s childhood and student years were spent in comfortable ease. However, at the age of twenty, because of the declining financial situation of his father, a landowner in Kaluga province, he lost the material support he had previously enjoyed. In spite of his needs, he did not start working. He did not orient himself towards practical engineering, a subject close to his heart and promising the earliest termination of his regrettable circumstances. Nor did he waste time on trifles.

Without allowing himself to be diverted, he persisted in studying the works of the great mathematicians, Euler, Legendre, Abel, and other classics. During this critical period of his life, the young Chebyshev, drawing on his own experience, brilliantly solved for the fist time an extremal problem, that of determining the best possible way of applying the natural talent which he clearly perceived in himself. We see that Chebyshev had no doubt that in the first place he was a mathematician rather than a technician. He understood that only mathematics could give him a clue to the solution of all the problems which interested him, not merely of those restricted to practical applications. Thus during three or four years which were especially difficult from the financial point of view, Chebyshev, with exceptional persistence assimilated the rich heritage of the past, and restrained the aspiration for independent work so basic to his nature.

But of course his idea of mathematics had nothing in common with the science defined by Bertrand Russell, which knows not what it is speaking about, nor whether what it is saying is true[2]. Chebyshev’s mind, clear, exact and concrete, was not inclined towards philosophizing[3]. On the basis of all his scientific work, we may describe his attitude to mathematics as intuitively materialistic.

He approached the study of his predecessors' contributions like a builder, selecting from the vast arsenal of tools and materials all that he found reliable and solid, and whatever justified itself by the irreproachable precision of the obtained results. Like Isaac Newton, he considered mathematics as a science of magnitudes having obvious properties, with concrete sense and meaning. Each relation between mathematical symbols represented the corresponding relation between real objects. A mathematical argument was equivalent to an experiment of irreproachable precision which was repeated an indefinite number of times[4] and led to logically and materially correct conclusions. Such, roughly speaking, were Chebyshev's requirements on mathematics.

His first short work appeared in 1843 [1][5]. Written in the spirit of the classics not typical for him, it testifies that Chebyshev declined, not without hesitation, to use the richest source of formal identities obtained by introducing complex parameters into definite integrals. The meaning and legitimacy of the pertinent transformations were dubious and easily led to unaccountable contradictions. In spite of all its seductiveness, this tool, unreliable at the time, was inadmissible for him, and he did not use it again.

The next paper of 1844 [2] shows that Chebyshev carefully studied Cauchy's theory of functions of a complex variable. Nevertheless, justly indicating some inaccuracy in Cauchy's reasoning, he felt himself unable to regard this theory as a working instrument. Having little inclination for general studies without a concrete aim, Chebyshev did not therefore embark on a critical justification of mathematical analysis in its entirety, a goal that characterized West European mathematics during the second half of the 19th century. Trusting in his own ingenuity, Chebyshev preferred to restrict his tools, and only made use of comparatively elementary means. Except for algebra, which he later enriched by new methods, in the analysis of infinitesimals he used only the most simple functions and methods. It is indeed surprising that he was able to erect great buildings with such a meagre set of instruments.

In 1845 he published his Master's dissertation [3] and defended it in 1846 at Moscow University. Its main interest is not in its new theorems, but in its aim, which consisted in freeing the theory of probability from transcendental methods and converting it into an exact mathematical science[6]. Probability was undoubtedly one of the fields of mathematics that immediately attracted Chebyshev's special attention. Previously, the formulation of its problems and the methods applied for their solution often did not comply with the requirements of mathematical rigour[7].

Although in the second half of the 19th century the most eminent West European mathematicians had begun a fundamental revision of mathematical analysis, they barely took notice of probability[8]. It is therefore remarkable that Chebyshev perceived the importance of the theory's practical applications and attempted to base it on a reliable foundation. For the first time ever, he offered the now generally accepted definition of this mathematical discipline[9]:

The theory of probability has as its object the calculation of the probability of an event, given its relation with events whose probabilities are known. [1845/1851, p. 29.]

He added that

We consider it approximately certain that events will, or will not occur if their probabilities only slightly differ from 1 or 0.

An appendix to his dissertation published in 1846 [4] deserves special attention. A new proof of the well-known Poisson theorem[1] was given and its necessity justified:

Clever though is the method used by the illustrious geometer, it does not provide the bound of the possible error of his approximate analysis. Thus, because of the uncertainty of the magnitude of the error, his proof does not have the appropriate rigour.

Chebyshev's original demonstration was based on the solution of a quite elementary problem. It was the first attempted application of the extremal method and furnished a very precise estimate of the error under consideration.

Thus, for Chebyshev, the law of large numbers, like all the limit theorems of the theory of probability, made sense not as an abstract characteristic of some infinite number of trials or objects[11]. It was rather an approximation to quantitative relations observed in sufficiently large finite aggregates of concrete random objects, whose properties were precisely described in terms of the theory. In addition, it was especially important for him to specify the word *sufficiently* by proper inequalities. Chebyshev's predecessors, in particular Poisson, had formulated the law of large numbers too generally and vaguely. This led to many misunderstandings which for a long time compromised the theory of probability[12]. Chebyshev, however, completely eliminated them. Equally vague were the conditions for applying the limit theorem for the sums of independent random variables. They stated that, in the limit, the Gauss – Laplace law, now usually referred to as the normal law of distribution, was appropriate for such sums when the number of terms tended to infinity.

The propositions put forward by Chebyshev in 1845 – 1846 were very restrictive. Indeed, he unconditionally required estimates but had not yet discovered the essentially new methods which later led him to a complete solution of both these problems[14].

One of the main aims of Chebyshev's future mathematical work is thus foreshadowed: to establish the most general but undoubtedly reliable and clearly formulated conditions for the applicability, in the above sense, of the law of large numbers and the Gauss – Laplace limit law of distribution.

Leaving aside the solution of this second problem which he completely achieved only towards the end of his life, Chebyshev then discovered the tool necessary for his purpose, namely, continued fractions. In his hands, they became an inexhaustible source of formal transformations and of the ensuing inequalities. While investigating integration in a closed form, the great Norwegian geometer Abel, the creator of the theory of algebraic integrals, had found a remarkable application for continued fractions However, his early death prevented him from proving one of the main theorems which led to the result of integration of the hyperelliptic integral

$$\int \frac{\rho}{\sqrt{R}}\,dx = A \ln \frac{p+q\sqrt{R}}{p-q\sqrt{R}}$$

in the required form.

It is clear that such a concrete problem would have interested Chebyshev. Indeed, he chose it as the subject of his next work [5] which he defended as a dissertation [*pro venia legendi*] in 1847 to obtain a professorship at the St. Petersburg University. In addition to proving the above theorem, Chebyshev essentially improved on Abel's method of continued fractions. In particular, he showed that the reducibility of elliptic integrals depended on whether it was possible to determine a constant *A* for which the integral

$$\int \frac{(x+A)dx}{\sqrt{x^4+\alpha x^3+\beta x^2+\gamma x+\delta}} \tag{1}$$

was represented by logarithms of algebraic functions. Chebyshev solved this problem for the case in which the coefficients of the polynomial were rational, and its roots were not represented by square roots, i. e., could not have been constructed by straightedge and compass.

This problem has not yet been solved in the general case, for any set of coefficients in the square root of (1). It is known that, once the constant *A* is approximately chosen, it is necessary and sufficient for the reducibility of an elliptic integral that that square root be expandable in a periodic continued fraction. However, except for the Chebyshev case, which E. I. Zolotarev had somewhat generalized, the prior determination of whether the relevant continued fraction is periodic, continues to present insurmountable difficulties.

This work of Chebyshev and several of his supplementary papers are characteristic of him in that he was not afraid of a difficult problem if he clearly perceived its meaning and significance both for mathematics and its applications. He was convinced that the more difficult was a precisely and naturally posed problem, the more important and fruitful will be the methods invented for its solution and later used to solve other problems of analysis.

Witness the continued fractions which became essential in Chebyshev's later work. When necessary, he narrowed the scope of his problem to obtain its final constructive solution. For him, general theories were never ends in themselves, but rather important insofar as they opened up new concrete facts or relations. Even if the particular problem that he was solving influenced the development of the general theory of algebraic or abelian integrals far less than the investigations of Abel, and later Riemann and Weierstrass, his results still remain beyond the reach of the general methods of this theory.

Chebyshev had an exceptional gift for obtaining unexpected new results by a profound analysis of simple elementary facts, to which most mathematicians pay no heed. This is evident with the greatest clarity in his later remarkable works on the theory of numbers, i. e., in his Doctoral dissertation of 1849 [6] supplemented by the memoir [7], and in contribution [8] of 1851. By empirically studying tables of prime numbers Legendre obtained an approximate formula for the number $\pi(x)$ of primes not exceeding a given value x:

$$\pi(x) \approx \frac{x}{\ln x - 1.08366}. \qquad (2)$$

However, during Chebyshev's time, the theoretical problem of the increase of $\pi(x)$ with x remained a tempting mystery that resisted the efforts of the greatest mathematicians. Several formal relations connecting the total numbers of all natural integers and all primes were known, and the most remarkable was Euler's celebrated identity

$$\varsigma(s) = \prod_{p=2}^{\infty} [1 - (\frac{1}{p^s})^{-1}] = \sum_{n=1}^{\infty} n^{-s} \qquad (3)$$

which determined the well-known function $\varsigma(s)$. Here, n represents any integer, p is any prime number, and $s > 1$. This identity potentially describes all the properties of the infinite number of primes. In particular, it leads to the following corollary: the series

$$\sum_{2}^{\infty} \frac{1}{p^s}$$

in which the sum is only taken over all the prime numbers p, behaves like the same series summed over all the integers, i. e., converges if $s > 1$ and diverges if $s = 1$.

For Chebyshev, however, convergence only made sense if its rate could be precisely estimated. He carried out a simple estimation of the ever worsening convergence of this series as $s \to 1$ for the sequence of all integers as compared with the sequence of only prime numbers. He used an elementary but remarkably profound application of the Euler identity. This enabled him to determine for the fist time and with full mathematical rigour that the function $\pi(x)$ obeys a certain inequality for infinitely many values of x. He then readily inferred that, if the asymptotic value of $\pi(x)$ can be represented by elementary functions to within $x/(\ln x)^n$ with an arbitrarily large n, it will coincide with the integral logarithm

$$\int_2^x \frac{dx}{\ln x} \qquad (4)$$

rather than with the Legendre formula.

Chebyshev's next work [8] is still more original and elementary. It includes an absolutely new formal identity that reflects the properties of not only the totality of all primes, but of finite parts of them as well.

Owing to this identity and its amazing clever application, Chebyshev proved the Bertrand hypothesis or postulate which states that a prime number is always present between integers *a* and 2*a* – 2. Later Chebyshev added but little to these two fundamental memoirs which marked the beginning of a new epoch in the theory of prime numbers. It is natural that these memoirs at once advanced Chebyshev in the eyes of the whole world to the rank of the best contemporary mathematicians.

Thus, in 1851 – 1852, the period of Chebyshev's formative years ended. He reviewed the entire mathematics of his time and became aware of the power and nature of his own genius. Relying now on himself only, he went his own way. He did not turn aside a single step under the influence of the works of other mathematicians which he preferred not to read[15]. He still supplemented his investigations in some directions in the number theory and algebraic integrals and every now and then wrote short notes on more or less random subjects.

But these efforts are minor as compared with the solution of extremal problems of the best approximation (in one or another sense) connected with the theory of mechanisms, with the interpolation mostly by the method of least squares and with the proof of the two main laws of the theory of probability. All these fundamentally important results are united mostly by the general methods [tools] used in these investigations, namely continuous fractions, whose properties were discovered while solving definite concrete problems. The lack of a systematic exposition of the theory of algebraic continuous fractions created by Chebyshev himself makes the reading of his memoirs difficult.

Chebyshev's second period began in 1853 [9], with a work which we have already mentioned. Although unsurpassed in the richness and novelty of its mathematical ideas, it does not regrettably contain the promised mechanical applications. Its concluding words are:

In the next sections we shall show the application of the formulas derived to determine the elements of the parallelograms which satisfy the conditions under which the precision of the motion of these mechanisms is greatest.

At the time, these sections were apparently not compiled in a form which satisfied Chebyshev. Indeed, they were not found in his posthumous manuscripts either, and it was only in 1861 that the first short note of an applied nature [12] appeared, and followed, when Chebyshev reached the age of 50, by a series of remarkable practical works on mechanisms for which he spared no expense and to which he devoted much time. Here, Chebyshev's theoretical research found a brilliant application, especially in the theory of mechanisms consisting of three elements. In particular, it enabled him to construct a mechanism for transforming the continuous circular motion of a wheel into a rectilinear oscillatory motion. Its precision was such that it was impossible to notice the deviation from rectilinearity by naked eye. For a segment of 20 *cm* the error did not exceed 1 *mm*. True, it was known that, theoretically speaking, the Lipkin – Poncelet inverter[16] which consisted of seven elements and was highly praised at the time by Chebyshev, precisely transformed circular into rectilinear motion. However, it could not work when the wheel was moving continuously in the same direction. Moreover, because of the large number of its parts, it was in practice much less precise than the Chebyshev mechanism. As testified by technicians, the unavoidable additional error connected with the used material, appreciably increased with the number of its elements.

Chebyshev's main mechanical contributions appeared at a later period, after 1870, which goes to show that he first attempted to improve his theory. In 1859, appeared his fundamental memoir [11], the lengthiest of his works. There, he proved the main general theorem on the necessary and sufficient conditions for a function $F(x)$ of a given kind with n parameters $p_1, p_2, \ldots, p_n$ to deviate minimally from zero. Chebyshev applied this theorem to solve three main algebraic problems whose importance it is difficult to overestimate: to determine a function minimally deviating from zero over the interval $[-h, h]$ and belonging to the classes

1. $x^n + p_1x^{n-1} + \ldots + p^n$
2. $(x^n + p_1x^{n-1} + \ldots + p^n)/R(x)$
where $R(x)$ is a given polynomial of a degree $m < n$ not vanishing over the given interval

3. $(x^m + A_1x^{m-1} + \ldots + A^m) + \dfrac{p_1x^{m-1} + p_2x^{m-2} + \ldots + p_m}{p_{m+1}x^{n-m} + \ldots + p_nx + 1}$

where $A_1, \ldots, A_m$ are given numbers.

The solution of the first problem, for example, for $h = 2$, is the polynomial

$$2\cos n\arccos(x/2) = x^n - nx^{n-2} + \ldots + (-1)^k \frac{n(n-k-1)!\,x^{n-2k}}{k!(n-2k)!} + \ldots, \quad (5)$$

whose modulus over the interval $[-2, 2]$ does not exceed 2; any other polynomial of this kind exceeds value 2 at some points in this interval.

It is interesting to note that in his first [relevant] publication of 1853 Chebyshev, in contrast to the approach in his later contribution of 1859, does not use continuous fractions. He obtains a certain simple differential equation whose later solution and investigation by other authors in more involved cases proved to be more fruitful than the method of continuous fractions. Chebyshev later solved a few more important particular problems, and a careful analysis of his concrete formulas served as a starting point for many subsequent generalizations.

In Chebyshev's works, the main significance of continued fractions is that they enabled him to create a general theory of orthogonal polynomials which led to the least (weighted in one or another way) mean square deviation from zero. It is hardly necessary to stress the exceptional importance of this discovery for every branch of analysis. Chebyshev was first led to it by solving the problem of interpolation by the method of least squares. The practical significance of his results in this field is well known. In particular, Chebyshev's memoir of 1875 [14] where, for the most important practical case of constant weight he provided an elegant expression for the needed polynomials. In the limit they coincided with the classical expression of the Legendre polynomials by derivatives of the *n*-th order. Once more, we see how concrete practical problems constantly nourished and directed Chebyshev's mathematical work.

To Chebyshev, the most profound investigation of the properties of continued fractions became essential for the solution of the second of the abovementioned problems of probability theory. The solution of the first of these, i. e., the determination of the most general conditions for the applicability of the law of large numbers to independent variables published in 1867 [13], is generally known. His strikingly simple proof is based on the same idea as in the second problem, that is, on the use of the consecutive moments of random variables for deriving the most precise bounds for the probability that the sought magnitude lies within some given interval. This study was one of the main aims of Chebyshev's life. It led him to investigate entirely new problems of definite integrals and is interwoven with the discovery of remarkable practical formulas for approximate quadratures.

Chebyshev's article [15] appeared only in March 1887. There, following the results of his previous work on continuous fractions and definite integrals, he proved the second limit theorem of probability theory. It must be clear that a mathematical genius of Chebyshev's calibre could not have failed to influence greatly the further development of mathematics and to impress all mathematicians, even those with interests remote from his own.

I only touch on four fields in which Chebyshev's ideas have imparted fundamentally new directions, namely the theory of mechanisms, function theory, the theory of probability and number theory. We should first note that none of his closest students or later followers inherited Chebyshev's peculiar harmonious accord of theory and practice. His theoretical mathematical research was later assimilated and developed far better than his practical investigations.

Professor Artobolevsky[17] reports on what was done after Chebyshev in the theory of mechanisms and mentions the inexhaustible possibilities in this area resulting from a fuller application of his technical heritage. I only remark that the theoretical problems of best approximation solved by Chebyshev have recently also found a direct application in electrical engineering.

Even during Chedbyshev's lifetime the most prominent representatives of the St. Petersburg mathematical school continued his theoretical investigations in many directions. In particular, Zolotarev and the brothers A. A. and V. A. Markov solved a number of very important algebraic problems about functions with minimal deviation from zero. Thus, A. A. Markov [17], while solving a problem posed by Mendeleev, showed that if there existed at least one point belonging to a segment of length L at which the derivative of a polynomial of order n attains the value l, then the polynomial cannot deviate from zero on this segment by less than $L/2n^2$ while at the same time possibly not exceeding this value[18].

A similar theorem due to V. A. Markov for the case of a derivative of any given order and the later generalization of these theorems proved very useful in the general theory of functions of a real variable.

I only mention the following proposition of the same type which was proved later. Let

$$f(x) = \sum_{k=0}^{\infty} A_k \cos(p_k x + \alpha_k) \qquad (6)$$

be some almost periodic function. If $0 \le p_k \le p$ for all k, and there exists at least one point x_0 at which the derivative of the s-th order $f^{(s)}(x_0) = 1$, then the deviation of $f(x)$ from zero over the entire real axis will not be less than $1/p^s$. Moreover, this deviation will not exceed $1/p^s$ only if

$$f(x) = \frac{\cos(px + \alpha)}{p^s}. \qquad (7)$$

West European mathematicians only took due notice of the Chebyshev theory of best approximation after Weierstrass had discovered the fundamental theorem which stated that any function $f(x)$, continuous over a given range, can be arbitrarily closely approximated there by an appropriate polynomial. If $E_n f(x)$ is the best approximation of the function $f(x)$ by polynomials of order n, then the property of $f(x)$ according to which

$$\lim E_n f(x) = 0 \text{ as } n \to \infty \qquad (8)$$

can be taken as a definition of a continuous function $f(x)$.

Thus, a new, Chebyshev constructive analytic direction of function theory had originated at the beginning of the 20th century. Many West European and Russian mathematicians after Borel and de la Vallée Poussin have participated in its development. In particular, beginning in 1912, Kharkov University became one of the main centres where Chebyshev's ideas in the fields of function theory and somewhat later of probability were being developed.

Without dwelling in detail on the numerous achievements of constructive function theory, which are also connected with the Chebyshev orthogonal polynomials, the problem of moments and the formulas for approximate quadratures, I consider it necessary to mention the outstanding work done in this field by N. I. Achieser. During the ten years before 1941, he successfully led, with the assistance of M. G. Krein, the Kharkov school of mathematical analysis.

Another area where Chebyshev's works were of basic importance, is the theory of probability. The most outstanding exponent of Chebyshev's ideas who shaped them with greatest skill in his classical course [18] was A. A. Markov. In many directions he went much further than his teacher and made up some deficiencies in Chebyshev's celebrated proof of the limit theorem for sums of independent random variables.

While continuing his investigations based on the same method of moments, Markov widely expanded the boundaries of probability theory by introducing classes of dependent random variables, which were important in applications. His most considerable works published at the beginning of the 20th century were devoted to his theory of chains of dependent random variables, namely Markov chains. They are methodologically very important as the natural stochastic modification of the concept of successive deterministic phenomena. No wonder that Markov chains have come to be used in various fields of natural science, especially physics.

During the twenty years after Markov's death [in 1922, 27 years rather than 20] the development and generalization of his ideas in various directions have occupied a central place in probability theory. This subject, however, will be adequately covered by Kolmogorov [16], the author of related works of outstanding importance. And I should like to indicate only one more fundamental contribution made by another celebrated student of Chebyshev, A. M. Liapunov, one of the greatest mathematicians of the end of the 19th and the beginning of the 20th century.

Accepting Chebyshev's main principles, he introduced into probability one of its most powerful methods, that of characteristic functions, as it is now called. Chebyshev had justly denounced its application in his time as not satisfying the requirements of mathematical rigour. However, imparting adequate rigour to this method, Liapunov proved the Chebyshev limit theorem and determined the error of the limiting formula for a finite number of terms under even more general conditions than Markov's. At present, it is known, however, that the most general form of the limit theorem can be derived from either set of conditions. The Liapunov method of characteristic functions was widely extended and applied to a number of other problems. Regrettably, however, his basic investigation, far more important than its later technical generalisations and simplifications achieved by other authors, is underrated not only in foreign literature, but also partly in the Soviet Union.

I am unable to discuss the theory of primes for which Chebyshev's research signified the beginning of a new epoch. Fortunately, I can report that by the end of this year the Academy of Sciences will publish a collection of articles in two books devoted to the analysis of Chebyshev's scientific heritage[19]. Hopefully, all problems touched upon here will be fuller treated there.

I conclusion, allow me to express my desire that our young mathematicians, following Chebyshev's example, will never separate general theoretical research from concrete facts.

Notes

1. To some extent Lobachevsky himself was the cause of his *arduous road*: he had not explained the aim of his geometry.

2. *The science defined by Bertrand Russell* conformed to the most general definition of mathematics: introduction and study of ever more abstract systems which possibly have no prototype in the real world.

3. See at least a non-positive comment by V. E. Prudnikov (Sheynin 2017, p. 328, Note 1). For my part, I agree only with a partly anti-philosophical attitude.

4. Why repeat an irreproachable experiment?

5. Above, the author mentioned the really first work of Chebyshev.

6. See also Sheynin (2017, p. 217). Chebyshev's contribution was not a proper gymnasium textbook, as was required by the proper educational authority: his reasoning was necessarily burdensome. For that matter, a general survey of probability theory would have been more useful.

7. Of course not! Beginning with Laplace, the theory of probability had been developing as a branch of applied mathematics [ii, § 4].

8. Indeed, *barely*, see Note 7.

9. Somewhat later Boole (1851/1952, p. 251) offered the same definition for *propositions* instead of *events*.

10. The Poisson theorem was forgotten. It was Bortkiewicz and then Newcomb who picked it up (Sheynin 2017, p. 249).

11. *Abstract characteristic* meant existence theorem. At the end of his paper the author highly praised the Weierstrass famous existence theorem.

12. Poisson followed Laplace in that he also considered probability theory as a branch of applied mathematics, but who had *compromised* probability?

13. The author never applied the term *central limit theorem* and described its proof by Russian mathematicians rather vaguely. These deficiencies were possibly occasioned by inadequate, in 1947, knowledge of probability theory by his readers. See a proper description of this subject by Kolmogorov (Chebyshev, *Polnoe Sobranie* …, vol. 3, 1948, pp. 404 – 409).

14. The author mentioned Markov and Liapunov at the end of his paper.

15. If the author was not mistaken, Chebyshev likely missed something essential.

16. L. I. Lipkin was Chebyshev's student. He was a Jew and in 1870 Chebyshev managed to secure for him the right to live in Petersburg and to sit for his Master's examination (Prudnikov 1964, p. 84).

17. Artobolevsky's article is published in the same collection as mine. S. N. Bernstein.

18. Here is Markov's result. Given, polynomial $\boldsymbol{f}(x) = p_0x_n + \ldots + p_{n-1}x + p_n$, such that $|\boldsymbol{f}(x)| \le L$ for $a \le x \le b$. Then for that same segment $[a, b]$,
$|\boldsymbol{f}'(x)|_{\max} = 2n^2L/(b - a)$.

19. I doubt that these collected articles were ever published.

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P. L. Chebyshev

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IX

Nekrasov's work on probability: the background

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1. Introduction

Pavel Alekseevich Nekrasov (1853 – 1924) was an outstanding mathematician who importantly contributed to algebra, analysis, probability theory and mechanics. However, he was unable to present satisfactorily his work on probability and Markov and Liapunov simply rejected it. Seneta (1981) was the first to discover Nekrasov's pertinent work and further explored this subject (Seneta 1984); Soloviev (1997) investigated Nekrasov's study of the central limit theorem (CLT) and I myself discovered archival and newspaper materials concerning him (Sheynin 1989a; 1993; 1995; 1996; and Chirikov & Sheynin (1994). I have also translated the entire debate of Markov and Liapunov with Nekrasov (**S, G,** 1) as well as the report of a commission denouncing Nekrasov's proposal on studying probability theory in school (Markov et al 1916) and one of Nekrasov's paper (1912 – 1914) on the method of least squares.

2. Biography

Several authors (Sheynin 1989b, p. 342; Sheynin 1993; Petrova & Soloviev 1997) sketched Nekrasov's life), see also the pertinent references in § 1. Nekrasov was certainly described in contemporaneous Russian encyclopaedias, but the best source for his life up to 1898 is an anonymous newspaper article of that year (1898) whose author expressed his hope that Nekrasov will continue to educate young people *in the spirit of duty to God, Czar and Fatherland*.

Nekrasov graduated from a Russian Orthodox seminary, entered Moscow University in 1874[1] and in 1885, several years after receiving his degree, became privat-dozent there. He became extraordinary professor in 1885 or 1886 and full professor in 1890. He also received his doctorate in pure mathematics by virtue of his remarkable memoir (1885b). In 1898 Nekrasov was appointed Rector. After completing his term of office he asked permission to resign, but the Czar (Alexander III) commanded him to continue, cf. Vygodsky (1948, p. 177) who refers to Nekrasov's file at the Archive of the University. In 1886 Nekrasov examined Chuprov (then a graduating student) and *accepted* his Candidate thesis[2], see however Sheynin (1996/2011, p. 161, Note 2.1).

During 1885 – 1891 he doubled at the Moscow Institute of Land Surveying where he taught theory of probability and higher mathematics.

From 1898 onward Nekrasov served as warden responsible for the Moscow educational district and was apparently fully occupied in administrative work[3]. In 1905 he moved to Petersburg as member of the Council of the Ministry of People's Education and had to leave his position as President of the Moscow Mathematical Society which he held since 1903, vice-president from 1891. In 1891 – 1894 Nekrasov was also vice-president of the Society of Friends of Natural Sciences.

As a student, he studied under Bugaev (1837 – 1903), a partisan of discrete mathematics, a philosopher, a *talented eccentric*, as he was called during his last years (Youshkevich 1968, pp. 483 – 486). Bugaev was the first who delivered a course in complex-variable theory at the University. Nekrasov (1905) published a booklet commemorating his teacher.

In 1910, complying with a request made by Ludwig Darmstädter (1846 – 1927), a chemist and a collector of autographs, Nekrasov sent him a handwritten letter[4]. Here it is (spelling preserved):

St. Petersbourg, 16.4.1910, Universitet

Je m'empresse à subvenir à Votre demande en Vous envoyant pour Votre collections quelques lignes autographiques en russe et en française, où sont mentionnés mes oeuvres les plus importantes. Dans mes travaux scientifiques j'ai toujours payé mon tribute d'admiration aux genie laborieux allemande. …

Ma carrière de professeur s'est acoulée principalement à l'Université de Moscou (1883 – 1905). Après m'être installé à St. Petersbourg comme membrte de conceil de ministre d'instruction publique , je ne lis qu'un petit cours de privat-docent à l'Université de St. Petersbourg.

Dans les volumes 11 – 25 de *Recueil de mathématiques* [*Matematich. Sbornik*], édité par la Société Mathématique de Moscou, dans les volumes 29, 31, 38 et 47 des *Mathematische Annalen* (Leipzig) [1887, 1888, 1891 (two papers), 1896] je publié mes dissertations et des nombreux mémoires, parmi lesquelles les plus importants sont [… 1909, 1895b, 1900a, 1900c, 1902].

As mentioned above, the memoir 1885b was indeed remarkable, I discuss it as well as the excessively long article 1900c in § 6. The two others are of little value[5] and Nekrasov's high opinion of them strongly testifies against his judgement.

Little is known about Nekrasov's life in Soviet Russia. In 1918 – 1919 he read a special course *On the branches of mathematics necessary for economic sciences* (whose title coincided with that of one of his papers of 1912) at Moscow University (Komlev 1989, p. 423). His only listener, A. A. Konüs[6] told me in 1989 that Nekrasov dealt in particular with the work of Walras, the founder of the mathematical school in economics.

The following lines, for what they are worth, are extracted from an obituary of Nekrasov by Uritsky (1924) discovered by Polovinkin (1994)[7].

The revolution had come, and Nekrasov decided to direct his entire talent towards serving the proletariat. He definitely attempted to grasp the Marxist system. He wrote a series of new monographs [where are they?] where he applied mathematical methods to the analysis of social phenomena. Some communists-mathematicians took upon themselves the problem of cleansing his works from their metaphysical shell [where are their results?] … A few days before his death, after catching pneumonia and entering a hospital, Nekrasov had time to write me his last note asking me to participate in creating a scientific Marxist group for studying and applying his works. He ended his letter by a request:

I am asking you to take all steps to ensure that the mathematical truths, valuable from the Marxist viewpoint, having been once discovered, will not be lost after my death, already sneaking up to me.

At that time the so-called Socialist Academy of Social Sciences was renamed Communist Academy and, perhaps later, a number of affiliated societies were created including a society of mathematicians-Marxists. A political purge of the students of Moscow University already took place in 1924 (Beskin 1993, p. 181)[8]. Nekrasov's family vainly attempted to turn over his rich collection of letters (e. g., from Markov and Zhukovsky)[9], now apparently lost, to several archives.

And here is another episode showing that everything connected with Nekrasov was still considered at about 1967. At a sitting of the seminar on the history of mathematics at Moscow University, Youshkevich informed its members (including me) that Kolmogorov had favourably mentioned Nekrasov's attempts at presenting probability as a science of mass random phenomena and expressed his desire to see a study of Nekrasov's work. Youshkevich then asked Maistrov (who died in 1986) whether he intended to examine this subject. No, he *shall wait until Kolmogorov repeats his wish ex cathedra*.

Seneta (1984, pp. 68 – 69) adduced a passage from an obituary by Sluginov (1927)[10]. He called Nekrasov *Professor at First Moscow University*[11], *Doctor of pure mathematics*, and mentioned a *yet unpublished* [Russian] memoir, *Anthropological Precis* which I did not find in the *Knizhnaya Letopis* (Book Annals) for 1923. Sluginov forgot Nekrasov's booklet (1923) listed in an abstracting journal (Seneta 1984) and in that *Letopis* for 1923, No. 7, p. 348. I think that neither Nekrasov's *Precis*, nor the *new monographs* on *analysing social phenomena*, mentioned by Uritsky, were ever published.

Beskin (1993, pp. 168 – 169) confirmed that Nekrasov had taught at Moscow University [but how?]:

> However strange it would seem, I, together with some other freshmen, attended Nekrasov's course *Theory of probability*. It was the last year that he delivered lectures[12]. He simply read aloud his book (I do not know which one), and, according to some indications, he did not go into the essence of the materials read. His course proved to be absolutely useless.

The first five words apparently meant that already then the University was not the right or proper place for Nekrasov. The *book* was probably Nekrasov's textbook (1896) rather than its second edition of 1912 which Markov et al (1916, p. 106, Note 5) called *full of absurdities*. Moreover, Nekrasov did not treat either Markov chains or the issue of axiomatising probability. Liusternik (1967, p. 222), an eminent mathematician, essentially complemented Beskin's picture. During the first half of the 1920s

> Nekrasov still attended the meetings of the Moscow Mathematical Society and sometimes even presented papers. A queer shadow of the past, he seemed decrepit, physically and mentally, and it was difficult to understand him. … This pitiful old man was like a shabby owl.

Seneta (1996, p. 258) quoted this passage from the English translation of the *Uspekhi* which omitted the following phrase.

He once declared that he made a mistake in his previous *works* [Liusternik's italics]: he selected the wrong sign of a square root. When replacing it by the contrary sign, he will be able to prove the need for social revolution.

We shall never know what kind of papers Nekrasov had presented.

3. The change of personality

Nekrasov's religious upbringing, his acquaintance with Bugaev's views, and his administrative duties strongly influenced his personality. From about 1900 he therefore underwent what may be called a change of personality. There are various indications for this change. His writings became unimaginably verbose[13], sometimes obscure and confusing with mathematics inseparably linked with ethical, political and religious considerations. In a letter to Florensky[14] of 13.12.1916 Nekrasov argued that he had reconciled mathematics with religion and politics *logically, correctly and rightfully*. A similar utterance by Nekrasov was in the newspaper *Novoe vremya* 7 (20) Dec. 1916, pp 7 – 8:

The mathematical language [must] … embrace supreme ethics, [be] together with conscience [with theology]. … However, the mathematical language of such pan-physicists[15] as Markov is of another kind, it is Nietzschean, and does not recognize supreme ethics [theology].

Markov, who allowed himself to doubt the texts of the Holy Writ, and in 1912 even asked the Most Holy Synod to excommunicate him from the Russian Orthodox Church (Sheynin 1989b, p. 340), hardly had anything in common with Nietzsche except a negative attitude towards religion. And I also doubt that religion or politics benefited from Nekrasov's contributions.

Moreover, Nekrasov's style became unbearable. Many commentators quoted his senseless (sometimes hardly translatable) phrases. Here is a comparatively mild example from 1906 (Chirikov et al 1994, p. 127)

[Mathematics accumulated …] psychological discipline as well as political and social arithmetic or the mathematical law of the political and social development of forces which depend on mental and physiological principles.

A third indication of the personality change is Nekrasov's obvious indifference toward his own mistakes. The most glaring illustration is his statement (1901, approximately p. 233) that, according to Chebyshev, Markov and Liapunov, the condition

$$\lim(P - L) = 0$$

is sufficient for the variable L to be the limit of variable P. For these scholars, Nekrasov explained, *any magnitude of the type* x^n *with* $n > 0$ *can, for example, be considered the limit of* $\sin x$ *as* $|x| \to 0$. Liapunov (1901, p. 62) noted that no one had ever proposed such a definition. He concluded:

All of Nekrasov's objections are based on various misunderstandings. Then, some of them are just unsubstantiated declarations … whereas the other ones either do not at all relate to the subject-matter of the criticized papers, or are distinguished by extreme vagueness.

Another example is Nekrasov's unjustified statement (1885a, p. 192) that the Seidel iterative solution of systems of linear algebraic equations with a positive definite and symmetric matrix undoubtedly converges, cf. Sheynin (1966) and Seneta (1984, § 3).

Not only Nekrasov erred in concrete mathematical derivations and arguments, he uttered incomprehensible declarations. Thus, he contrasted the *Bienaymé – Chebyshev – Markov* method to what he called the *Cauchy – Chebyshev – Nekrasov – Pearson* method. But there is no method common to latter four scientists since Nekrasov used complex-variable theory in probability whereas Chebyshev and Pearson did not[16].

All these traits led Mikhailov et al (1985, p. 225) to think that Nekrasov had become mentally ill. Another, at least partial explanation is that, perhaps semiconsciously, he extrapolated his religious feelings onto science. In Sintzov's opinion (1916)[17]

As usual, Nekrasov considers his view on events as an absolute truth and believes that, once he expresses it to someone, he had thus convinced the other man irrevocably.

A. V. Andreev (1999, p. 108) formulated a similar opinion:

Nekrasov's stylistic madness was directly connected with the essence of his philosophy. The unsatisfactory form which his writings came to acquire can be explained by irreconcilable intrinsic contradictions of his Weltanschauung.

Andreev indirectly connected this contradiction with the fuzziness of Nekrasov's philosophy. Neither he, nor Polovinkin (1991; 1994) to whom he referred, mentioned the eminent Russian religious philosopher V. S. Soloviev (1853 – 1900) whose statements (Radlov 1900) are highly relevant:

Philosophy offers its hand to religion. … Truth is integral essence. … The basis of veritable knowledge is mystic or religious perception. … Veritable knowledge is a synthesis of theology, rational philosophy and positive science.

I strongly suspect that Nekrasov, even though he hardly cited Soloviev, took up his idea about veritable knowledge and began to subordinate mathematics to religion and (obscure) philosophy[18]. And this reminds me Pearson's celebrated maxim (1892, p. 15): *The unity of all science* [all the more of a single contribution] *consists alone in its method, not its material*. But where can we find unity of method in Nekrasov's later writings?

4. Reactionary views

Nekrasov held to reactionary political views as manifested in his non-mathematical writings, in his thoughts about teaching probability theory in high school (§ 5) and in his letters to Florensky. Concerning the first two items, I refer to Bortkiewicz (1903) which is a review of Nekrasov's (1900c) and Markov et al (1916) respectively.

Bortkiewicz (1903) refuted Nekrasov's idealistic declarations that probability can *soften the cruel relations* between capital and labour. He stated that Nekrasov was attempting to justify stochastically *the principles of firm power and autocracy*. Cf. Markov et al (1916): Nekrasov attempted to

Exert influence, by means of mathematics, on the moral, religious and political Weltanschauung of the youth in a direction assigned in advance.

I select now two of Nekrasov's letters to Florensky (11.11 and 26.11 1916). In the former he *sympathizes* with his correspondent's plans for *teaching the mathematical encyclopaedia* at the Theological academy: *At your hands it will differ from an* [imaginary] *encyclopaedia of Markov & Co. inspired from Berlin*.

In the latter case Nekrasov (Sheynin 1993, p. 196) Nekrasov declared that the comparison of *Christian science* and the Moscow philosophical-mathematical school [Bugaev, Florensky and himself] with Karl Marx, Markov and another hardly known author *clearly shows the crossroads to which the German-Jewish culture and literature are pushing us*.

Earlier, however, Nekrasov (§ 2) paid his *tribut d'admiration aux genie laborieux allemand*, but what exactly did he mean now by *German-Jewish culture and literature*? I van only note that both letters were written during WWI and that Nekrasov's antisemitism has been a trait common to a great deal of Russian *patriots* to this very day (*Bash the Jew and save the nation*!)[19]. And I am unable to understand why Nekrasov linked Markov with Marx.

A. V. Andreev (1999, p. 104) and to a lesser extent Polovinkin (1991; 1994) attempted to explain Nekrasov's Weltanschauung by his philosophical and stochastic views. Andreev (pp. 105 – 106) also noticed that Nekrasov's economic concepts were *equally hostile to both capitalist and socialist principles*.

5. Educationist

Nekrasov (1916b, p. 51) declared that

At bottom, my official activities in defining the various types of schools and mathematical programmes … are reduced to an ideological struggle which aims to uphold entirely the classical values of mathematical education in all types of the general school.

In a letter to Florensky dated 2.11.1916 he also stated that

For the sake of our Fatherland, it is necessary to raise the standard of the mathematical education in the school, but protect it from the Markov & Co's frame of mind by those precepts, emblems and exercises which are included in our native tongue, in Magnitsky's Arithmetic[20], in Bugaev's arithmology, in the theory of probability of Buniakovsky, Chebyshev, Mendeleev and me.

Mendeleev did not contribute to the theory of probability. It is true that Nekrasov and Markov had conflicting *frames of mind*. The latter (Sheynin 1989b, p. 340) stated that seminarians were hardly fit for studying natural sciences or mathematics (Nekrasov was an outstanding exception) and Markov (cf. § 3) attempted to divorce himself from religion, but he hardly ever opposed *the classical values of mathematical education* (whatever this means). Still, he may have held other ideas than Nekrasov about teaching Latin. In a letter to the philologist F. E. Korsch (1843 – 1915) Nekrasov expressed his desire to strengthen the study of classical languages in gymnasiums. Markov (Sheynin 1993, p. 200), however, without considering this issue in general; declared in a newspaper letter of 1915, that *Latin … is not necessary for the physical and mathematical education.*

To get Nekrasov's statements in perspective, I (cf. Sheynin 1993, p. 198 with reference to rare Russian sources) note that in 1911 the Most Holy Synod, *in executing the Imperial will*, worked out new regulations for theological institutions *in the spiritual direction*, and that many members of their professorial staff had to abandon their positions. It can be assumed that a similar strengthening of *the spiritual direction* went on in the theological seminaries and in other types of school as well.

In 1915 Nekrasov has seconded a proposal made by P. S. Florov, an educationist, for introducing probability theory into the curriculum of the high school, and both the Ministry of Public Education and the Petrograd (formerly the St. Petersburg) Academy of Sciences considered this issue (cf. Sheynin 1993, p. 197 and Chirikov et al 1994 which includes a list of ten pertinent Nekrasov's writings).

Markov, the most eminent Russian specialist in probability, was not invited to the Ministry's conference by correspondence but in 1915 he published a paper denouncing that proposal. It was mainly he who killed that definite proposal. I restrict my description to several points, but see also § 4.

1. The Academic commission (Markov et al 1916) examined the definite proposal rather than the essence of the matter. However, *some* of its members *had indeed opposed in principle the introduction of* [probability theory] *in any form into the school curriculum*.

2. The commission indicated serious mistakes in both Nekrasov's understanding of the main objects of mathematical analysis (cf. § 3) and in the proposed programme.

3. The implementation of any essentially new programme, especially under war conditions, would have encountered great difficulties, for example, because of the lack of qualified teachers (see below Item 4)[21].

4. Earlier, in 1898, Nekrasov (Sheynin 1995) put forward a similar proposal concerning the Law Faculty at Moscow University and compiled an appropriate draft programme. However (p. 166, Note 15), during 1902 – 1904 and most of the period from 1912 to 1917 the theory of probability was not taught even at the Physical and Mathematical Faculty, so that the issue raised by Nekrasov was apparently premature to say nothing about studying probability in schools. Nekrasov's proposal of 1898 was hardly implemented.

5. Nekrasov (1916a, pp. 30 – 31) also proposed to include into the school curriculum elements of analytic geometry and analysis as well as the *successive approximate analysis*, whose essence he did not clarify. He related it to induction in a wider sense and mentioned Laplace, Poincaré and other scholars (p. 19). Nekrasov also attached much importance to the establishment of mathematical classrooms and the educational use of films (pp. 30 – 31).

Much earlier Nekrasov (1906, p. v) argued that school mathematics should be based on logic. Mathematical logic was then in the making and he had possibly thought about its elements.

6. Mathematics and related fields of knowledge

Among Nekrasov's papers in *Mathematische Annalen* the first was devoted to algebra, the last one, to mechanics, and the three others, to analysis. Youshkevich (1968, p. 539) and Soloviev (1997, p. 9) briefly described his algebraic contribution (Nekrasov 1885b) for which the Petersburg Academy of Sciences on its own initiative had awarded him the Buniakovsky prize (Nekrasov did not participate in the contest).

Concerning analysis, I remark once more that Nekrasov was proficient in complex-variable theory ands essentially applied it in his research in probability theory[22]. And he (1885b; 1900a) was the main author the main author of the method of saddle points, see Seneta (1984) and Petrova & Soloviev (1997). Nekrasov used it particularly in probability theory.

Mikhailov et al (1985, p. 234) described Nekrasov's studies (1892 – 1896) of the rotation of a solid body about a fixed point and called his analytical results remarkable. It was mainly Nekrasov who defended Golitzin, the future co-founder of modern seismology, when some most eminent Russian physicists had opposed his later exonerated research in mathematical physics[23].

6.1. The method of least squares (MLSq). In an addendum of 1914 to his earlier paper (1912) Nekrasov acknowledged his failure to notice a relevant paper of Yaroshenko, but still alleged (wrongly) to have considered the issue more generally. He also wrongly attributed to Legendre an interpolation-like application of the MLSq, inadequately described the difference between the approaches chosen by Laplace and Gauss[24], and threw in a few financial terms[25]. See Seneta (1984, § 4) about the previous work of Yaroshenko (and Sleshinsky).

6.2. The central limit theorem (CLT). Seneta (1984, § 6) and Soloviev (1997) described Nekrasov (1898; 1900c) on the CLT in the case of large deviations. At the time, this was absolutely new, only some 50 years later this subject began to be effectively studied.

Suppose that independent, non-identically distributed lattice variables $\xi_1, \xi_2, \ldots, \xi_n$ (linear functions of integral variables $\xi_k = b_k + h\eta_k$) have finite mean values a_k and variances σ_k^2 and denote $m = \xi_1 + \xi_2 + \ldots + \xi_n$. In the new case for lattice variables, the only one that Nekrasov considered, the magnitude x

$$x = \frac{|m - \sum a_k|}{\sqrt{\sum \sigma_k^2}}$$

remained less than n^p with $0 < p < 1/6$.

Nekrasov (1898) formulated six pertinent theorems and proved them later (1900c). Understandably, neither of the two commentators aimed at describing his work in sufficient detail and it is hardly possible to compare directly their accounts with the incomprehensible original writings. Soloviev indicated Nekrasov's shortcomings and mistakes and added (pp. 15 – 16) that he was

Firmly convinced that neither any contemporaneous mathematician, nor any historian of mathematics had examined the greater memoir in any detail.

He only *assumed* that the proofs of the six theorems formulated in 1898 and provided in 1900 – 1902 are correct *naturally without being completely certain about it*. Here are his conclusions (p. 21):

1. We do not doubt that Nekrasov proved the usual CLT although only for lattice random variables[26].
2. He imposed on them an excessively strict condition [the analyticity of the generating functions in some ring] which is much stronger than presuming the existence of all the moments of the random variables.
3. He also formulated other restrictions whose implementation is generally impossible to check …
4. In general, Nekrasov's work on the CLT should be considered unsatisfactory. … His contribution (1900c) was forgotten and did not at all influence the development of the theory of probability.

Seneta (1984, p. 55) says much the same in a much more general way:

Nekrasov's attempt was only partly successful, poorly presented, badly defended [against Markov], never understood by Markov and Liapunov[27] or noticed by their successors.

Nevertheless, Seneta also credited Nekrasov with being a *catalyst* whose work to a certain degree prompted Markov[28]. I quote Soloviev's pertinent remark (1997, p. 15):

It is possible to understand Markov, who was sick of unnecessary complications and muddle and in whose debates with Nekrasov his irritation clearly shows through. Political and religious discord was undoubtedly superimposed on their relations. However, when appraising their long-term debates, it can be said that in most cases (although not always, as Kolmogorov once remarked) he was in the right.

Seven papers comprise the debate on the CLT between Markov, Nekrasov and Liapunov (Markov 1899b; 1912; Liapunov 1901; Nekrasov 1899; 1900b; 1901; 1911). It was also reflected in the Markov – Nekrasov correspondence and in their letters to the Petersburg Academy of Sciences.

Markov (undated letter of 1898; **S, G,** 4) largely rejected Nekrasov's report (1898), stating that most of his theorems were either wrong or of small import (cf. Soloviev's conclusions 2 and 3 above). He also accused Nekrasov, who had dedicated this report to Chebyshev's memory, of failing to mention the latter's contribution (1891) to the CLT.

Nekrasov (letter of 11.10.1898, **S, G,** 4) insisted that his report was important and claimed that it should not be criticized until he published the proof of his theorems. On 18.12.1898, in a letter to the Permanent Secretary of the Academy Nekrasov (Ibidem) complained that Markov, first, had published two papers (1898; 1899a) repeating what he, Nekrasov, had *accomplished* earlier *in a somewhat different form*, and, second, had declared *in an indecent postcard*[29] that he did not mention *works* [like Nekrasov (1898)] *which do not deserve any attention*. Soon Nekrasov (1899) again claimed that Markov's memoirs *adjoined* his own *previously published works*[30].

In his next paper Nekrasov (1900b) stated that Chebyshev's contribution (1891) was

> Of minor importance since it contained that which was proved with sufficient rigour much earlier and included in generally known treatises.

He referred to Laurent (1873, pp. 144 – 165)[31]. He also somehow blamed one of his mistakes on his *excessive trust in … Laplace, Chebyshev et al.* Then, Nekrasov (1901) argued that Liapunov *had overlooked the well-known difficulties* encountered in applying the Dirichlet discontinuity factor and that his results had contained *all the main shortcomings* of his predecessors. These latter, Nekrasov continued, had wrongly understood the notion of limit (see § 3).

Answering Nekrasov, Liapunov (1901)[32] remarked that he did not prove his accusations and that he, Liapunov, had not actually applied the Dirichlet factor. Finally, Liapunov explained that he was not interested in the case of large deviations in the CLT since Chebyshev did not consider it. Nevertheless, he made a few interesting remarks.

Markov (1912) severely criticized Nekrasov (1911) in which the latter had applied his earlier terminology (*middling paradoxical instances*, *special cases of the first kind*, boundary paradoxical cases) never used by anyone else[33] and insisted that his differential approach was more advisable than the integral method. Nekrasov also declared that Liapunov did use the discontinuity factor, although in an unusually concealed way and that Chebyshev did not rule out the case of large deviations. Subsequently, however (Gnedenko 1959) Nekrasov had renounced his statement about the discontinuity factor. Again, Chebyshev said nothing directly about the case of large deviations. More precisely, the CLT states that, under certain conditions,

$$\lim P(t_1 < x < t_2) = \frac{1}{\sqrt{2\pi}}\int_{t_1}^{t_2}\exp(-t^2/2)dt\,,\ n \to \infty$$

in which x is given by formula (1). Chebyshev remarked that t_1 and t_2 where *any*, but had not considered them as variables.

I repeat that Nekrasov had evidently proved the CLT in their new case of large deviations for lattice variables. However (§§ 3 and 4) he was unable to leave a coherent description of his findings. Markov had no desire to study Nekrasov's horribly complicated work in detail, but at least he profited from it to some extent, even if indirectly. Liapunov, who had not returned to probability after 1901, was no longer interested in Nekrasov. Even though he understood that the latter was examining the case of large deviations, he did not take up this subject because his teacher, Chebyshev, gave it no attention.

Acknowledgment. Prof. H. Bos indicated several unclear or even wrong statements which are now clarified/corrected.

Notes

1. An official Russian source stated that in Russian universities in 1875 the alumni of theological seminaries comprised about 40% of the student body (Sheynin 1993, p. 143).

2. H ere is one of his remarks written in a margin of Chuprov's manuscript (Sheynin 1996/2011, pp. 110 – 111): *Concerning* [force, space, time, probability] *philosophers have written full volumes of no use for physicists or mathematicians. … Mill, Kant and others are not better but worse than Aristotle, Plato, Descartes, Leibniz.*

3. Here is a passage from a letter written by K. A. Andreev (a geometer, 1848 – 1921) to Liapunov (Gordevsky 1955, pp. 40 – 41): *Nekrasov reasons perhaps deeply but not clearly and expresses his thoughts still more obscurely. I am only surprised that he is so self-confident. In his situation, with the administrative burden weighing heavily upon him, it is even impossible, as I imagine, to have enough time for calmly considering deep scientific problems, so that it would have been better not to study them at all.*

4. Darmstädter did not ask such materials either from Markov or Liapunov.

5. Bortkiewicz (1903) ridiculed one of them (§ 4).

6. For his biography see Dickwert (1987).

7. The author of the obituary was a namesake or relative of the communist leader M. Uritsky (1973 – 1918). Polovinkin explained that the title of his paper was a quotation from Nekrasov's *psycho-arithmo- mechanician, or moralarithmetician*. I do not understand this expression (in which, to make matters even worse, *moralarithmetician* was a single word). Cf. § 3.

8. With regard to the situation in statistics see Seneta (1985, pp. 122 – 124) and Sheynin (1998).

9. Prof. S. S. Demidov, in a pivate conversation, perhaps in 1989.

10. Sluginov published a number of mathematical papers listed in *Matematika* (1930). His obituary of Nekrasov inadequately describes Nekrasov's work. Then, he applies to Nekrasov the phrase *He ceased to calculate and live* which appeared in one of Euler's obituaries, but this seems objectionable.

11. Perhaps from 1917 until 1930 the University consisted of two bodies (*Bolshaia* 1950, vol. 28, p. 413).

12. On p. 164 Beskin stated that he had entered Moscow University in 1921.

13. For a number of years, *Matematicheskiy Sbornik* published by the Moscow Mathematical Society almost exclusively consisted of his papers, some of them really monstrous. Polovinkin (1994) quoted Bugaev's son, the writer Andrei Bely (a penname): he stated that perhaps even in 1894 his father became utterly disappointed with Nekrasov. Unlike Polovinkin, I am inclined to believe this testimony.

14. Pavel Aleksandrovich Florensky, a religious philosopher and mathematician. His book (1914) contained an extensive supplement of a natural-scientific and mathematical nature. His early manuscript devoted to philosophy of mathematics was recently (1999) published. On his relations with Luzin see Demidov et al (1989) and Ford (1997). Petrova & Suchilin (1993) studied his geometric interpretation of complex numbers, He was born in 1882 and died in 1943 (*Bolshaia* 1969, vol. 27), but (Seneta 1996, p. 258) the foreword to the second edition of 1990 of Florensky (1914) states that he was shot in 1937. Nekrasov's letters to Florensky are kept by the family of the latter and S. S. Demidov acquainted me with them.

15. By pan-physicist Nekrasov apparently meant an atheistic natural scientist.

16. *In spite of his splendid analytical talent, Chebyshev was a pathological conservative* (Novikov 2002, p. 330).

17. On D. M. Sintzov (1867 – 1946) see Demidov et al (1994). Youshkevich (1968) repeatedly mentioned him.

18. Cf. his own opinion (Note 2) about philosophical works useless for positive science.

19. Florensky was an out-and-out anti-Semite. I have seen in the internet a testimony to the effect that he, had he been a Jew, would have himself killed that Christian boy (as was thought by his ilk about an ordinary and later exonerated Jew who allegedly killed that boy).

Markov battled against anti-Semitism (Sheynin 1989b, p. 341).

20. The *Arithmetic* of 1703 by L. F. Magnitsky (1669 – 1739) had been the main Russian mathematical (not only arithmetical) textbook up to the mid-18th century.

21. In 1989, in a conversation with me, Aleksandr Youshkevich, son of Adolph Y. and an eminent mathematician in his own right, expressed his utter regret that the Academy had not approved Nekrasov's very idea. However, even disregarding the considerations which opposed it, only the elements of probability could have been introduced.

22. Nevertheless, *Being a very powerful analyst, he chose an unfortunate, purely analytic rather than a stochastic approach towards solving the problem* [of the CLT] *which largely predetermined his failure* (Soloviev 1997, p. 21).

23. *Uchenye* (1894) documented the entire story. Incidentally, Markov (Sheynin 1990) successfully blocked Golitzin's election to full membership at the Petersburg Academy of Sciences because of the later's unsatisfactory paper on treating observations. Golitzin beame ordinary academician five years later (in 1908).

24. Here is a typically meaningless statement from his letter to Markov of 20.12.1913, see **S, G.** 4:

I distinguish the viewpoints of Gauss and Laplace by the moment with regard to the experiment. The first is posterior and the second one is prior. It is more opportune to judge a posteriori since more data is available, but this approach is delaying, it lags behind, drags after the event.

25. The second edition (1912) of Nekrasov's treatise (1896) was in part a (hardly successful) attempt in the same direction. Elsewhere Nekrasov (1916a, p. 29) mentioned problems concerning labour relations, public health and credit in connection with the statistical method. Recall also (§ 2) his interest in the application of mathematics to economics.

26. I emphasize: Soloviev refers to Nekrasov's final contribution rather than his report (1898). And I am adding Soloviev's own grain of salt (1997, pp. 13 – 14): Nekrasov wrongly (too extensively) understood the notion of lattice variable. All the terms here (CLT, large deviations, lattice variable) are of later origin.

27. I qualify this statement. First, I would say *therefore, never understood* … Second, Liapunov (1901) had indeed noticed the case of large deviations (cf. below).

28. Seneta (1984, p. 67). In 1910, in a letter to Chuprov (Ondar 1977/1981, p. 5) Markov stated that Nekrasov's wrong opinion about the law of large numbers *prompte*d him to explain the situation. Elsewhere he (Markov 1912, p. 77) declared that the connection between them consisted in that *when compiling some of my articles, I had in mind* Nekrasov's *wrong statements* whose refutation had been *one of my purposes*.

29. On 29.9.1915, in a letter to the Vice-President of the Academy, Nekrasov, see **S, G,** 4, had repeated this complaint and soon (1916b) published six postcards of 1915 and 1916 deleting, as he stated, the indecencies.

30. By that time, however, Nekrasov did not publish anything relevant except his report of 1898 and his declaration shows once more that everything he said should be considered doubtful. Nevertheless, that report possibly served Markov as a *catalyst*, cf. above.

31. Earlier Nekrasov (1899, pp. 31 – 32) had lamely stated that he had not included any references at all in his report (1898) because of its *conciseness*, but at least he should have made an exception for Chebyshev (cf. Markov criticism above in this section). Nekrasov's new statement was no better: Laurent (correct pages, 144 – 145) had indeed considered the general case of the CLT, but did not estimate the approximation of his calculations. It was also possible to mention Cauchy, who, in 1853 had allegedly proved that theorem rigorously (Freudenthal 1971, p. 142), but only in a particular case.

32. I quoted from this source in § 3.

33. More interestingly, in connection with the mathematical study of indeterminacies, Nekrasov (1916b, p. 23) mentioned the then not yet existing theory of catastrophes and even used the term *catastrophe*.

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IMI = *Istoriko-matematicheskie issledovania*
Kazan Izv. = *Izvestia Fiziko-matematich. obshchestvo,* K*azan Univ*.
MS = *Matematicheskiy sbornik*
Psb = Petersburg

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X

Markov: integrity is just as important as scientific merits

Intern. Z. f. Geschichte u. Ethik der Naturwissenschaften, Technik u. Medizin (NTM), Bd. 15, 2007, pp. 289 – 294

Abstract

The Russian mathematician Markov was born 150 years ago. Many commentators have been describing his life and work but the extraordinary traits of his strong personality are much less known, especially beyond Russia. I describe this aspect of Markov's life and note that to a certain extent his character influenced his work.

1. General Information

Andrei Andreevich Markov, 1856 – 1922, one of the best-known students of Chebyshev, was an outstanding mathematician, Professor at Petersburg University from 1886 to 1905 (Dozent since 1880) and a member of the Petersburg Academy of Sciences since 1886. He is meritorious for achievements in the number theory and, especially, theory of probability, where he initiated the study of dependent variables, in particular variables connected into *Markov chains*. His important papers are collected in Markov [1951] and his treatise on probability theory appeared in four editions, in 1900, 1908, 1913 and, posthumously, in 1924, and its second edition was translated into German [1912] together with three of his papers. On Markov's life and work see Markov Junior [1951], this being his biography written by his son, a mathematician in his own right, Andrei Andreevich Markov Junior, 1903 – 1979.

Grodzensky [1987]; Sheynin [1989]; Gnedenko & Sheynin [1992]; and Eugene Seneta [2001] as well as some other references listed below testify that my present note is a compilation, important since it concerns Markov.

Markov had reasonably been highly critical of Karl Pearson whereas Tschuprow or Chuprov with whom Markov corresponded in 1910 – 1917 [Ondar 1981; Sheynin 1996 and 2011, § 8], persistently but not really successfully attempted to convince him of the scientific importance of the Biometric school. Concerning the Continental attitude to Pearson, it is instructive to cite a letter of unknown date which Chuprov received from Georg Bohlmann, 1869 – 1928 (and quoted in his own letter to Leon Isserlis, 1881 – 1966, of ca. 1924) [Sheynin 2011, p. 76]:

Am Schluß Ihrer jetzigen Abhandlung [Tschuprow 1918 – 1919] *hat mich Ihr Eintreten für Pearson sehr verblüfft; denn es ist so vieles in seinen Ansätzen, was mir bisher überhaupt nicht ernst erschienen ist* […]. *Um so schöner wäre es, wenn ein Teil seiner Arbeiten doch auf eine wissenschaftlich zu rechtfertigende Basis gebracht werden könnte. Ich freue mich daher sehr darauf, die Begründung zu studieren, die Sie zu Ihrem Standpunkt geführt hat*.

(At the end of your paper I was perplexed by your pleading for Pearson; indeed, so much in his approach did not at all seem serious to me. […] Nevertheless, it would be all the better if a part of his work could be scientifically justified. Consequently, I am glad to study the motive which brought you to your point of view.)

Chuprov continued:

Markov regarded Pearson, I may say, with contempt. Markov's temper was not better than Pearson's, he could not stand even slightest contradictions [protivorechie] *either*.

Coupled with the extremely unfavourable conditions of life and scientific work in Russia since 1914 (World War I, revolution, Civil War), Markov's rigidity resulted in his having barely recognized the new stream; in particular, he never mentioned other English statisticians, for example Udny Yule or Gosset, pen-name Student.

The same rigidity is seen in Markov's failure to illustrate the possible application of chain-dependent variables in natural sciences although, for one thing, it would have been quite easy for him to justify much more easily why the asteroids were uniformly scattered across the ecliptic (Poincaré's celebrated example of uniform randomness). Indeed, Markov actually explained his general standpoint in a letter to Chuprov of 1910 [Ondar 1981, Letter 44]:

I shall not go a step out of that region where my competence is beyond any doubt.

It is known that, as an example of applying his chains, Markov had studied the distribution of vowels and consonants in two Russian classical texts, but this had nothing to do with natural sciences. True, Kolmogorov [1947, p. 59] remarked that the absence of applications to natural sciences had reflected the remoteness of Russian contemporary mathematics from *statistical physics*, but this seems to be only a partial explanation.

Just the same, Markov manifested his steadfastness in his high-principled attitude towards social conditions in his native country, see § 2. No wonder the press nicknamed him *Militant Academician* [Nekrasov 1916, p. 9] and *Andrew the Furious* [Neyman 1978, p. 486].

2. Attitude towards social conditions of life

2.1. Letters to Newspapers. Grodzensky [1987] published about 20 of Markov's letters to newspapers dated from 1904 to 1915 and devoted to burning social issues. He discovered them in two archives and noted, on p. 100, that *many of them* were not published owing to their sharpness; he did not elaborate, and it is unknown exactly which of them were rejected by the newspapers. Right now, I describe only one of them. In December 1904, Markov indirectly blamed Russia's *absolute despotism*, i. e., the monarchy, for the inevitable defeat in the war with Japan [Grodzensky 1987, p. 94]. My §§ 2.3 and 2.4 are largely based on the same source, and Grodzensky, in turn, referred to several other of Markov's newspaper letters.

Also known are three published letters on education [Sheynin 1993]. In one of them written in 1915 he protested against the waiving of entrance examinations for graduate seminarians who wished to study at physical and mathematical faculties of universities:

[Seminarians] *are getting accustomed by their schooling to a special kind of reasoning. A seminarian must subordinate his mind to* *the indications of the Holy Fathers and replace it by the texts from the Scripture. The seminary's wisdom … is far from real science …*

2.2. Attitude towards Religion. In part, this was seen just above. In 1901, the great Russian writer, Tolstoy, was excommunicated from the Russian Orthodox Church. During his last days, the Most Holy Synod discussed whether he should be *admitted to the bosom of the Church* and decided against this [Anonymous 1910]. This goes to show that in 1912 Tolstoy's excommunication was likely well remembered.

Yes, in 1912 Markov submitted a request to the Synod for excommunication. He quoted his treatise to the effect that

We should regard stories about incredible events allegedly having occurred in bygone times with extreme doubt and added that he did not *sympathize with religions which, like Orthodoxy, are supported by, and in turn lend their support to fire and sword*.

His request was not granted; the Synod resolved that Markov *had seceded from God's church* [Emeliakh 1954, pp. 400 – 401 and 408]. Markov was possibly prompted by the notorious blood libel case against an ordinary Jew, Beilis, see below.

A special point here is Markov's debates with Nekrasov, a talented mathematician who later, beginning with ca. 1900, underwent a change of personality and subordinated his stochastic studies to religion and shallow philosophy [ix, § 3]. This, indeed, was one of the reasons why Markov vehemently opposed him. And, although Markov could not have known it, Nekrasov once stated, in a letter of 1916, to Florensky, a religious philosopher and mathematician, who later perished in the Gulag, that Markov's contributions show

The crossroads to which the German – Jewish culture and literature are pushing us.

This statement can only be partly explained by World War I then going on.

The other cause for Markov's total rejection of Nekrasov's work on probability was that his writings became unimaginably verbose, obscure and confusing, corrupted by numerous mistakes and meaningless statements. And, finally, it was mainly Markov who killed a proposal, seconded in 1915 by Nekrasov, to introduce the theory of probability into the curriculum of the high school [ix, §5]. Here, I only note that Nekrasov was certainly not the person to supervise such an innovation and that he would have oriented the programme towards confirming religious truths and traditions.

2.3. Struggle against Anti-Semitism. In 1905, the Council of the Petersburg University decided to ask those responsible for permission to enrol all Jewish applicants irrespective of the quota (3% of the total). Markov and another member of the Council moved that the matter ought to be resolved without asking permission. Their move was defeated and Markov resigned from the Council's Commission [Zhurnaly 1906].

In 1907, the students of the Academy of Military Medicine turned out several members of a Black Hundred organization, the League of the Russian Nation, from the Academy's building. Markov publicly declared his approval of this action and the students expressed their gratitude to him. Grodzensky [1987, p. 96], who reported this episode, did not elaborate. The ousted students might have returned next day, or left for good because of the general bad feelings towards them.

In 1913, a certain Zhoftis sat for an entrance examination in mathematics at Kharkov Technological Institute and was asked to solve an equation of the tenth degree. He naturally failed and described this episode in letter to a newspaper. Markov, upon hearing about this, sent a letter to another newspaper where he called the examination a *humiliation* [Grodzensky 1987, pp. 102 – 104].

Also in 1913, the notorious Beilis case, a counterpart of sorts to the Dreyfus case of 1894 – 1899 in France, was being heard in Kiev. The defendant was charged with a ritualistic killing of a Russian boy (and acquitted). Before and during the trial an anti-Semitic campaign supported from above had been launched whereas Markov, together with many other public figures, actively protested against the charge. In particular, Markov sent an open letter to the leader of the extreme right wing in the Duma accusing him of organizing this campaign [Grodzensky 1987, pp. 104 – 105].

It is not amiss to add that about 1870 Chebyshev's student, Libman Israelevich Lipkin, 1841 – 1875 (co-inventor of a mechanical device for transforming circular motion into rectilinear motion but not yet a professional scholar), was allowed to live in Petersburg, i. e. beyond the Pale of Jewish settlement, and hold his Master's examination as a result of a solicitation made by several professors of the Petersburg University, – in the first place, by Chebyshev [Prudnikov 1964, p. 84]. Nothing is known about Lipkin's last years.

2.4. Other Episodes. In 1902 the Academy of Sciences elected Maxim Gorky, real name Aleksei Maksimovich Peshkov, Honorary Member but the President annulled the election on political grounds on demand of the Czar Nikolai II. Markov (unsuccessfully) protested against the annulment both then, and, under more favourable social conditions, in 1905. Gorky was only admitted in 1917 [Markov Junior 1951, pp. 604 – 606]. That during his last years Gorky had become an ardent partisan of Stalinism is another story altogether.

In 1903, Markov declared that he had no desire to be decorated by orders [Markov Junior 1951, pp. 606 – 607].

In 1907, Markov refused to vote in the elections of the Third Duma since its convocation was connected with a *violation of the law* [Markov Junior 1951, pp. 607].

In 1908, Markov refused to comply with an official demand to keep an eye on the students' political behaviour [Markov Junior 1951, p. 608]. In 1910 he protested against the expulsion of students for participating in unofficial gatherings [Grodzensky 1987, p. 48].

In 1912, Markov refused to be included in a commission of the Academy of Sciences elected for participating in the tercentennial celebrations of the House of Romanovs [Grodzensky 1987, p. 88]. He is known to have organized, in 1913, the bicentennial of the law of large numbers at the Academy and Markov Junior [1951, p. 610] states that he did that to *counterbalance* the official celebrations.

2.5. The Last Protest. In 1921, 15 professors of the Petrograd University made a statement whose main point was that those wishing to study must only be selected according to their knowledge rather than to class or political considerations. Markov was the first to sign this statement [Grodzensky 1987, p. 137], which could have been made time and time again during the Soviet period of Russian history.

Nothing more is known about Markov's possible protests after 1917. His life became certainly difficult and once in 1921 he even lacked footwear [Grodzensky 1987, p. 136].

As a fitting conclusion I cite Einstein's, letter of 1933 to the statistician Gumbel (Einstein Archives, Hebrew University of Jerusalem, 38615): *Characterleistungen sind ebenso viel Wert wie wissenschaftliche* (the title of this note is a translation of that phrase).

Acknowledgement. Nekrasov's letters to Florensky, one of which I quoted in § 2.2, are kept by the latter's family. It was Prof. Sergei Demidov who acquainted me with them. And I am grateful to the referee who led me to specify several points.

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XI

Statistics in the Soviet epoch

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Summary

This paper is based on a large number of Soviet and Western sources and describes the development of statistics in the Soviet Union. After ca. 1922, Russian statisticians were able to work successively by drawing on the contemporaneous national and foreign professional knowledge. However, from 1927 onward many of them were labelled saboteurs or enemies of the people, arrested and even shot. Previous statistics was denied, and its classics (Quetelet and even Süssmilch) were called ideologists of the bourgeoisie or (not better!) enemies of materialism (Pearson). The new crop of statisticians, largely composed of ignoramuses, restricted the aims of statistics to confirming Marxist political economy. In the post-war period ideology continued to dominate statistics, econometrics had to overcome great ideological resistance and genetics, crushed in 1945 had not returned to life until the 1960s.

Introduction

I am discussing the Soviet scene. Readers will find interesting points of similarity between the Soviet Union and communist China (Li 1962) or perhaps Eastern Germany. A particular noteworthy parallel existed between the Soviet abominable term *enemy of the people* (*vrag naroda*) coined in the early 1930s and possibly the somewhat later Nazi expression *Volksfeind*.

Abbreviation: CND = Central Statistical Directorate

OGPU = United State Political Directorate, the predecessor of the KGB.

See also Abbreviation in Bibliography

1.1. The prehistory. In pre-revolutionary Russia, local economics (especially agriculture) was studied by the so-called zemstvo statisticians (mentioned in § 1.2) whose work was described by several authors (Sheynin 1997, Note 8). It seems that zemstvo specialists regarded the peasants as a single class of population whereas Lenin (1899/1958) separately studied the poor peasants, those of average means and the rich peasants (the *kulaks*). This simple fact had great consequences: Soviet Marxists extolled Lenin's statistical prowess to the skies and proclaimed statistics to be a social discipline. They never thought that, at least from the mid-19th century, preliminary data analysis had become the usual business of statisticians. Neither had they noticed Lenin's *misleading use of means*, his *tendentious use of statistics* or *statistical and political apologetics* (Kotz & Seneta 1990, pp. 84 – 85, 78 and 86).

Lenin's philosophical outlook also proved extremely harmful for Soviet statistics: he (1909/1961, pp. 190 and 274) called Pearson *a conscientious and honest enemy of materialism* and *one of the most consistent and lucid Machians*. (Ernst Mach, 1838 – 1916, physicist and philosopher.). Understandably, Soviet statisticians rejected Pearson's statistical work out of hand. Chuprov's desire to combine the Lexian Continental direction of statistics with the English Biometric School was never mentioned.

1.2. Chuprov's students. Chuprov left Russia a short while before the Bolshevik coup d'état. He expected to return in a few months but remained beyond Russia for the end of his life. In a few years Chuprov (1922, p. 358) remarked about Russia's CSD:

Hervorragende statistische Kräfte […] *unter denen ich mehrere meiner besten Schüler finde, sind an ihn angeschlossen*.

The situation soon changed, and at least three of his former students were persecuted, together with dozens if not hundreds of other statisticians, zemstvo statisticians who were ideologically suspected[1], in the first place. His closest student N. S. Chetverikov spent four years in prison, apparently in 1931 – 1935, as a saboteur, and in 1937 or 1938 he was subjected to new repressive measures, i. e., was at best banned from living in large cities (Anonymous 1995). V. I. Khotimsky, *ein mathematisch hochbegabter und politisch sehr linkstehender Student* [of Chuprov] (Anderson 1959, p. 295) became an enemy of the people (Lozovoy 1938, p. 117) and was shot in 1939 (Kolman 1982, p. 132). And B. I. Karpenko was arrested in 1938 and had to live somewhere in exile until 1943 (Karlik 1992).

Anderson (p. 294) once more:

Könnte ich […] *eine ganze Reihe von in Russland früher sehr geschätzten Statistikern und viel versprechenden jüngeren Schülern* […] *Tschuprows aufzählen, deren Namen nach 1930 aus der sowjet-russischen wissenschaftlichen Literatur plötzlich ganz verschwanden*.

Compare this with Chuprov's earlier statement (1922, p. 358):

Distinguished statistical forces […] *among whom I find some of my best students are connected with the* [CSD].

1.3. An Utopian statistics. After ca. 1927 Soviet statistics became ever less reliable and statistical research in some vital areas was simply forbidden. Thus crime was explained away as a *survival of capitalism*. In the 1930s, prostitution was declared non-existent (Gorfin 1940); drug addiction and the spread of AIDS were grudgingly acknowledged only after further denial became impossible and the Chernobyl catastrophe was criminally played down.

In 1952, the official figure for the gross harvest of cereals was 130 *mln* tons, 33% higher than the real figure as later disclosed Khrushchev. Anderson (1959, p. 293n), who described this episode, concluded that this deception would have been impossible *ohne eine entsprechende Anweisung Stalins*. And Starovsky (1958, p. 13) let it be known that [apparently for quite a while] statisticians had only estimated the biological harvest.

Vast trustworthy statistics was not needed, it was *even dangerous* for the Soviet system (Orlov 1990, p. 67)[2]. He (p. 68) continued: there was no need to study the market, to improve the quality of goods, and mass falsification of data was rampant. Monopolization of statistics by the CSD and bureaucracy were additional reasons why scientific progress was difficult. And, finally, the Marxist philosophical viewpoint, or at least its official description, impeded a proper understanding of randomness and its role, cf. Lysenko's declarations in § 7.

Dikalov (1990) argued that the statistical service should be subordinated to the parliament. However, under the existing conditions the puppet Supreme Soviet would have hardly helped to improve the situation:

1.4. Mathematical statistics. How did the general situation influence mathematical statistics? In 1948 or 1949, W. Feller, in a letter to Huxley (1949, p. 170n) stated:

There is practically no statistics in Russia and it is a surprising feature that a country so strong in probability theory has made practically no contribution to mathematical statistics. Obviously, the political atmosphere is unfavourable to that type of application.

Feller's opinion was correct only in respect of statistics but not mathematical statistics, see Smirnov (1948), Gikhman & Gnedenko (1959) and Gnedenko (1950a, Chapter 11). Note that Gnedenko barely cited foreign authors and downgraded Fisher. Only one of his examples dealt with applications of statistics (a safe discussion of sample inspection of mass production). The situation did not change in the later editions of his book.

Romanovsky, who lived and worked in Tashkent, continued the Chuprov tradition (§ 1.1) of combining European (including Russian) and English trains of statistical thoughts, but in 1948 he had to apologize for his *ideological mistakes* (§ 3.4).

2. Several calm years

During several years Soviet statisticians were able to work as they did before 1917. Zarkovic (1956, p. 336) concluded that *Russian statistics in the early 1930s* was on a par *with the best in other countries* and Kotz (1965, p. 134) agreed with him. Jasny (1957, p. 1) states that *government statistics rose during the NEP*[3] *era to a high level, to a status of glory – only to be* […] *plunged into slavery*.

The happy period ended in 1928 or even 1927 (§ 3.1). And the years 1919 – 1922 were still very difficult, to say nothing about 1917 – 1918. Even the most important data were missing and

33 von der <u>mobilisierten</u> Statistikern, welche die [Berufs]*zählung durchzuführen hatten, ihr Leben an der <u>statistischen Front</u> lassen mussten* (Chuprov 1922, p. 358, issuing from the VS).

There also we read:

Die statistische Zentralbehörde steht oft vor geradezu unüberwindlichen Schwierigkeiten angesichts der fortschreitenden Desorganisation des Landes.

In 1926, the well-known Kondratiev asked Chuprov to return to Russia and fill a post at his Conjunction Institute in Moscow. It was Chetverikov, Kondratiev's assistant who conveyed that invitation to his former teacher. He added that the Kondratiev's researchers were working *with all their hearts* and that their *conscience is not violated even to the slightest degree* (Sheynin 1990/2011, pp. 38 – 39). Chuprov, however had serious reservations and, anyway, he was extremely ill (and died soon afterwards). Had he returned, he would have likely been at least exiled. Chetverikov himself, although he did not foresee the horrible future, warned Chuprov that the situation was not clear.

3. The two camps

3.1. The saboteurs. No, the situation was not clear! Kondratiev was soon elbowed out of science, sentenced (in 1931) to imprisonment, then shot (1938) for being too slow to die by himself (Makasheva 1988). The Year of the Great Change (1929) was approaching with its uprooting of millions of peasants and gloom for the entire nation. Quite appropriately, at about the same time ever more statistical data became classified and scientific criticism was largely replaced by dangerous political accusations (Bukhanov 1988, p. 53).

Also by the end of the 1920s a new crop of statisticians had gradually emerged, some of them vaguely familiar with their science, but all and sundry hell-bent on toeing the Party line and rooting out the *saboteurs*. Maria Falkner-Smit or Smit was a worthy representative of these troglodytes both as an ignoramus (§ 3.5) and as a ferocious persecutor of *bourgeois statistics*, see also [v, §6].

She (1931, p. 4) clumsily stated that *the crowds of arrested* [probably on her initiative] *saboteurs are full of statisticians*. At about the same time she chaired a conference whose proceedings were published under a expressive title *Planned sabotage and the statistical theory* (Smit 1930b). She began her opening speech by remarking that Marxist statistics was only in the making and concluded that old specialists were therefore able to carry out their subversive activities. And she (p. 168) closed the conference by declaring that Marxist statisticians should help the OGPU in exposing the saboteurs[4].

Many other authors expressed similar views (Brand 1931, p. 235)[5]; I quote now the preface to the first Soviet statistical textbook (Boiarsky et al 1930) as translated by Anderson (1959, p. 294) from its second edition of !931:

Die historische Rolle der Mathematik als Dienerin der bourgeoisen Wissenschaft verursacht unvermeidlich ein gewisses Misstrauen der Vertreter der marxistischen Wissenschaft gegen mathematische Methoden. […] *Nach erscheinen der 1. Auflage unseres Buches trafen Ereignisse ein, welche endgültig und augenfällig die Wurzeln jenes Klassenkampfes bloßlegten, der in der mathematischen Statistik als einem Teil der allgemeinen ideologischen front geführt wird.* […] *Die Ideologen der bürgerlichen volkswirtschaftlichen und statistischen Theorie in Russland* [*d. h. in erster Linie die Mitarbeiter des Moskauer Konjunkturinstituts Kondratjeff Tschajanoff, Basaroff und Rubin*] *offenbarten ihr wahres Gesicht, das Gesicht von Lakaien der vaterländischen und internationalen Bourgeoisie, das Gesicht von Spionen des französischen Generalstabs, das Gesicht von Interventen und Schädlingen …*[6]

3.2. The ideologists of the bourgeoisie. Apparently expressing the official viewpoint, Brand (1931, p. 234) declared that the saboteurs had followed the line of such *typical ideologists of the bourgeoisie* as Süssmilch and Quetelet. Later statisticians, as he added, attempted to substantiate the invariability of the existing capitalist relations. He also approvingly noted that Boiarsky et al (1930) had resolutely criticised Cournot and Mises.

These authors (pp. 4 – 5) had also indirectly rejected Pearson as a Machian and [Irwing] Fisher and von Bortkiewicz as bourgeois economists. They also discarded the theory of stability of statistical series and declared that the bourgeois economists, being *absolutely helpless*, had to apply mathematical methods.

Starovsky (1933, p. 280) followed suit: he alleged that the theoreticians of the bourgeois statistical science (Süssmilch, Quetelet, Lexis, von Bortkiewicz, Pearson […], Chuprov) had attempted to prove the *invariability* and the *eternity* of the capitalist system and the *stability* of its laws[7]. Smit (1934, p. 217) echoed his opinion almost in the same words. Even much later Starovsky (1960, p. 15) mentioned t*he antiscientific essence of the* <u>*theories*</u> *of Lexis and Pearson*. And Smit (1930a, p. 48) singled out Cournot and von Mises calling them, in connection with their economic views, *malicious Austrians*.

Quetelet maintained that the stability (e..g., of crime) took place only un der invariable conditions (Sheynin 1990/2011, Note 14.1 on pp. 171 – 172). Later statisticians beginning with Lexis and *including Markov* (though not Pearson), had indeed studied the stability of statistical series, but in a purely mathematical way. The only contemporaneous Soviet author who stated that the theories of the bourgeois scientists had nevertheless contained something useful was Lozovoy (1938, p. 118), a hunter for saboteurs!

3.3. A sabotaged census. After 1917, the first nation-wide censuses were carried out in 1920, 1926 and 1937. The last-mentioned, however, was proclaimed unsatisfactory and a new one was conducted instead in 1939. Vobly et al (1940, pp. 128 – 130), in describing the census of 1937, maintained that the enemies of the people had deranged it. A considerable number of people were left out, as they alleged, since the explanatory note for the registrars was hardly understandable and faulty, and the vocational structure of the population was distorted since such professions as prostitute (cf. however § 1.3!) and tramp were recognized.

The real story is quite different (Zaplin 1989; Volkov 1990). The census was planned for 1933 but repeatedly postponed: instead of a rapid increase in population, appropriate for a nation approaching socialism, there occurred a demographic catastrophe occasioned by arbitrary rule, uprooting of millions of people, mass hunger and savage witch-hunts. In mid-1936, in a lame attempt to save face, abortions were cruelly prohibited (which was too late).

The programme and the method of conducting the census were worked out at the highest level with Stalin himself making the most important decisions. Statisticians had little time for instructing the registrars, but they did their best: only 0.3 – 0.4% of the population was missing in the census, but only 162 million people were counted instead of the 170 – 172 *demanded*. This certainly was the doings of the saboteurs. Accordingly, the state statistical service was decimated and one of the victims was Brand (mentioned in §§ 3.1 and 3.2), referring to him and to several other statisticians, although without mentioning censuses, Lozovoy (1938, p. 117) plainly called them enemies of the people[8].

In 1930, a Demographic Institute was established under the Academy of Sciences[9] only to be abolished in 1934 (Tipolt 1972). In justifying its decision dissolve the Institute, the Academy's Presidium noted that

The attempts to introduce social-economic elements [to introduce conformity and Marxist ideology] *into the work of the Institute failed*.

Eminent demographers (S. A. Novoselsky, V. V. Paevsky) worked there. I. M. Vinogradov, and at least later a zoological anti-Semite, was the Director. He had not contributed anything to demography.

3.4. The new wave. WWII began and ended, but oppression had not ceased. In 1948, genetics was brutally uprooted, and a vicious campaign against cosmopolitism had started. Both these savage events had to be appropriately mentioned in the Resolution (1948) of a conference on mathematical statistics attended by such figures as Kolmogorov.

In 1948, the victimization of the bourgeois ideology, and for good measure of the Anglo-American statistical school had resumed in the context of the notorious campaign against cosmopolitanism. And so (p. 313), the conference resolutely condemned V. S. Nemchinov who had *attempted to justify reactionary genetics by statistical means* and spoke

From the position of the Machian Anglo-American school which appropriates for statistics the unnatural role of an arbiter situated above other sciences

[apparently: above the Marxist ideology, see § 7].

Then (p. 314), the Resolution denounced *servility and kow-towing to outlandish ideas*, worryingly noted that the *methods of bourgeois statistics were sometimes popularized and applied*, and put on record that V. I. Romanovsky, the well-known mathematician and statistician, had acknowledged his earlier ideological mistakes.

3.5. Political economy versus mathematics. Bearing in mind the previous material, it is not difficult to imagine that Soviet statisticians down-graded mathematics. Brand (1931, p. 235) stated that mathematical statistics was not a separate entity, but the more difficult chapters of the theory of statistics. More important, he (p. 234) declared that Lenin's works rather than the bizarre *achievements* of the bourgeois statisticians constituted the basis of the Marxist-Leninist statistics.

Actually, he expressed the idea that can be traced in many other contributions published from then onward well into the 1970s with relapses occurring until recently (Nazarov 1990, p. 36).

Boiarsky et al (1930, p. 4) bluntly declared that

The role of statistics is reduced to the measurement of the regularities revealed by the specific analysis of the pertinent discipline [of Marxism].

Starovsky (1933, p. 280) and Eidelman (1982), who discussed economic statistics, can also be cited. Here is one more familiar author, Smit (1934, pp. 218, 220 and 222): mathematical statistics cannot be the basis of economic statistics because a preliminary qualitative analysis is needed [which is the business of theoretical statistics]; the mass processes of social life cannot be described by the classical theory of probability based on equipossibility[10]. Indeed, where is equipossibility in a planned economy? Her final statement (pp. 227 – 228), also mentioned in [v, § 6], was a prime example of vulgarity:

Pearson is a Machian and his curves are based *On a fetishism of numbers, their classification is only mathematical. Although he does not want to subdue the real world as ferociously as Gaus* [!], *his system nevertheless rests only on a mathematical foundation, and the real world cannot be studied on this basis at all*.

Even Khotimsky (Smit 1930b, p. 145) declared that the bourgeois science makes use of mathematics *for the sake of propriety*. He added, however (p. 149), that it was *impossible to tolerate anymore the unbelievable mathematical illiteracy of our economists and social scientists*.

Strumilin (1969), the Editor of that source, left yet another example of the same kind: statistics is a class discipline (p. 11); it rests on political economy (p. 13), and the connection between these two disciplines becomes ever tightest (p. 16).

Strumilin himself (1952, p. 42) was against squeezing statistics into *the narrowest confines of political economy* and argued that it was not identical with, and should not be subordinated to it (1954/1958, pp. 118 and 120). Nemchinov (1952, pp. 105 – 106) held similar views.

Far back, in 1926, Osinsky (the future enemy of the people, see § 4 and Note 14) declared in the newspaper *Pravda* that statistics should not be subordinated to politics or simply justify the desired. My source is Bukhanov (1988, p. 54) who had not supplied an exact reference. Osinsky was of course forgotten, and even Strumilin or Nemchinov's opinion, in spite of their high scientific standing, was hardy ever repeated (§ 5.1). It is therefore all the more interesting that at least some Russian statisticians in the first half of the 19th century (T. F. Stepanov in 1831, K. S. Veselovsky in 1847) believed that statistics and political economy enjoyed equal rights, whereas I. I. Sreznevsky in 1839 declared that the former can manage without the latter (Ploshko 1964, pp. 29 – 31).

A mathematical or statistical study of an economic problem will not perhaps satisfy the economists and the reasonable thing to do is to change (some) assumptions, or take into account new social conditions and try once more. Statisticians, however, have another possibility: they may declare that the specialist in charge is an enemy of the people[11]. Two examples are in order.

Boiarsky (Smit 1930b, p. 160) criticized the conclusion made by Bazarov, an exposed enemy of the people, and naturally offered no reference[12]. Bazarov worked out a differential equation which described the gross national output. Its particular solution asymptotically tended to a *horizontal* line so that the rate of economic growth decreased with time (a terrible crime!). Boiarsky's description was however faulty: the graph of the solution did not tally with the equation. Furthermore, he (p. 163) stated that a straight line belonged to lines with decreasing rates of growth, which is difficult to understand. Nevertheless, his train of thought is clear and he did not forget to maintain that the enemies of the people were scientifically preparing an armed intervention from abroad (pp. 158 and 159).

Due to the Stakhanovite movement[13], which officially resulted in mass higher productivity, the Party resolved that statistically substantiated quotas in industry were hindering progress[14].

Lozovoy (1938, p. 120) appropriately concluded that the concept of mean output per person per shift was meaningless. In practical terms this or similar criticisms (Petrov 1940, p. 113) meant that changes of assumptions (see above) should have been made prior to experience …

3.6. We are the best ones. Some sober criticism was also offered. Thus, Khotimsky (Smit 1930b, pp. 145 – 146) indicated that American economists and statisticians had failed to predict the crisis which began in 1929. He properly argued that any purely empirical predictions were worthless, hat extrapolation of time series without a study of the underlying economic processes was meaningless. What he did not say was that neither had the Soviet specialists equipped with the true Marxist theory been able to predict that crisis. Only the future enemy of the people, Kondratiev (§ 3.1), manged to offer, in 1923, a partly correct picture of the imminent events (Belianova et al 1988). In about two decades extrapolation of time series was replaced by studies of econometric models, but not initially in the Soviet Union (§ 6)[15].

Much later Boiarsky et al (1947) accused the bourgeois statistics of deliberately diminishing the rates of unemployment, of failing to stress the difference between the longevities of the lives of white and black Americans and noted that the officially tolerated commercial secrecy impeded the compilation of industrial statistics. A really scientific statistics, as the authors (p. 75) solemnly concluded contrary to the real situation, can only be created under the conditions of the Soviet system[16]. Ten years later Boiarsky (1957) published a non-political textbook but concluded it by a wild attack against Western statistics. For example, he mentioned the pernicious *consequences of the Pearsonian Machian school*.

Gurevich (1938, p. 71) was no less resolute: he declared that Soviet statistics was *the most advanced in the world*. The unsinkable Boiarsky (1953, p. 43) somewhat carefully maintained that statistics *only achieves real flourishing under Socialism*. Similar pronouncements were due to Riabushkin (Anonymous 1954, p. 69) and Strumilin (1969, p. 11). Taken together, they remind readers of the Bandar-log pack of monkeys from Kipling’s *Jungle Book*:

We are great. We are free. We are wonderful. We are the most wonderful people in all the Jungle! We all say so, and so it must be true!

Here is Anderson’s sober assessment (1959, p. 297) of a textbook published by that same Boiarsky in 1957:

Seine […] *Leser nicht imstande sein werden, sich in den Werken der modernen mathematischen Nationalökonomie und Ökonometrie zurechtzufinden*.

Some accusations against Western statisticians were nevertheless true. Andreski (1972) had much to say about social science in general (he barely mentioned statistics) *where anybody can get away with anything* (p. 16). He explained the situation by *an endemic bureaucratic disease* leading to *safe mediocrity* (p. 194).

Truesdell (1981/1984, pp. 115 – 117), discussed the same subject, agreed with another author (E, Chargaff) ad noted: *wherever money is abundant, charlatans are brought forth by spontaneous generation*. He introduced the term *plebiscience,*

The science by, for and of the demos; […] *like everything dear to the plebs* [plebiscience] *is dear for the taxpayer*. […] *Plebiscience is an intermediate stage. The next and last is prolescience.* […] *Its function will be to confirm and comfort the proletariat in all that will by then have been ordered to believe. Of course, that will be manly social science*.

Truesdell did not mention Soviet statistics. By the end of the 1920s it had become prolescience.

4. Accounting

Describing 15th century Italy, Kendall (1960) remarked that

Counting was by complete enumeration and still tended to be a record of a situation rather than a basis for anticipation or prediction in an expanding economy. However, by the end of the 1920s, Soviet statisticians, living in an expanding economy, began to believe that statistical theory was developing into national accounting (Brand 1931, p. 236)[17]. One of the first, not yet direct pronouncements to this effect was due to Smit (1930b, p. 143):

The old theory of statistics is […] *a theory of the oscillations of random variables*. […] *It perfectly reflects the chaotic capitalism and its economy*[18]. *Statisticians, deliberately sabotaging accounting, made use of this tool*.

The main partisan of accounting was however Osinsky[19]. He (1932, pp. 6 – 7) maintained that

Market spontaneity is ousted by the activity of the organizations which are directly subordinated to planned management. […] *The statistical method begins to retreat in the fact of the method of direct accounting*.

He went on to say, although without any explanation that statistics still had a certain role to play.

Elsewhere, also in 1932, Osinsky (Strumilin 1935/1958, pp. 81 – 82) defined accounting in a national economy as *a qualitative-quantitative study of consciously contemplated* […] *actions and their results* and repeated his statement about the retreatment of statistics. Strumilin, and more definitely Bukhanov (1988) remarked that, however, Osinsky’s practical work did not confirm his proposition.

For a few years Osinsky’s statement remained in vogue. Starovsky (1933, p. 282) even maintained that

The method of quantitatively studying the processes going on in the national economy is accounting.

Then, however, everything changed. Lozovoy (1938, p. 118) mentioned *the wrecking proposition about the atrophy of statistics*[20], and the same Starovsky (1960, 16) declared that it was an ultra-leftist theory first put forward by Academician (!) Osinsky. He (1958, pp. 9 – 10) also recalled that [in spite of the official standpoint voiced earlier by Lozovoy] the CSD, merged with the Directorate of Accounting, was not re-established until 1948. Starovsky did not add that the most influential statistical periodical, *Vestnik Statistiki*, did not appear during 1930 – 1948. A meagre number of statistical papers (mostly inspired by Smit or her likes) was then published in *Planovoie Khoziastvo*.

In any case accounting was unable to provide reliable figures. Already in 1926 Dzerzhinsky (the head of the dreaded OGPU and of the Supreme Council of the National Economy), see Seliunin et al (1987, p. 188), concluded that the figures were *absurd*[21]. The reason was simple, explain those authors: upward distortion of information about output and other *positive* indicators became widespread, and, owing to manipulation of cost, financial reports did not at all characterize output in physical units.

It is therefore difficult to justify the use of statistics (much less, of accounting) under conditions of controlled processes. Boiarsky (1974, p. 195) claimed that the law of large numbers operated also under conditions of controlled processes. Thus, he added (p. 196), various degrees of exceeding the plan were possible. A repulsive statement! Strumilin (Anonymous 1948, p. 80) expressed a more cautious pipe-dream: the deviations from the plan were random. And hardly anything was published about sample accounting.

5. Sealing a Marxist definition

5.1. The conference. In 1954, a nation-wide statistical conference took place in Moscow[22]. It was organized by the Academy of Sciences, the Ministry for Higher Education and the CND, and an account of its proceedings was published (Anonymous 1954); see also Kotz (1965).

Some pronouncements made by its participants were quite reasonable. **M. V. Ptukha** (p. 44) argued that statistics should be on a par with other sciences, and **Strumilin** (p. 41) claimed that it was an independent science. **Starovsky** (p. 50) advocated the benefits of sampling. In Chuprov's fatherland it was all but forgotten.

The prevailing statements were, however, grotesque. Thus (**A. M. Vostrikova**, p. 41), *Only the revolutionary Marxist theory* is the basis for developing statistics as a social science. **V. A. Sobol** (p. 61); Statistics does not study mass random phenomena. **S. P. Partigul** (p. 74): Such phenomena do not possess any special features. **Smit**, alive and kicking (p. 46): The theory of stability of statistical series is a bourgeois theory and even its *honest representatives* are compelled to violate their professional duty.

K. V. Ostrovitianov (p. 62), the vice-president of the Academy of Sciences, also made a bizarre statement: *Lenin had completely subordinated* [adapted] *the statistical methods of research* […] *to the problem of class analysis of the rural population*.

He also ignorantly warned his listeners that it was impossible to maintain that *the same methods of research* were used in economics and stellar statistics[23]. In other words: Soviet statistics should reveal and quantify Marxist laws and regularities (cf. § 3.5) and it is hardly amiss to add that, according to Süssmilch and his contemporaries, statistics should reveal the Divine laws of population.

Scientifically speaking, the Conference had hardly achieved anything[24]. It was important mainly as a tool for demanding statisticians to toe the official line, and many authors from 1954 onward have referred to it[25]. Thus, Starovsky (1958, p. 13) alleged that the Conference had *cleared up* many theoretical problems and that (1969, p. 9) *harmonious work* [stagnation!] had followed it. Strumilin (1969, p. 9) quoted the adopted definition of statistics and noted that it was [still] generally adhered to. Here it is (Anonymous 1954, p. 87):

Statistics is an independent [not really in the Soviet Union] *social science. It studies the quantitative aspect of mass social phenomena in an indissoluble connection with their qualitative aspect*.

However, Orlov (1990, p. 69) saw fit to *reject the decisions* of the Conference. First and foremost he thus rejected the definition just quoted[26].

Lifshitz (1967, p. 20) had more to say. In 1950 – 1952, a number of authors (cf. Note 17), as he correctly stated, had maintained that statistics should only quantify the regularities described by the Marxist political economy (cf. § 3.5 and Ostrovitianov's threatening warning above). Therefore, they concluded, statistics did not need either mathematical methods or sampling. Objectively, they desired to *do away with the statistical science*. Lifshitz also noted that the decisions of the Conference were a compromise between such *abolitionists* and progressive statisticians (Nemchinov in the first place).

5.2. Kolmogorov's report. It (Anonymous 1954, pp. 46 – 47; Anonymous 1955, pp. 156 – 158) deserves to be described separately. He began by declaring that it was necessary

To reject sharply […] *the abuse of mathematics in studies of social phenomena, so characteristic of the bourgeois science*. Its representatives *apply without any foundation hypotheses of stationarity and stability of time series*

(cf. 3 3.6). Then Kolmogorov opposed

The wrong belief in the existence, in addition to mathematical statistics and statistics as a social-economic science, of something like yet another non-mathematical although universal general theory of statistics, which essentially comes to mathematical statistics and some technical methods of collecting and treating statistical data[27].

Kolmogorov went on to list important fields of application of the laws of large numbers under the Socialist system (telephone networks, life insurance) but did not mention demography. This subject was tricky since the bloody Stalinist regime decimated the entire population (§ 3.3).

Kolmogorov thus stressed the importance of mass random phenomena which many statisticians (§ 3.5) hardly recognized.

6. Econometrics

Nemchinov made an early attempt to introduce econometrics in the Soviet Union. The commentator (Anonymous 1948, p. 82) reported that he endeavoured *to transfer the arch-bourgeois mathematical school of economics to the Russian soil*.

In 1959, a conference on econometrics was held in Moscow (Anonymous 1959) to determine an attitude towards this new direction in economics. The participants unanimously agreed that in economics, mathematical methods should be wider applied. However, the main reporter, Boiarsky, as well as several others present, contended that econometrics cannot be recognized as a separate discipline. He (pp. 55 – 57 and 69 – 70) argued that its subject-matter still pertained to political economy which *does not restrict its research by purely qualitative reasoning*. He then remarked that Marxism will not change qualitatively, but neither he, nor any other Soviet scientist ever admitted that it was a rigid doctrine[28].

A. Kh. Karapetian (p. 61), apparently expressing the general belief, stated that Western economists were basing their research on *vulgar* [evidently: non-Marxian] *economics* and Ya. A. Kronrod accused Kantorovich, the originator of linear programming, of deviating from Marxism. The solution of problems belonging to the entire national economy, should be based on this teaching rather than on [alien] criteria.

Kantorovich (1959a) based his economic recommendations on his *objectively determined evaluations*, and stressed, as Nemchinov (1959, p. 8) remarked, their closeness to market prices.

Another such conference attended by scientists of the highest calibre was held in 1960 (Gerchuk & Minz (1961). Nemchinov. Kantorovich and V. V. Novozhilov reported. Nemchinov spoke about the determination of the minimal investment needed to achieve a specified aim. Kantorovich argued that new methods of planning, new economic and statistical indicators, and research in economics, statistics and mathematics were required.

Kolmogorov (p. 254) participated ín the discussion and even stated that

The joint work of economists and mathematicians should lead to an appreciable and essentially new stage in the development of the economic theory itself [of political economy!]. *Economics will apparently have to specify many of its formulations and concepts in the light of those demands which the application of mathematics will raise before it*.

In describing the same conference, Birman (1960, p. 44) additionally quoted Kolmogorov as saying that

The main difficult but necessary aim is to express the desired optimal state of affairs in the national economy by a single indicator.

Read: he rejected the Marxist theory of value.

The transactions of the conference (reports in full, discussion shortened) were published (*Obshchie Voprosy* …, 1961) and some movement followed. In 1963, the Central Mathematical-Economic Institute was established, and in 1965 the three reporters were jointly awarded the Lenin National Prize (Nemchinov, posthumously) for the S*cientific development of the method of linear programming and of economic models*[29].

I doubt, however, that essential progress was achieved. First, political decisions always overrode economic arguments. Second, trustworthy data were hardly available. And, third, Soviet economists had scarcely changed their attitude towards mathematical reasoning. The early pathological fear of mathematics (§ 3.5) and accusations of deviating from Marxism (Kronrod, see above) persisted, see Kantorovich (1959b, 1952/1990) and Campbell (1961). In my context, the most important point was that economists, who actually refused statistics the status of an independent scientific discipline, remained helpless in the face of new possibilities and requirements. Thus, Kantorovich (1959b), as quoted in translation by Campbell (p. 415):

In the 42nd year of the existence of the socialist state, our economic science does not know precisely what the law of value means in a socialist society or how it should be applied. It does not know what socialist rent is or whether in general there ought to be some calculations of the effectiveness of capital investment. […] *We are offered as the latest discovery in the field of economics, for example, the proposition that the law of value does not govern but only influences*.

Much the same is found in Kantorovich (1952/1990), but there, in addition, he stresses the subjective aspect of the situation: those on top of official economics were hell-bent on preserving the status quo.

Here, finally, is my only heroine, Maria Smit (1961, p. 294):

Utterly impotent, the adepts [why not stooges?] *of the bourgeois political economy are facing the dreadful, for them, reality. On the contrary, the power and vitality of the Marx and Lenin's economic teaching truly consists in the deepest penetration into the essence of the laws governing the economic development of the human society*.

Just where did she find even a trace of Lenin's economic teaching?

7. Genetics

By 1935, the Soviet Union became

A leading centre of Mendelian research and was so recognized by the whole world (Anonymous 1951, p. 5).

In 1939, and especially […] *after 1948 the development of Soviet genetics decelerated* (Beliaev 1975, p. 180).

This statement was a shameless smoke-screen. Vavilov, apparently in the beginning of the 1930s, unwisely promised to transform national agriculture. He was the most prominent Soviet geneticist, but his optimism proved premature, and from 1935 he came under fierce attacks. Genetics was called an idealistic science contrary to dialectical materialism (Adams 1981). In 1940 Vavilov was arrested and died in prison in 1943 (Ibidem, p. 511).

In 1939, a conference on genetics and selection took place in the editorial office of the periodical *Pod znamenem Marxisma* (Under the Banner of Marxism), see its NNo. 10 and 11 of that year. Vavilov was politely but severely criticized there, but the show-down occurred in 1948 at another conference attended by the highest-ranking specialists in the field, mostly anti-geneticists[30]. The chief and the most resolute opponent of genetics was Lysenko.

Kolman (1982, p. 213) seems to have understood him properly:

At first, he sincerely believed in his views. After gaining power, he turned to forcible methods of struggling with the opponents of his claims.

Here is Lysenko's statement (1948, p. 520):

Being unable to reveal the regularities in animate nature, they [the geneticists] *have resorted to the theory of probability* […]. *Physics and chemistry have rid themselves of the accidental. Therefore, they have become exact sciences.* […] *Science is the enemy of the accidental*[31].

Lysenko knew about probability or its role in physics not more than a newborn babe. Huxley (1949, p. 83) testified that

Lysenko and his followers refuse to utilize the statistical methods.

They reasonably believed (p. 82) that the leaders of the USSR felt that

There is no place either for chance or for indeterminacy in Marxist ideology in general, or […] *in science conceived of by dialectical materialism*.

For a detailed review of the battering of Soviet geneticists carried out with the Party's approval (and perhaps by its implicit command) see Cook (1949) with Leikind (1949) who provided the pertinent bibliography. Fisher (1948/1974, p. 61) should not be forgotten either:

Under the impulsion of his [Lysenko's] *attacks many Russian geneticists, and those among the most distinguished, have been put to death* [apparently in 1940 – 1941]. And on p. 64: *The reward he* [Lysenko, *the Grand Inquisitor*] *is so eagerly grasping is Power, power for himself, power to threaten, power to kill*[32].

The Grand Inquisitor (or rather the Damned One) was Stalin himself. For him, even Lysenko was only a pawn, and the campaign against geneticists was only one in a series of operations designed to win the cold war (Note 23) and prepare the nation for a Great War with the West. Cf. Chuprov (Sheynin 1990/2011, p. 35):

During all his stormy life, Lenin strove for power for power's sake without thinking about Russia or about the Russian proletariat. […] *He was indifferent to the fate of the people*.

Lysenko's irresponsible attitude towards probability theory led to a campaign against it waged by *hotheads* (Gnedenko 1950b, pp. 7 – 8). Gnedenko also mildly criticized Kolmogorov and several other leading Soviet mathematicians for their support of Mendelism and politely interpreted Lysenko's notorious opinion about science being an enemy of the accidental. He did not dare to point out Lysenko's glaring blunder about physics. See also Note 24.

Nemchinov attended the 1948 conference and delivered report. It was interrupted by numerous rude voices from the floor but he was able to say (*O polozhenii* 1948, p. 472) that

The chromosome theory of heredity has become a part of the gold fund of human knowledge. […] *I am in a position to verify this theory from the point of view of* […] *statistics. And it also conforms to my ideas*.

After the conference Nemchinov had to abandon his post of Director of the Timiriazev Agricultural Academy, and, six months later, to leave his chair of statistics there (Lifshitz 1967, p. 19). Moreover, he (1952, p. 104) had to confess publicly his guilt, see also Davies & Barker (1965).

One of the most eminent Soviet mathematicians who essentially contributed to genetics was Bernstein. Aleksandrov et al (1969, pp.

213 – 214) described his pertinent achievements and made known that in 1949 or 1950 a (certainly state-owned) publishing house had abandoned its intention to bring out a new edition of his course in probability theory since Bernstein *categorically refused* to suppress a few pages of its former edition dealing with Mendelism. More: L. N. Bolshev, the late corresponding member of the Academy of Sciences, told me that the proofs of the doomed edition had been already printed and sent to Bernstein. And on a like occasion Suslov, the Grey Eminence of the Kremlin, much later declared that money should not prevent ideology.

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Notes

1. It is instructive to recall Engels (1891/1979):

Kommen wir dagegen durch ein Krieg vorzeitig ans Ruder, so sind die Techniker unsre prinzipiellen Gegner, betrügen und verraten uns, wo sie können. Wir müssen den Schrecken gegen sie anwenden und werden doch beschissen.

In Soviet statistics, it was the state that deceived and betrayed the population (and itself!), cf. § 1.3 and Note 8.

2. Fresh thoughts have been appearing from 1989 onward, see for example the discussion entitled *Statistics and perestroika* in EMM, vol. 25, No. 5, 1989, pp. 900 – 931.

3. The New Economic Policy (1921 – ca. 1936). On the period 1921 – 1929 see Gozulov (1957, p. 132).

4. B. S. Iastremsky (Smit 1930b, p. 153) was another zealot of the OGPU. He criticized the already arrested V. G. Groman (Krylenko 1931), *einer der besten Semstvo Statisiker* (Anderson 1959, p. 294) and previous head of the State Planning Committee. Groman attempted to predict the relative yield of cereals, which he assumed random, given its previous values. For 1929 his prediction somehow came true, but it failed in 1930. Curiously enough, Iastremsky only remarked that Groman's reasoning was applicable to any random variable but noted that the OGPU had proved that Groman's mistakes were *not only methodological*. Proved, certainly by lawful methods …

5. Sabotage was of course revealed in every branch of science (Kolman 1931).

It is opportune to add that thee categories of people became repressed first of all. First, scapegoats for Stalin's own mistakes or shortages. Second, victims of sudden changes of state policy. As a prime example, I mention the notorious Molotov – Ribbentrop pact. Those who actively supported the previous attitude towards Nazi Germany became inconvenient and had to be suppressed (often shot). Third, victims of Stalin's pathologically suspicious mind.

6. The edition of 1936 (p. 27) mentioned the metaphysicist Leibniz-Wollf! S. A. Yanovskaia, the future renown specialist in mathematical logic, unrestrainedly praised that book: these authors were the first to discover how to insert dialectical materialism into statistics. To achieve this aim in a planned economy, as she added, the theory of probability (singular instead of the proper Russian plural) was insufficient. She really believed such nonsense.

7. No one seems to have noticed (or did not intentionally notice) the changes in the capitalist society since the 1870s when Lexis studied the stability of statistical series.

8. The census of 1939 obediently showed a population of 170.1 *mln*, 1.6% higher than the real figure (Volkov 1990).

9. In 1915 or 1916 Chuprov (Sheynin 1990/2011, pp. 130 – 131) argued that in *good time* the Academy should set up an institute for the statistical study of Russia.

10. Brand & Starovsky (1935, p. 191) lamely justified the use of equipossibility (and therefore the classical definition of probability) in economics. They did not mention statistical probability (which can be used almost on a par with theoretical probability). They rather referred to unconvincing indirect statements by Marx, Engels and Lenin (not yet by Stalin which was extremely unwise) who had discussed the compensation of individual deviations and thus made indirect use of equipossibility. Without mentioning them Lozovoy (1938, p. 118) repeated the attack against equipossibility.

11. The *enemies* were guilty only in that they did not consider assumptions leading to the impoverishment of the population.

12. All books published by such *enemies* and the issues of periodicals containing their papers were either destroyed or transferred to special libraries.

13. Stakhanov was a coal-miner, a cutter. In 1935, together with two timbermen, he extracted 102 tons of coal during a single shift (14 times the quota for a lone cutter), and later, eve 227 tons (Anonymous 1947). It is quite possible that special conditions (disrupting even the usual course of work at the mine) wee arranged for Stakhanov. He hardly had to wait for the timber, or for the coal to be carted away. No one asked what happened to the extra coal since the customers and their needs were stipulated from above. Apparently they also should have greatly increased their work. Lift yourself up by the hair! All over the nation happy workers from every branch of economy followed suit and thus the Stakhanovite movement had emerged. However, the state balance sheets did not change. During the short-lived period of de-Stalinization a Soviet newspaper called that movement a *Stalinist propaganda manoeuvre* (English Wikipedia, entry *Stakhanovite movement*).

14. Plenary Session, Central Committee, Communist party. Resolution. Newspaper *Izvestia*, 26 Dec. 1935, p. 1.

15. Grigory Feldman (1884 – 1958) should also be mentioned. He came near to the ideas of this discipline but then he apparently did time during 1937 – 1943 and only in 1953 was able to return to Moscow (Weinstein & al (1968). And Slutsky had to abandon economics.

16. Witness their loathsome statement (p. 74): Pearson was the author of *some ideas of a racist nature which for five decades forestalled the Göbbels department*.

17. Each factory etc. had fixed suppliers and buyers, fixed prices for paying the former and for receiving payment from the latter and more or less fixed wages of its workers, fixed almost everything.

18. In a few years, forgetting oscillations she denounced the bourgeois theory of stability (§ 3.2).

19. Real name, Obolensky. He held a number of most important posts; in 1926 – 1928 he headed the CSD. A full member of the Academy of Sciences from 1935. He was arrested in 1937 and died in 1938, see Bukhanov (1988). Seliunin et al (1987, p. 190) state, however, that Osinsky was arrested in 1935 and Kornev (1993) reports that he was shot.

20. At the same time, he (p. 116) defined statistics as a science that included *qualitative-quantitative* studies of *planned and controlled processes*. This is difficult to understand.

21. I have seen another edition of Dzerzhinsky's *Sel. Works* and did not find this statement. Anyway, he mercilessly criticized official industrial statistics elsewhere (1926).

22. Quite a few papers were published in 1951 – 1953 in VS and *Voprosy Ekonomiki* as forerunners to the conference.

23. Ostrovitianov attacked reasonable pronouncements made at the conference by Iastremsky (p. 43) and especially A. S. Mendelson (p. 57). He could have also cited Kolmogorov (p. 47).

24. Incidentally, Chuprov was not mentioned at the conference.

25. Some authors (Ostrovitianov 1954; Strumilin 1954) had published their reports in full; the former somewhat scaled down his ideological attacks, but did not in essence budge. See also excerpts from the reports dealing with the law of large numbers in Anonymous (1955).

26. In 1976 Riabushkin (1980a) repeated the definition of 1954 in an encyclopaedic entry but then (1980b) modified his viewpoint. He put down the same formula but all at once added that there also existed another definition which lacked the *indissoluble connection* and acknowledged the closeness of statistics and mathematics.

27. Kolmogorov was mistaken. Collection of data and their preliminary study (yes, mostly by *technical means*) is a very important stage of work of theoretical (yes, *general*) statistics.

28. In 1910 – 1911 von Bortkiewicz undertook *a lonely effort to construct a Marxian econometry* (Gumbel 1978, p. 26), but *his dry presentation prevented the Marxists* (*except Klimpt*) *from accepting his method* (p. 25). Other and perhaps more successful attempts would have surely been made by Soviet scientists, but the top *Parteigenossen* were always mortally afraid of any ideological innovation.

29. Newspaper *Izvestia*, 22 Apr. 1965, p. 4. In 1975, Kantorovich was awarded the Nobel Prize for inventing linear programming.

30. The cold war began in 1947 and those on the Soviet top started doing their damnedest to quench all international contacts; genetics which manifested such contacts was thus doomed (Krementsov 1996, p. 40).

31. True, other authors, for example Kedrov (1961, § 2) denounced this opinion but Lysenko was hardly mentioned on such occasions. Kedrov (p. 31n) also approvingly quoted an editorial which had appeared in the influential *Voprosy Filosofii* (1948, No. 2). Randomness of single events was correctly linked there with necessity on a large scale and it seems that Lysenko, who delivered his notorious speech later in 1948, did not see (or heed) that statement. The dialectics of those two categories was known even to laymen in the 18th century:

Der Weise […] *sucht das vertraute Gesetz in den Zufalls grausenden Wunders, Sucht den ruhenden Pol in der Erscheinungen Flucht* (Schiller, Der *Spaziergang*).

32. In 1955, after doing time in a labour camp. Efroimson compiled a lengthy essay (1989) on Lysenko. He wrote it like an indictment and naively attempted to bring the perpetrator to trial. The entire establishment was too deeply involved (as shown by Efroimson) to do anything. Nevertheless, Lysenko was gradually elbowed out of power. Efroimson (No. 3, pp. 106 – 107) also estimated the damage done by introducing all over the country and without any statistical confirmation Lysenko's own quack proposals. Here is just one item from the author's conclusions.

The total losses of grain up to 1955 amounted to ca. 150 *mln* tons, and more losses will inevitably follow at least until 1965. Recall for comparison the estimated harvest in 1952 (§ 1.3). Khrushchev's corn madness comes to mind. As a sideline, I note Efroimson's remark (No. 3, p. 102n), regrettably offered without a precise reference, to the effect that Hitler had done away with genetics in Germany.

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BSE = *Bolshaia Sovetskaia Enziklopaedia* (*Great Sov. Enc.*, GSE)
EMM = *Ekonomika i Matematich. Metody*
JNÖS = this periodical
JRSS = *J. Roy. Stat. Soc*.
MEMO = *Mirovaia Ekonomika i Medzhdunarodnye Otnosheniya*
PKh = *Planovoe Khoziastvo*
SSR, SSSR = Soviet Socialist Republic, USSR
Uch. Zap. = *Uchenye Zapiski po Statistike* (irregular edition)
VIET = *Voprosy Istorii Estestvoznania i Tekhniki*
VS = *Vestnik Statistiki*; from Sept. 1995, *Voprosy Statistiki*

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